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The Effect of M&A Delistings on Industry Peers' Information Environment

An Empirical Study of the Swedish Stock Market

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This study investigates the association between M&A delistings and the information environment of industry peers, and how this association is moderated by whether the M&A is concentric or conglomerate. We operationalize the information environment as the mean forecast error of equity analysts and use a panel fixed effects model to analyze Swedish M&A delistings from 2008 to 2024. Findings reveal an association between M&A delistings and a deterioration in the information environment, but no support for M&A type to be associated with that deterioration. The study is consistent with M&A delistings in Sweden having informational externalities, and results are partly consistent with prior research on the US market, although the estimated deteriorative effect is considerably larger.

Keywords: Negative information externalities, M&A, Delistings, Information Environment

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1. Introduction

This section provides a brief overview of M&A delistings, the information environment, and the Swedish context. It also presents the purpose and relevance of the study, as well as the research questions. Finally, we outline the scope and limitations, the results, and the contributions made.

1.1. Background

Mergers and acquisitions, hereafter M&A, refers to the combination of two separate firms into one. M&A can allow firms to achieve economies of scale, acquire new assets, or integrate supply chains, but can also be motivated by a wide range of other motives. An M&A event leading to the delisting of a public entity is called an M&A delisting. M&A delistings have well-documented effects on the competitive environment, but their effects on the information environment, hereafter IE, are not well understood (Fee & Thomas, 2004). The IE is a common concept in accounting and finance without a conclusive definition but can broadly be understood as the information relevant to assessing a firm's value.

There are reasons to suspect that M&A delistings have an impact on the IE. Financial reports of peers constitute an important part of the IE, and since reporting requirements are significantly less comprehensive for non-listed entities, M&A delistings might have a direct adverse effect on the IE. The competitive environment has effects on the IE, and since M&A delistings lead to changes in the competitive environment, there may also be an indirect effect of M&A delistings on the IE. Because the competitive environment is likely to be affected in different ways by different types of M&As, the effects of concentric (intra-industry) and conglomerate (extra-industry) M&A may also be different.

1.2. Purpose and Research Questions

According to McKinsey, from 2000 to 2020, the number of listed firms in the United States, hereafter US, decreased by about 25% (*A Closer Look at Trends in Public Company Listings and IPOs*, n.d.). Although Sweden has not seen the same trend over that period, similar mechanisms may explain the steady decline in the number of listed firms that the market has seen since 2022 (*Strong Stock Market Led to Increased Share Wealth*, 2024). Because of the positive information externalities associated with a higher number of listed firms, concern has been expressed regarding possible negative market-wide informational effects arising from the

decline in listed firms, for which delistings are an important mechanism (A. B. Brown et al., 2024; Cheng, 2021; White et al., 2013).

While the informational spillover effects of structural events have previously been studied, the current body of work is limited. We have only been able to locate three empirical papers on this topic, of which only one studies M&A delistings (A. B. Brown et al., 2024; Hinson & Piao, 2024; Kim & Ljungqvist, 2023). Consequently, there have been calls for more analysis of the topic (A. B. Brown et al., 2024). The three studies on such spillover effects have all been examining the US market. As such, the informational effects of M&A delistings have not been studied outside of the US context, and thus not in the Swedish context.

Apart from the novelty, studying the Swedish market may be especially illuminating for several reasons. Whereas private US firms do not have to disclose their financial statements to the public, all limited Swedish companies must do so (Gill, 2017; *Swedish Company Audit, Financial Statements, Accounting, Consulting in the Sweden | GSL*, n.d.). Because of this, the effect of delistings may be smaller in Sweden, as the absolute decrease in information following a delisting is smaller. On the other hand, it is plausible to expect Swedish firms to be smaller and fewer, with fewer analysts following them, and that analysts' collective forecasting abilities are worse due to there being fewer analysts in Sweden than in the US. Because of this, the IE of US firms might on average be better than the IE of Swedish firms, and hence items of information may be more valuable relatively in a Swedish context. Because of this, the effect of delisting may be greater in Sweden, as the relative decrease in information following a delisting is larger.

In terms of private equity fundraising relative to GDP, Sweden has the second most private equity relative to its size in Europe, trailing only Luxembourg ("The Economic Footprint of Swedish Venture Capital and Private Equity," 2022). While buying non-listed assets is a common private equity strategy, so is acquiring listed ones. As such, the Swedish market may experience more M&A delistings relative to its size, thus making the topic even more relevant in the context.

M&A delistings will inevitably have some effect on the competitive environment, and since the competitive environment affects the IE, there may also be indirect effect on the IE. That the effect on the competitive landscape differs between conglomerate and concentric M&A has been found in several inquiries (see e.g. Sheen, 2014). Since part of the effect that M&A delistings may have on the IE is indirect and dependent on the competitive effects, the effect

could also differ between conglomerate and concentric M&A. If one type of M&A is associated with a larger deterioration in the IE, that may help illuminate not only the informational costs of types of M&A, but also the mechanical differences.

The number of listed firms is decreasing across markets, and informational spillover effects of M&A delistings have not been studied extensively, or at all outside of the US market. While theories on information spillovers from M&A exist, how M&A delistings in general, and Concentric and Conglomerate M&A delistings in particular, impact the IE is not understood.

The differences in context between the US and Sweden, in addition to the limited prior research into these topics, underscores the relevance of the study. By studying informational effects of M&A delistings, and how these differ between conglomerate and concentric M&A, our study aims to address significant gaps in the literature and may further the understanding of this understudied topic. Ultimately, this may help understand the severity and nature of the informational costs of M&A delistings. As such, in this study we aim to answer the following research questions:

Research question 1: *Do M&A delistings have an impact on analysts' information environment for industry peer firms?*

Research question 2: *Does the effect of M&A delistings on analysts' information environment for industry peer firms differ between concentric and conglomerate M&A?*

Our study adopts a similar methodology to the one carried out by A. B. Brown et al. (2024). In doing so, we employ panel fixed effects regression models, using analysts' quarterly forecast error on earnings of industry peers to M&A delistings as a proxy for the IE of equity analysts. The mean forecast error of analysts is the dependent variable, and the independent variable for research question 1 is an indicator of whether the period is in the pre- or post-M&A period. To answer research question 2, we add an interaction term between the pre- or post-M&A period indicator and a variable indicating whether the M&A is concentric or conglomerate in nature. An extensive control structure with entity fixed-effects, time-fixed-effects, and cross-sectional controls is used, and the longer-term effects are studied by excluding the 2 quarters immediately after the M&A delisting. The analysis is conducted on Swedish public stock markets on quarters in the years 2008-2025, and 7,717 quarterly observations of analyst forecast errors for industry peers to M&A delistings.

The results support M&A delistings being associated with a deterioration in the IE of industry peers, but are inconclusive regarding whether this effect differs between concentric and conglomerate M&A. These findings expand the small body of research on how the IE is impacted by structural market events.

The study is divided into five sections. After the introduction, the second section examines and presents the theoretical framework. The third section outlines our methodology and hypotheses based on the body of previous research. In the fourth section, results are presented and described, while the fifth section discusses these results, their implications and limitations, and suggests areas of future research.

2. Theoretical framework

In this section we outline the theoretical framework of our study. We start with reviewing the concept of the IE and how analysts interact with it, followed by an overview of M&A in general. Finally, the theoretical expectations of how M&A delistings impact the IE, and how this effect is moderated by M&A type are outlined.

2.1. The Information Environment

The IE is a frequently occurring theme in accounting and finance research, but there is currently no consensus on its meaning (Jonnergård et al., 2020). The generally accepted notion is that IE is information sources relevant for assessing a firm's value, either as the de facto sum of all available data, or the result of information dissemination from the company to the public and investors (Bushman et al., 2003; Collins & Kothari, 1989).

Jonnergård et al. (2020) suggest limiting the usage of the concept IE to what they call "firm-level IE", describing it as "the phenomenon as such, the relations to transparency and disclosure and the relations needed to investigate a specific empirical level of the concept." In this version of IE, the most commonly utilized definition is the one made by Collins & Kothari (1989) who describe the IE as:

> All sources of information relevant to assessing firm value. It includes government reports on macroeconomic conditions, industry reports and trade association publications, firm-specific news in the financial press and reports issued by analysts

> and brokerage houses in addition to accounting reports, and vertical and intra-industry information transfers via sales and industry reports. (p. 145)

Jonnergård et al. (2020) expand on this and establish four key components of IE, namely:

Management - Described as a bridge between firms and the public, management has access and control over several important features of financial and non-financial information, including public disclosures, voluntary disclosures, management forecasts, conference calls, and earning warnings. These information sources all contribute to the firm-level IE.

Ownership - Different levels of institutional/foreign ownership will require different levels of disclosures, either through regulatory requirements or by ownership pressure to disclose additional information.

Financial Analysts - Described as intermediaries between companies and investors, financial analysts and their reports are a key aspect of a firm's IE.

Media - Like the function of financial analysts, media reports on companies provide specific information to outsiders.

2.2. Analysts' Interaction with the Information Environment

As Jonnergård et al. (2020) highlight, financial analysts are a key component of a firm's IE, serving as an intermediary between companies and the public. Several studies have been conducted connecting the level of analyst coverage to the firm's level of IE (see e.g. Armstrong et al., 2012; Jonnergård et al., 2020). Analysts' role in well-functioning financial markets have been highlighted, and they have been described as actors providing information discovery, interpretation, and dissemination (M. Bradshaw et al., 2017). Furthermore, analysts' usage of public and private information in creating their forecasts to some extent reflects the current IE of the company, as analysts have been shown to utilize many different forms of firm- and industry-level information in their estimations (M. Bradshaw et al., 2017).

The different information sources used provide a holistic picture of the firm's IE. As such, analysts' estimates are used in empirical research on financial markets and the IE in several ways; error in mean forecast is used as a proxy for mean precision of information known to all analysts, whereas forecast dispersion has been used as a proxy for precision of analysts' idiosyncratic information, both originally proposed in the framework developed by Barron et

al. (1998). The notion is that since analysts' precision is dependent on the IE, higher forecast errors indicate a worse IE. While analysts' error dispersion has been argued to not fully capture investor uncertainty due to measuring analysts' idiosyncratic information, analyst error measures have still prevailed as tools for estimating the IE, particularly the analysts' IE (Abarbanell et al., 1995; Heflin et al., 2001; Lang et al., 2003).

2.3. Mergers and Acquisitions

M&A refers to the combination of two separate entities into one. Merger typically refers to two entities ceasing to exist individually and creating a joint entity, whereas acquisition typically refers to the takeover of the target by an acquiring entity (Malik et al., 2014). Regardless, the result is that whereas there used to be two entities operating, the two form one joint entity. M&A typically involves some type of consideration transferred to the owners of the target entity of an acquisition, or to both sets of owners in a merger.

In M&A where the acquired or merged firm was traded on a public exchange, one or more firms usually delist because of the M&A. The exception occurs when an unlisted company buys or merges with a listed firm, where the resulting combined entity may be listed, in which case the M&A is a Reverse Takeover (RTO) (Gleason et al., 2005). All cases of M&A in which the target firm or one of the merging firms used to be listed, besides RTOs, thus result in a delisting of the target firm or at least one of the merging firms. These cases are a subset of M&A, denoted as M&A delistings, which result in fewer listed firms.

M&As are typically a significant business event for both parties of the transaction. As a result of M&A, the resulting firm may face a variation of outcomes (Hossain, 2021). M&A may also have effects beyond those affecting the parties involved in the transaction. The competitive landscape of an industry can change because of an M&A, and M&A in one industry may impact another industry (Fee & Thomas, 2004).

Economic theory posits, and empirical studies support, a list of different M&A motives. Andrade et al. (2001) report an extensive set of internal explanations including "efficiency-related reasons", other "synergies", and "attempts to create market power". Apart from internal conditions such as these, industry-wide shocks to factors such as technology, demand, or regulation can also result in M&A transactions (Harford, 2005; Mitchell & Mulherin, 1996). As such, any notion that M&A is randomly distributed is incorrect, and concerns about studying

the effects of M&A arise. It may be that some shocks are associated with both M&A delistings and changes to the IE.

M&A may be categorized based on the industries of the involved parties. Concentric M&A, also known as horizontal M&A, is M&A where the acquirer and target are in the same industry, whereas conglomerate M&A is M&A where the acquirer and target are in different industries (A. B. Brown et al., 2024). Concentric and conglomerate M&A have been found to have differences, for example, Sheen (2014) finds concentric M&A results in greater competitive effects.

2.4. Effects of M&A on the Information Environment

In a delisting event, when a publicly traded company stops posting financial disclosures in the detail required by regulation for traded firms, the IE is negatively impacted, which is expected to be reflected in analysts' estimates (A. B. Brown et al., 2024). This is because intra-industry information transfers have been shown to play an important role in capital markets, especially among sell-side analysts (Foster, 1981; Muslu et al., 2014). While Bushman (1991) finds that the value of public disclosures for analysts varies with the structure of the private information market in the industry, public disclosures are important for analysts' information production function (Muslu et al., 2014). The direct informational effect of not having access to peer firms' interim reports, hereafter the delisting effect, is therefore expected to negatively impact analysts' precision, increasing forecasting errors (A. B. Brown et al., 2024).

M&A has long been documented to have an impact on industry structure and competitive dynamics (Fee & Thomas, 2004; Mitchell & Mulherin, 1996). However, the impact of M&A on the IE moderated by competitive changes, hereafter the merger effect, has generally been under-explored (A. B. Brown et al., 2024). Various aspects of the competitive environment have been found to have an impact on the IE. Alshehabi et al. (2024) find that companies facing higher competition are more likely to report impairments relevant to equity valuation decisions. Bushee et al. (2021) report that sales forecasts made by firms central in a network of collaborating companies tend to be more accurate. Several studies, for example those by El Diri et al. (2020), and Laksmana & Yang (2014), find that the commonality of earnings management differs between industries based on the degree of competition. Bertomeu et al. (2015) find that the three biggest US carmakers are less likely to publicly share production forecasts when they have greater incentives to compete aggressively. That M&A has an impact

on industry structure and competitive environment is well documented, and that industry structure and competitive environment impact the IE has also been shown in several analyses. Thus, the notion that M&A may impact the IE is indirectly supported by the literature. Recent findings by A. B. Brown et al. (2024) also support the impact of competitive changes due to M&A on the IE explicitly. Whether the merger effect on aggregate has a positive or negative effect on the IE is unclear (A. B. Brown et al., 2024). If the merger effect leads to an increase (decrease) in the variance of future profits, it will be more difficult (easy) for analysts to forecast future earnings (Barefield & Comiskey, 1975; Hope, 2003). It is also important to note that since industry and competitive changes may differ between M&As, the merger effect may be heterogeneous (A. B. Brown et al., 2024). How the merger effect impacts the IE is ambiguous.

2.5. Effects of Concentric and Conglomerate M&A on the Information Environment

The delisting effect mechanism is expected to be similar between concentric and conglomerate M&A. This is because regardless of the type, less information is provided, adversely impacting the IE of peers. Two potential mechanical differences should however be noted.

Firstly, analysts tend to cover firms in the same industry (M. Bradshaw et al., 2017). Because of this, analysts may be less able to substitute the information provided by the target firm with information provided by the combined entity if the M&A is conglomerate than if it is concentric. In cases of conglomerate M&A, the combined entity may be outside the sector covered by analysts, hence causing analysts to face more difficulties in substituting the information of the acquired firm with the combined entity. This is because analysts exploit commonalities between the firms they cover in their information production, and are less likely to cover the combined entity after conglomerate M&A than they are after concentric M&A (Muslu et al., 2014). This applies if the combined entity provides more information after the M&A event than the acquiring firm did before.

Secondly, the change in provision of information by the combined entity may differ between conglomerate and concentric M&A. In concentric M&A the functional integration might on average be greater, and thus it may be that the financial reporting integration is also generally greater. In that case, less information would be provided by the combined entity after concentric M&A than after conglomerate M&A, which would make it more difficult for analysts to

substitute information from the target entity with information from the combined entity after concentric M&A than after conglomerate M&A.

The first difference would point towards a greater delisting effect of conglomerate M&A, whereas the second difference would indicate a greater delisting effect of concentric M&A. Thus, theory would indicate that the delisting effect differs between conglomerate and concentric M&A, but the direction of the difference, if any, is inconclusive.

With regards to the merger effect, Fee & Thomas (2004) find that concentric M&A has effects on the competitive environment, and Sheen (2014) reports that concentric M&A but not conglomerate M&A results in increased price competition within the industry. Since the merger effect is moderated by the competitive environment, and the competitive effects of concentric M&A have been found to be greater, we would expect the merger effect to be more statistically significant for concentric M&A than for conglomerate M&A. This is in line with findings by A. B. Brown et al. (2024). It should be noted that since the theoretical expectations of the direction of the merger effect are inconclusive, so is the expectation of the difference between conglomerate and concentric M&A's merger effect.

3. Data and Methodology

In this section we develop and present our hypotheses and explain the research design and sample derivation. Thereafter, the regression models are presented, and variables are explained and motivated, followed by a discussion of some methodological issues and how they are mitigated.

3.1. Hypothesis Development

Research question 1 is "Do M&A delistings have an impact on analysts' information environment for industry peer firms?" It could be argued that the theoretical expectation of the overall effect of M&A on analysts' IE of industry peers is ambiguous due to the obscure direction of the merger effect, even though we would expect the delisting effect to be associated with a negative impact on analysts' IE. A significant aggregate negative impact on analysts' IE has however been recorded by A. B. Brown et al (2024), and thus we would expect a significantly negative overall association between M&A delistings and analysts' IE.

Research question 2 is “Does the effect of M&A delistings on analysts’ information environment for industry peer firms differ between concentric and conglomerate M&A?” Most theoretical underpinnings would expect the merger effect to be significantly different between conglomerate and concentric M&A, but the expectation of the directionality of the difference is unclear. With regards to the delisting effect, the literature is inconclusive both in terms of differences in significance and directional impact. These factors would lead to an expectation of no significant differences or significant but directionally unclear differences between conglomerate and concentric M&A. Recent empirical findings by A. B. Brown et al (2024) do however find support for a significantly greater negative impact of concentric M&A on analysts’ IE, which is why we would expect findings to be in line with this.

From the research questions we have derived two hypotheses in which the normative directions are consistent with the theoretical framework:

Hypothesis 1: *M&A delistings are associated with a statistically significant increase in mean forecast error for industry peer firms.*

Hypothesis 2: *Concentric M&A delistings are associated with an increase in mean forecast error for industry peer firms that is statistically significantly greater than that of conglomerate M&A delistings.*

It should be noted that tests are performed on the null counterparts of these hypotheses respectively.

3.2. Research Design

To answer the derived hypotheses, our empirical analysis builds on a quasi-experimental panel setup that examines how analysts’ mean forecast error differs before and after M&A for hypothesis 1, and how the difference varies depending on whether the M&A is concentric or conglomerate for hypothesis 2.

We construct a panel dataset of firm-quarter observations covering all listed firms in Sweden from the third quarter of 1975 to the first quarter of 2025. Defining the M&A delisting quarter q as the first quarter in which an interim report is not released by the delisting entity, we designate quarters q and q+1 as the event period and exclude this event period to study the long-term informational effects of M&A delistings. This timeframe removes short-lived anomalies around the delisting and captures the more persistent informational spillover effects.

Quarters [q; q+1] are expected to capture short-lived anomalies due to competitive effects likely being anticipated in these quarters, and their capturing of short-term effects of the unavailability of interim reports on analysts' forecasts. As the study aims to capture changes to analysts' ability to forecast earnings over the longer term, it is desirable to exclude these quarters, which is in line with prior research (A. B. Brown et al., 2024). The actual date in which the security is delisted relates heterogeneously to quarter q, but most actual dates of delistings are in the span of [q; q+1].[1] In line with A. B. Brown et al (2024) we define the pre-M&A period as the 6 quarters before the event period, [q-6; q-1], and similarly define the post-M&A period as the 6 quarters after the event period, [q+2; q+7].

To operationalize the IE of analysts, we use the mean analyst forecast error, that is, the absolute number of the mean of analysts' earning expectation less the actual earnings reported. Unlike A. B. Brown et al (2024), we do not employ dispersion of analysts' forecast error, as it may proxy analysts' IE worse due to capturing idiosyncratic elements of analysts' information. We link the industry which the firm operates in with M&A delistings in that same industry. Firms with the same TRBC industry name as the delisting entity are defined as peers to an M&A delisting, and this gives a time-varying industry-level treatment variable, a post-M&A indicator.

For the second research question, we add an interaction term between the post-M&A indicator and a dummy variable for whether the M&A is classified as concentric or conglomerate, based on TRBC activity names. This adds a second time-varying treatment variable at the industry level, allowing the testing of whether the effects of M&A delistings on analysts' IE differ between conglomerate and concentric M&A. This is merged with firm-level measures of analysts' IE and determinants of the IE to create the sample.

TRBC is utilized to define industry, as no other unique industry classification was available for both private and public firms in the Swedish market. NACE classifies all European firms into industries, but in this system a firm can have multiple industries, which renders the method of matching inefficient, as the degree of activity in a specific NACE industry for any given firm is not assessable. MSCI's GICS and I/B/E/S' system provide a unique industry for each firm,

[1] A manual check found 7 out of 10 sampled delistings to have had their security delisting in [q, q+1]. The definition of quarter q as the first quarter in which an interim report is not released rather than as the quarter of the security delisting is in line with A. B. Brown et al. (2024).

but only do so for listed firms, rendering them unusable for identifying the industry of a private acquiring party. TRBC is a classification system from Refinitiv and has a five-level hierarchical structure with 10 Economic sectors, 33 Business sectors, 62 Industry groups, 154 Industries, and 898 Activities.[2] The Industry level "Industries" was chosen as it has the most similar number of groups to the level used by A. B. Brown et al. (2024). The unique combination of a specific year-quarter and industry defined by TRBC industry name is denoted as industry-quarter. Firms having the same TRBC industry name as the target of an M&A delisting, indicating that they are in the same industry as the target firm and thus are industry peers, are denoted as M&A-peers going forward.

The research design is quasi-experimental in that it leverages plausibly exogenous variation in the occurrence and nature of M&A delistings for identification. It should be noted that identification relies on the assumption that, conditional on controls, M&A delistings are plausibly exogenous with respect to changes in analysts' IE of industry peers. As earlier noted, shocks may cause M&A delistings and changes to the IE simultaneously, and therefore we acknowledge that the empirical strategy does not fully eliminate concerns about unobserved confounding factors.

Nevertheless, by controlling for observable characteristics and absorbing time- and firm-specific effects, the design seeks to isolate the association between M&A delistings and peer firms' IE of analysts. By comparing analyst forecast errors before and after delistings, and across M&A types, the research provides evidence on how these events are associated with the IE of analysts. Fixed effects, winsorization, and clustering of standard errors absorb unobserved heterogeneity, lessen concerns regarding heteroscedasticity, and improve the reliability of the results.[3] Robustness checks examining possible endogeneity and possible improper specification of industry-level or event window are undertaken to mitigate concerns. The research design and robustness measures are largely in line with previous literature on the subject, especially with the work by A. B. Brown et al. (2024).

[2] For further reading on the TRBC system, see (*TRBC Sector Classification*, n.d.).
[3] Standard errors are clustered at the M&A-peer level. For further reading on Fixed effects, Winsorization and Clustering of Standard Errors, see (Allison, 2005; Cameron & Miller, 2015; Dixon, 1960).

3.3. Sample Derivation

Table 1 summarizes the sample derivation process. To find industry-quarters with M&A delistings, we use Refinitiv EIKON datastream to extract a sample of 3,155 active and dead securities from the Swedish stock exchanges Nasdaq Stockholm, First North, NGM equity, and Spotlight over the period 1975 quarter 3 to 2024 quarter 3. We remove all securities without a TRBC industry classification, and all "active" securities. "Active" refers to a security having posted its latest interim report in quarter 3 of 2024 or later and having no other visual indication of a delisting taking place, such as "Delisted" or "Dead" appearing in the security name in the database. Removing securities without a TRBC industry classification limits the sample of delistings to quarter 1, 2008 and later quarters, as no earlier delistings in our sample had a TRBC industry classification.

We remove dead securities which do not constitute a delisting, for example delistings of preferred stock, where the entity is still listed afterwards. We then identify the cause of the delisting as either M&A or non-M&A (see table A1, appendix 1), and remove those observations where the cause is not M&A. We leverage, in descending order of preference; Skatteverket, Cision, and other financial publications[4] for identifying whether the delisting is due to M&A and the identity of the acquirer. Finally, we remove cases with incompatible or unusable data (see table A2, appendix 1).

We identify M&A delistings taking place in the same industry-quarter and exclude all but one of these M&A delistings within that specific industry-quarter. This is because M&A delistings within the same industry-quarter have the same event period [q; q+1] and M&A-peers. Therefore, these M&A delistings are excluded, to avoid estimating the effect of M&A on an industry-quarter multiple times.

In this stage, we also identify the acquirer's TRBC industry name using Refinitiv EIKON datastream. Cases where acquirers have the same TRBC industry name as the target are defined as concentric M&A, and other cases are defined as conglomerate M&A. In those cases where the acquirer or its industry code is unavailable, the observation is removed from the sample, resulting in 193 industry-quarters with M&A delistings.

[4] 96% of definitions used Skatteverket, 2% used Cision, and 0.67% used Breakit, Finwire and Aktiespararna respectively.

To find quarterly observations for the quarters around M&A delistings [q-6; q+7], we use Refinitiv EIKON datastream to extract all stocks that traded on the Swedish exchanges during the period from quarter 3 of 1975 to quarter 1 of 2025. For these firms, we extract Share price, Latest interim date, TRBC industry codes (Industry name and activity name), actual EPS, mean forecasted EPS of equity analysts, all on a quarterly level and utilizing opening values when applicable. We also extract Market Value, Trading Volume, the Market-to-Book ratio, and the number of equity analysts following the company. In this stage we calculate the forecast error for all firms, defined as the absolute difference between the mean forecasted EPS of equity analysts and the actual EPS of the period. Forecast error is then scaled by opening share price.

To create our final sample of M&A-peer quarter observations, we match the sample of industry-quarters with M&A delistings with the quarterly observations, utilizing the TRBC industry name as the match indicator. For each M&A delisting, we identify all M&A-peers and extract the quarterly observations of the periods related to the matched industry-quarters with M&A delistings [q-6; q+7]. We then filter the matched M&A-peer quarter observations based on the requirement of the M&A-peer having 3 or more quarterly observations of forecast error in the pre-M&A period [q-6; q-1], and 3 or more in the post-M&A period [q+2; q+7]. This is to avoid observing idiosyncratic noise from analysts covering only a few of the quarters and therefore being less accurate. Matching the industry-quarters with M&A delistings to M&A-peer quarter observations yields 7,717 M&A-peer quarter observations, which constitute our final sample.

Regarding hypothesis 2, the TRBC industry names are used to match acquirers and targets in the same industry. Defining concentric M&A as those cases where the acquirer and target have the same TRBC industry name, and conglomerate M&A as those cases where the acquirer and target have different industry names yields 3,024 M&A-peer quarter observations of concentric M&A, and 4,693 M&A-peer quarter observations of conglomerate M&A.

Table 1

Sample Derivation of Industry-Quarters With M&A Delistings and M&A-Peer Quarter Observations

Panel A. Sample Selection of M&A Delistings		
Extraction of active and dead securities between 1975-2024		3,155
(Did not have TRBC industry classification)	(1,704)	
Equities with TRBC Industry classifications		1,451
(Not delisted)	(747)	
Dead Equities		704
(Not delistings)	(294)	
Delisted companies		410
(Delistings for non-M&A reasons, see table A1 in appendix 1)	(182)	
Delistings due to M&A		228
(Removed due to data inconsistencies, see table A2 in appendix 1)	(14)	
(Removed due to multiple M&A in the same industry-quarter)	(11)	
(Acquirer TRBC not matched)	(10)	
Industry-quarters with M&A delistings		**193**

Panel B. Sample selection of quarterly observations		
Industry-quarters with M&A delistings		193
M&A-peer quarter observations matched to industry-quarters pre- and post-M&A		11,349
(Removed due to fewer than 3 quarters pre- or post-period)	(3,632)	
Final sample: M&A-peer quarter observations matched to industry-quarters with M&A		**7,717**
Of which are classified as Concentric M&A	3,024	
Of which are classified as Conglomerate M&A	4,693	

Table 1 represents the sample derivation process, utilizing data from EIKON for all securities trading on the Swedish stock exchanges Nasdaq Stockholm, First North, Spotlight and NGM Equity. *Industry-quarters with M&A delistings* are defined as the year-quarters in which a (several) firm(s) in a given TRBC industry ceases to report due to M&A. *M&A-peer* is defined as a company with the same TRBC industry name as the target of a M&A delisting. Matched to industry-quarters with M&A refers to observation in the pre- and post-M&A period of a M&A delisting, which are [q-6; q-1] and [q+2; q+7] respectively.

3.4. Diagnostics in Regression

To address the research questions and test the hypotheses, we use panel data OLS models to leverage the longitudinal structure of the data and to address heterogeneity across entities and time. The models have the following specifications:

Model 1: $\mathbf{ForecastErr_{mit} = \beta_1 * PostMA_{mit} + X'_{it} * B + \gamma_{mi} + \delta_t + \varepsilon_{mit}}$

Model 2: $\mathbf{ForecastErr_{mit} = \beta_1 * PostMA_{mit} + \beta_2 * (PostMA_{mit} \times ConcentricMA_m) + X'_{it} * B + \gamma_{mi} + \delta_t + \varepsilon_{mit}}$

In these models, the subscript m refers to a specific M&A, the subscript i refers to a specific M&A-peer, and the subscript t refers to a specific quarter.

For the first model, the dependent variable is $ForecastErr_{mit}$, the mean analyst forecast error defined as the absolute of the mean analyst earnings per share less actual earnings per share for the forecasted period. This variable is divided by the share price at the opening date of the quarter and is thus the error as a fraction of the share price. This ensures measured effects are not impacted by the volume and price of shares, nor the market value of the firm. The independent variable, $PostMA_{mit}$, is a dummy variable coded 1 if the observation is in the post-M&A period and coded 0 if the observation is in the pre-M&A period. The model also includes a range of controls, X'_{it}, in which all variables are in quarterly granularity for each M&A-peer pair. γ_{mi} is a set of M&A-peer fixed effects and absorbs any time-invariant M&A-peer specific characteristics, such as elements of industry positioning. δ_t is a set of quarterly fixed effects, and controls for any time-variant sample-wide effects such as shocks to the macroeconomic environment. For identification, this model relies on variation within M&A-peers over the pre- and post-M&A periods.

The second model is similar but also includes an interaction term between $PostMA_{mit}$ and $ConcentricMA_m$. $ConcentricMA_m$ is a dummy variable coded 1 for concentric M&A and 0 for conglomerate M&A. Concentric M&A is defined as M&A where the acquirer and target have the same TRBC industry name. For identification, this model compares changes in forecast error after M&A delistings, between concentric and conglomerate M&A.

Due to data availability, the mean forecast error is used instead of the median forecast error as the dependent variable, as opposed to the standard practice in this type of research. Using the mean forecast error has precedent in the literature (see e.g. L. D. Brown, 1997; Siegel et al., 2011). Utilizing the median is however the most common in prior research on this area (see e.g. M. T. Bradshaw & Sloan, 2002; A. B. Brown et al., 2024; Lim, 2001). The mean is more statistically efficient than the median and may give a fairer representation of analysts' aggregate IE. We would however expect median forecast error to be an option more likely to find significance, the median is more robust to outliers and reflects central tendencies even if data is skewed. Because of this, the usage of mean forecast error is likely to impact the significance of the findings negatively relative to using the median forecast error.

In the first model, the independent variable of interest is $PostMA_{mit}$, and thus, β_1 is the coefficient of interest. This coefficient captures the effect of an M&A-peer quarter being in the

post-M&A period on the mean forecast error. The literature suggests that this variable should be expected to have a positive coefficient and be statistically significantly different from zero.

In the second model, the independent variable of interest is the interaction term $PostMA_{mit} \times ConcentricMA_m$.[5] Here, β_2 is the coefficient of interest, capturing the differential effect of being in the post-M&A period on the mean forecast error for M&A-peers affected by concentric M&A, relative to those affected by conglomerate M&A. The literature suggests that this variable should be expected to be statistically significantly different from zero, but expectations for the sign of the coefficient are inconclusive.

Included in both models are quarterly fixed-effects, as well as fixed effects for each M&A-peer pair. Specifically, $\delta_t = \Sigma\beta_t * YearQtr_t$, where $YearQtr_t$ is coded 1 for year-quarter t and 0 otherwise, whereas $\gamma_{mi} = \Sigma\beta_{m,i} * MA_m * Peer_i$, where MA_m is a set of indicator variables coded 1 for each M&A delisting, and $Peer_i$ is a set of indicator variables coded 1 for each industry peer firm i. The fixed effects structure is consistent with prior literature and allows for controlling for sample-wide time-variant effects as well as time-invariant factors unique to each M&A-peer pair. Also included is a vector of cross-sectional control variables for each M&A-peer pair, X'_{it}, multiplied by a vector of coefficients corresponding to a control variable, B. The control variables are log(MktVal), log(Following), log(MTB), and log(Volume).[6] Doukas et al. (2002) find that Market Value and Market-to-book ratio are directionally associated with forecast error. Liu et al. (2014) report that the number of analysts following the firm is significantly negatively correlated with forecast error. Lehmer et al. (2022) find that forecast accuracy is significantly associated with larger trading volume. Financial variables and the number of analysts following a company tend to be skewed to the right, which is why using the logarithm of these variables is common practice in research concerning analysts' forecast errors (Chou & Shiah-Hou, 2010; Hilary & Hsu, 2013; Siegel et al., 2011). A. B. Brown et al., (2024) also control for the logarithm of average days since the analyst started covering the company, as well as for industry returns. Due to data availability, this paper does not include

[5] Denoted as "PostMA×Conc." In results.

[6] MktVal is the opening balance of the market value of the security in millions of SEK, Following is the number of published analyst EPS estimates for that given quarter, MTB is the opening balance of the market value of the ordinary (common) equity divided by the opening balance of the balance sheet value of the ordinary (common) equity in the company, and Volume is the total value of shares transferred during the quarter in thousands of SEK using closing price.

these variables as controls, but studying forecast errors without controlling for these factors is also commonly done in the literature (see e.g. Chou & Shiah-Hou, 2010; Doukas et al., 2002).

The sample size is small compared to that used in prior research, and some firms are missing values of control variables. Because of this, and to avoid restricting the sample size further, mean imputation is used, where missing values of control variables are replaced with the sample mean of that variable.

3.5. Methodological Issues and Mitigative Actions

Effects such as the potential outsized effect of outliers and the model not satisfying the assumption of spherical standard errors are mitigated through the usage of winsorization and clustering of standard errors within each M&A-peer pair. Also, there are methodological concerns of which we perform sub-analyses on some issues regarding endogeneity and mean imputation of control variables.

Our model is based on the classical linear model assumptions of spherical standard errors, that standard errors are independent and identically distributed. The model results may be adversely impacted by noise, which could skew coefficient estimates, inflate standard errors, and distort inference.

To relax the assumption of spherical standard errors, the standard errors are clustered by M&A-peer. This is critical in this study, as several reasons to suspect that the assumption of spherical errors is violated exist. Each M&A-peer pair appears in the data multiple times, and because of this, the error terms are likely correlated over time. To assume that analysts reset their forecasting behavior in each quarter is strong and accepting that this may not be true implies that their errors may be autocorrelated. To mitigate this, clustering on the M&A-peer level is appropriate. Since the treatment is at the M&A-peer level, shocks tied to an M&A delisting may affect a given M&A-peer consistently over time, and since the model observes several quarters for each M&A-peer pair residuals are likely correlated across time within each pair. Clustering by M&A-peer pair thus enhances inference of the post-M&A effects, as without clustering, the model might underestimate standard errors, leading to inflated t-statistics and overestimated p-values.

Outliers may have an outsized effect on the result and lead to unstable estimates. To mitigate this, we winsorize all variables at the 1% and 99% levels, which is in line with prior research (A. B. Brown et al., 2024).

M&A is unlikely to happen at random even conditional on the factors being controlled for, and this leads to concerns regarding endogeneity. Shocks to an industry may cause M&A and simultaneously lead to changes in the IE of analysts (A. B. Brown et al., 2024; Mitchell & Mulherin, 1996). The fixed-effects structure does not account for time-varying unobservables that differ between M&A-peer pairs. Three main concerns arise because of this. First, industry-specific shocks could be timed with M&A delistings and cause changes to analysts' IE at the same time. This would introduce omitted-variable bias. Secondly, target firms' forecast uncertainty might start to deteriorate before the M&A, and the M&A may be a consequence of a deteriorating IE. This would introduce reverse causality concerns. Lastly, compound treatments could be affecting the results, if more than one M&A delisting happens in the same industry in the same quarter, the effect may be different but still captured in our model.

To deal with concerns of endogeneity and concurrent treatment effects, we re-estimate the models using a subsample where all industry-quarters with multiple M&A delistings in the same industry are dropped. This results in a sample of 7,151 observations. If it is accepted that contemporary M&A delistings in the same industry are more likely to be driven by some endogenous shock also impacting the IE, such as regulatory, technological, or macroeconomic changes, this subsample excludes some endogenously driven M&A. While not excluding all endogenously driven M&A, comparing these results to baseline results may mitigate some concerns of omitted-variable bias. Also, since one type of compound treatment is excluded here, the results of this re-estimation also clarify whether the model has issues stemming from capturing the effect of compound treatments. To mitigate concerns of reverse causality, we plot and visually inspect mean forecast error prior to the M&A period. If there is reverse causality, we would expect the IE to start to deteriorate before the M&A delisting. These robustness checks have previously been undertaken in the most similar research (A. B. Brown et al., 2024).

Mean imputation is used to replace missing values for control variables to ensure a sufficient sample size is reached. Mean imputation may be problematic and does not influence results only if values are missing completely at random (MCAR). MCAR is unlikely to hold, as factors dependent on time and entity, such as quarter timing and size, are likely to impact the missingness of data. It is more reasonable to assume that values are missing at random (MAR), that missingness depends only on observed data. This is as time-invariant entity effects and entity-invariant time effects are controlled for with this research design. MAR is still unlikely

to hold completely, as time-variant entity effects may impact the missingness of variables.[7] Because of this, mean imputation may affect results. Of 7,717 total M&A-peer observations, 309 are missing some control variable, and thus some control variable value is imputed from the mean. The only affected control variables, with values missing, are MTB and Following. Thus, we re-estimate models 1 and 2 excluding the controls for these, to assess whether systematic bias introduced by mean imputation is affecting results. If coefficients and p-values are similar to those of the baseline regressions, that would support the notion that the results are not driven by bias introduced by mean imputation.

4. Results

In this section, descriptive statistics and correlation coefficients are presented for the sample, as are plots of the dependent variable over pre- and post-M&A quarters. The baseline models are specified and described, as are models undertaken for robustness analyses.

4.1. Descriptive Statistics

Table 2, panel A, shows descriptive statistics of our independent, dependent, and control variables. ForecastErr exhibits a higher mean than median, implying a positive skew. The mean is 0.027, whereas the standard deviation is 0.069, indicating large variation between M&A-peer quarters. The mean, median, and standard deviation are significantly larger in our sample than in that of A. B. Brown et al. (2024), something which may be explained by Swedish firms on average being smaller and less covered. Panel B in table 2 displays the Pearson and Spearman correlation coefficient between our dependent, independent, and control variables. The usage of both coefficients allows understanding levels of both linear and rank correlation. ForecastErr has absolute correlation coefficients below 0.5, and PostMA and ConcentricMA both display negligible correlation with other variables. While control variables are correlated at higher levels, this does not bias the coefficients or statistical significance of the independent variables. Thus, the correlations suggest sufficient independence between variables for model coefficient estimates to be reliable.

[7] For further reading on this topic, please see Van Buuren (2018).

Table 2

Descriptive statistics and correlations

Panel A. Descriptive statistics

	N	Mean	Std Dev.	P1	25%	50%	75%	99P
ForecastErr	7,717	0.027	0.069	9.62 E-05	0.003	0.010	0.026	0.298
PostMA	7,717	0.502	0.500	0	0	1	1	1
ConcentricMA	7,717	0.387	0.487	0	0	0	1	1
MktVal	7,717	20,351	42,188	118.2	1590.5	5,903	18,513	251,168
Following	7,717	4.983	5.637	1	2	3	6	26
MTB	7,717	2.954	3.266	0.29	1.08	1.86	3.38	18.29
Volume	7,717	3,354,408	7,604,652	3,739	76,517	457,637	2,301,174	41,462,258

Panel B. Correlation table

		(1)	(2)	(3)	(4)	(5)	(6)	(7)
ForecastErr	(1)		0.04	0.01	-0.14	-0.12	-0.22	-0.26
PostMA	(2)	0.03		0.00	0.00	-0.05	0.01	0.02
ConcentricMA	(3)	-0.03	0.00		-0.03	-0.11	0.01	0.03
log(1+Following)	(4)	-0.25	0.02	0.01		-0.04	0.75	0.71
log(1+MTB)	(5)	-0.18	-0.06	-0.14	-0.04		0.08	0.06
log(1+Volume)	(6)	-0.37	0.01	0.01	0.73	0.06		0.90
log(1+MktVal)	(7)	-0.42	0.03	0.04	0.70	0.04	0.90	

Panel A represents descriptive statistics for our baseline sample, after winsorization at a 99% and 1% level for testing model (1) and model (2). Panel B represents a Pearson and Spearman correlation matrices (in upper and lower triangles respectively) for this sample. For details on sample construction see table 1.

Figure 1 visualizes the mean forecast error of all M&A-peer pairs over quarters [q-6, q+7]. Consistent with a deterioration in the IE of M&A-peers, this plot shows an increase in ForecastErr between the pre- and post-M&A periods. This plot alleviates some concerns of endogeneity, if a deterioration in the IE leads to M&A delistings rather than the other way around, analysts' mean forecast error would be expected to increase before an M&A delisting, which they do not on average. Figure 2 visualizes the mean forecast error split over concentric and conglomerate M&As and shows forecast errors to generally be higher in both the pre- and post-M&A period. There is no clear indication from this plot that concentric M&A delistings are associated with a larger change in analysts' forecast errors, which is neither consistent nor inconsistent with the ambiguous theoretical framework.

Figure 1

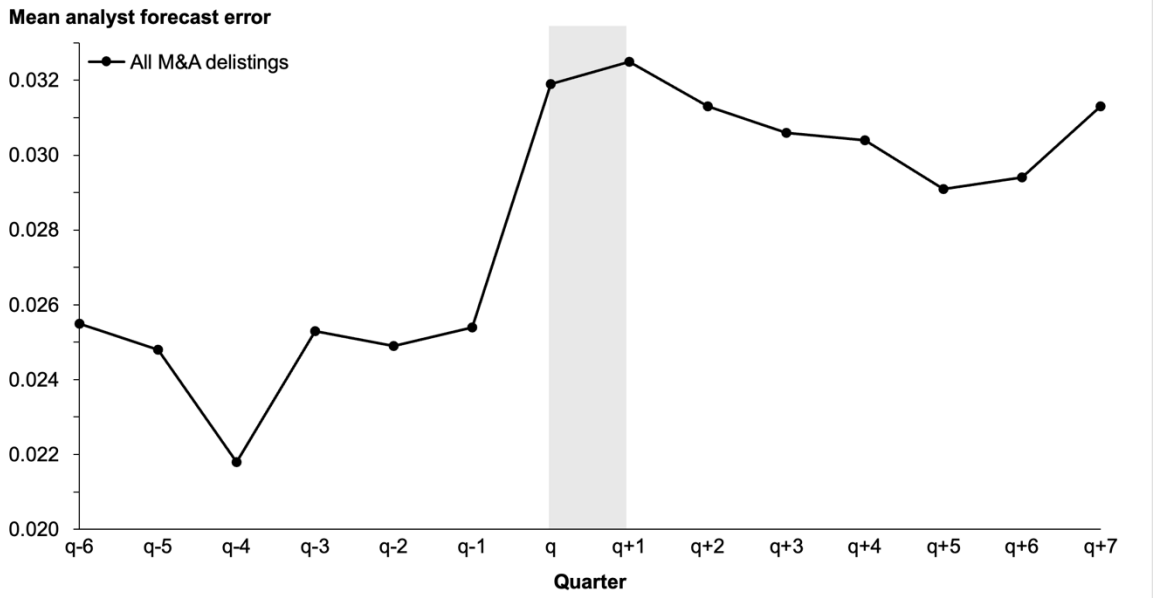

Figure 1 plots *ForecastErr,* which is the absolute of the mean analyst earnings per share less actual earnings per share for the forecasted period, scaled by share price, from q-6 to q+7. The sample is based on 7,717 of M&A-peer industry-quarter observations, see table 1 for further details. Data are winsorized at the 99% and 1% level.

Figure 2

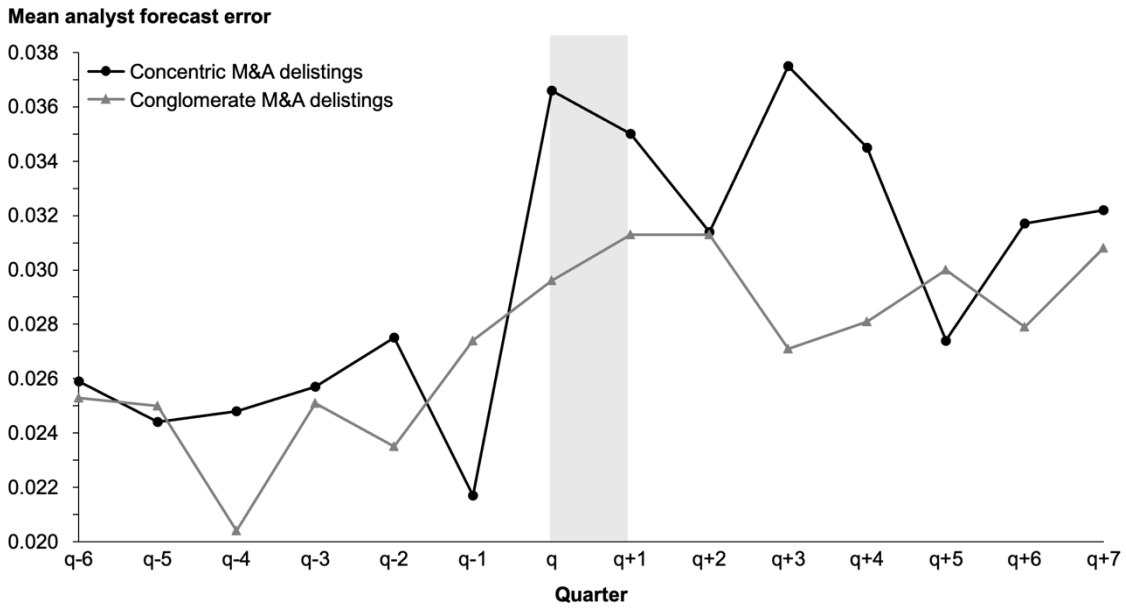

Figure 2 plots *ForecastErr,* which is the absolute of the mean analyst earnings per share less actual earnings per share for the forecasted period, scaled by share price, from q-6 to q+7. The plot is separated into two lines, *concentric M&A delistings* and *conglomerate M&A delistings*, visualizing ForecastErr separated by the dummy variable *ConcentricMA* where observations are coded (1) for concentric M&A and (0) for conglomerate M&A for each quarter. The sample is based on 7,717 of M&A-peer industry-quarter observations, see table 1 for further details. Data are winsorized at the 99% and 1% level.

4.2. Hypothesis Testing

The first hypothesis tests whether analyst forecast errors of M&A-peers increase after M&A delistings. The results of the regression are shown in table 3 column 1, and are consistent with results obtained from data using other levels of winsorization, both at the 5% and 95% levels and using data which are not winsorized (see table A3, column 1.1 and 2.1, appendix 2).

As the theoretical background would suggest, there is a statistically significant positive effect of the M&A delisting variable ($PostMA_{mit}$) on the mean forecast error (*ForecastErr)* at the 5%-level, with a t-stat of 2.11. Because of this, we reject the null of hypothesis 1, a rejection which is consistent with M&A delistings having an adverse effect on the IE of analysts. The economic interpretation of $PostMA_{mit}$ having a coefficient of 0.0055 is that on average, the mean analyst forecast error, scaled by share price, was 0.55 percentage points higher for industry peer firms after an M&A delisting than before.

The second hypothesis tests whether the effect of M&A delistings on analysts' forecast errors differs over types of M&A. The results of the regression are shown in column 2 of table 3, and are consistent with other levels of winsorization, both at the 5% and 95% levels and using data which are not winsorized (see table A3, columns 1.2 and 2.2, appendix 2). Contrary to the theoretical expectation, the interaction term capturing the effect of M&A being concentric ($PostMA_{mit} \times ConcentricMA_{m}$) is statistically insignificant at the 10%-level, leading us to fail to reject the null version of hypothesis 2. Had the effect of the interaction term been statistically significant, the economic interpretation of it having a coefficient of 0.0034 would have been that on average, the mean analyst forecast error in the post-M&A period, scaled by share price, was 0.34 percentage points higher for industry peer firms subject to concentric M&A than for industry peer firms subject to conglomerate M&A.

Table 3

Testing H1 and H2: Main OLS regression

	(1) Testing Hypothesis 1: *Positive effect of PostMA on ForecastErr*		(2) Testing Hypothesis 2: *Effect of PostMA×ConcentricMA on ForecastErr*	
	Coeff	t-stat	Coeff	t-stat
PostMA	0.0055**	(2.11)	0.0041**	(1.97)
PostMA×Conc.			0.0034	(1.58)
Control Var:				
log(1+MktVal)	-0.0259***	(-3.53)	-0.0296***	(-3.53)
log(1+Following)	-0.0094	(-0.90)	-0.0093	(-0.89)
log(1+MTB)	0.0056	(0.67)	0.0058	(0.68)
log(1+Volume)	0.0132**	(2.26)	0.0132**	(2.26)
Firm-fixed effect	M&A-peer		M&A-peer	
Time-fixed effect	Year-quarter		Year-quarter	
Adjusted std. errors	M&A-peer		M&A-peer	
R^2 (Between)	-5.54%		-5.74%	
R^2 (Within)	3.66%		3.68%	
R^2 (Overall)	0.90%		0.94%	
# of obs.	7,717		7,717	
F-stat (Robust)	4.79*** (df=5;7438)		4.08*** (df=6;7437)	

This table presents the PanelOLS regression results of model (1) and (2), to test (1) H1 and (2) H2. The estimates use our main baseline sample, details of which are in table 2. Our dependent variable is *ForecastErr*, defined as the absolute difference between actual EPS and the mean EPS forecast, scaled by the stock price at the beginning at the quarter. *PostMA* equals one if a M&A-peer quarter observation is for forecast made during the 6-quarter period after the M&A [q2; q7] and zero if it is made in the period before [q-6; q-1], where q is the quarter when the delisted firm stops issuing reports. We utilize the interaction term *PostMA×Conc*. in column 2, where *ConcentricMA* is a dummy variable coded 1 if the M&A delisting is concentric M&A, based on TRBC industry level, and 0 if it is conglomerate M&A, based on TRBC industry level. The control variables are as follows: (1) *MktVal* is the market value of a M&A-peer at the beginning of the quarter; (2) *Following* is the number of analysts releasing earnings forecasts for the M&A-peer in the given quarter; (3) *MTB* is the market-to-book ratio of equity of a M&A-peer; (4) *Volume* is the trading volume of the M&A-peer summed over a quarter. M&A-peer and year-fixed effects are included. Data are winsorized at the 1% and 99% level. Standard errors are clustered at M&A-peer level. Significance levels are based on a two tailed p-values; 10%, 5%, and 1% being indicated by *, **, and ***, respectively.

4.3. Robustness Analyses

To mitigate concerns of endogenous shocks or concurrent treatment impacting results, we create a restricted sample where all industry-quarters in which there was more than one M&A delisting in that same industry are dropped. This results in a sample size of 7,151 observations (see table A5, appendix 3). The results of re-estimating models 1 and 2 are shown in table 4 in columns 1 and 2 respectively. In both re-estimations, the coefficients of interest rise slightly, while p-values drop marginally. All cases of possible endogenous or concurrent treatment are

not removed from this sample, but that the magnitude and significance increase when removing some such cases is reassuring. The increased statistical significance and coefficient sizes in the restricted sample suggests that industry-wide shocks and overlapping delisting events are not causing the model estimate to be overly optimistic. If anything, the full-sample estimates may be conservative due to confounding effects of compound treatments. It should be noted that endogeneity and compound treatments remain possible issues, these results are mainly mitigative.

Table 4

Robustness Testing H1 and H2: Endogeneity-restricted sample

	(1) Testing Hypothesis 1: *Positive effect of PostMA on ForecastErr*		(2) Testing Hypothesis 2: *Effect of PostMA×ConcentricMA on ForecastErr*	
	Coeff	t-stat	Coeff	t-stat
PostMA	0.0067**	(2.39)	0.0052**	(2.06)
PostMA×Conc.			0.0035	(1.55)
Control Var:				
log(1+MktVal)	-0.0240***	(-3.26)	-0.0240***	(-3.35)
log(1+Following)	-0.0118	(-0.99)	-0.0118	(-0.98)
log(1+MTB)	-0.0020	(-0.25)	-0.0019	(-0.24)
log(1+Volume)	0.0120**	(2.30)	0.0119**	(2.29)
Firm-fixed effect	M&A-peer		M&A-peer	
Time-fixed effect	Year-quarter		Year-quarter	
Adjusted std. errors	M&A-peer		M&A-peer	
R^2 (Between)	18.54%		18.45%	
R^2 (Within)	3.55%		3.57%	
R^2 (Overall)	5.56%		5.63%	
# of obs.	7,151		7,151	
F-stat (Robust)	5.71*** (df=5;6866)		4.08*** (df=6;6865)	

This table presents the PanelOLS regression results of re-estimating model (1) and (2) to test (1) H1 and (2) H2, using our endogeneity-restricted sample, see table A5, appendix 3 for sample specification. The specifications are otherwise unchanged from the original models, see endnotes in table 3 for further information. M&A-peer and year-fixed effects are included. Data are winsorized at the 1% and 99% level. Standard errors are clustered at M&A-peer level. Significance levels are based on a two tailed p-values: 10%, 5%, and 1% being indicated by *, **, and ***, respectively.

Mean imputation is used to replace missing values for control variables to ensure a sufficient sample size is reached. Mean imputation may introduce bias, and because of this, we re-estimate our baseline models using a model where log(1+MTB) and log(1+Following), the controls for which values are imputed, are excluded. The results of these re-estimations of model 1 and 2 are shown in table 5, in columns 1 and 2 respectively. These results are similar in magnitude and significance to baseline results, which in combination with the plausibility

of MAR holding relatively well implies that p-values of the baseline regressions are likely valid and not spuriously influenced.

Table 5

Robustness Testing H1 and H2: Excluded control variables

	(1) Testing Hypothesis 1: *Positive effect of PostMA on ForecastErr*		(2) Testing Hypothesis 2: *Effect of PostMA×ConcentricMA on ForecasErr*	
	Coeff	t-stat	Coeff	t-stat
PostMA	0.0053**	(2.11)	0.0039*	(1.94)
PostMA×Conc.			0.0034	(1.58)
Control Var:				
log(1+MktVal)	-0.0287***	(-3.69)	-0.0287***	(-3.69)
log(1+Volume)	0.0128**	(2.30)	0.0127**	(2.29)
Firm-fixed effect	M&A-peer		M&A-peer	
Time-fixed effect	Year-quarter		Year-quarter	
Adjusted std. errors	M&A-peer		M&A-peer	
R^2 (Between)	12.10%		12.13%	
R^2 (Within)	3.64%		3.66%	
R^2 (Overall)	4.65%		4.74%	
# of obs.	7,717		7,717	
F-stat (Robust)	6.58*** (df=3;7440)		4.98*** (df=4;7439)	

This table presents the PanelOLS regression results of re-estimating model (1) and (2) to test (1) H1 and (2) H2, using our main baseline sample, see table 2. Control variables *MTB* and *Following* are excluded from the model. The specifications are otherwise unchanged from the original models, see endnotes in table 3 for further details. M&A-peer and year-fixed effects are included. Data are winsorized at the 1% and 99% level. Standard errors are clustered at M&A-peer level. Significance levels are based on a two tailed p-values: 10%, 5%, and 1% being indicated by *, **, and ***, respectively.

To mitigate concerns about the validity of research design, namely event period classification and industry classification level, models 1 and 2 are re-estimated on altered datasets. The changed event period classification sample changes the event period from [q; q+1] to [q-1; q], and contains 7,938 industry-quarter observations, see table A7, appendix 3. In the changed industry level classification sample, we change the TRBC industry level from "Industry" to "Activity", and obtain 2,385 industry-quarter observations, see table A6, appendix 3. The results of the re-estimations are shown in table A4, appendix 2. Results of both re-estimations are less significant than baseline results, alleviating concerns of other specifications being more appropriate.

5. Discussion

This section presents the analysis of our baseline results and the associated robustness checks and different specifications. Then, limitations to the study are recognized and discussed, after which some concluding remarks on the study are made. Finally, some suggestions for future research into this topic are offered.

5.1. Analysis

The R^2 (within) of our models is around 3.0%, which is considerably lower than what would be expected from the adjusted R^2 of 15-20% reported by A. B. Brown et al. (2024). This likely reflects differences in context, sample period, and methodological decisions, as well as the absence of some firm-level controls. However, in fixed-effects panel models, overall model fit is of less importance than the consistency and robustness of the key coefficients, which in our case remain stable across multiple different specifications. Also, the robust F-tests are significant at the 1% level for both models, with F-stats of 4.79 and 4.08 respectively, indicating that the regressors jointly explain some variation in analysts' mean forecast errors in both models. Heteroskedasticity is not a large concern because of the usage of winsorization at various levels, clustered standard errors, high robust F-stats, and multiple specifications of the model.

The estimate of the M&A delisting variable coefficient in model 1 is positive and statistically significant at the 5% level. The results hold over different levels of winsorization, and robustness analyses support that the association is not driven by endogenous shocks, concurrent treatment effects, or systematic bias introduced by mean imputation to replace missing control values. These results are consistent with hypothesis 1 and with M&A delistings causing a deterioration in analysts' IE. If M&A delistings cause a deterioration in analysts' IE, these results show quantifiable informational costs of M&A delistings and highlight the potential adverse effects of a decreasing number of listed firms.

This result is in line with the expectations. Prior empirical work has found M&A delistings to be associated with an increase in median analyst forecast error (A. B. Brown et al., 2024). The theoretical background supports the existence of the merger and delisting effects, and a joint effect of M&A delistings on the IE. While our results do not prove a causal effect of M&A delistings on the IE, they are consistent with this theoretical notion.

The coefficient of the M&A delisting variable is estimated at 0.0055 in our baseline regression of model 1. We gather that A. B. Brown et al. (2024) present their results as the percentage difference of percentage points between the pre- and post-M&A periods. As the untabulated mean forecast error of quarters in the pre-M&A period was 2.46%, scaling our estimated coefficient by the mean forecast error of pre-M&A quarters yields an estimate of 0.223. This means that the forecast error expressed as a percentage was 22.3% higher in the post-M&A than in the pre-M&A period. A. B. Brown et al. (2024) estimate the coefficient in these terms to be 2.2%, with regards to median forecast error.

Thus, our estimated effect is 10 times as high as the effect estimated by prior studies examining the US market. This difference may be explained by the IE of analysts, analysts' collective forecasting abilities, or analyst coverage being relatively better in the US than in Sweden, thus rendering the informational impact of M&A delistings more severe. Notions of differences in the IE of analysts are supported by A. B. Brown et al. (2024) reporting significantly lower mean forecast errors and a higher average number of analysts covering the firm. Methodological differences could also reconcile the different results, it is not implausible that the change in median forecast error would have been smaller and more like prior findings than we find the change in mean forecast error to be, especially as A. B. Brown et al. (2024) find the median to be significantly lower than the mean. A. B. Brown et al. (2024) also use additional controls and a time-variant industry classification system, which may reconcile the findings.

The estimate of the coefficient of the interaction term for concentric M&A is positive in the baseline estimation of model 2, but not statistically significant at the 10%-level. This result holds consistent when changing winsorization levels. This is also supported by robustness analyses suggesting that effects of endogenous shocks, concurrent treatment effects, and mean imputation are not impacting the result. The t-stat of the interaction term for concentric M&A in the baseline model is 1.58, corresponding to a p-value of 0.1146. While close to 0.1, the lack of statistical significance leads us to fail to reject the null of hypothesis 2. It is not implausible that the statistical insignificance is a product of the sample size being small relative to prior research.

The reported result of testing hypothesis 2 is not in line with prior empirical work by A. B. Brown et al. (2024), who find concentric M&A to be associated with a statistically significantly larger increase in the median forecast error of analysts. It should be noted that our estimated coefficient, while insignificant, is directionally comparable to A. B. Brown et al. (2024) in that

it suggests a larger change following concentric M&A as opposed to conglomerate M&A. Not rejecting the null of hypothesis 2 may be considered to be in line with theory to some extent, as the theoretical expectations for differences in effect on forecast error of concentric contra conglomerate M&A are conflicting and inconclusive. It should however be noted that this rejection is not what would have been expected given the prior empirical work by A. B. Brown et al. (2024). Our rejection of the null of hypothesis 2 may also be explained by differences in setting. It could be that any difference in effect across M&A types is less distinguished in the Swedish context relative to prior findings and theoretical expectations formed in a US context. Operationalizing the IE as mean rather than median analyst forecast error could also cause the conflicting findings. Other differences in methodology, such as a different industry classification system, may also explain the differences in findings.

5.2. Limitations

Our study suffers from several potential issues regarding data availability and sample derivation, as well as necessary methodological decisions and the inherent challenges of establishing causal relationships in this setting. These issues may adversely impact the results and their interpretation. Below we outline possible issues and how they may affect the results.

5.2.1. Sample Selection and Representativeness

The selection and derivation of the sample may introduce bias and other issues to the results, especially as the sample derivation process is relatively complicated. Sample selection bias is one concern, it may be that the sample is not representative of the population for several reasons. Observations where data was missing were excluded, but the lack of data is unlikely to be randomly distributed, factors such as size, age, and industry could be determinants of data availability and as such introduce bias. Observations could be skewed towards certain industry and industry-time combinations, and this may limit the generalizability of the findings if the sample is disproportionately drawn from sectors and periods with heterogeneous effects. Only firms covered by analysts are studied, and analyst coverage is almost certainly not randomly distributed, hence introducing sample selection bias.

Some control variables are unexpectedly insignificant in the baseline regressions, which raises concerns that the controls may be poorly specified or that there may be measurement error in the controls. It could also be that the sample has dynamics different to those previously studied, but regardless, the insignificance of those controls may cause variation not attributable to the

variables of interest to affect the results. Furthermore, the sample is small compared to previous similar studies, which might impact the results negatively, especially given that the extensive control structure reduces the degrees of freedom.

5.2.2. Data Quality and Measurement Issues

Data quality is another concern impacting interpretability. The estimates of analysts, and therefore the forecast errors, may be of various quality, for example, firms may choose to pay directly to be covered by an equity analyst, or indirectly by procuring corporate finance advice from the investment bank covering them. If analyst behavior is heterogeneous and dependent on the independent variables or an omitted variable, the interpretability of results is adversely affected. Furthermore, analysts may change their behavior over time, for example due to regulatory changes such as the introduction of the EU Market Abuse Regulation in 2016. If analyst behavior changes, the effect of the independent variables on forecast error may be time-dynamic, in which case the time-fixed effects do not capture the full effect, and residual bias could impact the results.

Industry belonging is complicated. The breadth of activities differs between firms, some firms may be active in more industries than others. Also, industry is time-dynamic, as firms evolve their activities may change. The TRBC industry name indicator used is not time-dynamic and only assigns one industry name to each listed firm. This is likely to adversely affect the significance of our results, as real industry-similarity, and therefore real industry peers to M&A delisting and real concentric M&A are not always captured by the model.

Mean forecast error is used less often in prior literature than median forecast error. Because of the usage of the mean, our results consider outlying forecasts more and may thus not as accurately represent the IE. Also, generally measures of forecast error are not perfect measures of analysts' IE or the broader IE, forecast error is simply the result of a process in which a better IE should yield more accurate forecasts, and this ought to be recognized.

5.2.3. Methodological Decisions

The study measures changes in forecast error with fixed event windows relative to the delisting quarter, but the informational effects of M&A delistings likely vary severely across delistings. Effects may arise outside of our event windows and therefore not be captured and using rigid windows could therefore lead to misestimation of the true effects. Whereas the direct informational effects of M&A delistings are likely to be the most outsized in quarters [q; q+1],

the effects on the competitive environment might be larger in [q-1; q]. This is because news of the impending delisting may be known in q-1, and as such analysts may anticipate competitive and informational effects in this quarter, thus making it reflect short-term abnormal analyst behavior. Thus, we re-estimate the models using [q-1; q] as the event period, to mitigate concerns of the event period specification being inappropriate. The results of the re-estimations are in table A4, column 10.1 and 10.2, in appendix 2. The event period specification leads to another methodological issue, related to the definition of quarter q. While q is the first quarter in which the delisting entity does not release an interim report for the first time, it is often not the quarter in which the firm's stock ceases to be traded. It would be implausible to expect the timing of those competitive effects driving the merger effect to be completely conditional on the time when the stock stops trading. Still, it could be that some other definition of quarter q might be more appropriate, and this presents a limitation of the study.

The study utilizes the TRBC industry name as an industry indicator, but even within the TRBC system, there are other hierarchical levels. Several activity names jointly make up an industry name group, and using the activity name, or a higher level than industry name, would lead to different results. Industry may be more accurately reflected by another level of the TRBC system, most likely in that case by activity name, as higher levels contain a very large number of firms. To mitigate concerns arising from this, we re-estimate the model using activity name for identification of peers instead, the results of which confirm that industry name is the more appropriate level to use, and are shown in table A4, column 9.1 and 9.2, appendix 2. It could be that other higher levels would have presented more significant effects, which remains a limitation of the study.

We perform a robustness check by dropping quarters with multiple M&A delistings in the same industry and see similar results, but there are still residual concerns regarding concurrent treatment effects. If M&A delistings occur in adjacent quarters, the measured effect of each delisting could be the result of multiple M&A delistings. While time-fixed effects absorb some general time-shocks, localized overlapping treatments might introduce residual bias and/or inflate the standard errors, reducing statistical power.

Whereas winsorization reduces the effect of outliers and improves the robustness of results, not adjusting outliers or winsorizing at different levels may more accurately reflect economic effects. Additionally, although prior research appears to winsorize not only the independent and control variables, but also the dependent variables, winsorizing the dependent variable may be

problematic. To alleviate these concerns, the baseline models are estimated on data which are winsorized at the 5%- and 95%-levels and data which are not winsorized, the results of which are largely similar and shown in table A3, appendix 2.

5.2.4. Causality Concerns and Confounding Factors

Given the complexity of firm and analyst behavior and the broader economic environment, establishing clear causal links between M&A delistings and forecast errors is challenging. Furthermore, analysts' IE is studied, and although this constitutes a key part of the broader IE, findings may not always be generalizable to the broader IE. Additionally, some points about heterogeneity and potential behavioral changes should be noted.

Our research design does not observe direct causal effects. Although we attempt to control for confounding factors by using control variables and robustness checks, we cannot conclusively rule out explanations alternative to M&A delistings causing the shift in forecast errors, and this should be considered in interpreting the results. Reverse causality, that impending deterioration of the IE causes M&A, cannot be conclusively ruled out even though Figures 1 and 2 support its absence. Similarly, simultaneity could also drive the results, and cannot be ruled out, especially not unobserved shocks causing both changes to the IE and M&A delistings.

Although analysts' IE is an important element of the broader IE, the documented association between M&A delistings and a deterioration of analysts' IE may not apply to the broader IE. While it seems implausible, it could be that other elements of the IE, such as media, ownership, and management, improve in association with industry peer M&A delistings.

The effects of M&A delistings are unlikely to be homogenous, but our research design does not explicitly model for this heterogeneity, and as such our reported mean results could mask underlying variation in effects. Furthermore, firms may change their informational behavior in response to M&A delistings, for example by changing disclosure practices or analyst guiding behavior. Such endogenous reactions could be reflected in the reported results, and therefore this should be considered in interpreting the results.

5.3. Concluding Remarks

In conclusion, this study identifies M&A delistings as being associated with a decrease in analysts' mean forecast error of industry peer firms. This finding aligns with prior empirical findings in other contexts and the theoretical framework and is consistent with adverse

informational spillover effects arising from M&A delistings. The estimated effect on analysts' mean forecast error is 10 times larger than the estimated effect on analysts' median forecast error found in a study on the US market. No statistically significant difference in changes in analysts' mean forecast error of industry peer firms is found between Concentric and Conglomerate M&A. This is inconsistent with empirical findings in other contexts but could be perceived as not contradicting the theoretical framework, which is inconclusive.

If the first result holds broadly, and particularly if the relationship is causal, it carries significant implications. If so, there are quantifiable informational costs of M&A delistings, highlighting the issue that a shrinking number of listed firms presents. This could warrant consideration in policymaking, and the effects of M&A delistings could perhaps to a greater extent be foreseen and mitigated by analysts. If the results hold broadly, and the 10 times larger estimated effect is indicative, the results are consistent with the informational effects of M&A delistings on the IE being significantly larger in Sweden than in the US. Because Swedish private firms are required to release financial reports while their American counterparts are not, there may be an argument for expecting a larger absolute informational loss from US M&A delistings than from Swedish ones. However, because some factors suggest that the average IE of analysts in Sweden is worse than in the US, an argument could also be made for expecting a larger relative informational loss from Swedish M&A delistings than from US ones. If these mechanisms both reflect reality, their reconciliation is contingent on the relative information loss being more important in this case.

The findings are timely and relevant to the current context, considering the trend of shrinking numbers of listed firms. Academically, the study contributes in several ways. We extend the results of A. B. Brown et al. (2024) in two ways, exploring a new context, and operationalizing the IE as the mean forecast error rather than the median. The study is the first of its kind in a non-US setting and addresses significant gaps in the existing literature. The findings demonstrate the external validity of the findings of A. B. Brown et al. (2024), and highlight consistency over different conditions.

Whereas there is room for improvement in several areas, the study provides a basis for studying the informational effects of structural market events in a Swedish context, and emphasizes the need for further understanding of the topic, particularly with regards to the different effects of concentric and conglomerate M&A.

5.4. Suggestions for Future Research

Future research could further this topic in several ways, because of the lack of prior empirical findings, many extensions would make valuable contributions to the literature. The context of our study could be explored further, for example through differentiation of M&A based on another characteristic than concentric/conglomerate or through employing cross-sectional analysis to understand what determines the extent of informational effects of M&A delistings. Whether there is a difference in effect between Concentric and Conglomerate M&A could also be explored to better understand the mechanisms at play. Other methodologies could also be used to further understand the dynamics of the changes to the IE, for example, with individual analyst data, the framework developed by Barron et al. (1998) could be utilized to understand how M&A delistings impact analysts' idiosyncratic and shared information respectively. Smaller methodological alterations, such as utilizing a different model, a different event period, a different peer identification strategy, or different controls would all further illuminate this topic.

The study, paired with that of A. B. Brown et al. (2024) could be perceived as consistent with a greater deterioration in analysts' IE following M&A delistings in Sweden than in the US, and possible mechanisms explaining this could be explored in future empirical and theoretical work.

Future research could also shift or broaden the scope of study. Other geographies and time periods could be explored to understand the applicability and context-dependent conditions of the results, and to understand how differences in context may interact with the mechanism at play. Other structural market events, such as other types of equity delistings, non-equity security delistings, and IPOs could also be studied to further understand the consequences of changes in the number of listed firms and securities for the IE. Furthermore, other elements of the IE could be studied to enhance confidence that the effects apply to the entirety of the IE, and not just to the IE of analysts.

Appendices

Appendix 1 - Expansion of Sample Derivation

Table A1. Clarification of Non-M&A Delistings

Bankruptcies	98
List-Switch	27
Liquidation	17
Voluntary Delisting	14
Misc. (State intervention, N/A, Domicile change, etc)	27
Total:	**182**

Table A2. Clarification of Data Inconsistencies

No data found for latest interim date	2
Still produces annual reports after M&A	10
Has a reporting period not ending in 31/3, 30/6, 30/9 or 31/12	2
Total:	**14**

Appendix 2 - Results of Robustness & Re-Specification Tests

Table A3

Robustness Tests of Model 1 and 2 for H1 and H2: Winsorization Levels

	95/5% level: *Data set winsorized at 95% and 5% level*				100/0% level: *Unwinsorized data set*			
	(1.1) Hyp 1		(1.2) Hyp 2		(2.1) Hyp 1		(2.2) Hyp 2	
	Coeff	t-stat	Coeff	t-stat	Coeff	t-stat	Coeff	t-stat
PostMA	0.0056**	(2.14)	0.0043**	(2.02)	0.0052**	(2.03)	0.0040*	(1.92)
PostMA×Conc.			0.0033	(1.53)			0.0031	(1.45)
Control Var:								
log(1+MktVal)	-0.0276***	(-3.21)	-0.0276***	(-3.21)	-0.0261***	(-3.37)	-0.0261***	(-3.37)
log(1+Following)	-0.0105	(1.03)	-0.0104	(1.02)	-0.0097	(-0.90)	-0.0096	(-0.90)
log(1+MTB)	0.0035	(0.36)	0.0036	(0.37)	-0.0029	(-0.34)	-0.0028	(0.32)
log(1+Volume)	0.0134**	(2.23)	0.0134**	(2.22)	0.0124**	(2.19)	0.0123**	(2.18)
Firm-fixed effect	M&A-peer		M&A-peer		M&A-peer		M&A-peer	
Time-fixed effect	Year-Quarter		Year-Quarter		Year-Quarter		Year-Quarter	
Adjusted std. errors	M&A-peer		M&A-peer		M&A-peer		M&A-peer	
R^2 (Between)	5.48%		5.55%		9.99%		9.93%	
R^2 (Within)	3.42%		3.44%		3.67%		3.69%	
R^2 (Overall)	3.96%		4.04%		4.25%		4.29%	
# of obs.	7,717		7,717		7,717		7,717	
F-Stat. (Robust)	4.98*** (df=5;7438)		4.27*** (df=6;7437)		4.46*** (df=5;7438)		3.75*** (df=6;7437)	

This table presents the PanelOLS regression results of Model (1) and (2), re-estimated; using our original sample winsorized at a 95% and 5% level we test (1.1) H1 and (1.2) H2 and using our original sample un-winsorized we test (2.1) H1 and (2.2) H2. Dependent, independent and controls are not correlated or acting in any way significantly different than the original sample, see table 2 for original reference. Model specifications are otherwise unchanged from original model, see endnotes of table 3 for further details. M&A-peer and year-fixed effects are included. Standard errors are clustered at M&A-peer level. Significance levels are based on a two tailed p-values: 10%, 5%, and 1% being indicated by *, **, and ***, respectively.

Table A4

Re-Specification of Models 1 and 2 for H1 and H2: Industry Level Classification & Event Period Classification

	(1) Industry level classification: *Eight-level TRBC - Activity Name*				(2) Event period classification: *[q-1; q0]*			
	(1.1) Hyp 1		(1.2) Hyp 2		(2.1) Hyp 1		(2.2) Hyp 2	
	Coeff	t-stat	Coeff	t-stat	Coeff	t-stat	Coeff	t-stat
PostMA	0.0098	(1.65)	0.0137*	(1.86)	0.0058*	(1.66)	0.0045	(1.37)
PostMA×Conc.			-0.0093	(-1.20)			0.0035	(1.44)
Control Var:								
log(1+MktVal)	-0.0139	(-1.55)	-0.0134	(-1.50)	-0.0268***	(-3.29)	-0.0268***	(-3.30)
log(1+Following)	-0.0141	(-1.16)	-0.0146	(-1.18)	-0.0138	(-1.03)	-0.0137	(-1.03)
log(1+MTB)	-0.0032	(-0.23)	-0.0039	(-0.28)	0.0010	(0.16)	0.0011	(0.17)
log(1+Volume)	0.0073	(1.21)	0.0077	(1.26)	0.0160**	(2.09)	0.0160**	(2.08)
Firm-fixed effect	M&A-peer*		M&A-peer		M&A-peer		M&A-peer	
Time-fixed effect	Year-Quarter		Year-Quarter		Year-Quarter		Year-Quarter	
Adjusted std. errors	M&A-peer*		M&A-peer		M&A-peer		M&A-peer	
R^2 (Between)	10.99%		9.32%		15.29%		15.90%	
R^2 (Within)	2.18%		2.47%		3.37%		3.39%	
R^2 (Overall)	5.21%		5.04%		3.01%		3.07%	
# of obs.	2,385		2,385		7,938		7,938	
F-Stat. (Robust)	1.47 (df=5;2190)		1.94* (df=6;2189)		4.39*** (df=5;7650)		3.73*** (df=6;7649)	

This table presents the PanelOLS regression results of model (1) and (2), re-estimated using new samples and model specifications. For (1) Industry level classification, a sample based on *M&A-peers** is used to test (1.1) H1 and (1.2) H2, see table A6, appendix 3 for sample details. *M&A-peer** refers to a firm which have the same TRBC activity name as the targeted firm of a M&A delisting. This model changes the interaction term *PostMA×Conc.*, as *ConcentricMA* now is coded 1 if the M&A is concentric, based on TRBC activity level, and 0 if it is not concentric (conglomerate), based on TRBC activity level. All control variables are now based on *M&A-peer** instead of *M&A-peer* as in previous models. The specifications are otherwise unchanged from the original models, see endnotes of table 3 for further details. M&A-peer *and year-fixed effects are included. Standard errors are clustered at M&A-peer* level. For (2) Event period classification, a sample based on a different event period specification [q-1; q] utilizing data gathered in periods [q-7; q+6] is used to test (2.1) H1 and (2.2) H2, for sample details see table A7, appendix 3. The event period specification changes the *PostMA* indicator, where it equals one if a M&A-peer quarter observation is made during the 6-quarter period after the M&A [q+1; q+6], utilizing the new event specification, and zero if it is made in the period before [q-7; q-2], where q is the quarter when the delisted firm stops issuing reports. The specifications are otherwise unchanged from the original models, see endnotes of table 3 for more information. M&A-peer and year-fixed effects are included. Standard errors are clustered at M&A-peer level. For both samples, data is winsorized at the 1% and 99% level. Significance levels are based on a two tailed p-values: 10%, 5%, and 1% being indicated by *, **, and ***, respectively.

Appendix 3 - Sample Selection for Specification Tests

Table A5. Sample Selection of M&A Delistings and Quarterly Obs. for Endogeneity-Restricted Sample Test		
Industry-quarters with M&A delistings		193
(Industry-quarters with more than one M&A)	(11)	
Industry-quarters with only one per-industry M&A delisting		182
M&A-peer quarter observations matched to the industry-quarters pre- and post-M&A		10,288
(Removed due to fewer than 3 quarters pre- or post-period)	(3,137)	
Final sample: M&A-peer quarter observations matched to industry-quarters with M&A		**7,151**
Of which are classified as Concentric M&A	2,893	
Of which are classified as Conglomerate M&A	4,258	

Note: Dependent, independent and controls are not correlated or acting in any way significantly different than the original sample, see table 2 for original reference.

Table A6. Sample selection of M&A Delistings and quarterly obs. for ind. classification specification test		
industry-quarters with M&A delistings		193
No longer multiple M&A in same ind.-quarter due to activity-quarter classification	3	
Activity-quarters with only one per-industry M&A delisting		196
M&A-peer* quarter observations matched to the activity-quarters pre- and post-M&A		3,641
(Removed due to fewer than 3 quarters pre- or post-period)	(1,266)	
Final sample: M&A-peer* quarter observations matched to activity-quarters with M&A		**2,385**
Of which are classified as Concentric M&A	1,050	
Of which are classified as Conglomerate M&A	1,335	

Activity-quarter refers to the unique combination of a year-quarter and an TRBC activity name. *M&A-peer** refers to a firm which have the same TRBC activity name as the targeted firm of a M&A delisting.

Note: Dependent, independent and controls are not correlated or acting in any way significantly different than the original sample, see table 2 for original reference.

Table A7. Sample selection of M&A Delistings and quarterly obs. for event period specification test		
Industry-quarters with M&A delistings		193
M&A-peer quarter observations matched to the industry-quarters pre- and post-M&A		11,448
(Removed due to fewer than 3 quarters pre- or post-period)	(3,510)	
Final sample: M&A-peer quarter observations matched to* industry-quarters with M&A		**7,938**
Of which are classified as Concentric M&A	2,984	
Of which are classified as Conglomerate M&A	4,954	

*Matched to** refers to observation in the pre- and post-M&A period of a M&A delisting, which are, in the current definition of the event period, [q-7; q-2] and [q+1; q+6] respectively.

Note: Dependent, independent and controls are not correlated or acting in any way significantly different than the original sample, see table 2 for original reference.

Appendix 4 – Declaration of AI usage

Students at Stockholm School of Economics are allowed to use generative artificial intelligence (AI) tools, contingent on the disclosure of use. Therefore, this following section is an overview on how AI tools have been utilized, together with their risks and benefits.

For this paper, several AI tools have been used. GitHub's *Copilot* have been used for coding assistance in Python. DeepSeek's *DeepSeek V3* and OpenAI's *ChatGPT4o* have also been used for limited coding assistance in Python, but have generally been used for grammatical and text-structuring assistance, locating relevant literature and further research, as well as assistance in interpreting results and methodological development. The key benefit of these tools was enhanced efficiency, serving as a valuable supplementary resource throughout the thesis process. However, AI also poses a risk for academic integrity, as outputs such as references, ideas and code may be factually incorrect or "hallucinated" (generated without any underlying evidence). To mitigate these concerns, all AI-generated content was rigorously scrutinized. Code was tested, statements were cross referenced, all reference papers were read without AI assistance, and no output was adopted without verification. This approach ensures the reliability of the contributions assisted by AI in the thesis process.

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