Three papers in Empirical Finance on Information Processing

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1 Introduction

1.1 Common theoretical background

Information efficiency has been a central theme in finance for decades. Although the Efficient Market Hypothesis in its original form as proposed by Fama (1970) has been challenged early on (Basu, 1977; Grossman and Stiglitz, 1980; Shiller, 1981), empirical evidence for weak or semi-strong forms of market efficiency is found, especially in developed and liquid markets (Schwert, 2003).

Despite the theoretical underpinnings of market efficiency, practical realities include that information is processed by individuals, which do not possess unlimited cognitive capacity. Instead, they often rely on heuristics or rules of thumb, as described by Kahneman and Tversky (1979) in their pioneering work on prospect theory. These cognitive limitations can lead to systematic biases and deviations from rational behavior, influencing how information is processed and subsequently reflected in market prices.

Adding to the limited capacity to process information, different market participants often have access to varying levels of information, leading to what is commonly referred to as information asymmetry. This concept is highly relevant in corporate finance, where transactions are often influenced by information asymmetries between different parties, such as management and investors. The seminal work by Akerlof (1970) suggests significant imbalances in information can lead to market failure. Building on this, later work e. g. by Myers and Majluf (1984) suggests that the less informed party may need incentives, often in the form of a reduction in price in their favor, to compensate for their informational disadvantage.

The papers in this dissertation aim to analyze different specific aspects of information processing by separate groups of actors in the face of information asymmetry within the domain of corporate finance and financial markets. Each paper focuses on a different subgroup of market participants (institutional investors, systematic investors, financial analysts) and a different corporate event (Seasoned Equity Offerings (SEOs), filings of insider trades, Mergers and Acquisitions (M&A) transactions), thereby covering a wide range of aspects. By bridging theoretical concepts with empirical analysis, this research aspires to contribute to the broader academic discourse as well as provide insights that carry significant practical implications.

In one way or another, all the papers in this dissertation are touching on the group of subjects mentioned above and are contributing to a better understanding of these phenomena. The first paper sheds light on the role of financial report age for institutional investors in accelerated SEOs. Although classical theories based on information asymmetry and demand elasticity predict that investors should demand higher discounts for SEOs where the last publication of financial figures has been longer ago, this is not what we find in our analysis. We focus specifically on accelerated SEOs in Germany, as this allows us to further isolate a potential effect that has not been investigated before. Nevertheless, we find no meaningful connection between the age of an issuers last financial report and discounts in their SEOs, indicating that institutional investors are sufficiently sophisticated to stay informed about the company regardless of report age.

The second paper takes the perspective of a systematic investor, i.e., an investor who bases their decisions on a predetermined set of (mostly quantitative) rules. In this context, we analyze whether a profitable trading strategy could be achieved by copying selected insider trading filings on the US stock market. Although there should be a near instantaneous price reaction, once the new information in the insider trading filing is released, prior research, as well as ours, finds persistent abnormal percentage returns days after the filing. Our analysis takes a new angle in that it focuses on US-dollar returns as well. This enables us to uncover that trading on those signals would be neither scalable nor profitable. As this also dampens the profitability of potential arbitrage trades, the persistent percentage returns can be explained.

The third and final paper shifts the focus away from investors to financial analysts. Previous research has shown that financial analyst estimates and recommendations are important for a wide range of market participants. Therefore, it should be important to know in which circumstances the accuracy of these estimates might be impeded. In our paper, we consequently analyze the impact of the announcement of larger M&A transactions on the accuracy of analyst estimates. Those events increase uncertainty for all involved parties, as well as the information asymmetry between analysts and management. Both of those factors should lead to a higher forecast error in analyst estimates, which is precisely what we find. This increase in forecast error appears to be the result of an increase in overoptimism among analysts. Furthermore, we are able to identify three moderating factors for the increase in forecast error: The effect is stronger for transactions with a larger relative deal size (compared to the market capitalization of the acquirer) and lower for acquirers with a larger following of analysts as well as more positive share price reactions to the M&A announcement.

In the following sections, I will briefly summarize the ideas and results of all three papers. While these summaries highlight key references, please refer to the respective papers for full relevant citations.

1.2 Summary - Does financial report age matter to institutional investors in accelerated SEOs?

The paper with the full title *"Does financial report age matter to institutional investors in accelerated SEOs? Evidence from SEO discounts in Germany"* was written in late 2022 together with my coauthor and doctoral supervisor Prof. Dr. André Betzer. The idea for this paper arose from a combination of my practical work experience in equity capital markets at Bankhaus Lampe KG and my (then) newfound academic pursuits. As a practitioner, I was particularly interested in the reasons behind the discounts of equity capital markets transactions, focusing on those factors that could be influenced by the issuer and thus reduce indirect transaction costs. For instance, the size or industry of a given company might influence the discount, but is not easily changeable by the issuer to reduce discounts and as such of little relevance to practitioners.

As this was my first academic paper, Prof. Dr. André Betzer assisted me in structuring my thoughts and ideas and developing a viable approach to answer our research question. Upon reviewing previous research, we identified numerous influencing factors, yet one aspect remained unexplored: The timing of the transaction with respect to the publication of the latest financials. This effect should be more pronounced in the absence of new information in the transaction context. A capital increase structured as a rights issue typically provides new information within the transaction through a prospectus, thereby mitigating any delay effects. Accelerated, overnight transactions lack such updates, making the timing of financial publications potentially more critical. For this reason, we focus on this transaction type in our analysis. Using data from 311 accelerated SEOs in Germany between 2007-2021, we employ multiple regression analyses to examine the influence of the age of financial reports on discounts in equity capital market transactions, while controlling for variables established by prior research.

We focus on German data for three main reasons: My prior practical expertise in this area, the relative lack of research for this market and, most importantly, peculiarities of this market compared to the US and UK. Those allow us to further isolate our analyzed effect: (1) The issue method (rights vs. no rights) is mostly chosen based on needed capital, not needed time for marketing, (2) due to legal contestation risk, virtually all SEOs without rights are done overnight, (3) bought deals are not as common as in the US and UK, offer prices are mostly the result of a bookbuilding process.

Based on established theories, the expected impact of this factor on discounts of accelerated SEOs in Germany is not clear-cut. Different theories based on information asymmetry (Myers and Majluf, 1984; Parsons and Raviv, 1985; Rock, 1986) and demand elasticity (Armitage et al., 2014; Huang and Zhang, 2011; Gao and Ritter, 2010; Corwin, 2003; Intintoli et al., 2014) suggest higher discounts with increasing time passed since the last publication of financials. However, the transactions we analyze are exclusively marketed to likely highly sophisticated institutional investors. Previous research suggests that this investor group is well informed regardless of the publication of financials and might be able to anticipate corporate events to a certain degree, even capital increases (Szewczyk et al., 1992). We are thus not able to predict the impact of financial report age on discounts, but assume it to be dependent on the degree of sophistication of the involved investors.

Our analysis reveals a statistically significant, but economically negligible linear relationship between the age of financial reports and discounts in the expected direction. However, this is only observable when including an interaction with the issues demand elasticity (measured as percentage of the number of shares offered to shares in free float). Taken together, both effects indicate that, in most cases, the age of financial reports does not influence discounts. For very large offerings with low demand elasticity, there appears to be a reverse influence, although this is based on very few observations. This leads us to conclude that institutional investors, who are the primary target for these transactions in Germany, are indeed highly sophisticated and have alternative means of staying informed which do not rely on the latest financials of the company. The reverse influence for transactions where a significant percentage of share capital is offered could be explained by a likely higher degree of direct, unobserved communications from the firm to investors around the transaction.

1.3 Summary - Insider filings as trading signals

Insider filings as trading signals - does it pay to be fast was written by my coauthor Dr. Steffen Möllenhoff and myself in the first half of 2024. Again, the motivation was a synthesis of theory and possible practical application.

A large body of literature has analyzed abnormal returns around the announcement of insider trades, which are generally found to be significantly positive for insider purchases (Lakonishok and Lee, 2001; Jeng et al., 2003; Aktas et al., 2008; Brochet, 2010; Tavakoli et al., 2012; Aussenegg et al., 2018; Dardas and Güttler, 2011; Betzer and Theissen, 2009; Fidrmuc et al., 2013; Friederich et al., 2002; Cohen et al., 2012; Amel-Zadeh et al., 2019). For us, the natural next question was whether a profitable trading strategy based on those announcements as trading signals could be established.

Although there are studies on the potential returns of outside investors copying the insiders' trades (Bettis et al., 1997; Dickgiesser and Kaserer, 2010; Friederich et al., 2002), their results are inconclusive and do not offer any insight on potential scalability. Therefore, we follow a new approach and analyze the potential US-dollar returns of such a trading strategy instead of percentage returns, taking into account the individual stocks liquidity and thus a realistically achievable trading volume for each signal.

To test this potential trading strategy, we use Form 4 filing data published via

SEC EDGAR for a total of 25,636 insider trades. This data set is the result of filters based on previous research to optimize for the strongest and most informative possible signal. Extreme outliers in this data set are manually checked to ensure that they genuinely represent real trading activity. Contrary to most existing research in this field, we use tick data instead of end-of-day data. In our main analysis, we assume a fast reaction to signal, and thus use a buying window of 30 minutes directly after the publication of the insider trade (during normal trading hours). In the same time frame, we establish a Fama-French-five-factor weighted short position in S&P 500 futures to calculate abnormal returns. We assume to be able to capture 25% of the stocks' overall trading volume in that time frame and use the corresponding volume-weighted average price to calculate abnormal percentage and US-dollar returns for different holding periods, ranging from market close on the same day to market close 20 days later. To the best of our knowledge, we are the first to look at US-dollar returns of outsider trades emulating insider trades and thus shedding light on the potential scalability of such a strategy.

We do find statistically significant positive percentage returns for shorter holding periods, in line with previous research, although on a lower level. However, these percentage returns do not translate into meaningful positive US-dollar returns. Average US-dollar returns for the shorter time frames are negligible and insignificant and even turn significantly negative for longer holding periods. The insignificant, positive US-dollar returns are not scalable, as they are already based on the assumption of capturing 25% of the overall market volume and do not yet incorporate transaction costs.

We go on to analyze the relation between stock liquidity and returns, as this might help explain the discrepancy between percentage and US-dollar returns. Grouped averages and regression analysis reveal that there is a linear negative relation between a stocks' liquidity (measured by the overall trading volume in the two days before publication of an insider trade) and the achievable abnormal percentage return, as expected. However, this relation is not to be found for US-dollar returns. Both high- and low-volume stocks exhibit negative abnormal US-dollar returns, with intermediate cases yielding only insignificant positive return. Thus, a focus on low-liquidity stocks would not be profitable as well, despite percentage returns suggesting otherwise.

Our results are robust to different lengths of buying windows (5 minutes, 60 minutes, end-of-day). While percentage returns are better with shorter windows, indicating that a fast reaction to signal helps, they are still not economically viable even before including transaction costs. Eliminating observations from 2020, which were potentially biased due to the COVID crisis, as well as using different calculations of market betas does not affect our results.

In summary, our research shows that optimizing a trading strategy with Form 4 filings as signals based on abnormal percentage returns does not optimize it with regard to US-dollar returns. In addition to this practical implication, our results also shed light on market efficiency. In an efficient market, the reaction to new information should be near instantaneous. We and others, however, find significant abnormal returns to the new information contained in insider filings days later. Our results in US-dollar now show that arbitrage trades based on those returns would not be profitable, potentially slowing down the price reaction.

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This article is accessible at `https://doi.org/10.1016/j.frl.2024.106514`

1.4 Summary - Accuracy of research analyst estimates surrounding M&A-transactions

I wrote the paper *"Accuracy of research analyst estimates surrounding M&A-transactions"* together with Michael Strauß in the first half of 2024, based on ideas arising from discussions we had about his Master's thesis at University of Wuppertal.

In it, we focus on the effect of announcements of larger Mergers and Acquisitions (M&A) transactions on the accuracy of research analysts' forecasts. As these forecasts aid the decision making processes of many market participants, it is important to know in which circumstances their reliability might be impeded.

The outcomes of large M&A transactions and their effect on the acquirer's financials are inherently uncertain, primarily due to the uncertain nature of the realization of synergy effects. Additionally, M&A transactions ceteris paribus increase information asymmetry between management and other market participants, such as analysts. Both of these effects should increase the forecast errors of analysts.

To gain a better understanding of this phenomenon, we analyze the announcements of 2,612 M&A transactions with acquirers listed in the USA and the difference in average forecast errors before and after those announcements. In line with Ma et al. (2019), we focus on majority acquisitions with a substantial deal value, both absolute ($\geq$ 10m USD) and relative ($\geq$ 10% of acquirer market value). We exclude finance and education industries as well as intra-company deals. This allows us to focus on those deals, where the announced acquisition should have a meaningful impact on the acquirer's financials and thus needs to be reflected in the analysts' forecasts. We define forecast errors as the percentage absolute deviation of a (consensus) estimate from its actual value. In our main analysis, we focus on the average forecast error of quarterly one-year-ahead estimates for Earnings per Share (EPS) over four calendar quarters before and four quarters after the announcement.

As expected, we find a highly significant and robust increase in forecast error following M&A announcements, which is directly visible in the quarter following the announcement and persistent for at least two years. By conducting a supplementary analysis with an alternative, directed measure of forecast error, we are able to determine that this increase in errors is mainly driven by an increase in overoptimism amongst analysts. We are also able to identify moderating influences on this effect: (1) Higher relative deal values (as a percentage of acquirer market capitalization) lead to a stronger increase in forecast errors, which is to be expected, as relatively larger deals will also have a larger impact on the acquirer's financials and thus introduce more uncertainty. (2) A higher number of covering analysts and (3) a positive perception of the transactions by the capital market (as indicated by positive announcement effects in the acquirers stock price) are associated with a lower increase in forecast errors. This could be due to the increased public awareness in both cases, which might help reduce information asymmetries. Contrary to our expectation based on prior research, we find no moderating effects for the target's public status, M&A type, cross-border transactions or the acquirers market capitalization. Our findings are robust to different regression techniques (classical OLS and bootstrapping), alternative definitions of forecast errors, alternative handling of outliers, various control variables and fixed effects.

To the best of our knowledge, this is the first broad, quantitative analysis in this context. Thereby, we are able to build on and add to previous case study-based research by Andersson et al. (2020) and help generalize their findings. Furthermore, our findings should have practical implications for all market participants relying on analyst forecast, as we uncover circumstances in which their accuracy might be impeded. This is important for acquirers (who should increase communication around M&A announcements to dampen information asymmetry), investors (who should be aware of the overoptimism of analysts in these circumstances) and analysts themselves (who can use our results to reflect on their estimation process).

Does financial report age matter to institutional investors in accelerated SEOs? Evidence from SEO discounts in Germany

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Abstract

This paper tests a potential new determinant of discounts commonly found in Seasoned Equity Offerings (SEOs): The time distance between SEO announcement and the last publication of quarterly financial statements by the issuer. Contrary to theories of information asymmetry and demand elasticity, but in line with theories of superior sophistication of institutional investors, we find no robust significant effect of the time distance on discounts. We do find a significant linear effect as predicted by the first two theories, but only when including an interaction of our main variable of interest. This introduces a non-linear effect, which overlaps the linear one and taken together negates the influence of time distance on discounts. Our analysis utilizes peculiarities of the German stock market, and thus focuses on accelerated SEOs in Germany between 2007 and 2021.

Keywords: Seasoned Equity Offering, Discount, Underpricing, Information asymmetry, Information efficiency, Investor sophistication

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2.1 Introduction

Seasoned Equity Offerings (SEOs) as the main way for public companies to raise equity have been studied extensively for several decades, for an overview of foundational studies see e.g. Masulis and Korwar (1985). The discount in those transactions, commonly defined as the percentage difference between the offer price and the last trade price, is of particular interest, as it represents indirect costs for issuers, existing shareholders and accompanying banks (see e.g. Krakstad and Molnar, 2014). With discounts too high, issuers leave "money on the table", holdings of existing shareholders are diluted more heavily, and banks' fees are lower, as they are set at a percentage of the money raised for the issuer.

However, the vast majority of existing research on this topic focuses on the US equity market. While this is understandable given the size and global importance of this market, it might lead to a biased identification regarding determinants of discounts. Due to international differences in the institutional setting, an analysis of international markets might uncover factors, that are not visible in US-market data. Additionally, leading theories regarding SEO discounts have been developed several decades ago and the institutional setting in the US has evolved substantially since then (see section 2.2.3).

Against this background this paper analyzes the potential impact of transaction timing on discounts. More specifically, we investigate the timing of a transaction measured by the time distance between transaction announcement and a corporate news event such as the publication of quarterly financial statements. To the best of our knowledge, we are the first to propose and test such a relationship. This time difference could have an impact on discounts for two reasons: (1) Information asymmetry: Transactions conducted more closely after the publication of financial figures should suffer less from uncertainty about the current status of the issuer as financials are more informative with regards to the current situation. (2) Demand Elasticity: Public corporate events have a marketing effect and call the issuer to investors minds. This might ease the marketing effort in bookbuilding as it increases demand elasticity. Following the argumentation laid out in Armitage et al. (2014) we do not try to distinguish empirically between demand elasticity and liquidity as explanations for discounts as the two are closely linked and used interchangeably by several authors.

However, both of those reasons depend on a lack of information efficiency and general sophistication of participating investors. If investors are constantly well informed about the current situation of the issuer because they are tracking them on a regular basis, the time distance to the last publication of financials might not matter as much. Thus, we do not formulate expectations regarding the result of our analysis. The effect should depend on the degree of sophistication or rather informedness of investors in our sample.

To empirically analyze this relationship, data from German SEOs between 2007 and 2021 is used in a multiple regression setting. This analysis focuses on capital increases without subscription rights. In Germany those transactions are normally conducted overnight using an accelerated bookbuilding approach. Transactions are announced past market close, accompanying banks contact their investors and conduct a classical bookbuilding, which is normally finished the same evening or before market opening the next day (see Schlitt, 2014). This setting differs from the US and UK (see section 2.2.3) and does not allow for additional transaction related marketing efforts to increase demand elasticity. Thus, issuers might be tempted to utilize non-deal related publicity and time their transactions accordingly, especially those with inelastic demand in need of additional marketing efforts. Conversely, those transactions are exclusively marketed to professional, institutional investors, who could be well informed regardless of publication dates.

While we do find a statistically significant linear relation in the direction predicted by information asymmetry and demand elasticy between the above mentioned transaction timing and discounts, its impact is economically small, and it only emerges when also including an interaction between timing and demand elasticity. The non-linear effect introduced by this interaction overshadows the linear one, resulting in an overall non-existant influence of timing on discount. This finding is contrary to theories of information asymmetry and demand elasticity but points to a high degree of sophistication or informedness of the participating investors regardless of the age of financial information.

The remainder of this paper is organised as follows: Section 2.2 contains the background for our analysis by giving a brief literature review and showing the established theories on determinants of discounts in SEOs in 2.2.1, prior research on the sophistication of institutional investors in 2.2.2, as well as explaining the different institutional settings and their evolution over time in 2.2.3. Section 2.3 explains our data sources, variable implementation and methodology, before empirical results are presented and discussed in section 2.4. Finally, section 2.5 concludes with a summary and provides impulses for future research.

2.2 Background and prior research

2.2.1 Possible explanations for SEO discounts

While there are numerous empirical studies on discounts in SEOs, direct theoretical explanations for them are surprisingly rare. Most studies base their theoretical background on theories originally developed for underpricing of IPOs (not discounts, as they are not observable in this case) or announcement effects of SEOs. We share the view that at least aspects of those theories are applicable to our context. There is in fact empirical evidence, that discounts and underpricing move in parallel (see Kim et al., 2010), and Altınkılıç and Hansen (2003) even use the two interchangeably. We follow the same approach of transfer of theories and focus on two possible drivers of discounts: Information asymmetry and demand elasticity. While there are more theoretical explanations for discounts, those do not apply to the German setting in our view. We follow the same logic as Armitage et al. (2014) do for the UK market. They also give an overview of the theories not considered.[1]

2.2.1.1 Information asymmetry

Classical theories regarding the impact of information asymmetry on prices in equity issues look at different dimensions of asymmetry as well as different price impacts.

The well known model of Myers and Majluf (1984) for example looks at information

[1]In contrast to Armitage et al. (2014) we do not include financial distress as a possible explanation. Issuers in financial distress are likely to be in need of a higher degree of communication surrounding their issue and in need of a higher number of shares in the issue, as their share price is under pressure. Thus, they would likely choose a rights issue in the German setting, and drop from our analysis. This argumentation is similar to the one in Ursel (2006) for the US market.

asymmetry between management and investors, stating that investors are aware of the superior information of management and thus view an announcement of an equity issue as "bad news", as management seems to view the stock of the company as overvalued. This effect should increase with the level of information asymmetry. Their paper directly predicts a negative announcement effect, i.e. a drop in the trading price of the issuers shares after the announcement, but before the actual start of a transaction. It does not directly apply to issue discounts. However, the theory could be extended to discounts, especially in the German setting for SEOs without subscription rights (see 2.2.3). As there is no official trading between announcement and conclusion / pricing of a new issue, there can by definition be no announcement effect in this setting. Thus, any compensation that investors would have gotten via a drop in share price and thus a lower purchase price could be carried over to an explicit discount. In fact, several studies on the US market find an increase in discounts in the same time frame as overnight transactions gained traction (e.g. Corwin, 2003; Mola and Loughran, 2004).

A second view of information asymmetry is proposed by Parsons and Raviv (1985), who look at asymmetry between individual investors. In the US setting at their time, underwriters would set a price and try to sell all offered shares at that price. If that is not possible, the offer price would have to be lowered. As each investors has their own valuation of the issuer and does not know the valuation of each other market participant, setting the issue price is done under uncertainty. Bookrunners try to set a price, that extracts the valuation surplus of those investors with a high internal valuation, while at the same time does not incentivize investors to wait for a lower price. This price is always at a discount to the current trading price according to the model of Parsons and Raviv (1985). Due to the specific setting in which this theory is developed, it does not directly apply to the German setting. However, even in bookbuilding processes a price range is set by the underwriter. This price range suffers from the same uncertainty about investors individual valuations as described by Parsons and Raviv (1985), pushing the underwriter to offer a discount in order to increase the success chances of the transaction.

The classical underpricing theory of Rock (1986) is based on underpricing of IPOs, not discounts of SEOs. However, its basic assumption, that discounts are a way to both compensate informed investors for the cost of information acquisition and to entice uninformed investors into the offer, also holds true for discounts in an SEO setting.

All of those theories predict an increase in discounts with higher information asymmetry in the form of investors uncertainty about the issuers financial performance. This uncertainty should increase with the age of the financial figures last reported by the issuer, which poses the core variable of interest in this paper.

2.2.1.2 Demand elasticity

Demand elasticity (or price elasticity of demand) in our context refers to the sensitivity of demand for shares for a given change in price. In terms of equity issues, a high elasticity is desirable, as a small discount (i.e. a small change in price) would elicit a high change in demand, allowing the transaction to be completed successfully. Demand elasticity for shares is closely related to liquidity of those shares. The higher the liquidity of a given share (measured e.g. in daily turnover in relation to shares outstanding), the higher c.p. its demand elasticity, i.e. the demand generated by changes in price. This leads to several researchers using the terms interchangeably or at least viewing them as closely related (see Armitage et al., 2014, for an overview), which we will also do in this paper.

Armitage et al. (2014) argue that demand for shares is inherently less than perfectly elastic due to a limit to the numbers an individual investor is willing to buy at a given price (e.g. due to diversification considerations) and a limit to the number of investors willing to buy any shares at a given price. Given that an issue of new shares typically encompasses many times the normal daily trading volume in these shares, a discount is the natural conclusion. Huang and Zhang (2011) and Gao and Ritter (2010) both provide overviews of evidence for the less than perfectly elastic nature of stock prices. Both also argue that marketing of the offer by the bookrunner(s) is done to increase demand elasticity and thus decrease discounts. While Huang and Zhang (2011) focus on the number of bookrunners and thus on the number of investor contacts, Gao and Ritter (2010) go as far to say that there is a trade-off between speed and time for marketing activities in SEOs, both influencing discounts, and that elasticity thus is an endogenous choice variable. As discussed in section 2.2.3, this is different in the German market, where time for deal related marketing can not be freely chosen. Gao and Ritter (2010) further propose that a bookbuilding in and of itself is not only conducted to measure demand, but also poses a marketing activity and thus creates demand.

Apart from the papers mentioned above, others find a connection between demand elasticity and discounts as well: Corwin (2003) finds a significant impact of elasticity in connection with offer size, Intintoli et al. (2014) find share demand to be the reason for lower discounts in follow-on SEOs compared to mature SEOs, while Intintoli and Kahle (2010) state that price pressure is especially high for issuers with low free float, supporting the demand elasticity argument.

Hence, based solely on the arguments made in this section and (for the moment) ignoring the factors mentioned in section 2.2.2 regarding investor sophistication, we would expect a negative relationship between the time passed since the last publication of financial figures and the discount in an SEO.

2.2.2 Prior research on sophistication of institutional investors

When discussing the ability of investors to gather and analyze information, a distinction is often made between institutional "professional" investors on one side and individual "retail" investors on the other. The basic argument is that institutional investors are able to gather and analyze potentially price sensitive information more cost efficiently due to economies of scale as well as better access to management and financial analysts due to higher market influence. In the US, institutional ownership has risen to more than 60% in the last decades (Amin et al., 2015), with a preference for large, liquid stocks (Gompers and Metrick, 2001).

A number of empirical studies around corporate events have confirmed this conventional wisdom. Amihud and Li (2006), Alangar et al. (1999) and Amin et al. (2015) for example have studied the information content of dividend-change announcements in connection with institutional holdings. They come to the conclusion that a higher percentage of institutional holdings in a companies shares leads to a lower price reaction to dividend-change announcement, implying that institutional investors have anticipated this change to a certain degree and already incorporated it in the share price. Amihud and Li (2006) use this effect to explain the diminishing dividends in the 1980s and 90s, stating that dividends have lost information content due to the rising share of institutional investors. Amin et al. (2015) on the other hand use it to explain the reappearance of dividends since 2002 by incorporating the time-horizon of institutional investors (short-term vs. long-term) in their analysis. Studies connecting institutional holdings to the information content of announcements of quarterly financials (Bartov et al., 2000; Kim et al., 1997), complex financial information (Hand, 1990) and common stock offerings[2] (Szewczyk et al., 1992) come to similar conclusions. In summary, institutional investors seem to be able to anticipate corporate events to a certain degree and incorporate this information in the share price before the announcement.

In addition to the above, institutional investors also seem to have superior trading skills (when looking at interim results, see Puckett and Yan, 2011) and move market prices more than other market participants (Chakravarty, 2001). Shares with a high percentage of institutional holdings incorporate more forward looking information in their prices (Jiambalvo et al., 2002) and face less adverse effects in the event of termination of research coverage by financial analysts (Ellul and Panayides, 2018).

Given that overnight SEOs are exclusively marketed to institutional investors (as there is simply no time for any involvement of the general public and thus retail investors), the findings above are of high importance to our analysis. If institutional investors are able to gather information about a given company completely regardless of the age of the last reported financial figures, the arguments made in section 2.2.1.1 regarding information asymmetry would be irrelevant. In the same vein, if institutional investors are always perfectly informed and up-to-date regarding companies in their investment universe, there would be no marketing effect by the publication of financial figures as assumed in section 2.2.1.2 regarding demand elasticity. Hence, based solely on the arguments made in this section, we would expect no relationship

[2] In contrast to our study, Szewczyk et al. (1992) look at announcement effects, i.e. the market price reaction to the announcement, not the discount based on the actual offer price. Those are two separate mechanisms, as laid out in section 2.2.1.

between the time passed since the last publication of quarterly financials and the discount in an SEO. However, in practice the sophistication and information efficiency of institutional investors is likely not perfect, nor is it nonexistent. When combining arguments of this and the previous section, we are thus not able to predict the existence or nonexistence of our analyzed relationship, but expect it to be dependent on the degree of investor sophistication in our sample.

2.2.3 Differences in institutional settings between markets and over time

While recent decades have shown a common trend in heavily declining fully marketed offers (USA) / rights issues (EU) and a rise of accelerated transactions across all major equity markets (for an overview see Bortolotti et al., 2008), some important differences still remain.

In the US, rights issues as an issue method for SEOs have all but disappeared since the 1970s and 1980s. Their place was taken by fully marketed transactions without subscription rights (see e.g. Eckbo, 2008; Gao and Ritter, 2010). In more recent years, the trend in the US has shifted from fully marketed to accelerated offers with a marketing time frame of only several days, and further on to overnight shelf offers, of which a majority is structured as bought deals. Autore (2011) for example documents this trend starting in the 2000s and finds overnight shelf offers to account for 18.4% of all equity offers in his sample from 2000-2006, while they only accounted for 3.9% in the 6 years before. Roughly 55% of those overnight offers were bought deals. In bought deals, the whole issue is bought by the accompanying underwriter(s) at a price negotiated between underwriters and issuer, and subsequently placed with investors. Due to this structure, the issue discount is not a direct result of investor demand determined in a bookbuilding, but of investor demand assumed by the banks. Gustafson (2018) documents a further increase of this trend, finding 75% of all SEOs between 2009 and 2014 to be overnight transactions in the US, up from 27% between 2000 and 2008.[3]

Compared to the US, rights issues still played a more important role in Europe

[3] His evidence on the percentage of bought deals is not clear cut. He notes roughly 50% of overnight transactions being bought deals up until 2006, in accordance with Autore (2011) and Gao and Ritter (2010), with a sharp drop from 2007 onward. However, he admits this drop to be magnified by his sample filters

for some time, but are also increasingly vanishing in most markets. Armitage (2010) for example find that rights issues as the most common issue method in the UK have been replaced by open offers, a combination of "a placing via negotiation with an offer to the existing shareholders in proportion (pro rata) to their existing holdings". Accelerated bookbuildings play an increasing role in the UK as well.

Some European markets however still legally require rights issues in some circumstances. In Germany for example rights issues are still the legal base case for any capital increase of listed companies (Section 186 (1) AktG). Issuers are able to receive an inventory resolution by the annual general meeting for up to five years to exclude the subscription rights of existing shareholders in SEOs to come. However, they may legally not issue more than 10% new shares and may not deviate substantially from the reference price[4] in order to limit dilution for existing shareholders. This leads to virtually all capital increases above 10% of existing share capital in Germany being rights issues. This divides equity issues in Germany into two distinct categories: On the one hand larger rights issues with a full security prospectus and full marketing activities including roadshows in addition to a subscription period spanning roughly two weeks, on the other hand smaller accelerated transactions without subscription rights and without prospectus. Thus, the issue method is not chosen based on perceived need for marketing activities as is possible in the UK and US, but on the amount of new equity capital needed. SEOs without subscription rights are almost always structured as accelerated bookbuildings overnight outside market hours (see Schlitt, 2014), bought deals as seen in the US are not common in Germany. Although not specifically legally required, any deviation from an overnight placing would lead to a substantial legal contestation risk for the issuer: With overnight placings and bookbuildings conducted outside of trading hours being market standard, leaving the books open past market open on the day following the announcement would cast doubt on the success of the placement and put pressure on the market price of the share. This in turn would put pressure on the offer price determined in the bookbuilding. With new (lower) market

[4]The relevant legal standard, Section 186 (3) sentence 4 AktG, does specify neither "substantially" nor "reference price", leaving some leeway. In practice, this rule is interpreted as no more than 3-5% below the last closing price before the offer (for a full overview of the legal background see Schlitt, 2014)

prices since the last close price, the offer price can now theoretically be set more than 5% below the last close. This in turn is prone to legal contest, leading to a litigation risk for the issuer (see Schlitt, 2014). As the bookbuilding is thus normally completed in just several hours and outside market hours, any inclusion of retail investors is not feasible and those transactions are marketed exclusively to institutional investors or in rare cases high net-worth individuals classified as professional investors.

In bought deals in the US banks are bidding on the issue, take over all issued shares at an agreed price and place them with investors afterwards. The equivalent in Germany is a "hard underwriting", in which the underwriters guarantee a certain minimum price. In contrast to the US structure, there is still a bookbuilding with investors, which determines the offer price. The minimum price is typically not disclosed and only relevant, if there is not enough investor interest at or above the minimum price. In this case, the underwriters take over all shares not bought by investors and try to sell them in the market afterwards.

To summarize, despite a common evolution in the last years, the equity market in Germany still differs from the US and UK markets in several ways, which can be exploited for analyzing determinants of SEO discounts:

1. Due to legal restrictions, the issue method (rights vs. no rights) is chosen mainly based on needed capital, not needed marketing effort

2. Due to legal contestation risk, virtually all SEOs without rights are conducted overnight, giving no time for additional deal related marketing efforts

3. Bought deals are not as common as in the US and UK, offer prices seen in overnight transactions are thus a direct result of investor demand (to a certain extent even in hard underwriting scenarios)

Based on those differences, we should be able to see an effect of time passed since the last publication of quarterly financials on the discount, if this effect is not negated by the sophistication and informedness of the investors involved in the transaction. The following analysis tries to determine whether any such effect can be observed or not.

2.3 Data and Methodology

2.3.1 Data Sources

Basic transaction data has been sourced from Refinitiv Deal Screener, which lists a total of 1,666 follow-on transactions at German stock exchanges from 01/01/2007 to 07/13/2021. From this, all transactions with "Rights" or "Subscription" in their offer method text, pure firm commitments, issues with no new shares in the offering (i.e. pure block trades of existing shares) and issues with missing filing date have been excluded. In cases of inconsistencies the transaction type has been verified and corrected by hand using the transactions' ad-hoc release. This only leaves SEOs in the form of capital increases without subscription rights, a total of 539 relevant transactions as the maximum attainable dataset.

Data on reporting dates and reported figures has been obtained from Compustat-Capital IQ (Standard & Poor's) and matched on Refinitiv data via issuer ISIN. Data for various control variables (details below) has been selected from separate Refinitiv databases and matched on transaction data via Refinitiv PermIDs. Relevant data has been verified and corrected with the transactions' ad-hoc announcements or press releases, where available, which where mainly obtained from www.dgap.de.

Relevant control data from Compustat and Refinitiv for the base model has been available for 311 transactions or 58% of all transactions identified. Data availability is lowest (below 50%) for the years 2007-2009 and 2021, see figure 2.1 for a visual overview of data coverage over time. Results are essentially unchanged when only transactions from the years with the best data coverage (2010 to 2020, both included) are used, see table 2.4. The comparatively low coverage of total transactions is not surprising, given that no size restriction was imposed on the initial Refinitiv Deal Screener and data availability especially for micro-stocks is likely to be scarce.

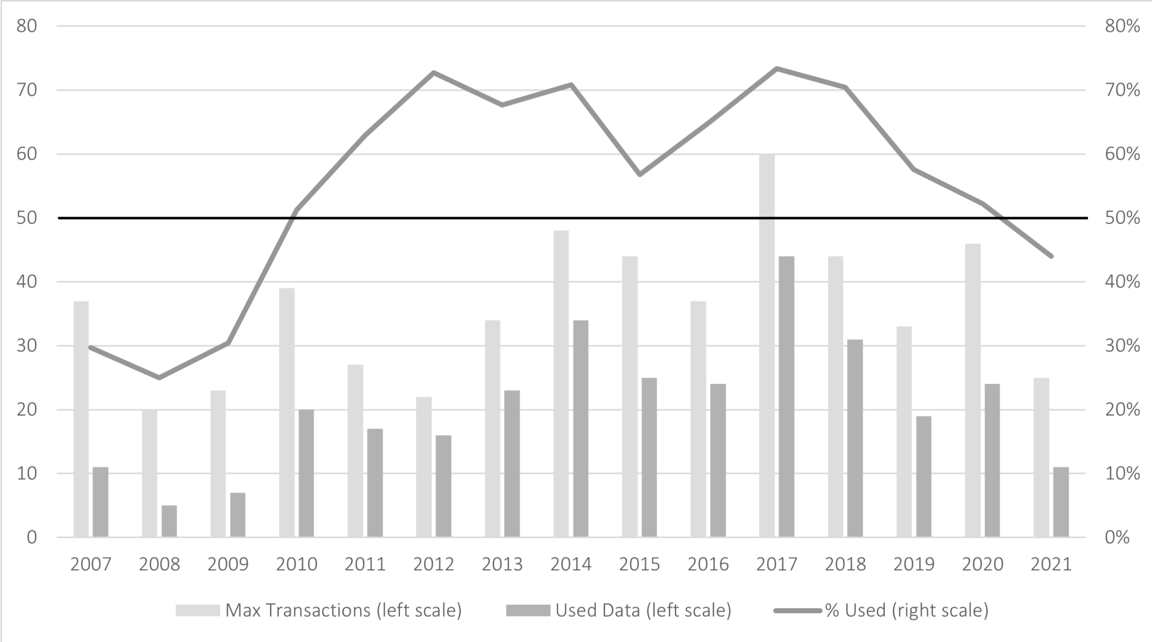

Figure 2.1: Data coverage compared to total relevant transactions over time
This figure shows the number of transactions in the sample used in the analysis and the total number of relevant transactions as listed in Refinitiv Deal Screener each year as bar charts. The line illustrates the percentage data coverage derived from the data in the bar charts.

When comparing deal value distributions in the total and the selected dataset, it becomes obvious that mainly very small transactions have been dropped from the analysis. The mean deal value (in raw, non-winsorized data) rises from EUR m 119.8 to EUR m 145.7, the median from EUR m 9.6 to EUR m 15.2. As very small transactions are likely highly illiquid and mostly excluded upfront in other analysis, this drop in datapoints should not negatively influence our results quality. At the same time transactions with extremely high premiums or discounts were dropped due to data availability in Refinitiv and Compustat. The mean discount (in raw, non-winsorized data) increases from -0.4% to -3.3%, while the median stays at -3.9%. The extreme values combined with limited data availability in other databases seem to point to data errors, thus the exclusion of those transactions also should not negatively influence our analysis.

2.3.2 Methodology

In order to expose an influence of timing with regards to financial publication dates on discounts in transactions, a multiple regression approach is used. *Discount* as the explained variable is defined as the percentage change from the price of a given security at the last trade before offer announcement to the offer price. Positive values indicate a premium in the offer price compared to the last trading price, whereas negative values indicate the (more common) discount.

The main explanatory variable of interest in our analysis is the time difference between the transaction announcement and a corporate news event. As publication of data on events such as non-deal roadshows or conference attendance is not mandatory and its availability thus very heterogeneous, we focus on dates of corporate events that are available for all companies: The publication dates of quarterly financials, which go hand in hand with accompanying press conferences and analyst / investor calls. The variable $log(Time)$ captures the natural log of days between the announcement of the transaction and the last publication date of quarterly financials. To the best of our knowledge, the impact of this or a comparable factor on discounts in SEOs has not been explored before.

As laid out in 2.2.1.2, demand elasticity of the issue could have an impact on the discount. For this reason we include the ratio of new shares in the offer to total shares in free float as $PercOfFF$ in our analysis. We follow the argumentation of Intintoli and Kahle (2010), that this ratio is a better proxy for price pressure due to liquidity (and thus the demand elasticity of the offer) than the ratio on new shares to total shares outstanding.[5] Due to the size restriction of 10% of existing share capital in Germany most issuers maximize the offer size when doing a SEO without rights anyhow, eliminating effects of relative issue size in shares compared to total shares outstanding. Thus we focus on the ratio to shares in free float and do not include the ratio to total shares in our base model.

We expect the elasticity changing effect of transaction timing to be more pronounced for issuers facing a low demand elasticity to begin with. Therefore, we

[5] We further follow the argumentation of Armitage et al. (2014) and do not try to distinguish empirically between demand elasticity and liquidity

include an interaction term for the two variables mentioned above.

In our base model, we control for two additional factors that have shown influence on discounts of SEOs in previous studies:

- Size, measured by the natural log of the market capitalization (shares outstanding pre offer times offer price) in EUR m ($log(MarketCap)$), as similarly used in Kim et al. (2010) (use inverse of inflation adjusted deal value), Intintoli et al. (2014) (use IPO size, assets and market capitalization), or Autore (2011), Gebhardt et al. (2001), Cline et al. (2012) and Corwin (2003) (all use market capitalization).

- Share price "run up", measured by the raw (not market adjusted) return of the issuers shares in the last five trading days before announcement of the offer ($RunUp_{5Days}$), as used similarly in Armitage et al. (2014) (although measured differently), Kim et al. (2010) (market returns instead of issuer stock returns), Cline et al. (2012) (use market adjusted return between filing and offer as well as six month to offer), Corwin (2003) (uses dummies for positive or negative abnormal returns five days prior to offer), or Intintoli et al. (2014), Kim and Shin (2004) and Autore (2011) (all use abnormal returns five days prior to offer).

Table 2.1 shows summary statistics for all variables used in the base regression as well as additional information and controls. All data has been winsorized at the 1% and 99% percentile level to minimize the effect of outliers.

Summary statistics raw data								
	N	$Mean$	$Std.$	$Min.$	$Q1$	$Median$	$Q3$	$Max.$
$Discount$	311	-4.09	3.73	-17.09	-5.58	-3.88	-2.03	7.71
$Time$	311	74.49	64.69	0.00	27.50	56.00	102.50	307.60
$PercOfFF$	311	0.23	0.43	0.00	0.10	0.12	0.19	3.16
$DealValue$	311	142.11	354.52	0.32	4.10	15.22	86.14	2,000.90
$MarketCap$	311	1,937.52	4,856.60	3.12	58.58	187.38	1,086.30	28,669.08
$RunUp_{5Days}$	311	0.00	0.07	-0.14	-0.03	-0.00	0.03	0.31
$Revenue/MarketCap$	311	0.28	0.38	0.00	0.06	0.14	0.35	2.07
$RevenueGrowth_{4Q}$	308	0.68	2.86	-0.98	-0.05	0.10	0.34	20.98
$EquityRatio$	218	0.41	0.25	-0.13	0.24	0.38	0.58	0.93
$\#ofBanks$	311	1.37	1.00	0.00	1.00	1.00	2.00	5.00
$\#ofAnalysts$	300	7.17	8.46	0.00	1.00	3.00	10.25	33.02
$FreeFloat$	311	67.46	28.77	3.32	45.78	70.04	96.18	100.00

Table 2.1: Summary statistics raw data
This table reports summary statistics for our analyzed data. $Discount$ denotes the percentage premium (positive values) or discount (negative values) of the offer price compared to the last trade price before announcement, $Time$ is the time difference in days between announcement and the last publication date for quarterly financials, $PercOfFF$ denotes demand elasticity / liquidity of the issuers shares as measured by the relation between new shares in the offer to total shares outstanding in free float, $DealValue$ and $MarketCap$ are the value of the relevant transaction and of all issuers shares outstanding before the transaction respectively, both in EUR m, $RunUp_{5Days}$ is the percentage difference of the last closing price before announcement of the offer and the closing price five trading days prior, $Revenue/MarketCap$ denotes the relationship of last reported revenue and market capitalization at the offer price, $RevenueGrowth_{4Q}$ is the percentage difference between the last reported quarterly revenue and the quarterly revenue four quarters ago, $EquityRatio$ is the last reported equity ratio, $\#ofBanks$ denotes the number of banks involved in the offer in any role and $\#ofAnalysts$ the number of research analysts covering the issuer, $FreeFloat$ is the percentage of shares outstanding held by free float investors (i.e. not blockholders). All variables are winsorized at the 1% and 99% percentile to dampen the influence of outliers. The sample period is January 2007 - July 2021.

Further control variables and different measurements for the base controls have been used for robustness tests, see 2.4.2.

2.4 Results

2.4.1 Base Regression analysis

In our main analysis, we test the effect of transaction timing related to financial publication dates on discounts using the following regression, with variables defined as laid out in 2.3.2 as well as time and industry fixed effects. Standard errors have been clustered on industry level:

$$Discount_i = Intercept + \beta_1 log(Time)_i + \beta_2 PercOfFFi + \beta_3 log(Time)_i x PercOfFF_i \\ + \beta_4 log(MarketCap)_i + \beta_5 RunUp_{5Days,i} + TimeFE + IndustryFE + \epsilon \quad (2.1)$$

Table 2.2 shows the results for the base regression in formula 2.4.1, building up from a univariate regression of $log(Time)$ on $Discount$ and adding each element subsequently. As can be seen, $log(Time)$ is statistically significant in each step of the build up. However, in the first two versions the sign of its coefficient is, surprisingly, positive, indicating lower (more positive) discounts with longer time distances. Upon adding the interaction term between $log(Time)$ and $PercOfFF$, the sign changes to the expected negative, i.e. longer time distances lead to higher (more negative) discounts. However, due to the interaction term, the effect of $log(Time)$ on $Discount$ can no longer be considered in isolation. More on the overall effect including the interaction term below.

Base regression build up					
	(1) : *Time*	(1)+ *Elasticity*	(2) + *Interaction*	(3) + *MarketCap*	(4)+ *RunUp*
Intercept	-4.8736***	-4.8208***	-3.5133***	-2.9754***	-2.4914***
	(-15.463)	(-17.424)	(-8.4684)	(-5.3462)	(-5.1909)
log(*Time*)	**0.2018****	**0.2038****	**-0.1469***	**-0.1730****	**-0.2479*****
	(2.4706)	(2.4119)	(-1.6636)	(-2.2130)	(-3.6150)
PercOfFF		-0.2627	**-6.5921*****	**-6.6742*****	**-7.6972*****
		(-0.3749)	(-2.9953)	(-3.0184)	(-6.0948)
PercOfFF : *log*(*Time*)			**1.6985*****	**1.7071*****	**1.9940*****
			(3.4809)	(3.5181)	(7.6416)
log(*MarketCap*)				-0.0773	-0.1055
				(-0.9204)	(-1.0394)
$RunUp_{5Days}$					-13.198***
					(-3.6274)
Time & Industry FE	*yes*	*yes*	*yes*	*yes*	*yes*
R^2	0.0037	0.0046	0.0281	0.0298	0.0850
Obs	311	311	311	311	311

Table 2.2: Effect of time since publication on discounts - base regression
This table reports the regression estimates for the effect of time since last financial publication date on SEO discounts. Building upon a univariate regression, control variables are added. The dependent variable is *Discount* as measured by the percentage difference between the offer price and the last trading price before announcement of the offer. The independent variables of interest (in bold) are: i) *log*(*Time*) the natural log of the time difference in days between the last publication date and the announcement date, and ii) *PercOfFF* which denotes demand elasticity / liquidity of the issue as measured by the relation between new shares in the offer to total shares outstanding in free float, as well as iii) *PercOfFF* : *log*(*Time*), the interaction between i) and ii). Additionally, the following control variables are used: i) *log*(*MarketCap*), the natural log of the Market Capitalization at the offer price in EUR m as an indicator for size, and ii) $RunUp_{5Days}$, the percentage difference of the last closing price before announcement and five trading days prior as an indicator for share price momentum. All variables are winsorized at the 1% and 99% percentile to dampen the influence of outliers. Additionally we add time fixed effects and industry fixed effects. For the latter, the 25 industries provided by Refinitiv have been mapped manually on SIC 1 classifications to reduce dimensions and to increase comparability with other studies. Standard errors are clustered on industry level. *t*-statistics are reported in parentheses, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

log(*MarketCap*) is not significant in any of our models, which is surprising, as previous studies (although mainly for the US market) showed a robust influence of size on discounts. $RunUp_{5Days}$ on the other hand is significant and the sign of its coefficient is negative, as expected. A higher run up directly before an offer leads to an investor expectation of higher discounts, as they will still be anchored on the lower share price a few days before the offer and not only compare the offer price to the last price.

To better understand the overall relation between *log*(*Time*), *PercOfFF* and *Discount*, figure 2.2 shows the joint influence of the main coefficients of *log*(*Time*) and *PercOfFF* as well as their interaction on *Discount* as estimated in the base regression (i.e. including the base control variables). For low values of $PercOfFF$ (below 1 / 100%), i.e. a low number of new shares compared to overall free float and thus high liquidity / elasticity, there seems to be no noticeable influence of $log(Time)$ on $Discount$. For high values of $PercOfFF$, Discounts are predicted to fall (i.e. get more positive) with increases in $log(Time)$. This is the inverse of the expected effect, as already seen in the univariate and non-interaction Regressions. As can be seen in table 2.1, most values for $PercOfFF$ are far below 1, with the 75%-percentile at 0.19, i.e. new shares in the offer amount to 19% of all shares in free float before the offer. Thus, the effect seen in figure 2.2 for high values of $PercOfFF$ stems from few extreme observations (with the number of new shares more than two times higher than all shares in free float before the offer) and most observations fall in the region predicting no noticeable effect between $log(Time)$ and $Discount$.

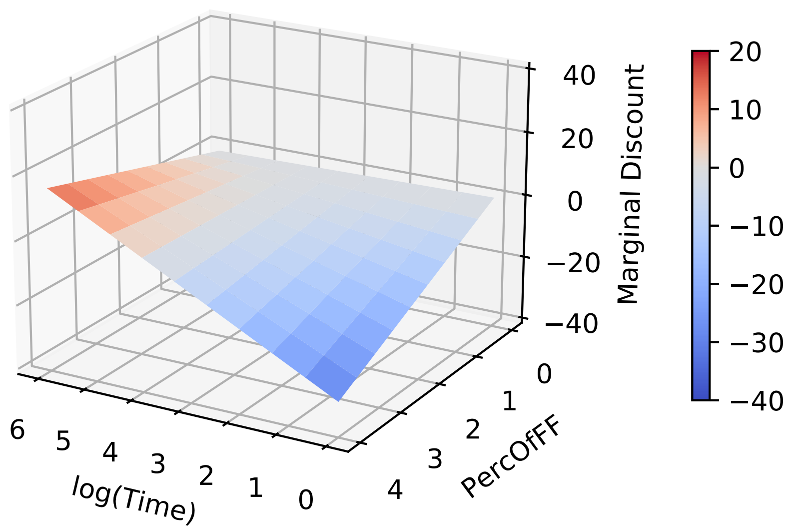

Figure 2.2: Joint influence of $log(Time)$ and $PercOfFF$ on $Discount$ in base regression
This figure shows the joint influence of $log(Time)$ and $PercOfFF$ on $Discount$ in base regression, i.e. the marginal discount for pairs of values for $log(Time)$ and $PercOfFF$ as predicted by their individual and interaction term coefficients. Coefficients are taken from the base regression model, i.e. including the effects of base controls.

In conclusion, although the base regression (4) in table 2.2 shows a statistically significant linear relation in the expected direction between the main variable of interest and *Discount*, this relation only exists when also including an interaction term with $PercOfFF$. The linear relation is then overlapped by the non-linear effect introduced by the interaction. Taken together, there seems to be no effect of $log(Time)$ on *Discount* for most cases and even an inverse relation for outlier cases of $PercOfFF$. Even if one only paid attention to the linear main effect of $log(Time)$, the coefficient indicates an economically negligible effect, a doubling of days (i.e. roughly a log increase of 1) is predicted to lead to an increase in *Discount* of only 25 basis points. This effect is overcompensated by the interaction term.

2.4.2 Robustness tests

In addition to the base regression, several additional regressions have been tested to increase robustness of our results. Table 2.3 for instance tests the addition of several potential control variables, which have not been included in the base regression due to limited data availability:

- $Revenue/MarketCap$ is included as a proxy for the valuation level of the issuer. Revenue has been used instead of more common valuation metrics such as EBITDA because of its better data availability. Higher valuation levels could lead to higher discount expectations of investors, as the issuers shares could be perceived as overpriced to begin with

- $RevenueGrowth_{4Q}$ is included as a proxy for the growth of the issuer and measured as the percentage change of revenue compared to four quarters ago

- *EquityRatio* is included as a measurement of leverage. Leverage is also used as a control by Kim et al. (2010) (in the form of the ratio Debt / Assets)

- $\#ofBanks$ indicates the number of banks involved in the SEO in any role and serves as a proxy for information asymmetry and marketing effort. A similar measure, the number of managing underwriters, is used by Intintoli et al. (2014)

- *#ofAnalysts* indicates the number of analysts following a given issuer and is included for a similar reason as *#ofBanks*. This control is also used by Armitage et al. (2014) and Intintoli et al. (2014) .

Base regression with additional controls						
	(1) : *Base*	(1)+ *Valuation*	(1)+ *Growth*	(1)+ *Leverage*	(1)+ *#Banks*	(1)+ *#Analysts*
Intercept	-2.4914***	-2.6995***	-2.6432***	-3.2554***	-2.4900***	-2.8139**
	(-5.1909)	(-4.3345)	(-4.1711)	(-5.6624)	(-5.1124)	(-2.3117)
$log(Time)$	-0.2479***	-0.2428***	-0.2390***	0.0065	-0.2540***	-0.2010***
	(-3.6150)	(-3.4191)	(-3.5640)	(0.0414)	(-3.5146)	(-2.8222)
$PercOfFF$	-7.6972***	-7.5614***	-7.5808***	2.0537	-7.7054***	-5.8836***
	(-6.0948)	(-5.4039)	(-5.2148)	(0.5843)	(-6.0827)	(-9.3723)
$PercOfFF : log(Time)$	1.9940***	1.9576***	1.9743***	-0.1656	1.9982***	1.4612***
	(7.6416)	(6.6124)	(6.2487)	(-0.1847)	(7.5563)	(11.445)
$log(MarketCap)$	-0.1055	-0.0932	-0.0880	-0.2136**	-0.0825	-0.0951
	(-1.0394)	(-0.8409)	(-0.7335)	(-2.3789)	(-0.6057)	(-0.3755)
$RunUp_{5Days}$	-13.198***	-13.166***	-13.026***	-17.353***	-13.219***	-13.136***
	(-3.6274)	(-3.5666)	(-3.1406)	(-6.1352)	(-3.6210)	(-3.8783)
$Revenue/MarketCap$		0.4283				
		(0.8686)				
$RevenueGrowth_{4Q}$			-0.0457			
			(-0.2970)			
EquityRatio				-0.2321		
				(-0.1850)		
#ofBanks					-0.0778	
					(-0.4335)	
#ofAnalysts						0.0083
						(0.1817)
Time & Industry FE	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
R^2	0.0850	0.0867	0.0843	0.1218	0.0853	0.0719
Obs	311	311	308	218	311	300

Table 2.3: Effect of time since publication on discounts - additional controls
This table reports the regression estimates for the effect of time a since last financial publication date on discounts and introduces additional control variables. Base variables are the same as in table 2.2. Additional control variables added in this table include i) $Revenue/MarketCap$, the relationship of last reported revenue and market capitalization at the offer price as an indicator for valuation levels, ii) $RevenueGrowth_{4Q}$, the percentage difference between the last reported quarterly revenue and the quarterly revenue four quarters ago, as an indicator for company growth, iii) *EquityRatio* the last reported equity ratio as an indicator for leverage levels and iv) *#ofBanks*, the number of banks involved in the offer in any role as well as v) *#ofAnalysts*, the number of research analysts covering the issuer, both as indicators for information asymmetry and intensity of marketing efforts. All variables are winsorized at the 1% and 99% percentile to dampen the influence of outliers. Additionally we add time fixed effects and industry fixed effects. For the latter, the 25 industries provided by Refinitiv have been mapped manually on SIC 1 classifications to reduce dimensions and to increase comparability with other studies. Standard errors are clustered on industry level. t-statistics are reported in parentheses, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

None of these additional control variables had a statistically significant effect. Nor did they change significance or signs of the base regression model, except for the inclusion of $EquityRatio$, but this coincides with a significant reduction in the number of observations.

Table 2.4 builds on this analysis and restricts the time frame for our analysis to 2010-2020, as discussed in section 2.3.1. Results are in line with the base regression.

Table 2.5 illustrates effects of exchanging measurements for control variables in the base regression. Exchanging $PercOfFF$, i.e. the ratio of new shares offered to shares outstanding in the issuers free float, with the ratio to total shares outstanding ($PercOfTotal$) leads to a loss of significance for both $log(Time)$ and the interaction effect (where $PercOfFF$ has also been exchanged). $PercOfTotal$ itself is statistically significant, as was $PercOfFF$. Using $log(DealValue)$ instead of $log(MarketCap)$ does not change the non-significance of size. $RunUp_{5Days}$ however can be exchanged with its market adjusted excess return version (compared to the German leading stock index DAX) without meaningful impact on significance or coefficients.

Table 2.6 shows the effects of changing the included fixed effects as well as the calculation of standard errors in the base regression. All possible combinations of time fixed effects, industry fixed effects, standard errors clustered on industry levels and robust standard errors as in White (1980) are calculated. The effects for the main variables of interest as discussed in section 2.4.1 seem to rely on time fixed effects and clustered standard errors, all other variations show no or reduced significance for the main variables.

Base regression with additional controls, only data from 2010-2020						
	(1) : *Base*	(1)+ *Valuation*	(1)+ *Growth*	(1)+ *Leverage*	(1)+ *#Banks*	(1)+ *#Analysts*
$Intercept$	-3.0152***	-2.9541***	-3.1428***	-4.0887***	-3.0162***	-3.5996**
	(-4.0241)	(-4.0849)	(-3.3867)	(-3.0373)	(-3.9459)	(-2.3141)
$log(Time)$	-0.2187***	-0.2189***	-0.2108***	0.0731	-0.2196***	-0.1493**
	(-3.1050)	(-3.1116)	(-3.4769)	(0.4053)	(-2.7614)	(-2.0899)
$PercOfFF$	-7.0437***	-7.0444***	-6.9435***	5.2703	-7.0438***	-4.6847***
	(-4.2222)	(-4.2183)	(-3.8552)	(1.0211)	(-4.2188)	(-3.5356)
$PercOfFF : log(Time)$	1.8114***	1.8104***	1.7969***	-1.0284	1.8118***	1.1095***
	(4.6732)	(4.7206)	(4.2025)	(-0.8067)	(4.6374)	(3.3733)
$log(MarketCap)$	-0.0531	-0.0564	-0.0364	-0.1799	-0.0487	0.0099
	(-0.3582)	(-0.4005)	(-0.2109)	(-1.4864)	(-0.2229)	(0.0313)
$RunUp_{5Days}$	-14.496***	-14.551***	-14.412***	-18.405***	-14.502***	-14.346***
	(-3.5681)	(-3.7117)	(-3.1178)	(-5.3258)	(-3.6063)	(-3.6602)
$Revenue/MarketCap$		-0.1523				
		(-0.1985)				
$RevenueGrowth_{4Q}$			-0.0616			
			(-0.3973)			
$EquityRatio$				0.4214		
				(0.3107)		
$\#ofBanks$					-0.0142	
					(-0.0530)	
$\#ofAnalysts$						-0.0043
						(-0.0776)
Time & Industry FE	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
R^2	0.0885	0.0887	0.0902	0.1255	0.0885	0.0774
Obs	277	277	275	193	277	268

Table 2.4: Effect of time since publication on discounts - data from 2010-2020
This table reports regression estimates for the same regressions as table 2.3 but only uses data from 2010 to 2020 (both included). This excludes the years with the lowest data coverage. Results are essentially unchanged compared to using the whole dataset. t-statistics are reported in parentheses, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

Base regression, alternative measurements of variables				
	(1) : *Base*	(2) : $FF \leftrightarrow Total$ $shares$	(3) : $MarketCap \leftrightarrow$ $DealValue$	(4) : $Raw \leftrightarrow$ $excess\ return$
$Intercept$	-2.4914***	-1.3380*	-2.6424***	-2.5761***
	(-5.1909)	(-1.8268)	(-8.4100)	(-5.6166)
$log(Time)$	-0.2479***	-0.3223	-0.2539***	-0.2568***
	(-3.6150)	(-1.0542)	(-3.4781)	(-3.1216)
$PercOfFF$	-7.6972***		-7.6978***	-7.4477***
	(-6.0948)		(-6.1445)	(-4.9593)
$PercOfFF : log(Time)$	1.9940***		2.0029***	1.9366***
	(7.6416)		(8.0839)	(5.9813)
$log(MarketCap)$	-0.1055	-0.1362*		-0.0978
	(-1.0394)	(-1.6998)		(-1.0118)
$RunUp_{5Days}$	-13.198***	-12.570***	-13.176***	
	(-3.6274)	(-3.6979)	(-3.6485)	
$PercOfTotal$		-25.223***		
		(-6.0149)		
$PercOfTotal : log(Time)$		4.4911		
		(1.6114)		
$log(DealValue)$			-0.1332	
			(-1.6168)	
$RunUp_{excess5D}$				-12.171***
				(-3.0711)
Time & Industry FE	*yes*	*yes*	*yes*	*yes*
R^2	0.0850	0.0712	0.0861	0.0763
Obs	311	311	311	311

Table 2.5: Effect of time since publication on discounts - alternative measurements
This table reports regression estimates for the base regression and three versions of it with alternative measurements of individual variables. Base variables are the same as in table 2.2. Model (2) exchanges $PercOfFF$ with the ratio of new shares to total shares outstanding before the offer, both as an individual variable and in the interaction term. Model (3) exchanges $log(MarketCap)$ as a measure of size with $log(DealValue)$, calculated as shares offered times the offer price. Model (4) exchanges $RunUp_{5Days}$ with its excess version, i.e. the return of a reference index (DAX) for the same time frame is deducted from $RunUp_{Days}$. As in previous regressions, all variables are winsorized at the 1% and 99% percentile, time fixed effects and industry fixed effects are added and standard errors are clustered on industry level. t-statistics are reported in parentheses, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

Base regression, different fixed effects and standard errors								
	(1) : *Base*	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	-2.4914***	-3.0369***	-2.7707***	-3.2866***	-2.4914**	-3.0369***	-2.7707***	-3.2866***
	(-5.1909)	(-6.9250)	(-4.9479)	(-6.1639)	(-2.2226)	(-2.6827)	(-2.5951)	(-3.0551)
$log(Time)$	-0.2479***	-0.1113*	-0.2100***	-0.0844	-0.2479	-0.1113	-0.2100	-0.0844
	(-3.6150)	(-1.7540)	(-3.7862)	(-1.5731)	(-1.2157)	(-0.5443)	(-1.0664)	(-0.4251)
$PercOfFF$	-7.6972***	-6.9048***	-7.4220***	-6.6226***	-7.6972***	-6.9048**	-7.4220***	-6.6226**
	(-6.0948)	(-6.9829)	(-5.7176)	(-6.5615)	(-2.7805)	(-2.5527)	(-2.7940)	(-2.5452)
$PercOfFF : log(Time)$	1.9940***	1.7483***	1.9326***	1.6902***	1.9940**	1.7483**	1.9326**	1.6902**
	(7.6416)	(7.3284)	(7.0972)	(7.1734)	(2.5611)	(2.3069)	(2.5704)	(2.3088)
$log(MarketCap)$	-0.1055	-0.0962	-0.0829	-0.0720	-0.1055	-0.0962	-0.0829	-0.0720
	(-1.0394)	(-1.1917)	(-0.8077)	(-0.8415)	(-0.8994)	(-0.8461)	(-0.7762)	(-0.6943)
$RunUp_{5Days}$	-13.198***	-12.768**	-13.307***	-12.863***	-13.198***	-12.768**	-13.307***	-12.863**
	(-3.6274)	(-2.5813)	(-3.6098)	(-2.5976)	(-2.7016)	(-2.4824)	(-2.7208)	(-2.4825)
Time FE	*yes*		*yes*		*yes*		*yes*	
Industry FE	*yes*	*yes*			*yes*	*yes*		
Std. Errors	*clustered*	*clustered*	*clustered*	*clustered*	*robust*	*robust*	*robust*	*robust*
R^2	0.0850	0.0821	0.0845	0.0809	0.0850	0.0821	0.0845	0.0809
Obs	311	311	311	311	311	311	311	311

Table 2.6: Base Regression with different fixed effects and standard errors
This table reports regression estimates for the base regression and alternatives using different combinations of fixed effects and calculation of standard errors. Variables are defined the same as in table 2.2. Models (1) to (4) use standard errors clustered on industry level and variate fixed effects. Models (5) to (8) use White (1980) heteroskedasticity robust standard errors and variate fixed effects as well. As in previous regressions, all variables are winsorized at the 1% and 99% percentile. t-statistics are reported in parentheses, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

2.5 Conclusion

This paper set out to study the effect of "unofficial" marketing and reduced information asymmetry by a reduced time distance between SEO announcement and publication of financials on SEO discounts. Given the tight guardrails for official deal-related marketing in German SEOs without subscription rights, a transaction announced shortly after the last publication of financials by the issuer could be expected to have a lower discount than transactions with "older" financials. Both theories of information asymmetry and demand elasticity point in this direction. However, sophistication of institutional investors could negate those effects.

Based on our empirical analysis, we are able to find a small, but statistically significant linear relation in this regard. However, this relation is only visible when including an interaction with the issues demand elasticity. The linear effect is then overlapped with a nonlinear effect introduced by the interaction. Taken together, both effects predict no meaningful impact of the time distance for the most common transactions. For transactions with very inelastic demand, an even opposite relation is predicted, although this is based on relatively few observations.

Further robustness checks where not able to negate this finding. They rather underscored the fragility of the relation found, further pointing to no meaningful impact of time passed since the last publication of financials on SEO discounts.

Hence, it seems that the sophistication of the institutional investors involved in this kind of transactions is so high, that the time passed since a financial publication date does not matter. Investors seem to be able to stay informed regardless of those dates and our analysis does not show a "marketing effect".

Another possible explanation is the existence of other non-deal related elasticity increasing corporate events, such as non-deal roadshows, conference attendances or investor days. An SEO announcement could have a large time distance to the last publication of financials, but a short one to any of those events, without us being able to see the difference. Those events were not analysed due to the scarcity of available data for German issuers in this regard.

A further possible explanation is that the time difference to published financials is not as important as the financials themselves and the resulting share price reaction.

This would partially be captured by our factor $RunUp_{5Days}$, but would negate any influence of the time distance itself.

Our analysis is based on a relatively low number of observations, given by the low number of relevant transactions in Germany. Further research could be done to expand our analysis to other markets, although adjustments would need to be made to account for differences in the institutional setting. This further research might help clarify the underlying mechanisms, by which institutional investors keep themselves informed in absence of new financial information.

Insider filings as trading signals - does it pay to be fast?

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May 2024

Abstract

We test an intraday trading strategy based on SEC Form 4 insider trading filings in the post Sarbanes-Oxley Act period. Using tick data, we analyze whether a prompt reaction to the announcement would earn abnormal returns. We find positive but lower abnormal percentage returns than in previous studies for short holding periods, but they disappear and even become negative when limiting the tradable dollar amount for each signal to a reasonable size. Moreover, we find that the returns in our setup are negatively correlated with stock liquidity, negating any potential profitable and scalable trading strategy even before considering transaction costs.

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3.1 Introduction and Literature

According to a large body of literature, corporate insiders reveal significant information to investors through their trades, whose announcements are associated with subsequent significant abnormal returns. For US stocks, this has been documented in a pre-Sarbanes-Oxley period (Lakonishok and Lee, 2001; Jeng et al., 2003; Aktas et al., 2008), with the effect becoming stronger after the tightening of disclosure rules by this reform (Brochet, 2010; Tavakoli et al., 2012). Similar results have been found for Europe in general (Aussenegg et al., 2018; Dardas and Güttler, 2011), for Germany (Betzer and Theissen, 2009), and for the UK (Fidrmuc et al., 2006; Friederich et al., 2002). The absence of insider trades also conveys information (Marin and Olivier, 2008).

However, there are differences in the amount of information conveyed, and thus in the subsequent stock returns. Although insider purchases convey new information, insider sales are less informative (Lakonishok and Lee, 2001; Brochet, 2010; Dardas and Güttler, 2011; Tavakoli et al., 2012) and further lose informativeness in a market environment influenced by algorithmic trading (Chang et al., 2022). Furthermore, insider purchases are found to earn significant positive abnormal returns for insiders themselves, whereas sales do not (Jeng et al., 2003).

Within insider purchases, several additional factors are found to explain abnormal announcement returns. These include, i.a., the position of the insider within the firm (Fidrmuc et al., 2006; Betzer and Theissen, 2009; Cohen et al., 2012; Tavakoli et al., 2012) and if the trade is classified as routine or opportunistic (Cohen et al., 2012; Amel-Zadeh et al., 2019)[6].

Although the announcement effects of insider transactions are mostly undisputed, the question of whether outsiders are able to gain abnormal returns by reacting to these announcements has not yet been clearly answered. Bettis et al. (1997) found that outsiders would be able to earn significant positive returns by copying insider trades. In more recent studies for Germany (Dickgiesser and Kaserer, 2010) and the UK (Friederich et al., 2002) the opposite has been found: When considering arbitrage

[6]For further factors see Roth and Saporoschenko (1999); Fidrmuc et al. (2006); Betzer and Theissen (2009, 2010); Dardas and Güttler (2011); Cohen et al. (2012); Tavakoli et al. (2012); Fidrmuc et al. (2013); Aussenegg et al. (2018); Sabherwal and Uddin (2019)

risk or transaction costs in terms of bid-ask spreads, no significant return could be found for outsiders. Both studies use daily data, leaving the potential for a profitable trading strategy when reacting fast enough, that is, right after the announcement.

Intraday stock data has been used in an insider trading context by Inci et al. (2010) and Aktas et al. (2008), but in both cases focused on the actual time of the insider trade, not around the time of publication of this trade. According to Aktas et al. (2008), the discovery of the price after the publication of insider trades is not instant, but takes several days after the reporting. Rogers et al. (2016) found that prices adjust more rapidly to insider trading SEC filings when there is accompanying media coverage, indicating that not all market participants react to the primary source of information, potentially giving an exploitable advantage to those who do.

We build on the existing literature by using the identified drivers of high announcement returns mentioned above to filter for the most promising insider filings. We then test a simple long stock - short market strategy based on intraday data and a fast response to the signal. We analyze both abnormal percentage returns based on buying at the volume-weighted average price (VWAP) within 30 minutes[7] following the exact publication time of a filing ("buying window") and US dollar (USD) returns, taking into account the tradable amount of a stock at the time of filing. USD returns are defined as follows (with t-1 indicating the buying window and V the total USD trading volume for this stock within this period; more details in Section 3.2):

$$r_{\text{USD, t}} = r_{\%,t} \cdot V_{\text{USD, t-1}} \cdot 0.25 \tag{3.1}$$

USD returns in the context of insider trading have previously been analyzed (Aktas et al., 2008; Cziraki and Gider, 2021), but for the insiders themselves. The latter study finds that in this context the "correlation between dollar gains and percentage returns is moderate". If this is also true for outside trades that copy the insider, abnormal percentage returns might not translate into meaningful USD returns.

To the best of our knowledge, this is the first study to examine the USD returns of outsiders emulating insider trades immediately after the announcement of the filing. Our results negate a profitable trading strategy based on these signals, even

[7]For robustness purposes, 5 and 60 minutes have also been tested.

before considering transaction costs and despite positive average percentage returns for shorter holding periods. This provides relevant information for categorizing the economic relevance of insider transactions and new evidence on the possibility of implementing a trading strategy based on the announcement effects of insider filings.

3.2 Data and Methodology

Single stock quotes are taken from polygon.io using VWAP and volumes of one minute intervals. High-frequency S&P 500 benchmark returns are obtained from FirstRate Data. For our analysis, we used one-minute bars from the continuous futures time series. We did not employ any statistical cleaning technique on the single-stock and benchmark data. Extreme values in single-stock data have been manually checked one by one and erroneous data points have been corrected[8].

Form 4 submissions are sourced directly from SEC EDGAR[9]. To obtain the strongest and most informative possible signal, we filtered the filings based on criteria identified in the literature discussed in Section 3.1:

1. Only purchases, no sells or mixed transactions[10]
2. The reporting person is Director or Officer
3. Only filings which have been disclosed no more than 2 trading days after the trade
4. Only discretionary trades[11]

Based on those filters, we obtain a total data set of 58,732 filings between November 2018 and November 2023.

[8] This only affects data errors caused by incorrectly calculated (reverse) stock splits.

[9] The Electronic Data Gathering, Analysis and Retrieval (EDGAR) system of the U.S. Securities and Exchange Commission. Although commercial databases also offer Form 4 data, they sometimes contain minor errors (Sidgman, 2015)

[10] See Lakonishok and Lee (2001); Brochet (2010); Dardas and Güttler (2011); Tavakoli et al. (2012)

[11] Filtered by transaction code "P": "Open market or private purchase of non-derivative or derivative security", used in the spirit of Marin and Olivier (2008), similar in Amel-Zadeh et al. (2019), Cohen et al. (2012)

Abnormal percentage and USD returns have been calculated for different time periods by emulating a simple long-stock - short-market strategy, taking into account the stock's 1-year beta derived from the Fama and French (2015) five-factor-model and ignoring transaction costs. Risk factors are obtained from Kenneth French's website[12]. The exact time of publication provided by the SEC's acceptance date time was used to determine a potential purchase price and volume for an outside investor.

We calculate the returns as follows:

- The buying price has been set as the VWAP in the 30 minutes following the time of publication[13]

- The tradable volume for calculating USD returns has been set at 25% of the individual stocks' trading volume in that time frame. This is an arbitrary assumption, however, any other percentage value would simply lead to a difference in scaling of our results

- The "market" selling price has been set at the VWAP of S&P 500 futures in the 30 minute time frame defined above. Market returns times beta have been subtracted from stock returns to obtain abnormal returns

The holding period differs from "until market close of the same trading day ($t_{0\ days}$)" to "until market close 20 trading days later ($t_{20\ days}$)" with steps $t_{1\ day}$, $t_{5\ days}$ and $t_{10\ days}$ in between. We assume that we will sell our positions in full at the official closing price, ignoring any market impact from this transaction (as opposed to our buy window). If the filing has been published outside normal trading hours or less than 30 minutes before market close, the 30 minute window starts at the beginning of the (next) trading day. This approach has been chosen due to the limited liquidity during extended trading hours – especially for small cap stocks – and the fact that the SEC accepts live submissions only until 5:30 pm.

After ignoring filings where there has been no trading activity in the specified

[12] `https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html`

[13] 30 minutes have been used, as Chordia et al. (2005) found it takes "more than five minutes but less than sixty minutes" for sophisticated investors to undertake enough countervailing trades in the face of order imbalances to incorporate information; for robustness purposes, 60 minutes and 5 minutes have also been tested. Results for 5 minutes can be found in the Appendix

time window or where price and volume data were not available, we are left with 48,704 observations. In multiple cases, there have been several filings a day for the same company by different individuals in a short time frame (i.e. minutes after one another). To avoid overlapping buying windows and double counting, we only used the first filing of the day for each company, leaving us with a total of 25,636 observations.

3.3 Results

3.3.1 Abnormal Returns

In line with the literature, Table 3.1 reports a positive significant abnormal return, but only up to $t_{5\ days}$, with negative insignificant values for $t_{10\ days}$ and negative significant values for $t_{20\ days}$. The returns reported below are smaller than those of some previous studies. However, based on the positive abnormal mean returns for shorter periods and their statistically significant difference from zero, a profitable trading strategy could be expected.

Abnormal percentage returns when selling the position at close on...					
	$t_{0\ days}$	$t_{1\ day}$	$t_{5\ days}$	$t_{10\ days}$	$t_{20\ days}$
count	25,636	25,636	25,636	25,636	25,636
mean	0.21%	0.33%	0.23%	-0.05%	-0.40%
p-value	0.0000***	0.0000***	0.0016***	0.5895	0.0010***
t-statistic	7.61	7.65	3.15	-0.54	-3.30
std	4.45%	6.86%	11.56%	15.76%	19.32%
min	-51.17%	-66.55%	-78.22%	-77.32%	-85.44%
25%	-1.28%	-2.15%	-4.25%	-6.02%	-8.73%
50%	0.02%	0.00%	-0.21%	-0.55%	-1.02%
75%	1.42%	2.35%	3.85%	4.78%	6.13%
max	95.12%	158.56%	377.31%	942.16%	631.01%

Table 3.1: Summary statistics abnormal percentage returns
This table reports summary statistics of abnormal percentage returns achieved when buying stock at the 30 minute VWAP after the publication of an insider trade and selling at the close price after the shown number of trading days. Abnormal returns are calculated by subtracting the FF-5-Factor beta weighted S&P 500 return. p-values and t-statistics shown are associated with tests for the statistical significance of the differences from zero, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

As reported in Table 3.1, the distribution of the returns contains sizable outliers both negative and positive. Although there is a tendency in the academic literature to clean high-frequency data with statistical methods (Olsen, 2001; Brownlees and Gallo, 2006), our intention is to test the practical implementation of a potential trading strategy. In consequence, we need to keep even extreme values as long as they reflect actual trading activity. Consequently, we conducted a manual review of the 10% most extreme outliers in our data set to ensure that they genuinely represent real trading activity.

The results for percentage returns discussed above change drastically when switching the calculations to USD returns, even without including transaction costs. Table 3.2 reports the resulting distributions using the 25% trading volume approach discussed in Section 3.2. To the best of our knowledge, this is the first study to look at USD returns of outsiders that emulate insider trades.[14]

Abnormal USD returns, when selling the position at close on...					
	$t_{0\ days}$	$t_{1\ day}$	$t_{5\ days}$	$t_{10\ days}$	$t_{20\ days}$
count	25,636	25,636	25,636	25,636	25,636
mean	2,565	3,234	-6,365	-12,071	-21,030
p-value	0.1348	0.1931	0.2077	0.0217**	0.0037***
t-statistic	1.50	1.30	-1.26	-2.30	-2.91
std	274,615	397,810	808,897	841,755	1,158,539
min	-12,403,180	-20,348,528	-70,597,359	-49,363,899	-129,854,089
25%	-669	-1,295	-2,788	-4,101	-6,264
50%	1	0	-7	-25	-56
75%	980	1,496	2,310	2,696	2,965
max	22,665,359	43,366,298	43,781,670	63,844,867	48,573,759

Table 3.2: Summary statistics abnormal USD returns
This table reports summary statistics of abnormal USD returns achieved, when buying stock in the volume of 25% of the trading volume in the 30 minutes after the publication of an insider trade at the VWAP of that time frame and selling at close after the shown number of trading days. Abnormal returns are calculated by subtracting the FF-5-Factor beta weighted S&P 500 return from the percentage return before multiplying by the above mentioned USD trading volume. p-values and t-statistics shown are associated with tests of statistical significance of differences from zero, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

We find that the mean abnormal USD returns for the holding periods $t_{0\ days}$ and $t_{1\ day}$ are positive, but not statistically significant with values of less than 3,300 USD. It should be noted that this is not a scalable profit, as it already hypothetically consumes 25% of the trading volume on every available signal. USD returns for $t_{5\ days}$ turn negative (but still insignificant) despite positive and significant percentage returns, and the USD returns for $t_{10\ days}$ and $t_{20\ days}$ are both statistically significant and negative. In short, although we find positive percentage returns for some periods,

[14]Cziraki and Gider (2021) and Aktas et al. (2008) have analyzed USD returns of insider trades, but for the insiders themselves and with their corresponding trade volume

the resulting USD returns are not significantly different from zero or even negative. Furthermore, since the median returns for shorter holding periods are almost zero, a trading strategy would be no better than a coin flip, suggesting that the positive mean is influenced by outliers. Figure 3.1 shows the returns for different holding periods and highlights their substantial and increasing volatility over time.

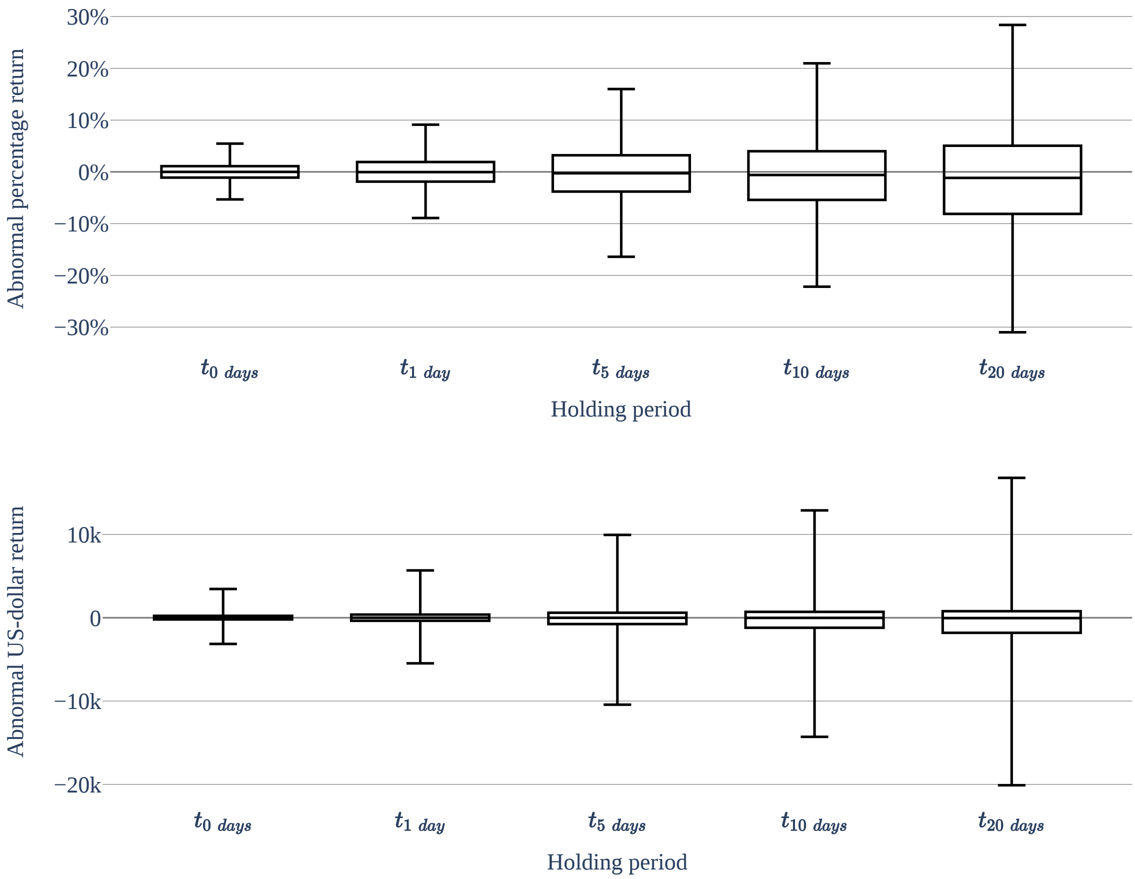

Figure 3.1: Distribution of percentage and USD returns for different holding periods
These figures show the distributions of the abnormal percentage returns and the abnormal returns achieved in USD when buying stock at the VWAP price of 30 minutes after the publication of an insider trade and selling at close after the indicated number of trading days. Abnormal returns are calculated by subtracting the FF-5-Factor beta weighted S&P 500 return. Returns in USD are calculated by multiplying by 25% of the USD trading volume in the buying time window. Boxes indicate the central 50% of the data, from the first to the third quartile. Whiskers extend to the smallest and largest value within 1.5 times the interquartile range. Outliers beyond those points have been excluded to increase readability.

In additional analyses, we examined buying windows of 5 minutes (included in the Appendix) and 60 minutes instead of 30 minutes and found similar results. Percentage returns are worse for longer buying periods and better for shorter time frames, supporting our hypothesis that being fast helps, but apparently not enough to generate sufficient USD returns. Naturally, both negative and positive USD returns moved closer to zero with a shorter window, as only smaller positions could be bought. In additional unreported results, we also excluded observations from the year 2020 from our analysis to eliminate a potential influence of the COVID crisis. Again, our main findings were unaffected.

3.3.2 Connection between trading volume and returns

Based on the results shown in the previous section, a link could be assumed between a stock's trading volume and the abnormal return to be gained after an insider trading publication. The discrepancy between percentage and USD returns could be explained if high percentage returns tend to be associated with less liquid stocks, having a lower impact on USD returns.

For Germany, Dickgiesser and Kaserer (2010) did not find an effect of trade volume on abnormal percentage returns, while Dardas and Güttler (2011) found that company size (correlating with volume) has a direct and negative influence on announcement returns "for most countries" in Europe. This could be explained by a higher degree of information asymmetry between insiders and outside investors in smaller companies with less liquid stock. Several studies found that a higher degree of information asymmetry (measured by various proxy) leads to higher announcement returns (Cohen et al., 2012; Alldredge and Cicero, 2015; Alldredge and Blank, 2024).

In summary, a negative connection between size (or liquidity) and abnormal percentage returns should not be surprising and is indeed what we find as well (see Table 3.3). However, as Cziraki and Gider (2021) have pointed out, there is not necessarily a correlation between percentage and USD returns, and thus the influence on USD returns remains an open question.

Table 3.3 aims to address this question by dividing our data set into the upper and lower quartile, and the middle half, measured by the trading volume of the respective stocks in USD in the two trading days before the insider trade announcement. We repeat this analysis for the shortest, longest, and middle holding periods. As expected, mean percentage returns are highest for the lower quartile and lowest (and indeed negative for $t_{5\ days}$ and $t_{20\ days}$) for the highest quartile, with the middle half falling in between. The same does not hold for USD returns: Both extremes, the lower and upper quartiles, show negative USD returns (with the exception of $t_{0\ days}$). However, the middle half shows a positive USD return in addition to its positive percentage return in two cases. A trading strategy focusing only on mid-cap or mid-liquid stocks would still not be advisable, as the mean USD return is not statistically significant, ranging from USD 70 to USD 1,378, and the median is close to zero. Bearing in mind that we already assumed to be able to capture 25% of all relevant trades in our trading window and ignored transaction costs, this strategy would still be neither scalable nor feasible.

	Panel 1: Zero days holding period ($t_{0\ days}$)					
	Lower 25%		25% - 75%		Upper 25%	
	%	*USD*	%	*USD*	%	*USD*
mean	0.28%	-529	0.22%	70	0.13%	10,648
p-value	0.0000***	0.5000	0.0000***	0.8800	0.0126**	0.1100
t-statistic	4.36	-0.67	5.83	0.15	2.50	1.58
std	5.10%	62,872	4.30%	51,810	4.05%	540,628
min	-51.17%	-2,530,841	-35.32%	-5,357,283	-47.85%	-12,403,180
25%	-1.59%	-43	-1.27%	-710	-1.03%	-13,963
50%	-0.04%	-0	0.03%	5	0.05%	470
75%	1.62%	41	1.46%	911	1.19%	16,871
max	93.97%	3,584,979	65.14%	1,144,151	95.12%	22,665,359

	Panel 2: Five days holding period ($t_{5\ days}$)					
	Lower 25%		25% - 75%		Upper 25%	
	%	*USD*	%	*USD*	%	*USD*
mean	0.83%	-1,797	0.29%	1,378	-0.50%	-26,420
p-value	0.0000***	0.0309**	0.0061***	0.1500	0.0002***	0.1900
t-statistic	5.87	-2.16	2.74	1.45	-3.71	-1.31
std	11.34%	66,632	12.01%	107,223	10.82%	1,609,219
min	-78.22%	-3,295,692	-69.51%	-7,810,168	-64.35%	-70,597,359
25%	-4.12%	-111	-4.37%	-2,744	-4.18%	-60,049
50%	-0.12%	-1	-0.26%	-48	-0.19%	-1,636
75%	4.67%	135	4.00%	2,402	3.10%	42,517
max	105.24%	89,582	377.31%	5,357,849	214.32%	43,781,670

	Panel 3: 20 days holding period ($t_{20\ days}$)					
	Lower 25%		25% - 75%		Upper 25%	
	%	*USD*	%	*USD*	%	*USD*
mean	0.48%	-1,581	-0.67%	-369	-0.73%	-81,802
p-value	0.0871*	0.3000	0.0001***	0.6900	0.0004***	0.0046***
t-statistic	1.71	-1.04	-3.97	-0.40	-3.57	-2.84
std	22.28%	121,249	19.08%	105,285	16.34%	2,308,176
min	-85.44%	-6,444,538	-81.59%	-5,426,065	-82.07%	-129,854,089
25%	-9.20%	-258	-9.13%	-5,919	-7.46%	-109,206
50%	-0.98%	-10	-1.22%	-291	-0.63%	-5,760
75%	7.11%	174	6.04%	3,115	5.61%	76,129
max	631.01%	5,475,783	352.40%	5,312,965	259.54%	48,573,759

Table 3.3: Returns at three different holding periods for the lower and upper quartile and middle half by trading volume before insider trade publication
This table reports the same values as tables 3.1 and 3.2 for different holding periods, but divided into subsets for the lower and upper quartile and middle half of the original data set with respect to the stocks' trading volume in the two trading days before publication. p-values and t-statistics shown are associated with tests of statistical significance of differences from zero, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

A linear regression analysis of the abnormal percentage returns on the liquidity of stocks before the announcement shows a significant negative influence for $t_{0\,days}$ to $t_{10\,days}$ (see Table 3.4). Including control variables based on the studies discussed in Section 3.1 does not change this relation. As most of the filings in our sample have been published outside of trading hours and we started our trading window the next trading day, we included a dummy variable *Overnight* to control for a potential bias. This inclusion also did not change the above relationship. The tables below show only the full regression model for all the holding periods analyzed. In unreported results, we individually added control variables to a univariate model. None changed the influence of stock liquidity on returns.

Dependent variable: Abnormal percentage return, when selling the position at market on close...					
	$t_{0\,days}$	$t_{1\,day}$	$t_{5\,days}$	$t_{10\,days}$	$t_{20\,days}$
$log(TradeVol)$	-0.0003*	-0.0008***	-0.0017***	-0.0017***	-0.0007
	(-1.79)	(-2.93)	(-3.72)	(-3.31)	(-1.23)
$IsOfficer$	0.0005	0.0056	0.0107	0.0095	-0.0239**
	(0.18)	(1.41)	(1.54)	(1.02)	(-2.09)
$log(InsideVol)$	0.0005**	0.0014***	0.0022***	0.0018***	-0.0002
	(2.32)	(4.04)	(4.30)	(2.93)	(-0.23)
$IsOff : log(InsVol)$	-0.0000	-0.0004	-0.0010	-0.0011	0.0018*
	(-0.12)	(-1.12)	(-1.58)	(-1.34)	(1.67)
$Overnight$	0.0001	-0.0029**	-0.0030**	-0.0042**	-0.0079***
	(0.14)	(-2.40)	(-2.00)	(-2.12)	(-3.10)
R^2	0.0011	0.0017	0.0030	0.0069	0.0134
Observations	25,636	25,636	25,636	25,636	25,636

Table 3.4: Influence factors for abnormal percentage returns - regression results
This table reports multivariate linear regression results for the abnormal percentage return when buying within 30 minutes of publication of an insider trade and selling at close after the indicated number of trading days. Returns as explained variables have been log-transformed to continuous returns. Explanatory variables are defined as follows: $log(TradeVol)$ is the natural log of the trading volume in the given stock in USD during the last two trading days before the publication of the insider trade plus one. $IsOfficer$ is a dummy variable that is equal to 1 if the reporting insider is an Officer in the respective company. As the data set only contains trades by Officers or Directors, a value of 0 indicates a Director position. This information is provided directly in the Form 4 filings. $log(InsideVol)$ is defined as the natural log of the reported USD volume of the insider trade plus one, $IsOff : log(InsVol)$ is the interaction of the last two variables. *Overnight* is a dummy variable that is equal to 1, if the filing has been published near market close or outside of trading hours and the trading window was started at the beginning of the next trading day. Standard errors are clustered by time (by day). t-statistics are reported in parentheses, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

When repeating the same analysis for USD returns, the significance of all independent variables vanishes (see Table 3.5). Therefore, a trading strategy optimized on the basis of prior percentage-return related research may not be appropriate for generating USD returns. This weak connection between percentage and USD returns is also expressed by a low correlation, ranging from 0.12 to 0.14, and low R^2 values for regressions of one in the other, ranging from 0.015 to 0.020. Those findings further underscore that a trading strategy based on insider trade reports would not be profitable.

Dependent variable: Abnormal USD return, when selling the position at market on close...					
	$t_{0\ days}$	$t_{1\ day}$	$t_{5\ days}$	$t_{10\ days}$	$t_{20\ days}$
$TradeVol$	0.0001	0.0002**	-0.0001	-0.0001	-0.0007
	(1.28)	(2.43)	(-0.18)	(-0.36)	(-1.42)
$IsOfficer$	286.1	4,914.7	-2,531.8	-6,949.5	-7,991.5
	(0.11)	(0.94)	(-0.42)	(-0.91)	(-0.91)
$InsideVol$	0.0000	0.0000**	0.0000	-0.0000	-0.0000
	(0.69)	(1.98)	(0.14)	(-0.09)	(-0.21)
$IsOff : InsVol$	0.0000	0.0001	-0.0000	-0.0001	-0.0001
	(0.74)	(0.87)	(-0.22)	(-1.10)	(-0.73)
$Overnight$	-9,905.9	-19,896.9**	-211.7	-400.5	53,322.0
	(-1.07)	(-2.41)	(-0.01)	(-0.01)	(1.05)
R^2	0.0639	0.0795	0.0024	0.0065	0.1333
Observations	25,636	25,636	25,636	25,636	25,636

Table 3.5: Influence factors for abnormal USD returns - regression results
This table reports multivariate linear regression results for the abnormal USD return when buying within 30 minutes of publication of an insider trade and selling at close after the indicated number of trading days. Explanatory variables are defined as in table 3.4, with the difference that neither the returns nor any of the explanatory variables have been logarithmically transformed, to allow a more intuitive interpretation of the coefficients. Standard errors are clustered by time (by day). t-statistics are reported in parentheses, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

3.4 Conclusion

This paper aims to examine whether the often cited positive abnormal returns following insider trading publications can be exploited in a trading strategy by reacting as early as possible to these signals. The selection of trading signals is based on previous empirical research, which mainly focuses on percentage returns.

Our results indicate a statistically significant positive percentage return, consistent with previous studies, when a simple Fama-French-Beta-adjusted long-stock-short-market strategy is employed. However, this strategy does not generate significant positive USD returns when considering the volume of stocks that could realistically be acquired based on the signal. In fact, the average USD return becomes negative for both high- and low-volume stocks, while intermediate cases yield an insignificant positive return. Contrary to what the percentage returns might imply, these USD returns are not scalable, as we have already assumed that we will be able to capture 25% of the total trading volume in the period immediately following the filing announcement.

Returns based on our immediate response to the signal with a 30-minute buy window outperform those of end-of-day trades, and a 5-minute window shows better percentage returns, suggesting that rapid execution yields better results. However, none of the USD returns were sufficient for a profitable trading strategy and most were negative. We conclude that a trading strategy based on insider trade reports and optimized according to the results of previous research is neither practical nor scalable. This finding holds even before considering transaction costs or risk-adjusted returns.

Our study focuses on the US market. Given differences in disclosure requirements and market efficiency across countries, further research in other markets may be necessary.

Accuracy of research analyst estimates surrounding M&A-transactions

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Abstract

We find a significant increase in analyst forecast errors after announcements of corporate takeovers, with analysts overestimating earnings per share more than before. This finding is robust with alternative measurements and with respect to industry and firm characteristics. We show that the effect is more pronounced for transactions with a large relative size. It is less pronounced for buyers with a larger analyst following and more positive share price reactions to the announcement. The results shed light on the informational role that analysts play after announcements of large mergers and acquisitions.

Keywords: Mergers & Acquisitions, Analyst Estimates, Research Estimates, Firm Complexity

JEL classification: G34, G17, G14

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4.1 Introduction

Previous research has uncovered consistent and persistent differences in the accuracy of sell-side research analyst[15] estimates, without providing a complete understanding of the underlying causes for these differences. In this study, we analyze the impact of large and sudden changes in the analyzed firm, in our case larger mergers and acquisitions (M&A) transactions, on analysts' earnings forecasts. To this end, we examine the differences in average forecast errors (FE) before and after the announcement of such transactions. Through this, we aim to fill a part of the existing gaps in the understanding about the reliability of analyst reports. As analyst estimates have an important impact on many decision-making processes, our findings are expected to have practical implications for investors, firms, and other capital markets participants.

In the context of larger M&A transactions, we assume that the accuracy of analysts' earnings per share (EPS) estimates for acquiring companies decreases. We measure accuracy by using the average FE, i. e. the percentage deviation of a consensus estimate from its corresponding actual value. Analysts are generally faced with considerable challenges, as they have to forecast numerous factors simultaneously. The inherent complexity of this task is further intensified by major events such as M&A transactions, as they generate considerable uncertainty and increase information asymmetries between management and analysts. Based on prior research, we postulate that the expected increase in complexity and thus in FE is influenced by the specific conditions of a transaction. In order to gain a deeper understanding of the changes in FE for the acquiring company due to an M&A announcement, this study examines various scenarios, transaction characteristics and profiles of the acquiring and target companies. To draw appropriate conclusions about the magnitude and direction of the change in FE, we conduct a broad empirical study encompassing 2,612 M&A transactions with US listed acquirers between 2004 and 2022.

Our primary finding is that large M&A announcements have a highly significant increasing impact on the average FE of analysts' estimates of acquirers' EPS, driven by too optimistic estimates. This result is robust to incorporating different control

[15] In the following, we use the short version "analyst" to refer to sell-side equity research analysts covering the acquiring firm.

variables, fixed effects, and alternative measures. We find higher relative deal values to be associated with a stronger increase in FE. A higher number of covering analysts conversely tends to have a dampening effect on the increase in FE. Additionally, a positive perception of transactions by capital markets, as indicated by positive cumulative abnormal returns (CAR) around the announcement, is also associated with a lower increase in FE. Our analysis indicates that the target public status, M&A type, cross-border status and acquirer market capitalization are not significantly influencing this change in FE, despite different expectations based on previous research.

To the best of our knowledge, our research is the first broad, quantitative analysis regarding the effects of M&A announcements on FE for the acquiring firm. This facilitates the generalization of our findings and of the corresponding practical implications. In this, our analysis differs from and adds to relevant previous research by Andersson et al. (2020), who choose a qualitative approach with specific case studies to answer a similar question. In contrast to our broader approach, they limit themselves to a smaller sample of three Swedish companies with high market-to-book ratios and high profitability. Barinov et al. (2024) and Brown et al. (2024) follow a more similar approach to our quantitative research. However, the first focus on firm complexity of conglomerates. Their primary conclusion is that increased organizational complexity impairs the market's and analysts' ability to accurately interpret and price earnings. Brown et al. (2024) focus on the information environment of industry peers after M&A-related delistings, not on the direct impacts regarding the accuracy of the acquirer's analysts.

Our results should be of interest to a wide range of capital market participants. In addition to demonstrating the increase in FE, we also quantify the extent of this phenomenon, highlight various scenarios and show the influence of certain transaction characteristics. This allows us to provide more insight into the informational role of analysts in the context of large M&A transactions, which can increase firm complexity and information asymmetries.

4.2 Background and prior research

4.2.1 Importance of research estimates

Equity research analysts are important intermediaries between capital market participants. They attract the attention of investors and issuers and have an impact on their decision-making processes. Thus, analysts' publications influence the performance of financial instruments and cause price reactions (Jegadeesh and Kim, 2006; Piotroski and Roulstone, 2004). Analysts provide equity and debt investors with important information about companies and help them select appropriate investments (Chen et al., 2017; To et al., 2018). In addition to interpreting earnings forecasts and recommendation revisions, markets also assess the depth of information provided in the analyst report. This applies in particular to downgrades (Asquith et al., 2005). Other studies have also shown that the market reacts more sensitively to negative information than to positive information. Although analysts are often accused of being overly optimistic, their reports receive significantly more attention in negative cases (Kothari et al., 2009; Womack, 1996).

Higher forecast accuracy helps to reduce information asymmetry between market participants. Analysts act as information brokers and external monitors, helping discipline managements, which enables more efficient investment decisions (Chen et al., 2017; To et al., 2018). This leads to lower costs of capital and allows companies to invest in more efficient projects (Amir et al., 2003; Ferrer et al., 2019; Jung, 2015; O'Brien, 1990; Chen et al., 2017). Chen et al. (2017) recognize that more accurate forecasts result in higher investments when companies previously tended to underinvest, and to lower investments when companies previously tended to overinvest. In this context, research by Balakrishnan et al. (2021) and Kothari et al. (2009) shows that analysts play a critical role in providing valuable benchmarks for assessing the cost of debt and equity and for valuing financial assets. They find a significant correlation between analysts' estimates of the cost of capital and key financial indicators such as beta, firm size, book-to-market ratio, and leverage.

Analyst estimates are also indicators of the market's return expectations. They are more accurate than historically based models and therefore represent a possible alternative to risk measures such as market beta (Gouret and Hollard, 2011; Givoly and Lakonishok, 1984). Kadan et al. (2012) find that analysts possess the ability to provide insights across various industries. This enables them to identify more targeted, sector-specific investment opportunities for investors. The integration of sector-specific and company-specific information enhances the value of analysts' insights. Thus, analysts play a critical role in forecasting investment returns and improving the effective and efficient allocation of capital.

Their role in reducing information asymmetry is also illustrated by Hutton et al. (2012). They find that analysts are able to generate informational advantages on a macroeconomic level. Analysts' EPS estimates are more accurate than management's forecasts when macroeconomic factors are closely related to a company's performance. In addition, analysts can reduce information asymmetries between professional and less experienced investors (Amiram et al., 2016). Professionals process new information faster, using their skills, resources and insider access. This gives professionals an informational advantage. Analysts help to disseminate new information to less experienced investors.

According to Bradley et al. (2017b) and Bradley et al. (2017a) analysts are not only important for the preparation of forecasts. They show that analysts with sufficient industry experience positively impact the quality of financial reporting, the appropriateness of management remuneration, and CEO change decisions by enhancing external monitoring and limiting management opportunism. Analysts also fulfill this monitoring role in the context of M&A. Cortes and Marcet (2023) show that transactions in which the acquirer and target are monitored by joint analysts are concluded less frequently. However, if they are closed, the acquirer's performance is higher than that of transactions without joint analysts. Joint analysts can also help reduce uncertainties about the acquirer's cash flows post acquisition. Through them, acquiring firms can gain information advantages or compensate for informational disadvantages in order to make a better target selection.

Equity research analysts are also relevant for their peers. Firstly, Jegadeesh and Kim (2010) identify a herding behavior among analysts, which means that they tend to follow the assessments of other analysts. This applies in particular to analysts from larger brokers, analysts who follow stocks with a lower spread of recommendations and analysts who revise their recommendations less frequently. If analysts deviate from consensus estimates, the share price reaction is stronger. Secondly, Mikhail et al. (1999) and Call et al. (2009) find that high forecast accuracy can positively influence analysts' careers. In contrast, low accuracy has a negative impact and increases the likelihood of termination.

4.2.2 Complexity of research estimates

While a wide range of market participants relies on analyst estimates, the process of creating them is influenced by a high degree of complexity. The prediction of future outcomes is inherently uncertain, relying predominantly on historical data, current expectations and company-specific insider information. Analysts undertake the task of sifting through substantial data to formulate their recommendations and prognoses. They gather insights from corporates and their management through various channels, including earnings calls, analyst briefings, conferences, and privileged communications (Brown et al., 2015). Brown et al. (2015) and earlier studies (e. g. Ruland, 1978) point out that the precision of an analyst's estimates is notably influenced by the depth of their access to insider information. Thus, the complex task of the analyst is to derive future predictions from partially incomplete and uncertain information.

Analysts play a crucial role in reducing the information asymmetry between the capital markets and a firm (Amir et al., 2003; Ferrer et al., 2019; Jung, 2015; O'Brien, 1990). However, they do not receive enough information to completely eliminate it. On the one hand, there is an information asymmetry between the managers of a firm and capital markets in general, which analysts can partially reduce. On the other hand, there is an information asymmetry between managers and analysts themselves. The reliability of the information provided by the management and, as previously mentioned, the degree of insider information have a significant impact on forecast errors (FE). Furthermore, analysts' forecasts are less accurate than management forecasts when the actions of the management are more difficult to predict. This phenomenon is particularly evident in companies that exhibit unusual inventory levels, excess capacity, or operating losses (Hutton et al., 2012).

Analysts also seem to be prone to behavioral biases, as are most market participants. Easterwood and Nutt (1999) for instance recognize that analysts tend to underestimate negative information and overestimate positive information. Ham et al. (2022) show that analysts tend to underweight short-term information in their forecasts for the current year. In contrast, they overweight information related to longer time periods. They emphasize that this imbalance is more prevalent when the more recent information is negative, which is consistent with the findings of Easterwood and Nutt (1999). These studies are consistent with other studies which generally attest that analysts are overly optimistic (Bradshaw et al., 2001, 2006). On the one hand, this may indicate a lack of ability on the part of analysts to process negative information correctly. On the other hand, it is possible that analysts receive less adverse information compared to positive information. A possible explanation is given by Richardson et al. (2004), who find that managers possess the ability to strategically influence analysts' expectations to their advantage.

The discussion in Call et al. (2009) further illustrates the complexity of research estimates. Complexity increases as the amount of information to be processed increases. However, limiting the scope to specific analyses, such as earnings forecasts, might use fewer resources but also ignores possible interactions and correlations. This restriction reduces the accuracy of estimates. Call et al. (2009) show that analysts exhibit a lower FE when they calculate both cash flow and earnings forecasts rather than just earnings forecasts. They link this to the fact that the combination of income statement, balance sheet, and cash flow statement forecasts represents a more structured approach. When analysts include all components of a financial statement in their estimates, they tend to gain a better understanding of the individual earnings components, such as operating cash flows. Estimating all three items together requires greater discipline in forecasting earnings because it requires the presentation of three financial statements.

However, analysts do not take all publicly available information into account (Cheng, 2005). In this context, it should also be considered that analysts must work with limited resources. Clement (1999) measures the skills and abilities of an analyst based on experience, the resources available based on the size of the employer, and the complexity of the estimate based on the number of companies and sectors observed by the analyst. He recognises that the FE is negatively correlated with experience and employer size. However, the FE is positively correlated with the number of companies and sectors observed by the analyst. The limited resources available consequently lead to an increase in complexity, which in turn results in less accurate estimates.

The FE rises, if more influencing variables have an impact on the position to be estimated and thus must be taken into account in the estimate. This increases FE for positions "further down" the balance sheet or profit and loss / cashflow statement. Studies such as Cheng et al. (2020) show that forecasting profit margins is more difficult than forecasting sales. The varying complexity of the methods used to calculate cash flows and financial ratios is illustrated by Hodder et al. (2008). With increasing complexity, analysts process less information. Reasons for this may be limited capacity and an efficient cost-benefit ratio (Plumlee, 2003). Chemmanur and Liu (2011) conclude that information processing is more challenging in more complex organizational structures. Their findings indicate that splitting an organization into smaller, more straightforward units with their own financial reports leads to lower information production costs for analysts. Additionally, with more focused companies, analysts can use their expertise more effectively. Chang et al. (2016) add in this context that the complex accounting treatment of derivatives leads to more inaccurate estimates. Nevertheless, they show that certain accounting standards help analysts to improve their forecasts. Filzen and Peterson (2015) show that as financial reporting becomes more complex, firms are more likely to exceed analysts' expectations. This complexity requires analysts to rely more on information provided by management. Their findings suggest that firms are aware of this and employ a strategy to reduce analysts expectations in order to beat them. As a result, this scenario exacerbates the challenges analysts face in accurately forecasting firm performance.

To illustrate the complexity of research estimates, previous literature has also referred to accruals. Although existing research has shown that high accruals tend to reduce earnings, Bradshaw et al. (2001) find that analysts do not take this into account. Using I/B/E/S consensus forecasts of annual earnings, they show that the FE for portfolios with high accruals is larger on average than for portfolios with lower accruals. They also find that the FE decreases over time. Analysts do not initially anticipate the impact of last year's accruals on the following year's earnings, and only recognize it in subsequent releases during the following year. This illustrates the complexity of making estimates over longer periods. Particularly in the case of increased uncertainty, which is reflected in higher accruals.

4.2.3 Expected impact of M&A-transactions on research estimates

4.2.3.1 Main hypothesis

The literature discussed above indicates that analysts play a significant role for capital market participants and serve as an important source of information. However, their ability to generate accurate forecasts is constrained by various uncertainties, limited resources, and information asymmetries. Major events can influence these factors, thereby increasing the difficulties for analysts. Consequently, we hypothesize that an increase in forecast error (FE) will be observed around M&A activity.

M&A transactions necessitate meticulous planning of the acquisition itself and the following post-merger integration process in order to achieve the often ambitious synergy goals. Previously separate management teams, divergent cultures, and the consolidation of reporting represent significant challenges. Consequently, merging companies is frequently more challenging and costly than anticipated (Renneboog and Vansteenkiste, 2019). For analysts, the complexity of the forecasts increases. The previous uncertainties that analysts have had to deal with are further intensified. At the same time, however, analysts gain broader access to new information, as M&A transactions are likely to be accompanied by extensive management communication. This prompts the question to which extent FE change in the course of M&A transactions.

As demonstrated by Barinov et al. (2024), the FE tends to increase with increasing company complexity, a phenomenon observed by them in growing conglomerates. An organizational structure that is difficult to understand and a broad portfolio with different business areas present analysts with greater challenges. This increased complexity would also be expected in M&A transactions, yet Barinov et al. (2024) do not find an increased FE due to M&A in their data. Instead, they emphasize that the ability of the market and analysts to accurately interpret and evaluate earnings is primarily affected by the inherent complexity of the conglomerate structure itself. This complexity has a greater impact than changes resulting from transaction events. The analysis suggests that while M&A transactions add complexity, it is the pre-existing organizational complexity that primarily causes the overall high FE for conglomerates. To show this, they measure share price drift after earnings announcements and define complexity using various measures, including the number of business segments and the company's diversification.

Brown et al. (2024) find that analyst performance in a given industry sector declines after an M&A transaction when both firms were previously publicly traded. This is explained by the fact that analysts lose an important source of information when one of the firms involved is delisted. As a result, the quality of their information environment deteriorates, leading to less information about the industry as a whole. In contrast to our research, they concentrate on the change in forecast accuracy for the peer group of the previously publicly listed target. We focus our analysis on the acquirer.

Andersson et al. (2020) examine the challenges equity research analysts face during M&A transactions due to the uncertainties and information asymmetries that characterize these periods. They consider M&A transactions as heterogeneous events in their analysis of three frequently acquiring firms. The study shows that analysts are often limited to a superficial analysis at the time of the announcement due to a lack of relevant information. It also shows that analysts' valuation methodologies are often too rigid to capture all the effects of transactions. The study by Andersson et al. (2020) aims to capture qualitatively how analysts deal with uncertainty and increased information asymmetry and what practices they use at different stages of takeovers. They do not provide quantitative data on the extent to which the challenges of M&A affect the accuracy of analysts' estimates. In addition, the limited number of observations makes it challenging to generalize their results. This is the gap we are filling with our research. Based on the findings of Andersson et al. (2020), Brown et al. (2024) and Barinov et al. (2024), we expect that the average FE increases following M&A announcements.

Hypothesis 1: *The FE for the acquirer increases after the announcement of M&A transactions.*

To gain a deeper understanding of the changes in FE for the acquiring company, we extend our analysis with additional hypotheses. This way we can take into account various scenarios as well as different transaction characteristics and profiles of the acquiring and target companies. Furthermore, we incorporate different fixed effects in our analysis. This comprehensive approach will help us identify key factors driving forecast inaccuracies and improve future estimates.

4.2.3.2 Additional hypotheses

Analysts do not process all available information. On the one hand, this is due to their individual selection of the information to be processed. On the other hand, they are unable to do so due to limited resources and the varying availability of (insider) information. The studies by Clement (1999) and Call et al. (2009), discussed in section 4.2.2, also show that resource capacity significantly influences the FE. According to them, the skills and resources of an individual analyst are limited. Fang and Hope (2021) recognize that analysts working in teams provide more accurate EPS estimates than individual analysts. They are able to cover more companies and publish more timely information. As a result, the market reacts more strongly to estimates provided by teams of analysts. Most importantly for our research, Li (2020) finds that firms which are followed by a large number of analysts are less overvalued than firms with low analyst coverage. Consequently, we assume that an increased number of analysts will result in a reduction of the FE in general and also in the change of FE, as information asymmetries should be less pronounced.

Hypothesis 2: *The FE for the acquirer and its M&A related change will decrease if the total number of analysts increases.*

In this context, it can be reasonably assumed that companies with a higher market capitalization will be observed by a greater number of analysts. Larger and more complex companies are also more in the public eye and are expected to publish more detailed information. This could lead to more efficient information processing and less information asymmetry.

> ***Hypothesis 3:*** *The FE for the acquirer and its M&A related change will decrease if the market capitalization of the acquirer is higher.*

Alexandridis et al. (2013) observe a negative correlation between the relative deal size and the buyer's return. Therefore, and in view of the aforementioned hypotheses, we posit that larger transactions are associated with greater complexity and consequently, a larger increase in FE. In line with Alexandridis et al. (2013), we utilize the relative size as a benchmark for measuring the transaction size. The relative size is defined as the ratio of the transaction volume to the market capitalization of the buyer one month before announcement date.

> ***Hypothesis 4:*** *The FE for the acquirer will increase more if the relative deal size is larger.*

Renneboog and Vansteenkiste (2019) show that acquirers in public takeovers perform worse than companies with no or non-public takeovers. The synergies of public takeovers are often overestimated and positive announcement returns often do not last. If analysts followed management expectations in their estimates, they should thus exhibit a higher FE. Additionally, as mentioned, Brown et al. (2024) observe the effect that the FE of a peer group increases when an M&A-related delisting takes place. This effect is particularly pronounced if the delisted firm had previously been an above-average contributor to the information environment. However, we take the acquirer's point of view in our study. Therefore, we conclude that a public target which has been an information provider for the peer group should also provide more information before the acquisition than a private target. As this reduces information asymmetry in the context of an M&A transaction, we therefore expect the increase in FE to be less pronounced for public targets.

> ***Hypothesis 5:*** *The FE for the acquirer will increase less if the target is publicly listed.*

Malloy (2005) suggests that analysts who are geographically closer to the subject have an informational advantage over other analysts. This implies lower FE for analysts who are geographically closer to the company than for other analysts. Jennings et al. (2017) confirm that focusing on firms in the same geographic area leads to lower FE. They combine geographical proximity with easier and cheaper access to information. Geographic focus therefore also has a positive effect on analysts' financial resources. According to Chen et al. (2010), country-specific factors also influence the information base of analysts and lead to country-specific differences in forecasting accuracy. International transactions do not exhibit a geographical focus. They increase geographical as well as cultural and other distances. Due to this, we expect transactions with a foreign target to lead to a larger increase in FE.

> ***Hypothesis 6:*** *The FE for the acquirer increases more in the case of cross-border M&A transactions (target outside the US) compared to national M&A transactions (target inside the US).*

In light of the above, the complexity of forecasts in individual sectors varies, necessitating the involvement of a diverse range of expertise (Kini et al., 2009). The majority of analysts are sector-focused (Dang et al., 2021). As explained in section 4.2.1, analysts with extensive industry experience are more likely to produce more accurate estimates, which points to differences between industries regarding estimates (Clement, 1999; Kadan et al., 2012; Piotroski and Roulstone, 2004). As demonstrated by O'Brien (1990), industry-specific characteristics, such as market conditions, regulatory density, economic volatility, and data availability and quality, can result in discrepancies in the precision of estimates. The findings by Kadan et al. (2012) indicate that analysts tend to exhibit greater optimism regarding sectors that exhibit higher levels of investment, earlier profitability, and earlier returns. A review of previous studies indicates that different sectors present varying challenges and degrees of difficulty for analysts. Thus, we posit that M&A transactions in sectors that are more challenging to estimate will also result in a larger increase in FE.

> ***Hypothesis 7:*** *The FE for the acquirer and its M&A related change are influenced by the industry sector of the acquirer firm.*

When analyzing the buyer's industry, it is also essential to consider the target's industry. The relationship between these industries is coded by us as the M&A type. We distinguish between horizontal, vertical and lateral acquisitions.[16] To the best of our knowledge, clear evidence regarding which type is more successful is not provided by existing research. The results by Fee and Thomas (2004) indicate that horizontal transactions can lead to enhanced production efficiency and augmented market power. Additionally, other studies demonstrate that horizontal transaction structures result in abnormal returns and economic success on average (Bernile and Lyandres, 2019). Raudszus et al. (2014) examine the M&A type for the construction industry. Their findings indicate that horizontal structures tend to have a positive effect on company value, vertical structures have a significantly positive effect, and lateral structures have a negative effect. Previous studies show that the success of transactions and the realization of synergies can depend on M&A type. Consequently, they could also have an effect on the complexity of estimates and FE. Therefore, analyzing the type of M&A is essential for a comprehensive understanding.

> ***Hypothesis 8:*** *The type of M&A transaction has an impact on the change in FE for the acquirer.*

Assuming a medium level of information efficiency, investors should be able to anticipate the success of M&A transactions to some extent. If that is the case, a positive perception of an M&A transaction could indicate the degree of realization of planned synergies. By extension, it could also reflect the accuracy of forecasts, as those synergies are likely included in the estimates. Bens et al. (2012) specifically examine the information asymmetry between management and shareholders following M&A transactions. They find that firms whose transactions are perceived negatively by the capital market and increase pressure on managers are more likely to misreport. With respect to the relationship between analysts and managers, these results suggest that analysts are also less well informed in such scenarios.

[16] horizontal structure: acquirer and target are both operating within the same macro industry and at the same stage of the value chain (i. e. mid industry); vertical structure: acquirer and target are both operating within the same macro industry but at a different stage of the value chain; lateral structure: acquirer and target are both operating within different macro industries.

Hypothesis 9: *The change in FE for the acquirer is higher for M&A transactions with lower cumulative abnormal returns around the announcement.*

In the following, our main hypothesis (section 4.2.3.1) and the additional hypotheses (section 4.2.3.2) are systematically tested through a series of comprehensive analyses.

4.3 Data and Methodology

4.3.1 Data Sources

M&A transaction data is taken from LSEG Deals Screener, which utilizes the SDC Platinum database. Deals are filtered using the following criteria[17]:

- Deals announced between 01/01/2004 and 12/31/2022 (to allow for analyst estimates up to 12/31/2023 in our main analysis)
- Only completed or unconditional deals (to filter out rumoured transactions)
- Acquirer listed on stock exchange in the USA (acquirer needs to be listed in order to have analyst estimates)
- Acquirer with headquarter in the USA (to filter for non-US companies with listing in the USA)
- Majority acquisition of target (to ensure full consolidation and thus impact on acquirer financials)
- Industry group "industrials" (no finance or education companies)
- Deal value $\geq$ 10m USD (only substantial deal size)[18]
- Deal value $\geq$ 10% of acquirer market value (significant deal size in relation to acquirer)
- Target parent company is not equal to acquirer or acquirer parent company (i. e. no intracompany deals)

[17] The data set is narrowed down based on the approach described by Ma et al. (2019). The threshold values are not applied in their original form; rather, they are adapted to align with the specific conditions of our research.

[18] We are aware that 10m USD in 2004 had a different weight than 10m USD in 2023. However, as this filter only has an insignificant effect on sample size, we choose not to adjust this factor for inflation over time.

These criteria lead to an initial dataset of 4,503 deals. After eliminating deals where data for one of our main variables, as outlined in section 4.4.2, were unavailable, we are left with 2,612 transactions as observations.

Analyst estimates and actuals for both quarterly earnings per share (EPS) and quarterly earnings before interest, taxes, depreciation and amortization (EBITDA) are also taken from LSEG, this time using the I/B/E/S database. Both estimates and actuals refer to the end of a calendar quarter. We choose to focus on quarterly rather than annual estimates to achieve more uniform distances between the M&A announcement dates and the first estimate thereafter in our dataset. This is important, as the time passed since the announcement should have an effect on the amount of information available to the analyst, which might possibly distort our results. The distance between an estimate and its corresponding actual value is always one year ahead. Estimates are consensus estimates. The number of individual analyst estimates contained in each consensus value is also obtained and used as a control variable. For each transaction and metric (EPS and EBITDA), eight pairs of pre-announcement estimates and actuals and eight pairs of post-announcement estimates and actuals are used. The last actual of the first eight pairs corresponds to the last quarter end before the announcement date. On the other end, the first estimate of the second eight pairs corresponds to the first quarter end after the announcement date. If the announcement is made within one month of the next calendar quarter end, the subsequent quarter end is taken instead. This is done to ensure that analysts have had enough time to incorporate the new information regarding the announced transaction into their estimates. The analysis presented in this paper focuses on the eight pairs of estimates and actuals directly around the announcement (i. e. four before and four after, corresponding to one year each). The four pairs each one year earlier and one year later are only used for a further longer-term trend analysis over four years in section 4.3.3. Consequently, deals with missing values in those outer time frames are not excluded from the main analysis.

Both estimates and actuals are taken from the same source (I/B/E/S) to ensure they have undergone the same adjustments, if any. Any per share data (in our case EPS and in supplemental analysis share prices) is adjusted for any capital measures and to the number of shares at the time of data retrieval. This eliminates any discrepancies caused by variations in the share count between the estimated and actual dates.

The I/B/E/S data are matched to the SDC platinum data by the PermID of the acquirer. In total, at least one pair of estimate and actual both before and after the announcement date and all main control variables in the relevant time frame could be obtained for 2,612 deals, which are included in our main analysis. A full set of four pairs each before and after the announcement was available for 1,821 deals. The main results of our analysis are virtually unchanged when restricted to this narrower dataset, therefore we focus on the broader one. Full results for the narrower dataset are available upon request.

4.3.2 Methodology

Our main variable of interest is the forecast error (FE) of analyst estimates. We define the FE as the percentage absolute deviation of a one-year-ahead (consensus) estimate for a quarterly company figure and calculate it as follows for our main analysis (with i for different companies and q for respective calendar quarters):

$$FE_{i,q} = \frac{|estimate_{i,q-1year} - actual_{i,q}|}{|actual_{i,q}|} \tag{4.1}$$

In further analysis in section 4.4.3.1, we also calculate a version of the FE, which retains the direction of the error. This enables us to infer if estimates are rather too optimistic or too pessimistic. By doing this, we can test if the results presented by Easterwood and Nutt (1999), Ham et al. (2022) and Bradshaw et al. (2001) (section 4.2.1) hold for our data as well. However, as this approach also leads to a netting of positive and negative errors when aggregating the data, we only use it in the supplemental analysis. Our calculation for the directed percentage FE is similar to the absolute percentage FE:

$$FE(directed)_{i,q} = \frac{estimate_{i,q-1year} - actual_{i,q}}{|actual_{i,q}|} \tag{4.2}$$

We consciously choose to analyze consensus estimates rather than individual analyst estimates. In this context, we refer to the findings of O'Brien (1990), which indicate that there is no evidence of consistent differences among analysts in terms of forecast accuracy. Additionally, in the I/B/E/S dataset, all analysts that are not identified by their name cannot be reliably identified over time by other means (Roger, 2017). This may result in a loss of data and potentially introduce a selection bias. As the focus of our study is not on characteristics or performance of individual analysts but rather the effect of an external event on analyst estimates for one company as a whole, we choose to avoid all identification problems by using consensus values. Therefore, the often used proportional mean absolute FE ($PMAFE_{i,j,t}$) established by Clement (1999), cannot be calculated for our data set. Nor would it be helpful for our intents, as it measures the performance of an individual analyst compared to other analysts for this company, and not the change in performance before and after an event. We are aware that I/B/E/S consensus estimate data is rounded to two figures after the decimal point, leading to a loss in precision (Payne and Thomas, 2003). The effect we find in our analysis is, however, economically large enough to be unaffected by this rounding.

For robustness purposes, Formula 4.1 is also adapted (1) by putting the absolute FE in relation to the share price rather than the absolute actual value and (2) by using EBITDA instead of EPS. Neither of those approaches changes the main outcome of our analysis. Further details can be found in section 4.4.3.

FE are analyzed both individually and as aggregates (averages) over quarters and one-year intervals. We compare pairs of estimates and actual values occurring before the announcement of a larger M&A-transactions with those occurring after it. The last pair before the announcement is the one, whose actual value refers to the last calendar quarter end before the announcement date. The first one after the announcement is the one whose estimate value has been published on the first calendar quarter end after the announcement date (or one quarter later, if the announcement date has been close to a calendar quarter end). We do not include pairs, where the estimate has been made before and the actual value refers to a point after the announcement date. It is reasonable to assume that those pairs naturally should have a larger FE, as the M&A-transactions have not been known at the time of the estimate's publication. However, it is possible that they already have an influence on the actual value.

After comparing the means of FE before and after the announcement, several multiple regression analyses are conducted to identify factors influencing the effect.

At announcement date, there is not necessarily a guarantee that the transaction will be completed. Therefore, a cautious analyst might choose not to reflect the expected effects of the transaction in their estimates at that point in time. Additionally, if the time delay is long enough, the transaction might not even be finished by the time the first actual values are published in our "after" period. Nevertheless, we still focus on this date and not the completion date as our cut-off for the before and after periods due to two reasons: Firstly, for most transactions in our data set, the time difference between announcement and completion date ("merger window") is less than 181 days[19], which is significantly less than the one year ahead time frame we use for our estimate - actual pairs. Secondly, the observed increase in FE is clearly visible after the announcement date already, as shown in section 4.3.3.

4.3.3 Descriptive Statistics

Table 4.1 shows the descriptive statistics for our full data set as described in section 4.3.1 and the numeric variables of our main analysis. FE_{before} and FE_{after} denote the average forecast error (FE, as defined in Formula 4.1) for quarterly earnings per share (EPS), calculated as the average over four calendar quarters before or, respectively, after the announcement date. FE are winsorized at a 2% level, to dampen the influence of outliers. Num_{before} and Num_{after} show the number of individual estimates in each consensus estimate, aggregated analogously to the FE.

[19] 181 days is the 90% quantile in our data. Mean is 81 days, median 54 days. The average "merger window" in the data of Macias and Moeller (2016) is about 4.5 months. It should be noted that our data is based on a relative deal size of at least 10 percent, which may differ from other averages for merger windows.

	$Count$	$Mean$	Std	Min	25%	$Median$	75%	Max
FE_{before}	2,612	0.86	1.29	0.04	0.16	0.35	0.92	6.33
FE_{after}	2,612	1.12	1.75	0.04	0.17	0.41	1.21	8.92
Num_{before}	2,612	3.96	3.91	0.25	1.50	2.75	5.00	31.08
Num_{after}	2,612	4.35	3.84	0.25	1.83	3.25	5.50	29.42
$DealValue$	2,612	1,666.28	5,677.44	10	114.98	335	1,018.12	86,831.16
$MktCap_{acquirer}$	2,612	5,195.64	17,078.99	0.20	426.56	1,193.66	3,299.10	353,340.33
$CAR_{(-1\|+1)}$	2,612	0.01	0.12	-0.38	-0.02	0.01	0.04	4.29

Table 4.1: Summary statistics main data set
This table reports the distribution of our numeric variables of interest. FE is the average FE (as defined in Formula 4.1) for quarterly earnings per share (EPS). FE are winsorized at the 2% level. Num is defined as the number of individual estimates in each consensus estimate. Indices $before$ and $after$ indicate whether the variable in question shows the average of four calendar quarters before or after the announcement date. $DealValue$ is the deal value of the M&A transaction in USDm, $MktCap_{acquirer}$ the market value of the outstanding shares of the acquirer 4 weeks before the announcement date in USDm. $CAR_{(-1|+1)}$ is the announcement effect on the acquirer's share price, measured as the cumulative abnormal return (CAR) from one day before to one day after announcement, relative to S&P500 market returns.

As can be seen, the distribution of FE is influenced by outliers even after winsorization. This is mainly due to the fact, that the absolute FE is scaled by the actual value. Since actual EPS values can be close to zero, large percentage deviations can result. In robustness tests, we cut off every FE over 2 (which would be a deviation of 200%) and scale the absolute FE by the share price instead of the actual value. Both approaches do not change the main outcome of our analysis. Therefore, and as scaling by the actual value allows for a more straightforward interpretation (percentage deviation of the estimate from the actual value), we use the values shown here for our main analysis despite the visible outliers.

Figure 4.1 shows the distribution of our main variables of interest, FE_{before} and FE_{after}, winsorized at a 2% level. As illustrated, the distribution after the announcement is shifted to the right, with both the mean and median statistically significantly higher. The difference is economically meaningful as well, amounting to 0.28 when looking at means and 0.06 for medians. This indicates a significant increase in average FE following the announcements of large M&A deals, with an even more pronounced increase in large FE, i. e. outliers.

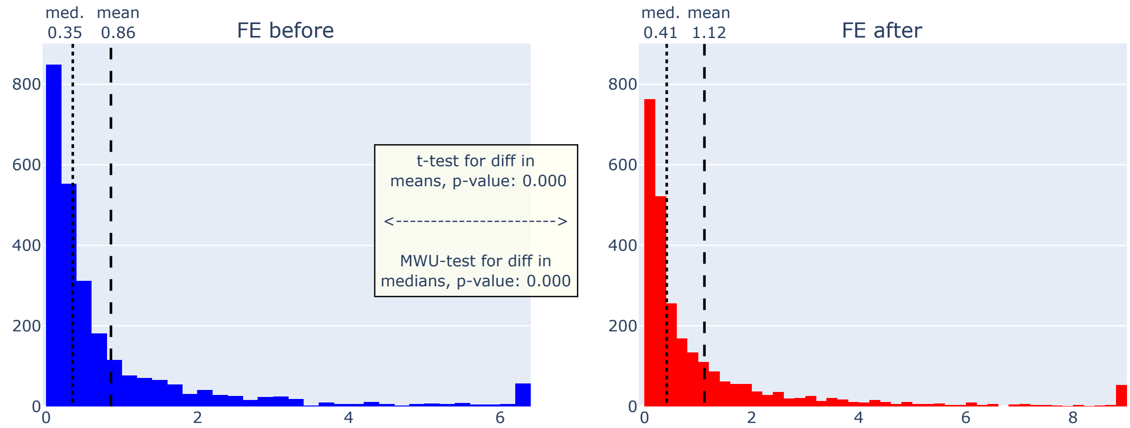

Figure 4.1: Distribution of EPS FE before and after M&A announcement
This figure shows the distribution of our main variable of interest, the average FE (see Formula 4.1) for quarterly earnings per share (EPS) over four calendar quarters before and after the announcement date. Data is winsorized at the 2% level. p-values shown are associated with tests for the statistical significance of the differences in means (t-test) and medians (mann-whitney-u-test) respectively.

Figure 4.2 shows the distribution of means and medians of quarterly FE for eight calendar quarters each before and after the announcement date. In addition, it presents the test statistics for a test of difference in means and medians for the averages of four quarters each. When examining medians, which is recommended due to the non-normal distribution observed in Figure 4.1, there is no statistically significant difference between the first and the second four-quarter interval (Years -2 and -1, with -1 being FE_{before} in our main analysis) and the third and fourth ones (Years +1, equal to FE_{after}, and +2). However, as expected and shown in Figure 4.1 already, there is one between the second and third (-1 and +1, or FE_{before} and FE_{after} respectively), with the announcement date between them.[20] As can be seen, this effect is virtually instant from calendar quarter -1 to +1 relative to the announcement date, with quarterly FE staying on the higher level for several consecutive quarters. This is why we choose the announcement date rather than the completion date as our cut-off point for the before and after time frames. The mean FE starts to drop in year +2, although not back to pre-announcement levels. The median stays on its elevated level.

[20]Different values for means and medians in Figure 4.2 compared to Table 4.1 and Figure 4.1 result from the winsorization being applied to quarterly FE instead of average FE over four quarters.

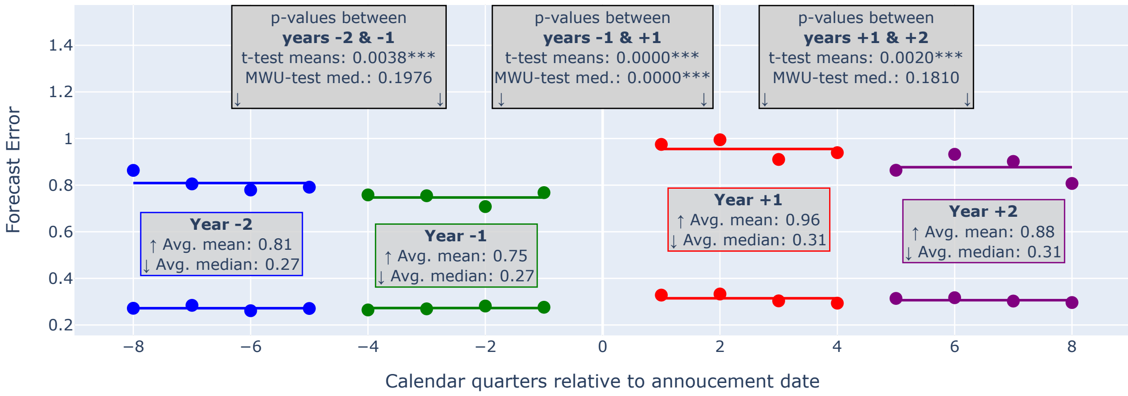

Figure 4.2: Distribution of EPS FE before and after M&A announcement
This figure shows the distribution of our main variable of interest, the average FE (see Formula 4.1) for quarterly earnings per share (EPS) for 8 individual calendar quarters before and after the announcement date. Colored groups show FE averaged over 4 calendar quarters. Data is winsorized at the 2% level. *p*-values shown are associated with tests for the statistical significance of the differences in means (t-test) and medians (mann-whitney-u-test) respectively, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

These initial results show a connection of the announcement of a larger M&A transaction to an increase of analyst FE, supporting our Hypothesis 1. Yet, we do not know at this point if there is a direct causal influence. As seen in Table 4.1, there is also an increase in the number of estimates included in the consensus estimates following an M&A announcement. Thus, it might be possible that the increase in FE is due to new analysts covering the acquirer and them making worse estimates than the analysts who have covered the company before. However, this would be in contradiction to our Hypothesis 2. This is why we are following up with several multiple regression analyses in section 4.4.2.

We also do not yet know whether the observed effect can be found in our entire data set or if it is only present or more pronounced in certain subsets. As this information might lead to valuable insights for our regression analysis, we start by analyzing several subsets of our data set in section 4.4.1.

4.4 Results

4.4.1 Grouped averages comparisons

Table 4.2 shows our main variables of interest, FE_{before} and FE_{after} as means and medians, as well as the average difference between them and the test statistics regarding this difference being different from zero for several subgroups. These subgroups will be used as dummy or categorical variables in the multiple regression analysis (or, in the case of acquirer macro industry, as fixed effects). This table illustrates two main characteristics of the subgroups: (1) different absolute levels of FE, be they before or after an announcement, and (2) different levels of changes from before to after the announcement, both in the size and in the significance of this change.

For instance, while FE seem to be lower for acquirers of public targets than those of private targets in general (lower mean and median FE, both before and after), the effect of the announcement seems to have roughly the same size (0.27 or 0.23 for means, 0.07 or 0.04 for medians). Differences from before to after are statistically significant in both cases. Thus, we do expect an influence of a dummy variable private/public on the level of the FE itself, but not necessarily on the change in FE due to an M&A announcement, although this would be in contradiction to Hypothesis 5.

The M&A type category is approximated based on the relation of Macro- and Mid-Industries of acquirer and target, as set out in footnote 16. With regard to FE, this distinction shows no obvious differences between the subgroups both in the absolute level and in the differences between before and after. Based solely on this, there is no support for our Hypothesis 8 so far.

Cross-border status shows the inverse of target public status: Absolute levels are similar for the subgroups, but differences are more pronounced for cross-border transactions, at least concerning means. Thus, in this case, we do not expect an influence on the level of the FE itself, but on the change in FE due to an M&A announcement. Given that complexity is likely to increase FE, this is not surprising and supports our Hypothesis 6. A cross-border transaction should, ceteris paribus, be more complex than a national one and its effects therefore harder to forecast.

The split by acquirer macro industry shows substantial variation in all dimensions, supporting our Hypothesis 7. There are even some subgroups where the overall effect of increased FE is not statistically significant at all. In most cases, those are industries with a lower number of observations. A notable exception is "Healthcare" with 412 observations and no noticeable effect at all. Due to the substantial variation across industries, we include industry fixed effects in our regression analysis and cluster standard errors on the industry level. Company and time fixed effects are added at a later stage as well, where appropriate.

		Mean				*Median*			
	Count	FE_{before}	FE_{after}	Δ	$p-value$	FE_{before}	FE_{after}	Δ	$p-value$
Full data set	2,612	0.86	1.12	0.26	0.000***	0.35	0.41	0.06	0.000***
By target public status									
private	1,948	0.90	1.17	0.27	0.000***	0.38	0.45	0.07	0.000***
public	664	0.74	0.97	0.23	0.002***	0.30	0.34	0.04	0.058*
By M&A type									
horizontal	1,400	0.90	1.15	0.25	0.000***	0.38	0.44	0.05	0.003***
lateral	728	0.79	1.01	0.22	0.004***	0.32	0.39	0.07	0.006***
vertical	484	0.84	1.17	0.34	0.001***	0.34	0.40	0.06	0.015**
By cross-border status									
national	2,189	0.87	1.10	0.24	0.000***	0.36	0.42	0.06	0.000***
cross-border	423	0.81	1.20	0.39	0.001***	0.31	0.39	0.08	0.017**
By acquirer macro industry									
Cons. P&S	200	0.67	0.89	0.23	0.130	0.32	0.31	-0.01	0.848
Cons. Staples	159	0.50	0.71	0.21	0.086*	0.23	0.24	0.01	0.303
Energy/Power	323	1.33	1.66	0.33	0.021**	0.74	0.92	0.17	0.054*
Financials	14	0.18	0.26	0.08	0.455	0.09	0.11	0.01	0.597
Healthcare	407	0.84	0.86	0.03	0.776	0.32	0.39	0.07	0.220
High Tech	581	0.80	1.03	0.23	0.006***	0.34	0.38	0.04	0.249
Industrials	385	0.81	1.18	0.37	0.002***	0.26	0.36	0.09	0.001***
Materials	215	0.81	1.26	0.44	0.005***	0.34	0.38	0.04	0.049**
Media & Ent.	173	0.92	1.13	0.21	0.176	0.40	0.55	0.15	0.242
Retail	70	0.73	1.46	0.73	0.024**	0.32	0.32	-0.00	0.241
Telecom.	85	1.09	1.36	0.26	0.261	0.46	0.94	0.48	0.046**

Table 4.2: Grouped average FE
This table reports the distribution of the average (mean and median) FE (see Formula 4.1) for quarterly earnings per share (EPS) over four calendar quarters before and after the announcement date, grouped by our categorical variables, including counts of observations and the average difference between FE_{before} and FE_{after} for each subgroup. FE are winsorized at the 2% level. "Cons. P&S" is short for "Consumer Products and Services". p-values shown are associated with tests for the statistical significance of the differences in means (t-test) and medians (mann-whitney-u-test) respectively, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

4.4.2 Base Regression analysis

For our first regression analysis, we choose the absolute level of the forecast error (FE) as the dependent variable. We essentially double our dataset by splitting each transaction observation into two observations, FE_{before} and FE_{after}, which are treated as two instances of the same dependent variable FE in this regression. This dependent variable is winsorized at the 2% level and log transformed by applying the natural log of the FE +1. The distinction in before and after is coded as the dummy variable $AfterM\&A$, which is our main independent variable of interest. Results are shown in Table 4.3.

This main variable is highly significant in all model specifications from univariate to the full model with all control variables, both with industry as well as company and time fixed effects. The coefficient remains stable around 0.073, which translates to an increase in FE after M&A announcement of 7.6 percentage points after reversing the log transformation. $AfterM\&A$ only loses significance once multiple interaction terms are introduced which include this variable. This is not surprising. Those interactions are discussed further below.

$AfterM\&A$ especially does not lose significance when introducing the number of individual estimates ($NumOfEstimates$) as a control variable. As discussed in section 4.3.3, the increase in FE might have been due to the introduction of new, potentially less precise analysts. As the factor $NumOfEstimates$ is only weakly significant in a single model specification and does not alter the effect of $AfterM\&A$, this explanation can now be ruled out. However, this also means that we do not find support for our Hypothesis 2.

Of the other non-interaction control variables, which are the same as shown in Tables 4.1 and 4.2, only the market cap of the acquirer (as natural log, $log(MarketCap)$) is of notable significance. Its coefficient stays stable at around -0.09 (dropping to around -0.05 with company and time fixed effects), indicating that FE tend to be lower for larger companies. This could be explained by a more transparent communication of larger companies and diversification effects, which help smooth the company figures. This finding supports the first part of our Hypothesis 3.

All other control variables show no significant impact, which was expected based on Table 4.2 for the M&A type dummies $HorizontalDummy$ and $VerticalDummy$ as well as the $CrossBorderDummy$, but not for $PublicDummy$.

To further analyze the impact of an M&A announcement for different subgroups, we add interaction terms of $AfterM\&A$ and the control variables to the full model, both with industry as well as company together with time fixed effects. The results are similar for both types of fixed effects: The interaction between $AfterM\&A$ and $NumOfEstimates$ is (weakly) significant with a negative coefficient, indicating a weaker effect for companies with a larger following of analysts. This was expected and supports our Hypothesis 2, as a larger number of analysts might reduce information asymmetries and allows for diversification of individual larger FE. The interaction with $log(MarketCap)$ is highly significant with a positive coefficient. This indicates that while larger companies tend to have lower overall FE (see above), the impact of a large M&A transaction announcement is more pronounced for larger companies (measured by market capitalization). These results are in direct contradiction to our Hypothesis 3. However, additional analysis below shows that this finding is not robust. Interaction with $RelativeValue$ is highly significant and positive, indicating that transactions with a larger deal size relative to the market value of the acquirer have a larger impact on FE. The results indicate that the complexity increases in relation to the size of the transaction. This was expected and supports our Hypothesis 4, as transactions larger in relation to the acquirer also have a higher impact on the consolidated figures after the transactions and, ceteris paribus, on expected synergies. The interaction term with the announcement effect on the acquirer's share price is also highly significant, but negative. This indicates that the increase in FE is lower for transactions that are viewed favorably by investors and thus create a positive price reaction, as expected and stated in Hypothesis 9. This is indirectly in line with Bens et al. (2012), whose results show that negative capital market reactions to M&A announcements are associated with more misreporting by managers (section 4.2.3.2). Consequently, analysts would also be less informed. Of these four moderating effects, all but $log(MarketCap)$ remain robust in further analysis. In untabulated results, interactions of $AfterM\&A$ with the remaining control variables are also tested, but found to be insignificant. These model specifications are excluded from Table 4.3 for brevity and ease of reading.

To assess the validity of our findings, we employ various regression diagnostics. In this regard, we primarily follow the approach proposed by Kennedy (2008). The full tables for these diagnostics can be found in the appendix (Tables A.4.1 and A.4.2). The main implications are discussed in the following.

Rainbow tests for linearity (Utts, 1985) show no non-linearity for most of our models. Only models "Co.+Time" and "Inter 2" show strong evidence of non-linearity, which we attribute to over-specification of these models, as they are the only ones with company and time fixed effects instead of industry fixed effects. Our data set contains only a very limited number of observations per company, potentially leading to overspecification.

The tests developed by Breusch and Pagan (1979) and Levene (1960) hint to heteroscedasticity in our data, both within industries as well as between. For this reason we employ standard errors clustered on the industry level to account for within-cluster correlation and heteroscedasticity. In unreported results, we also test heteroscedasticity-robust standard errors based on MacKinnon and White (1982) to account for overall heteroscedasticity. Our findings remain largely unchanged. Only the interaction between $After$ and $log(MarketCap)$ loses significance.

Variance inflation factors (VIF) are analyzed to test for multicollinearity. Following Kennedy (2008), we assume that VIF values below 10 are unproblematic. High VIF's are only found for the models with interaction terms, which is in line with expectations as they include multiple interactions with one variable. All other models do not show signs of multicollinearity.

Durbin and Watson (1950) test statistics indicate that there is no autocorrelation in the residuals of any of our models. However, despite log-transforming relevant variables, Kolmogorov-Smirnov tests indicate that the residuals are not normally distributed. This is further confirmed by (unreported) Q-Q plots showing deviations from the normal line. Given the obviously non-normal distribution of FE visible in Figure 4.1, this is not surprising, although we already log-transformed and heavily winsorized this data. To address this issue, we use robust standard errors, as discussed above. To further ensure the robustness of our results, a bootstrap method with 1,000 iterations per model is applied to obtain coefficients and confidence intervals that are less sensitive to non-normality. Again, our main findings remain unaffected, apart from the interaction between $AfterM\&A$ and $log(MarketCap)$, which once more loses significance. Bootstrapping is not applied to models with company and time fixed effects due to insufficient observations per company, which lead to convergence issues during the resampling process, necessitating their exclusion from the bootstrap analysis. For the sake of readability and familiarity, we choose to show the traditional OLS-regression in this section. A full table of coefficients and confidence intervals derived from bootstrapping can be found in the appendix in Table A.4.2.

	Dependent variable: FE averaged over four calendar quarters, winsorized at 2% and log-transformed by applying log(FE + 1)											
	AfterM&A	+Industry	+NumEst	+Size	+DealSize	+Public	+CrossBord	+Type	+CAR	Co.+Time	Inter 1	Inter 2
Intercept	0.4816***	0.3818***	0.4217***	0.9777***	0.9673***	0.9753***	0.9740***	0.9570***	0.9628***	0.8400***	1.0135***	0.8840***
	(15.2434)	(53.4049)	(16.3606)	(14.0025)	(15.5286)	(13.3624)	(13.1373)	(11.9058)	(12.2346)	(5.7954)	(13.6846)	(6.2676)
AfterM&A	0.0751***	0.0751***	0.0799***	0.0727***	0.0728***	0.0728***	0.0728***	0.0729***	0.0730***	0.0711***	-0.0249	-0.0204
	(5.2585)	(5.2534)	(5.9234)	(5.6207)	(5.5951)	(5.5870)	(5.5882)	(5.5909)	(5.5916)	(4.4556)	(-0.8677)	(-0.5717)
NumOfEstimates			-0.0121*	0.0061	0.0059	0.0058	0.0059	0.0055	0.0054	0.0101	0.0109	0.0151
			(-1.8100)	(0.8575)	(0.8527)	(0.8328)	(0.8355)	(0.7752)	(0.7675)	(1.0926)	(1.2074)	(1.3588)
log(MarketCap)				-0.0888***	-0.0881***	-0.0893***	-0.0893***	-0.0892***	-0.0896***	-0.0524**	-0.0971***	-0.0584**
				(-7.2494)	(-7.6976)	(-7.1526)	(-7.1447)	(-7.0804)	(-7.1921)	(-2.0915)	(-7.6834)	(-2.3665)
RelativeValue					0.0185	0.0132	0.0137	0.0100	0.0112	0.0018	-0.0482	-0.0568
					(0.4404)	(0.2940)	(0.3046)	(0.2260)	(0.2503)	(0.0417)	(-1.2587)	(-1.2465)
PublicDummy						0.0134	0.0133	0.0123	0.0093	-0.0316	0.0087	-0.0318
						(0.4600)	(0.4598)	(0.4223)	(0.3210)	(-0.8308)	(0.2988)	(-0.8315)
CrossBorderDummy							0.0110	0.0108	0.0099	-0.0351	0.0098	-0.0353
							(0.5870)	(0.5827)	(0.5289)	(-1.1428)	(0.5313)	(-1.1432)
HorizontalDummy								0.0406*	0.0410*	0.0188	0.0411*	0.0185
								(1.8782)	(1.8826)	(0.8118)	(1.8711)	(0.7989)
VerticalDummy								0.0424	0.0429	0.0149	0.0430	0.0149
								(1.0499)	(1.0519)	(0.3749)	(1.0485)	(0.3748)
$log(CAR+1)$									-0.1380	-0.2268**	0.0962	0.0117
									(-1.1592)	(-2.1452)	(0.9461)	(0.2268)
$After: NumOfEstimates$											-0.0110**	-0.0088*
											(-2.3545)	(-1.8132)
$After: log(MarketCap)$											0.0149***	0.0127**
											(3.8394)	(2.4822)
$After: RelativeValue$											0.1166***	0.1176***
											(4.3459)	(3.7313)
$After: log(CAR+1)$											-0.4613***	-0.4649***
											(-3.1726)	(-2.7268)
Fixed Effects	-	Industry	Industry	Industry	Industry	Industry	Industry	Industry	Industry	Company +Time	Industry	Company +Time
R^2	0.0055	0.0434	0.0510	0.1086	0.1087	0.1088	0.1089	0.1101	0.1105	0.5139	0.1140	0.5168
Observations	5,224	5,224	5,224	5,224	5,224	5,224	5,224	5,224	5,224	5,224	5,224	5,224

Table 4.3: Influence factors for FE - regression results
This table reports multiple linear regression results for FE (see Formula 4.1) for quarterly earnings per share (EPS) over four calendar quarters as the explained variable, either before or after the announcement date. This variable is winsorized at the 2% level and transformed by applying log(FE +1). Explanatory variables are defined as follows: $AfterM\&A$ is a dummy variable with value 1 if the observed FE is the average of four calendar quarters after the announcement date. $NumOfEstimates$ denotes the average number of individual estimates contained in the consensus estimates used for the FE. $log(MarketCap)$ is the natural log of the acquirer's market capitalization 4 weeks prior to announcement date. $RelativeValue$ is the deal value of the M&A transaction divided by the market capitalization of the acquirer 4 weeks prior to announcement date (clipped at 100% to dampen the influence of outliers). $PublicDummy$ is a dummy variable with value 1 if the target has been publicly listed at the time of announcement. $CrossBorderDummy$ is a dummy variable with value 1 if the targets nationality is different from the acquirers (i. e. non-US). $HorizontalDummy$ and $VerticalDummy$ are dummies for the categorical variable "M&A type" in Table 4.2, with lateral being the base value. $log(CAR+1)$ is the natural log of the acquirer stocks cumulative abnormal return (CAR) from one day before to one day after the announcement, relative to the return of the S&P500, +1. Variables starting with "$After:$" show interaction effects of $AfterM\&A$ with the named variables. t-statistics with standard errors clustered on industry level are reported in parentheses, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

Table 4.4 shows our second regression analysis, this time with the change in FE from before to after the announcement as the dependent variable, winsorized at the 2% level, but not log transformed, as this variable can contain negative values.

In accordance with our findings in Table 4.3 for interaction terms, the coefficient for Num_{before}, i. e. the number of individual analysts behind a consensus estimate before the M&A announcement, is significant at the 5% level in this analysis. The coefficient is negative, indicating a lower increase in FE for companies with a larger analyst following. We expected this based on interaction terms in the first regression analysis and it further supports our Hypothesis 2. $log(MarketCap)$ shows the same positive relation as in the first regression analysis, but only weakly significant in only two models and completely losing significance, once fixed effects are added. This is in line with what we expected based on regression diagnostics and bootstrapping regression. All other variables show the same behavior as the interaction terms in the first analysis: $RelativeValue$ is highly significant with a positive coefficient, $log(CAR+1)$ highly significant with a negative coefficient, all others show no significant impact, verifying our previous results. We do not include company, but industry and time fixed effects in this second regression analysis. This is done because there is only one observation for the difference of before to after for most companies in our data set. Including company fixed effects would therefore likely result in overfitting.

Regression diagnostics are applied to this second regression in the same way as for the first. The full tables for these can be found in the appendix (Tables A.4.3 and A.4.4).

The results are similar: Rainbow tests for linearity (Utts, 1985) show no non-linearity for any of our models in this regression and variance inflation factors (VIF) show no signs of multicollinearity. Again, strong hints to heteroscedasticity in our data are found (Breusch and Pagan, 1979; Levene, 1960). Therefore, we once more employ standard errors clustered on industry level and, in unreported results, test heteroscedasticity-robust standard errors based on MacKinnon and White (1982). Our findings remain unchanged, only the cross-border dummy loses its (weak) significance.

Durbin and Watson (1950) test statistics again indicate no autocorrelation in the residuals of any of our models, but residuals remain not normally distributed. When applying a bootstrapping method with 1,000 iterations to the regressions in Table 4.4, our main results remain unchanged (see Table A.4.4 in the appendix). Once more, only the cross-border dummy variable loses its significance, which was below the 5% level.

Dependent variable: Change in FE averaged over four calendar quarters from before to after M&A announcement, winsorized at 2%									
	Only NumEst	+Size	+DealSize	+Public	+CrossBorder	+Type	+CAR	+IndFixed	+TimeFixed
$Intercept$	0.3614***	0.2459*	0.0174	-0.0180	-0.0512	-0.0844	-0.0476	-0.0522	0.0163
	(3.9948)	(1.9325)	(0.1095)	(-0.1088)	(-0.3459)	(-0.5659)	(-0.3302)	(-0.3579)	(0.0857)
Num_{before}	-0.0234*	-0.0267*	-0.0287*	-0.0280*	-0.0274**	-0.0275**	-0.0287**	-0.0319**	-0.0246*
	(-1.8356)	(-1.8806)	(-1.9522)	(-1.9341)	(-1.9860)	(-1.9831)	(-2.0543)	(-2.0517)	(-1.6996)
$log(MarketCap)$		0.0180	0.0307	0.0365	0.0361*	0.0369*	0.0345	0.0351	0.0338
		(1.0066)	(1.5457)	(1.6301)	(1.6614)	(1.7276)	(1.6445)	(1.5615)	(1.5447)
$RelativeValue$			0.4080***	0.4355***	0.4478***	0.4515***	0.4635***	0.4708***	0.4721***
			(3.2604)	(3.3214)	(3.4600)	(3.6210)	(3.6829)	(3.9986)	(4.1111)
$PublicDummy$				-0.0734	-0.0746	-0.0795	-0.1053	-0.1056	-0.0996
				(-1.0392)	(-1.0496)	(-1.1345)	(-1.4688)	(-1.4037)	(-1.3565)
$CrossBorderDummy$					0.1839*	0.1817*	0.1719*	0.1619*	0.1961**
					(1.9132)	(1.8754)	(1.7759)	(1.7010)	(2.5309)
$HorizontalDummy$						0.0152	0.0182	0.0662	0.0442
						(0.1567)	(0.1900)	(0.7037)	(0.4315)
$VerticalDummy$						0.1063	0.1092	0.1737*	0.1608
						(1.0145)	(1.0480)	(1.7029)	(1.5720)
$log(CAR+1)$							-1.2503***	-1.2950***	-1.0618**
							(-2.8272)	(-3.0998)	(-2.1739)
Fixed Effects	-	-	-	-	-	-	-	Industry	Industry +Time
R^2	0.0021	0.0022	0.0053	0.0055	0.0066	0.0070	0.0094	0.0157	0.0356
Observations	2,612	2,612	2,612	2,612	2,612	2,612	2,612	2,612	2,612

Table 4.4: Influence factors for difference in FE - regression results
This table reports multiple linear regression results for the difference in FE (see Formula 4.1) for quarterly earnings per share (EPS) over the four calendar quarters before and after the announcement date respectively as the explained variable, winsorized at the 2% level. Explanatory variables are defined as in Table 4.3. t-statistics with standard errors clustered on industry level are reported in parentheses, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

4.4.3 Supplemental analysis

4.4.3.1 Alternative measures for FE

As discussed in section 4.3.3, our calculation of the forecast error (FE) according to Formula 4.1 leads to potential distortions for actual values close to zero. In our main analysis, this is addressed by winsorizing (and for the first regression log-transforming) our data. For robustness purposes, alternative measures for the FE are analyzed and presented in this section. Figure 4.3 repeats Figure 4.2, but for three alternative measures still based on EPS estimates and actuals, while Figure 4.4 is based on EBITDA estimates and actuals. Regression results for the main models using those measures are shown in Tables 4.5 and 4.6. Full regression tables for the alternative measures are available upon request. Our main finding, the statistically and economically significant positive influence of an M&A announcement on the FE of analysts in accordance with our main Hypothesis 1, holds in all variations.

The first alternative measure does not change the calculation of FE, but rather the handling of the resulting outliers. Winsorizing at the 2% level leads to a maximum FE of 6.33 in our before-announcement and 8.92 in our after-announcement window. As these are still substantial outliers, we repeat our analysis with FE cut at 2, corresponding to an estimate deviating 200% from the actual value. This of course leads to a drop in observations, which is why we do not base our main analysis on this measure. As can be seen in Figure 4.3, the quarterly averages show the same general behavior as the winsorized ones, albeit on a lower overall level due to the stricter exclusion of outliers. The cut-off is even clearer: Only the difference (i. e. the increase in both mean and median) from year -1 to +1 is statistically significant, changes from years -2 to -1 and years +1 to +2 are entirely negligible. Results for repeating our regression analyses for the absolute level of and difference in FE with this measure vary only in the degree of influence of some variables. $log(MarketCap)$ switches significances: The interaction term loses and the factor in the second regression gains significance. As this factor loses its significant influence on the absolute level of FE, the corresponding relation found in our main analysis is likely driven by outliers with respect to the FE. The public status of the target ($PublicDummy$) and horizontal deal structures ($HorizontalDummy$) gain in significance, while the number of analysts ($NumOfEstimates$) and announcement stock return ($log(CAR+1)$) loses some significance. The direction of influence remains unchanged in all cases except for $HorizontalDummy$, which shows a statistically significant negative influence on the change in FE, that is not evident in our main analysis.

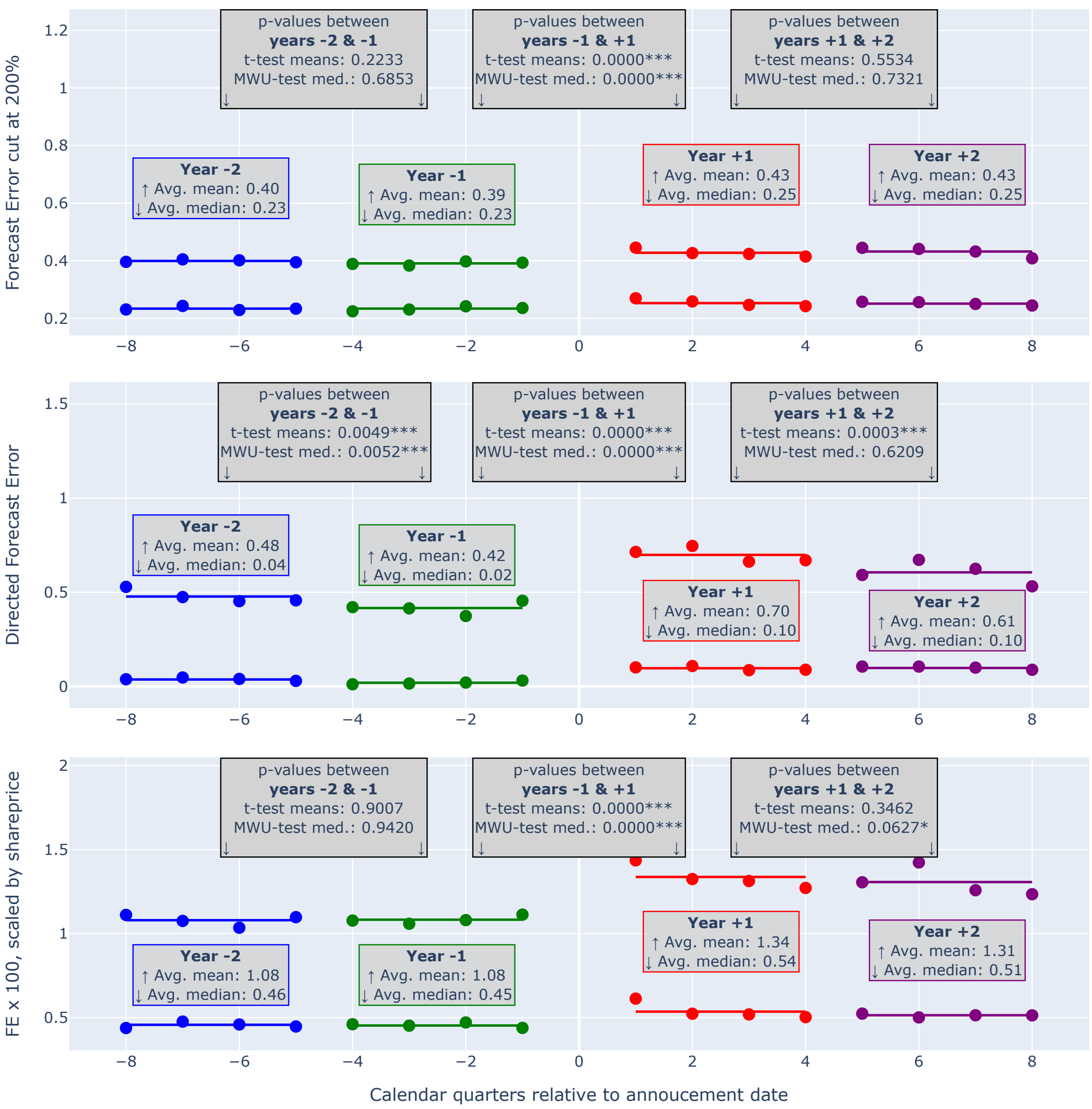

Figure 4.3: Distribution of FE with alternative calculation
These figures show the distributions of the average FE with alternative calculations used to the one in our main analysis and shown in Figure 4.2. For the first figure, all values above 2 (i. e. FE over 200%) are excluded instead of winsorizing the distribution, for the second and third figure, data is winsorized at the 2% level as in the main analysis. The second figure shows the directed FE (Formula 4.2). The third shows FE that are multiplied by 100 and then scaled by the corresponding share price at the time of estimation instead of the actual value. p-values shown are associated with tests for the statistical significance of the differences in means (t-test) and medians (mann-whitney-u-test) respectively, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

The second alternative calculates the FE according to Formula 4.2 instead of 4.1, i. e. it includes the direction of the FE instead of the absolute deviation. This of course leads to negative and positive deviations partially canceling each other out, but allows conclusions regarding the direction of the mean deviation, i. e. if analysts are rather too optimistic or too pessimistic. As can be seen in Figure 4.3, mean estimates are too optimistic in all quarters, with a substantial jump from year -1 to +1. Median FE on the other hand are very close to zero before the announcement, indicating that the average overoptimism of analysts is due to positive outliers and the non-outlier estimate is simply sometimes too high and sometimes too low. This changes immediately after the M&A announcement, as median FE rise from 0.04 and 0.02 to 0.10 in years +1 and +2, indicating an increase in over-optimism across the board following the announcement. Regression results in Table 4.5 show that $NumOfEstimates$ gains explanatory power for the overall FE. Additionally, the interaction terms with $RelativeValue$ and announcement stock return ($log(CAR + 1)$) lose explanatory power. In the second regression (Table 4.6), however, the $RelativeValue$ shows the same effect as in our main analysis. The announcement return remains insignificant in this specification. The corresponding models in the first regression table (Table 4.5) additionally differ from our main analysis in the FE not being log-transformed. This is not possible, as the directed FE can also be negative.

The third alternative deviates from Formula 4.1 in the denominator: Absolute differences from actual to estimate are not scaled by the actual value, but rather by the share price at the time of estimation. This measure is closer to the proportional mean absolute FE ($PMAFE_{i,j,t}$) established by Clement (1999) and used by multiple others. Before dividing, FE are multiplied by 100, to make the resulting figures more comparable to the other measures. The behavior shown in Figure 4.3 is unchanged: A significant increase both in means and medians from year -1 to +1 with a distinct jump right around the announcement date. Regression results (Tables 4.5 and 4.6) are again similar regarding our main hypothesis, with the difference that the main variable is also highly significant after inclusion of all interaction terms. The following differences in control variables can be observed: $NumOfEstimates$ and the $CrossBorderDummy$ gain explanatory power regarding the absolute value of the

FE, both with a negative coefficient. This is expected for the number of estimates (as per Hypothesis 2), but not for the $CrossBorderDummy$, which should only have an influence on the change in FE, not the absolute level (Hypothesis 6). With regard to interaction terms, the interaction with $NumOfEstimates$ loses significance and the one with $log(MarketCap)$ changes its sign, further invalidating the robustness of this influence and thus Hypothesis 3 overall. $RelativeValue$ and the announcement effect on the acquirer's share price ($log(CAR+1)$) behave similarly to our main analysis. These differences and similarities are also to be found in the second regression analysis, with the addition that the $CrossBorderDummy$ is not significant in this specification.

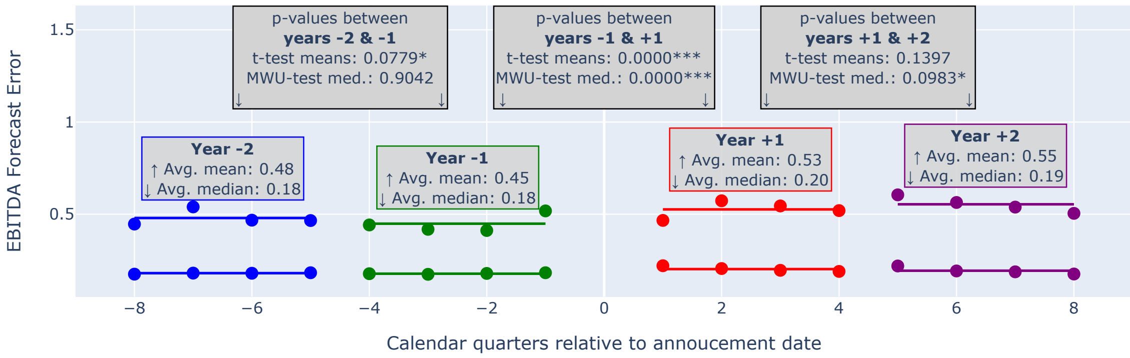

Figure 4.4: Distribution of FE based on EBITDA
This figure shows the distribution of the average FE (see Formula 4.1) for quarterly EBITDA (instead of earnings per share (EPS) as in our main analysis shown in Figure 4.2) for 8 individual calendar quarters before and after the announcement date. Colored groups show FE averaged over 4 calendar quarters. Data is winsorized at the 2% level. p-values shown are associated with tests for the statistical significance of the differences in means (t-test) and medians (mann-whitney-u-test) respectively, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

Finally, Figure 4.4 shows average quarterly FE according to Formula 4.1, but based on EBITDA estimates and actuals rather than EPS. This measure has the advantage of being closer to cash flow and thus less prone to earnings management, but the disadvantage of a lower number of observations and a potential selection bias[21]. Both the number of companies with any EBITDA estimates at all and the number of individual estimates in each consensus figure are lower than with EPS, which is why we choose the latter in our main analysis. The general development in Figure 4.4

[21] We refrain from using direct cash flow estimates, as their availability is even lower than for EBITDA estimates, further increasing this problem.

is the same as with EPS, although the increase in median FE is less pronounced with EBITDA estimates (but still highly statistically significant). Regression results show a similar picture regarding our main hypothesis as before. However, the control variables for the absolute level of FE are now all insignificant, except for the $CrossBorderDummy$, which has a significant negative impact. Additionally, most control variables lose significance with regards to the change in FE. This could be a result of the reduced sample (1,813 instead of 2,612 transactions), which probably suffers from selection bias. $log(MarketCap)$ again switches direction of influence, further weakening its overall assessment.

	Dependent variable: FE averaged over four calendar quarters, alternative calculations as defined below							
	FE cut at 200%		FE directed		FE by shareprice		FE EBITDA	
	Base	Int	Base	Int	Base	Int	Base	Int
Intercept	0.5626***	0.5881***	0.5878	0.7338*	1.5214***	1.4957***	1.4425***	1.4386***
	(11.4487)	(9.6067)	(1.3043)	(1.7722)	(12.5046)	(12.7589)	(11.8390)	(9.3148)
AfterM&A	0.0246**	-0.0270	0.2719***	-0.0507	0.0898***	0.1473***	0.0266***	0.0309
	(2.4841)	(-0.6295)	(5.9324)	(-0.2910)	(5.1587)	(3.3988)	(3.0943)	(0.3194)
NumOfEstimates	-0.0014	0.0010	0.0495**	0.0684**	-0.0152***	-0.0139***	0.0032	0.0055
	(-0.4011)	(0.2868)	(2.4211)	(2.4597)	(-4.8741)	(-4.4675)	(0.3308)	(0.6508)
log(MarketCap)	-0.0122	-0.0156	-0.0688	-0.0927	-0.0609***	-0.0544***	-0.0145	-0.0139
	(-1.0483)	(-1.3418)	(-1.0003)	(-1.4342)	(-3.2161)	(-3.1570)	(-1.0636)	(-0.8037)
RelativeValue	0.0160	-0.0121	0.0533	-0.0887	0.0037	-0.0650**	-0.0190	-0.0405
	(1.2876)	(-0.8315)	(0.2986)	(-0.4394)	(0.1275)	(-2.0010)	(-0.7259)	(-1.1497)
PublicDummy	0.0027	0.0027	-0.1481	-0.1493	-0.0271	-0.0269	-0.0250	-0.0249
	(0.2316)	(0.2285)	(-1.1488)	(-1.1484)	(-0.8166)	(-0.8089)	(-0.9309)	(-0.9253)
CrossBorderDummy	-0.0028	-0.0031	-0.0789	-0.0795	-0.0466**	-0.0466**	-0.0420**	-0.0422**
	(-0.2219)	(-0.2414)	(-0.6661)	(-0.6697)	(-2.1017)	(-2.1019)	(-2.0009)	(-2.0114)
HorizontalDummy	0.0102	0.0101	0.0449	0.0443	-0.0037	-0.0040	0.0056	0.0057
	(0.6760)	(0.6728)	(0.6010)	(0.5933)	(-0.1936)	(-0.2099)	(0.2817)	(0.2842)
VerticalDummy	-0.0040	-0.0039	0.0710	0.0717	0.0090	0.0087	0.0104	0.0104
	(-0.1841)	(-0.1827)	(0.7660)	(0.7721)	(0.2407)	(0.2335)	(0.3830)	(0.3830)
$log(CAR+1)$	-0.1208	-0.0155	-0.5982***	-0.4193*	-0.1583	0.1620	-0.1527	-0.0911
	(-1.4339)	(-0.2486)	(-2.7243)	(-1.8918)	(-1.3500)	(1.0631)	(-1.4668)	(-0.7612)
After : NumOfEstimates		-0.0042***		-0.0373*		-0.0005		-0.0044
		(-3.3126)		(-1.8254)		(-0.1198)		(-0.9589)
After : log(MarketCap)		0.0071		0.0531**		-0.0138**		-0.0008
		(1.4434)		(2.3864)		(-2.3608)		(-0.0756)
After : RelativeValue		0.0560***		0.2852		0.1376***		0.0404
		(3.2361)		(1.6134)		(3.0074)		(1.0216)
$After : log(CAR+1)$		-0.2039*		-0.3170		-0.6354***		-0.1168
		(-1.9301)		(-1.1232)		(-4.5664)		(-0.7015)
Fixed Effects	Company +Time	Company +Time	Company +Time	Company +Time	Company +Time	Company +Time	Company +Time	Company +Time
R^2	0.5966	0.5987	0.3961	0.3983	0.6934	0.6969	0.6130	0.6133
Observations	3,990	3,990	5,224	5,224	5,224	5,224	4,070	4,070

Table 4.5: Influence factors for FE, alternative measures - regression results
This table reports multiple and multivariate linear regression results for FE for quarterly earnings per share (EPS) over four calendar quarters as the explained variable, either before and or after the announcement date as in Table 4.3. Explanatory variables are defined in Table 4.3. FE are defined in four alternative ways to Table 4.3 for two models (without and with interaction terms) each: (1) FE cut at 2 (i. e. 200%) instead of winzorisation, log transformed, (2) FE directed according to Formula 4.2, winsorized at 2% but not log-transformed, (3) FE scaled by shareprice instead of actual, winsorized at 2% and log transformed, (4) FE according to Formula 4.1, but using EBITDA instead of EPS, winsorized at 2% and log transformed. t-statistics with standard errors clustered on industry level are reported in parentheses, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

Dependent variable: Change in FE averaged over four calendar quarters from before to after M&A announcement, alternative measures as defined below	FE cut at 200%		FE directed		FE by shareprice		FE EBITDA	
	Ind.	Ind.+Time	Ind.	Ind.+Time	Ind.	Ind.+Time	Ind.	Ind.+Time
$Intercept$	-0.1667**	-0.1169	0.0191	0.0636	0.6324***	0.5984***	3.9937*	15.1299
	(-2.4449)	(-1.1836)	(0.1017)	(0.3077)	(4.4184)	(3.6571)	(1.8964)	(1.2917)
Num_{before}	-0.0025	-0.0008	-0.0471***	-0.0401**	0.0089	0.0146	0.0479	0.1444***
	(-1.1524)	(-0.4066)	(-2.8185)	(-2.4807)	(0.8086)	(1.4082)	(1.1431)	(3.0140)
$log(MarketCap)$	0.0168**	0.0154	0.0508**	0.0390	-0.1095***	-0.1244***	-0.5364**	-0.5301**
	(1.9679)	(1.5945)	(2.1136)	(1.4641)	(-4.0820)	(-3.9782)	(-2.1845)	(-2.1305)
$RelativeValue$	0.1329***	0.1173***	0.3352**	0.3192**	0.5596***	0.4539***	-0.2099	-0.4344
	(4.6602)	(4.0064)	(2.0109)	(2.0007)	(3.1534)	(3.2511)	(-0.2732)	(-0.4882)
$PublicDummy$	-0.0783***	-0.0816***	-0.0356	0.0021	-0.0247	-0.0593	0.1845	0.0956
	(-4.5851)	(-4.2238)	(-0.4348)	(0.0320)	(-0.5666)	(-1.0972)	(0.5822)	(0.2952)
$CrossBorderDummy$	-0.0050	0.0016	0.2158	0.2550**	0.0321	0.0708	1.3181	1.4335
	(-0.2018)	(0.0726)	(1.5796)	(2.2338)	(0.2111)	(0.4652)	(1.2448)	(1.4821)
$HorizontalDummy$	-0.0337*	-0.0382*	0.0121	0.0039	-0.0512	-0.0607	-0.6490	-0.8743
	(-1.8720)	(-1.7954)	(0.1007)	(0.0316)	(-0.8185)	(-0.9340)	(-0.7687)	(-1.0527)
$VerticalDummy$	0.0389	0.0343	0.2211*	0.2198	0.1247	0.1228	1.0186	0.8786
	(1.4183)	(1.0592)	(1.7257)	(1.6356)	(1.1249)	(0.9583)	(0.8972)	(0.8179)
$log(CAR + 1)$	-0.3679**	-0.3163*	-0.1453	0.1561	-2.2850***	-2.0110***	-2.1852	-0.9426
	(-2.2486)	(-1.7270)	(-0.5288)	(0.8074)	(-5.1106)	(-4.7230)	(-0.5434)	(-0.2225)
Fixed Effects	Industry	Industry +Time	Industry	Industry +Time	Industry	Industry +Time	Industry	Industry +Time
R^2	0.0211	0.0443	0.0128	0.0443	0.0326	0.0741	0.0108	0.0322
Observations	1,995	1,995	2,612	2,612	2,612	2,612	1,813	1,813

Table 4.6: Influence factors for difference in FE, alternative measures - regression results

This table reports multiple and multivariate linear regression results for the difference in FE for quarterly earnings per share (EPS) over four calendar quarters before and after the announcement date respectively as the explained variable, as in Table 4.4. Explanatory variables are defined in Table 4.4. FE are defined in four alternative ways to Table 4.4 for two models (industry fixed effects and industry + time fixed effects) each: (1) FE cut at 2 (i. e. 200%) instead of winzorisation, (2) FE directed according to Formula 4.2, winsorized at 2%, (3) FE scaled by shareprice instead of actual, winsorized at 2%, (4) FE according to Formula 4.1, but using EBITDA instead of EPS, winsorized at 2%. t-statistics with standard errors clustered on industry level are reported in parentheses, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

4.4.3.2 Additional control variables

For further robustness, we test additional control variables beyond the ones used in Tables 4.3 and 4.4, which are shown in Tables 4.7 and 4.8. The step-wise addition of those supplemental control variables and their interaction with $AfterM\&A$ to the full base model including company and time fixed effects do not change our main finding: the statistically and economically significant influence of an M&A announcement on the FE of analysts.

Those variables include, i. a., the ratio of research and development (R&D) expenses to revenues as well as intangible to total assets. Both of these measures are proxies for the opaqueness of a business model and could therefore influence the ability of analysts to forecast figures of a given company. Kimbrough (2007) illustrates the complexity of R&D and intangible assets and shows that a correct valuation of these items is particularly dependent on insider information and internal estimates. Bena and Li (2014) demonstrate that R&D-intensive companies that file fewer patents are more frequently acquired by companies with extensive patent portfolios and low R&D expenditure. If such entities merge, this is likely to impact the complexity of analysts' estimates. Interestingly, we find none of the two to have an influence on overall FE, but both to have a significant impact in an interaction term with $AfterM\&A$ and in the regression of differences in FE. One would expect a positive influence, i. e. a stronger increase in FE for more opaque business models. This is also what we find for R&D expenses. Intangible assets, however, exhibit a negative influence in our sample. That said, those results are not directly comparable, as R&D expenses were only available for less than half of the companies in our sample. If both variables are included at the same time (in the smaller data set), their influence vanishes.

Another supplemental control variable is $Margin$, which we calculate as Net Income divided by Revenue, using the latest available annual figures as of the announcement date. According to Easterwood and Nutt (1999) and Bradshaw et al. (2001), analysts tend to be overly optimistic with less profitable companies, which should lead to an increased FE for companies with low $Margin$. Based on the results shown in Table 4.7, we are unable to support this finding. In contrast, we find a (weakly) significant positive influence of profitability on the size of the increase in FE.

The final supplemental control variable is *AnalystRec*, the consensus analyst recommendation at the time of the last estimate before the announcement date, scaled from 1 (=strong buy) to 5 (=strong sell). Bradshaw et al. (2001, 2006) find that analysts are generally overly optimistic, be it with regard to financial forecasts, buy/sell recommendations or share price forecasts. This might indicate a correlation between too optimistic recommendations and too high forecasts. Therefore, it could reasonably be assumed that the FE is higher and increases more for companies which are generally viewed more favorably by analysts. Hence, we include the analyst recommendation as an additional control variable. However, we find no significant influence of this variable on FE in general or on the change in FE following an M&A announcement.

	Dependent variable: FE averaged over four calendar quarters, winsorized at 2% and log-transformed by applying log(FE + 1)											
	Base	Base Int	+R&D	+Int	+Intang	+Int	+Margin	+Int	+Rec	+Int	+all	+Int
<——— All factors of full model in Table 4.3 included here in all specifications. Not shown explicitly for the sake of readability and clarity ———>												
AfterM&A	0.0711***	-0.0204	0.0521*	-0.0466	0.0754***	0.0151	0.0738***	0.0039	0.0705***	-0.0088	0.0514*	0.1114
	(4.4556)	(-0.5717)	(1.8829)	(-0.6621)	(4.9632)	(0.4102)	(4.8677)	(0.1098)	(4.5579)	(-0.1104)	(1.6874)	(1.1891)
R&DRatio			0.0015	0.0001							-0.0680	-0.0681
			(0.5162)	(0.0381)							(-1.5707)	(-1.5503)
After:*R&DRatio*				0.0028***								0.0007
				(3.9563)								(0.3384)
IntangibleRatio					0.0923	0.1408					0.1524	0.1783*
					(0.7916)	(1.1910)					(1.3998)	(1.6616)
After : IntangibleRatio						-0.0991***						-0.0513
						(-2.6647)						(-0.8996)
Margin							-0.0646	-0.0982*			-0.0720	-0.0907
							(-1.3707)	(-1.8041)			(-0.6311)	(-0.7967)
After : Margin								0.0676**				0.0373**
								(2.3761)				(2.3342)
AnalystRec									0.0362	0.0381	0.0457	0.0753
									(0.9041)	(0.8509)	(0.8394)	(1.5662)
After : AnalystRec										-0.0068		-0.0615**
										(-0.1849)		(-2.3133)
Fixed Effects	Company +Time	Company +Time	Company +Time	Company +Time	Company +Time	Company +Time	Company +Time	Company +Time	Company +Time	Company +Time	Company +Time	Company +Time
R^2	0.5139	0.5168	0.5489	0.5515	0.5239	0.5271	0.5165	0.5200	0.5134	0.5163	0.5586	0.5610
Observations	5,224	5,224	2,182	2,182	4,606	4,606	5,022	5,022	5,206	5,206	2,066	2,066

Table 4.7: Influence factors for FE, additional control variables - regression results
This table reports multiple linear regression results for FE (see Formula 4.1) for quarterly earnings per share (EPS) over four calendar quarters as the explained variable, either before and or after the announcement date. This variable is winsorized at the 2% level and transformed by applying log(FE +1). Explanatory variables for the base model are defined in Table 4.3. Additional control variables are all winsorized at the 2% level (except for *AnalystRec*) and defined as follows: *R&DRatio* is calculated as R&D expenses / Revenue, *IntangibleRatio* as Intangibles / Total Assets, *Margin* as Net Income / Revenue, all using the latest available annual figures as of announcement date. *AnalystRec* is the consensus recommendation ("strong buy" to "strong sell", coded as 1-5) at the time of the last estimate used before the announcement date. Variables starting with "*After :* " show interaction effects of *AfterM&A* with the named variables. t-statistics with standard errors clustered on industry level are reported in parentheses, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

Dependent variable: Change in FE averaged over four calendar quarters from before to after M&A announcement, winsorized at 2%						
	Base	+R&D	+Intang	+Margin	+Rec	+all
Intercept	0.0163	-0.0878	0.1563	0.0299	0.0468	0.2909
	(0.0857)	(-0.5339)	(0.6783)	(0.1346)	(0.1881)	(1.1912)
Num_{before}	-0.0246*	-0.0242***	-0.0253*	-0.0254*	-0.0239*	-0.0194**
	(-1.6996)	(-2.6302)	(-1.6519)	(-1.7191)	(-1.6756)	(-2.1553)
$log(MarketCap)$	0.0338	0.0577**	0.0280	0.0246	0.0377**	0.0588**
	(1.5447)	(1.9958)	(1.5362)	(1.0066)	(2.0030)	(2.1112)
$RelativeValue$	0.4721***	0.1816	0.4658***	0.4932***	0.4632***	0.0374
	(4.1111)	(0.6539)	(4.0111)	(4.3165)	(4.1385)	(0.1116)
$PublicDummy$	-0.0996	0.0369	-0.0993	-0.0679	-0.0944	0.0343
	(-1.3565)	(0.3459)	(-1.5393)	(-0.9304)	(-1.2801)	(0.2879)
$CrossBorderDummy$	0.1961**	0.2938*	0.1701**	0.2041***	0.2058***	0.2997*
	(2.5309)	(1.9144)	(2.1070)	(2.8616)	(2.6540)	(1.7783)
$HorizontalDummy$	0.0442	-0.0046	0.0189	0.0231	0.0373	-0.0055
	(0.4315)	(-0.0504)	(0.2110)	(0.2139)	(0.3935)	(-0.0601)
$VerticalDummy$	0.1608	-0.0143	0.1479	0.1557	0.1598	-0.0399
	(1.5720)	(-0.2280)	(1.5062)	(1.4852)	(1.5859)	(-0.3958)
$log(CAR+1)$	-1.0618**	-1.0435***	-1.2956*	-1.0580***	-1.0494**	-0.9509**
	(-2.1739)	(-3.9450)	(-1.9271)	(-2.8353)	(-2.2351)	(-2.4279)
$R\&DRatio$		0.0110***				-0.0009
		(4.7268)				(-0.1612)
$IntangibleRatio$			-0.3677**			-0.1479
			(-2.4655)			(-0.7490)
$Margin$				0.1572*		0.0863
				(1.6835)		(1.2655)
$AnalystRec$					-0.0286	-0.1593*
					(-0.3580)	(-1.8543)
Fixed Effects	Industry +Time	Industry +Time	Industry +Time	Industry +Time	Industry +Time	Industry +Time
R^2	0.0356	0.0426	0.0314	0.0370	0.0357	0.0428
Observations	2,612	1,091	2,303	2,511	2,603	1,033

Table 4.8: Influence factors for difference in FE, additional control variables - regression results
This table reports multiple linear regression results for the difference in FE (see Formula 4.1) for quarterly earnings per share (EPS) over the four calendar quarters before and after the announcement date respectively as the explained variable, winsorized at the 2% level. Explanatory variables for the base model are defined as in Table 4.3. Additional control variables are defined as in Table 4.7. t-statistics with standard errors clustered on industry level are reported in parentheses, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

4.5 Limitations and research outlook

As any empirical study, ours suffers from several limitations. We limit ourselves to large majority acquisitions by US acquirers between 2004 and 2022. This could introduce a selection bias. Further research might be warranted that includes other transaction sizes, market phases and more regional diversification (i. e. different countries for acquirers).

Our leading question is the influence of larger, sudden changes in a given company on the forecast error (FE) of analyst estimates for that company. We choose to focus our analysis on M&A transactions. Other events might also be worthy of further investigation, such as e.g. the introduction of a new main product line or changes in management and strategy. In this context, it would be useful to investigate which events have a larger influence on FE. This would allow more differentiated statements about the use of analyst reports.

We also choose to limit ourselves to consensus estimates rather than individual ones. Although we do so consciously and for specific reasons (which are laid out in section 4.3.2), an analysis based on individual analysts might help uncover additional moderating factors for the overall increase in FE following an M&A announcement. One potential area of research in this regard might be the analysis of changes in FE for joint analysts, similar to Cortes and Marcet (2023). Moreover, it would be beneficial to analyze which analysts and research firms provide more accurate estimates. With regard to Jegadeesh and Kim (2010), who recognize a herding behavior of analysts, it could be worthwhile to investigate whether the significant increase in FE observed in our study is due to individual analysts, or if the trend is independent of this.

Based on our findings, future studies could also examine other qualitative aspects similar to the analysis by Andersson et al. (2020). This could help to identify specific challenges and uncertainties that equity research analysts face when forecasting synergy benefits and integration costs in the context of M&A transactions. Approaches could be developed that contribute to improved methodologies and more efficient information gathering. This could be used to improve the forecasting accuracy of equity research analysts in general and surrounding major events.

Despite the limitations above, our study provides valuable insights into the use of analyst reports and establishes a solid foundation for future research. Thus, this work makes a valuable contribution to the broader academic discourse on analyst forecasts.

4.6 Conclusion

We find a highly significant positive influence of large M&A announcements on the forecast error (FE) of analyst estimates concerning the acquirer's earnings. This result is robust to including a wide range of control variables and fixed effects as well as alternative measurements for the FE itself in our regression models.

This finding is in line with our main hypothesis, that M&A announcements increase the complexity and / or information asymmetry for analyst forecasts and thus increase the difficulty of accurately estimating future earnings. Following an M&A announcement, analyst estimates tend to become more overly optimistic. This might be due to synergy effects being valued too high and cost of post merger integration too low.

The size of the increase in FE in our data set rises with the relative deal value (relative to the market value of the acquirer), which is in line with our expectations. Relatively larger transactions lead to, ceteris paribus, larger expected synergies and larger post merger integration effort, both increasing complexity and uncertainty. Dampening influences are found for higher numbers of analysts following a given company and higher effects on the acquirer's share price (CAR) around the M&A announcement date. This is again in line with our expectations, as both of those factors increase awareness for the transaction and thus can help to decrease information asymmetries. However, influence factors for the size of the increase are not as robust as the increase itself, opening avenues for further research.

Our findings directly add to those of Andersson et al. (2020), which are derived by a qualitative approach in form of a case study. As our analysis takes a broader and quantitative approach, our results are more easily suited for generalization. In addition, we extend the findings of Brown et al. (2024), who approach the accuracy of analyst forecasts from a different perspective. While Brown et al. (2024) observe an indirect increase in FE for industry peers in the context of M&A-related delistings, our analysis indicates that the FE also rises directly for the acquiring party. Furthermore, the main effect we find seems to contradict Barinov et al. (2024), who find no effect of M&A activity on FE. However, our data sets differ in two main aspects: (1) Barinov et al. (2024) focus solely on conglomerates and (2) we limit ourselves to majority acquisitions with a relative size of at least 10% of the acquirer's market value and thus a substantial impact on their financials.

Our study offers important practical implications that are highly relevant for several capital market participants, especially investors, companies and financial analysts themselves. The significant increase in FE and overoptimism among analysts can most likely be attributed to the increased complexity caused by M&A announcements. It follows that analysts' forecasts should not be used as the sole basis for investors' decisions in phases of intensive M&A activity. The existing literature demonstrates that analysts exert a significant influence on the decisions of investors and that the cost of capital is influenced by this. As higher forecast accuracy can lead to a reduction in the cost of capital, companies should be encouraged to provide more transparent information to analysts, especially surrounding M&A transactions. Our research also provides important information for analysts: It increases awareness for the average increase in FE and overoptimism due to M&A activities, which in turn could lead to better estimates.

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Appendices

Appendix to Section 3

Abnormal percentage returns when selling the position at close on...					
	$t_{0\ days}$	$t_{1\ day}$	$t_{5\ days}$	$t_{10\ days}$	$t_{20\ days}$
count	23,114	23,114	23,114	23,114	23,114
mean	0.29%	0.41%	0.34%	0.03%	-0.35%
p-value	0.0000***	0.0000***	0.0000***	0.8007	0.0072***
t-statistic	7.82	7.98	4.17	0.25	-2.69
std	5.72%	7.82%	12.30%	15.30%	19.59%
min	-98.23%	-97.77%	-99.98%	-100.16%	-101.45%
25%	-1.41%	-2.22%	-4.34%	-6.09%	-8.88%
50%	0.06%	0.06%	-0.15%	-0.46%	-0.98%
75%	1.72%	2.62%	4.10%	4.98%	6.31%
max	103.56%	168.09%	366.43%	422.39%	568.56%

Table A.3.1: Summary statistics abnormal percentage returns - 5 minute buying window
This table reports summary statistics of abnormal percentage returns achieved when buying stock at the 5 minute volume weighted average price after the publication of an insider trade and selling at the close price after the shown number of trading days. Abnormal returns are calculated by subtracting the FF-5-Factor beta weighted S&P 500 return. p-values and t-statistics shown are associated with tests for the statistical significance of the differences from zero, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

Abnormal USD returns, when selling the position at close on...					
	$t_{0\ days}$	$t_{1\ day}$	$t_{5\ days}$	$t_{10\ days}$	$t_{20\ days}$
count	23,114	23,114	23,114	23,114	23,114
mean	1,502	1,803	-1,205	-2,893	-5,718
p-value	0.0135**	0.0223**	0.4412	0.0772*	0.0128**
t-statistic	2.47	2.29	-0.77	-1.77	-2.49
std	92,432	119,921	237,932	248,815	349,059
min	-2,494,003	-3,359,772	-21,477,677	-15,151,029	-39,069,441
25%	-277	-484	-1,053	-1,528	-2,278
50%	2	1	-3	-11	-29
75%	500	701	981	1,148	1,260
max	7,448,248	11,912,855	11,617,151	17,011,053	12,903,492

Table A.3.2: Summary statistics abnormal USD returns - 5 minute buying window
This table reports summary statistics of abnormal USD returns achieved, when buying stock in the volume of 25% of the trading volume in the 5 minutes after the publication of an insider trade at the volume weighted average price of that time frame and selling at close after the shown number of trading days. Abnormal returns are calculated by subtracting the FF-5-Factor beta weighted S&P 500 return from the percentage return before multiplying by the above mentioned USD trading volume. p-values and t-statistics shown are associated with tests of statistical significance of differences from zero, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

	Panel 1: Zero days holding period ($t_{0\ days}$)					
	Lower 25%		25% - 75%		Upper 25%	
	%	*USD*	%	*USD*	%	*USD*
mean	0.17%	-129	0.39%	74	0.22%	5,988
p-value	0.0751*	0.3100	0.0000***	0.7500	0.0011***	0.0121**
t-statistic	1.78	-1.02	8.19	0.32	3.26	2.51
std	7.14%	9,562	5.18%	24,451	5.13%	181,279
min	-95.78%	-474,206	-98.23%	-2,494,003	-94.89%	-2,377,190
25%	-1.83%	-27	-1.36%	-294	-1.15%	-4,570
50%	-0.06%	-0	0.10%	8	0.10%	267
75%	1.92%	23	1.80%	478	1.43%	6,301
max	96.24%	362,549	89.15%	343,378	103.56%	7,448,248
	Panel 2: Five days holding period ($t_{5\ days}$)					
	Lower 25%		25% - 75%		Upper 25%	
	%	*USD*	%	*USD*	%	*USD*
mean	0.78%	-271	0.46%	421	-0.35%	-5,393
p-value	0.0000***	0.0751*	0.0001***	0.3300	0.0174**	0.3800
t-statistic	4.70	-1.78	3.94	0.98	-2.38	-0.87
std	12.59%	11,568	12.62%	46,206	11.29%	471,200
min	-97.37%	-646,432	-99.98%	-3,420,470	-95.30%	-21,477,677
25%	-4.50%	-60	-4.40%	-1,048	-4.04%	-18,028
50%	-0.07%	-0	-0.18%	-12	-0.13%	-325
75%	4.96%	70	4.33%	1,024	3.19%	13,920
max	101.38%	19,191	366.43%	2,491,771	226.03%	11,617,151
	Panel 3: 20 days holding period ($t_{20\ days}$)					
	Lower 25%		25% - 75%		Upper 25%	
	%	*USD*	%	*USD*	%	*USD*
mean	0.12%	-299	-0.43%	-123	-0.65%	-22,326
p-value	0.6800	0.2300	0.0184**	0.7700	0.0033***	0.0146**
t-statistic 0.41	-1.21	-2.36	-0.30	-2.94	-2.44	
std	22.18%	18,743	19.52%	44,706	16.76%	694,746
min	-101.45%	-935,592	-98.27%	-2,530,741	-96.23%	-39,069,441
25%	-10.19%	-145	-9.08%	-2,209	-7.43%	-33,562
50%	-1.15%	-7	-1.11%	-101	-0.52%	-1,267
75%	7.33%	81	6.36%	1,346	5.54%	23,165
max	568.56%	743,083	342.01%	2,696,863	272.96%	12,903,492

Table A.3.3: Returns at three different holding periods for the lower and upper quartile and middle half by trading volume before insider trade publication - 5 minute buying window

This table reports the same values as tables A.3.1 and A.3.2 for different holding periods, but divided into subsets for the lower and upper quartile and middle half of the original dataset with respect to the stocks' trading volume in the two trading days before publication. p-values and t-statistics shown are associated with tests of statistical significance of differences from zero, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

Dependent variable:	Abnormal percentage return, when selling the position at close on...				
	$t_{0\ days}$	$t_{1\ day}$	$t_{5\ days}$	$t_{10\ days}$	$t_{20\ days}$
$log(TradeVol)$	-0.0000	-0.0006	-0.0015**	-0.0016**	-0.0003
	(-0.13)	(-1.28)	(-2.49)	(-2.44)	(-0.41)
$IsOfficer$	-0.0064	-0.0014	0.0064	0.0052	-0.0323**
	(-1.25)	(-0.23)	(0.72)	(0.47)	(-2.49)
$log(InsideVol)$	0.0003	0.0012*	0.0022***	0.0018**	-0.0007
	(0.54)	(1.86)	(2.70)	(2.10)	(-0.88)
$IsOff : log(InsVol)$	0.0007	0.0003	-0.0006	-0.0006	0.0026**
	(1.40)	(0.47)	(-0.71)	(-0.62)	(2.21)
$Overnight$	-0.0029**	-0.0054***	-0.0059***	-0.0077***	-0.0093***
	(-2.15)	(-3.08)	(-2.70)	(-2.99)	(-3.21)
R^2	0.0005	0.0010	0.0021	0.0058	0.0123
Observations	23,114	23,114	23,114	23,114	23,114

Table A.3.4: Influence factors for abnormal percentage returns - regression results for 5 minute buying window
This table reports multivariate linear regression results for the abnormal percentage return when buying within 5 minutes of publication of an insider trade and selling at close after the indicated number of trading days. Returns as explained variables have been log-transformed to continuous returns. Explanatory variables are defined as follows: $log(TradeVol)$ is the natural log of the trading volume in the given stock in USD during the last two trading days before the publication of the insider trade plus one. $IsOfficer$ is a dummy variable that is equal to 1 if the reporting insider is an Officer in the respective company. As the data set only contains trades by Officers or Directors, a value of 0 indicates a Director position. This information is provided directly in the Form 4 filings. $log(InsideVol)$ is defined as the natural log of the reported USD volume of the insider trade plus one, $IsOff : log(InsVol)$ is the interaction of the last two variables. $Overnight$ is a dummy variable that is equal to 1 if the filing has been published near market close or outside of trading hours and the trading window was started at the beginning of the next trading day. Standard errors are clustered by time (by day). t-statistics are reported in parentheses, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

Dependent variable: Abnormal USD return, when selling the position at close on...					
	$t_{0\ days}$	$t_{1\ day}$	$t_{5\ days}$	$t_{10\ days}$	$t_{20\ days}$
$TradeVol$	0.0000	0.0001**	-0.0000	-0.0000	-0.0002
	(1.38)	(2.47)	(-0.16)	(-0.28)	(-1.31)
$IsOfficer$	545.9	1,803.7	38.2	-655.9	-1,513.2
	(0.56)	(1.06)	(0.02)	(-0.26)	(-0.49)
$InsideVol$	0.0000	0.0000*	0.0000	-0.0000	-0.0000
	(0.71)	(1.73)	(0.10)	(-0.05)	(-0.26)
$IsOff : InsVol$	0.0000	0.0000	0.0000	-0.0000	-0.0000
	(0.88)	(0.88)	(0.17)	(-0.98)	(-0.12)
$Overnight$	-3,283.4	-5,476.3**	277.0	-307.9	15,933.2
	(-1.02)	(-2.12)	(0.02)	(-0.03)	(0.99)
R^2	0.0799	0.0888	0.0022	0.0047	0.1277
Observations	23,114	23,114	23,114	23,114	23,114

Table A.3.5: Influence factors for abnormal USD returns - regression results for 5 minute buying window
This table reports multivariate linear regression results for the abnormal USD return when buying within 5 minutes of publication of an insider trade and selling at close after the indicated number of trading days. Explanatory variables are defined as in table A.3.4, with the difference that neither the returns nor any of the explanatory variables have been logarithmically transformed, to allow a more intuitive interpretation of the coefficients. Standard errors are clustered by time (by day). t-statistics are reported in parentheses, with ***, **, * denoting statistical significance at the 1%, 5%, and 10% level.

Appendix to Section 4

List of tables in the Appendix to Section 4

	Dependent variable: FE averaged over four calendar quarters, winsorized at 2% and log-transformed by applying log(FE + 1)											
	AfterM&A	+Industry	+NumEst	+Size	+DealSize	+Public	+CrossBord	+Type	+CAR	Co.+Time	Inter 1	Inter 2
Rainbow test for linearity												
Test stat.	0.9589	0.9579	0.9640	0.9683	0.9678	0.9676	0.9695	0.9706	0.9693	1.2304	0.9671	1.2242
p-value	0.8584	0.8636	0.8255	0.7946	0.7984	0.7999	0.7857	0.7766	0.7871	0.0000	0.8030	0.0000
Breusch-Pagan test												
Lagrange-Mult. stat.	16.88	51.33	57.84	185.70	187.00	186.87	191.68	193.96	195.16	2,206.07	198.15	2,195.86
p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Levene's test												
Test stat.	6.8637	6.1618	7.1063	6.9679	6.9734	6.9922	6.9732	6.833	6.8626	11.5978	6.7833	11.2173
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Variance inflation factors (VIF)												
$Intercept$	2.0000	14.0600	14.8715	35.6170	40.0800	42.3021	42.3971	43.3969	43.6904	-	69.5276	-
$AfterM\&A$	1.0	1.0	1.0029	1.0038	1.0038	1.0038	1.0038	1.0039	1.0039	-	26.5960	-
$NumOfEstimates$	-	-	1.1155	1.4465	1.4560	1.4597	1.4608	1.4788	1.4795	-	2.6703	-
$log(MarketCap)$	-	-	-	1.3415	1.3878	1.5123	1.5125	1.5136	1.5162	-	2.7900	-
$RelativeValue$	-	-	-	-	1.0557	1.1278	1.1290	1.1340	1.1348	-	2.1645	-
$PublicDummy$	-	-	-	-	-	1.1966	1.1966	1.1976	1.2115	-	1.2117	-
$CrossBorderDummy$	-	-	-	-	-	-	1.0288	1.0292	1.0302	-	1.0302	-
$HorizontalDummy$	-	-	-	-	-	-	-	1.5136	1.5139	-	1.5140	-
$VerticalDummy$	-	-	-	-	-	-	-	1.4861	1.4865	-	1.4865	-
$log(CAR+1)$	-	-	-	-	-	-	-	-	1.0356	-	2.0487	-
$After: NumOfEstimates$	-	-	-	-	-	-	-	-	-	-	4.0279	-
$After: log(MarketCap)$	-	-	-	-	-	-	-	-	-	-	28.2356	-
$After: RelativeValue$	-	-	-	-	-	-	-	-	-	-	3.8055	-
$After: log(CAR+1)$	-	-	-	-	-	-	-	-	-	-	2.0403	-
Durbin-Watson test												
Test stat.	1.9973	1.9978	1.9912	1.9883	1.9884	1.9878	1.9879	1.9861	1.9864	1.9812	1.9930	1.9906
Kolmogorov-Smirnov test												
Test stat.	0.1617	0.1618	0.1609	0.1546	0.1539	0.1552	0.1547	0.1541	0.1555	0.1169	0.1542	0.1155
p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

Table A.4.1: Influence factors for FE - regression diagnostics

This table reports various regression diagnostics corresponding to the regressions shown in Table 4.3. All variables and models are defined as shown there. VIF not meaningful for models with company and time fixed effects.

	Dependent variable: FE averaged over four calendar quarters, winsorized at 2% and log-transformed									
	AfterM&A	+Industry	+NumEst	+Size	+DealSize	+Public	+CrossBord	+Type	+CAR	Inter 1
Intercept										
mean	0.4813	0.3830	0.4230	0.9791	0.9662	0.9762	0.9759	0.9577	0.9651	1.0147
std	0.0093	0.0221	0.0238	0.0390	0.0426	0.0416	0.0438	0.0431	0.0438	0.0554
CI Lower	**0.4637**	**0.3387**	**0.3754**	**0.8985**	**0.8818**	**0.8970**	**0.8922**	**0.8752**	**0.8848**	**0.9038**
CI Upper	**0.4992**	**0.4269**	**0.4695**	**1.0503**	**1.0513**	**1.0533**	**1.0592**	**1.0357**	**1.0509**	**1.1248**
AfterM&A										
mean	0.0752	0.0748	0.0805	0.0733	0.0727	0.0729	0.0724	0.0728	0.0730	-0.0256
std	0.0144	0.0138	0.0136	0.0134	0.0137	0.0134	0.0130	0.0133	0.0134	0.0714
CI Lower	**0.0479**	**0.0476**	**0.0537**	**0.0473**	**0.0447**	**0.0463**	**0.0467**	**0.0465**	**0.0473**	-0.1684
CI Upper	**0.1036**	**0.1015**	**0.1065**	**0.0989**	**0.0995**	**0.0988**	**0.0978**	**0.0980**	**0.0996**	0.1084
NumOfEstimates										
mean	-	-	-0.0121	0.0060	0.0060	0.0058	0.0060	0.0055	0.0054	0.0109
std	-	-	0.0020	0.0025	0.0025	0.0025	0.0024	0.0025	0.0026	0.0033
CI Lower	-	-	**-0.0160**	**0.0015**	**0.0010**	**0.0011**	**0.0014**	**0.0007**	**0.0004**	**0.0048**
CI Upper	-	-	**-0.0081**	**0.0112**	**0.0108**	**0.0108**	**0.0110**	**0.0106**	**0.0101**	**0.0175**
log(MarketCap)										
mean	-	-	-	-0.0890	-0.0881	-0.0894	-0.0895	-0.0892	-0.0899	-0.0972
std	-	-	-	0.0050	0.0052	0.0053	0.0054	0.0053	0.0053	0.0070
CI Lower	-	-	-	**-0.0987**	**-0.0982**	**-0.0995**	**-0.1002**	**-0.0998**	**-0.1006**	**-0.1119**
CI Upper	-	-	-	**-0.0789**	**-0.0778**	**-0.0797**	**-0.0787**	**-0.0786**	**-0.0793**	**-0.0841**
RelativeValue										
mean	-	-	-	-	0.0181	0.0135	0.0150	0.0103	0.0127	-0.0481
std	-	-	-	-	0.0249	0.0259	0.0259	0.0267	0.0267	0.0328
CI Lower	-	-	-	-	-0.0298	-0.0361	-0.0350	-0.0394	-0.0401	-0.1110
CI Upper	-	-	-	-	0.0663	0.0644	0.0640	0.0603	0.0623	0.0183
PublicDummy										
mean	-	-	-	-	-	0.0136	0.0123	0.0116	0.0091	0.0083
std	-	-	-	-	-	0.0161	0.0154	0.0157	0.0164	0.0163
CI Lower	-	-	-	-	-	-0.0181	-0.0169	-0.0185	-0.0200	-0.0239
CI Upper	-	-	-	-	-	0.0432	0.0412	0.0427	0.0406	0.0419
CrossBorderDummy										
mean	-	-	-	-	-	-	0.0127	0.0106	0.0098	0.0098
std	-	-	-	-	-	-	0.0196	0.0190	0.0187	0.0185
CI Lower	-	-	-	-	-	-	-0.0216	-0.0268	-0.0255	-0.0228
CI Upper	-	-	-	-	-	-	0.0532	0.0474	0.0484	0.0477
HorizontalDummy										
mean	-	-	-	-	-	-	-	0.0400	0.0408	0.0411
std	-	-	-	-	-	-	-	0.0156	0.0165	0.0164
CI Lower	-	-	-	-	-	-	-	**0.0085**	**0.0083**	**0.0080**
CI Upper	-	-	-	-	-	-	-	**0.0700**	**0.0725**	**0.0718**
VerticalDummy										
mean	-	-	-	-	-	-	-	0.0419	0.0429	0.0433
std	-	-	-	-	-	-	-	0.0215	0.0211	0.0214
CI Lower	-	-	-	-	-	-	-	**0.0029**	**0.0037**	-0.0007
CI Upper	-	-	-	-	-	-	-	**0.0851**	**0.0857**	0.0842
log(CAR + 1)										
mean	-	-	-	-	-	-	-	-	-0.1364	0.0887
std	-	-	-	-	-	-	-	-	0.0941	0.1209
CI Lower	-	-	-	-	-	-	-	-	-0.3306	-0.1532
CI Upper	-	-	-	-	-	-	-	-	0.0439	0.3086
After : NumOfEstimates										
mean	-	-	-	-	-	-	-	-	-	-0.0108
std	-	-	-	-	-	-	-	-	-	0.0043
CI Lower	-	-	-	-	-	-	-	-	-	**-0.0189**
CI Upper	-	-	-	-	-	-	-	-	-	**-0.0022**
After : log(MarketCap)										
mean	-	-	-	-	-	-	-	-	-	0.0149
std	-	-	-	-	-	-	-	-	-	0.0098
CI Lower	-	-	-	-	-	-	-	-	-	-0.0041
CI Upper	-	-	-	-	-	-	-	-	-	0.0341
After : RelativeValue										
mean	-	-	-	-	-	-	-	-	-	0.1162
std	-	-	-	-	-	-	-	-	-	0.0512
CI Lower	-	-	-	-	-	-	-	-	-	**0.0220**
CI Upper	-	-	-	-	-	-	-	-	-	**0.2205**
After : log(CAR + 1)										
mean	-	-	-	-	-	-	-	-	-	-0.4542
std	-	-	-	-	-	-	-	-	-	0.1813
CI Lower	-	-	-	-	-	-	-	-	-	**-0.8049**
CI Upper	-	-	-	-	-	-	-	-	-	**-0.0895**

Table A.4.2: Influence factors for FE - bootstrapping regression
This table reports coefficients, their standard deviation and 95% confidence intervals using a bootstrapping method for regressions shown in Table 4.3 over 1,000 iterations. Models with company and time fixed effects are excluded, as insufficient numbers of observations per company lead to convergence issues during the resampling process. All variables and models are defined as shown in Table 4.3. Bold formatting indicates statistical significance at the 5% level.

	Dependent variable: Change in FE averaged over four calendar quarters from before to after M&A announcement, winsorized at 2%								
	Only NumEst	+Size	+DealSize	+Public	+CrossBorder	+Type	+CAR	+IndFixed	+TimeFixed
Rainbow test for linearity									
Test stat.	1.0232	1.0264	1.0201	1.0190	1.0168	1.0160	1.0105	1.0048	0.9977
p-value	0.3395	0.3189	0.3600	0.3668	0.3821	0.3871	0.4253	0.4658	0.5168
Breusch-Pagan test									
Lagrange-Mult. stat.	2.19	34.13	35.81	36.71	38.97	40.52	43.84	80.34	96.00
p-value	0.1392	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Levene's test									
Test stat.	5.59	5.59	5.50	5.44	5.44	5.42	5.39	5.34	4.70
p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Variance inflation factors (VIF)									
$Intercept$	2.0259	22.2320	26.5482	27.9931	28.2334	30.1940	30.3336	42.6979	55.3617
Num_{before}	1.0000	1.2401	1.2456	1.2564	1.2574	1.2631	1.2655	1.4598	1.5344
$log(MarketCap)$	-	1.2401	1.2741	1.3712	1.3713	1.3738	1.3752	1.5095	1.5856
$RelativeValue$	-	-	1.0275	1.0925	1.0950	1.1060	1.1071	1.1367	1.1860
$PublicDummy$	-	-	-	1.1772	1.1773	1.1804	1.1934	1.2132	1.2351
$CrossBorderDummy$	-	-	-	-	1.0033	1.0041	1.0054	1.0301	1.0375
$HorizontalDummy$	-	-	-	-	-	1.3802	1.3804	1.5125	1.5381
$VerticalDummy$	-	-	-	-	-	1.3623	1.3624	1.4865	1.5056
$log(CAR + 1)$	-	-	-	-	-	-	1.0249	1.0366	1.0568
Durbin-Watson test									
Test stat.	1.9903	1.9915	1.9910	1.9910	1.9938	1.9948	2.0014	1.9937	2.0357
Kolmogorov-Smirnov test									
Test stat.	0.2176	0.2154	0.2163	0.2149	0.2132	0.2162	0.2106	0.2102	0.1950
p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

Table A.4.3: Influence factors for difference in FE - regression diagnostics
This table reports various regression diagnostics corresponding to the regressions shown in Table 4.4. All variables and models are defined as shown there.

Dependent variable: Change in FE averaged over four calendar quarters from before to after M&A announcement, wins. at 2%								
	Only NumEst	+Size	+DealSize	+Public	+CrossBorder	+Type	+CAR	+IndFixed	+TimeFixed
$Intercept$									
mean	0.3617	0.2505	0.0168	-0.0090	-0.0507	-0.0819	-0.0445	-0.0483	0.0267
std	0.0577	0.1821	0.1945	0.1935	0.1938	0.1960	0.1996	0.2424	0.2732
CI Lower	**0.2534**	-0.0970	-0.3678	-0.3951	-0.4242	-0.4470	-0.4278	-0.5057	-0.5159
CI Upper	**0.4722**	0.6157	0.4037	0.3761	0.2999	0.3280	0.3582	0.4276	0.5512
Num_{before}									
mean	-0.0234	-0.0265	-0.0288	-0.0278	-0.0265	-0.0277	-0.0285	-0.0321	-0.0243
std	0.0102	0.0108	0.0109	0.0109	0.0108	0.0108	0.0110	0.0119	0.0122
CI Lower	**-0.0436**	**-0.0477**	**-0.0493**	**-0.0498**	**-0.0476**	**-0.0487**	**-0.0503**	**-0.0555**	-0.0479
CI Upper	**-0.0044**	**-0.0066**	**-0.0065**	**-0.0062**	**-0.0047**	**-0.0077**	**-0.0063**	**-0.0096**	0.0003
$log(MarketCap)$									
mean	-	0.0170	0.0307	0.0357	0.0354	0.0366	0.0339	0.0358	0.0331
std	-	0.0239	0.0243	0.0249	0.0252	0.0242	0.0250	0.0252	0.0263
CI Lower	-	-0.0303	-0.0187	-0.0157	-0.0114	-0.0135	-0.0119	-0.0128	-0.0194
CI Upper	-	0.0615	0.0798	0.0843	0.0826	0.0832	0.0849	0.0854	0.0860
$RelativeValue$									
mean	-	-	0.4116	0.4357	0.4493	0.4545	0.4658	0.4615	0.4740
std	-	-	0.1510	0.1619	0.1536	0.1538	0.1583	0.1568	0.1575
CI Lower	-	-	**0.1167**	**0.1378**	**0.1410**	**0.1502**	**0.1474**	**0.1617**	**0.1665**
CI Upper	-	-	**0.7178**	**0.7545**	**0.7550**	**0.7586**	**0.7762**	**0.7815**	**0.7818**
$PublicDummy$									
mean	-	-	-	-0.0762	-0.0746	-0.0775	-0.1017	-0.0998	-0.1018
std	-	-	-	0.0920	0.0948	0.0918	0.0922	0.0930	0.0883
CI Lower	-	-	-	-0.2444	-0.2519	-0.2523	-0.2791	-0.2891	-0.2732
CI Upper	-	-	-	0.1000	0.1212	0.0994	0.0810	0.0837	0.0729
$CrossBorderDummy$									
mean	-	-	-	-	0.1833	0.1882	0.1703	0.1604	0.1939
std	-	-	-	-	0.1179	0.1135	0.1110	0.1107	0.1114
CI Lower	-	-	-	-	-0.0363	-0.0380	-0.0453	-0.0446	-0.0258
CI Upper	-	-	-	-	0.4116	0.4020	0.3907	0.3897	0.4195
$HorizontalDummy$									
mean	-	-	-	-	-	0.0125	0.0201	0.0641	0.0434
std	-	-	-	-	-	0.0891	0.0912	0.0912	0.0957
CI Lower	-	-	-	-	-	-0.1596	-0.1414	-0.1211	-0.1481
CI Upper	-	-	-	-	-	0.1812	0.2060	0.2434	0.2182
$VerticalDummy$									
mean	-	-	-	-	-	0.1041	0.1104	0.1702	0.1559
std	-	-	-	-	-	0.1170	0.1140	0.1228	0.1254
CI Lower	-	-	-	-	-	-0.1316	-0.1054	-0.0608	-0.0779
CI Upper	-	-	-	-	-	0.3405	0.3303	0.4140	0.3979
$log(CAR+1)$									
mean	-	-	-	-	-	-	-1.2453	-1.2989	-1.0493
std	-	-	-	-	-	-	0.4990	0.4828	0.4976
CI Lower	-	-	-	-	-	-	**-2.1907**	**-2.2101**	**-1.9765**
CI Upper	-	-	-	-	-	-	**-0.1860**	**-0.3245**	**-0.0784**

Table A.4.4: Influence factors for difference in FE - bootstrapping regression
This table reports coefficients, their standard deviation and 95% confidence intervals using a bootstrapping method for regressions shown in Table 4.4 over 1,000 iterations. All variables and models are defined as shown in Table 4.4. Bold formatting indicates statistical significance at the 5% level.