The Determinants of Financial Analysts' Performance:

Analyses using Quasi-Natural Experiments

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Table of Contents

List of Figures and Tables

Abstract

This thesis consists of three studies that utilize financial analyst career events as quasi-natural experiments to examine the factors that explain analyst forecasting performance. The purpose of this thesis is to minimize endogeneity problems that have hampered the financial analyst literature and at the same time add to the literature by showing that important life events can have a significant impact on analyst forecasting performance.

First, I examine how employment change affects analyst herding behavior in their forecasts. My results show that analysts exhibit stronger herding behavior following an employment change. Specifically, they have a greater tendency to imitate other analysts' earnings forecasts. Also, relative to their peers, they are slower in issuing forecasts and, as a result, issue revisions less frequently. This has a consequential negative effect on the market impact of their forecasts. I argue that the results are due to the need for newcomers to contend with the unfamiliarity of their new workplace environment and demonstrate that my results hold across several robustness tests, including a quasi-natural experiment using brokerage firm M&As that utilizes the estimation of an average treatment effect. This study raises a significant human resource question on how brokerage firms should support employees who have recently switched jobs.

Second, I examine the impact that work specialization has on the performance of superior and inferior analysts. My results show that the forecast accuracy of superior analysts improves when their coverage is more concentrated within a few industries. However, there is no evidence of an equivalent improvement for inferior analysts. I argue that this is due to superior analysts being better able to utilize intra-industry relevant information when pricing stocks within the same sector, leading them to benefit more

from specialization. My results are robust when I conduct quasi-natural experiments by utilizing brokerage firm M&As to capture changes to the work specialization of analysts who continue to work in the merged firms after the M&A events. The findings of this study have implications for how brokerage firms allocate coverage to analysts with different abilities.

Third, I examine a channel that can explain analyst forecast pessimism. Specifically, I investigate the forecasting performance of analysts who have been rehired after experiencing a recent job loss following their brokerage firm closures and find that their forecasts will be more pessimistic relative to both their peers and actual earnings. Importantly, this leads to a decline in the accuracy of their forecasts at their new job. These results are theoretically supported by the career transitions literature, which shows that a job loss will affect the mental disposition of an employee and which I argue leads to analysts providing more pessimistic recommendations. This raises an important question as to how brokerage firms should support new employees who have recently experienced a job loss to avoid any negative impact it might have on their performance.

Declaration

I certify that this work contains no material which has been accepted for the award of any other degree or diploma in my name, in any university or other tertiary institution and, to the best of my knowledge and belief, contains no material previously published or written by another person, except where due reference has been made in the text. In addition, I certify that no part of this work will, in the future, be used in a submission in my name, for any other degree or diploma in any university or other tertiary institution without the prior approval of the University of Adelaide and where applicable, any partner institution responsible for the joint-award of this degree.

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Signature: Thi Mai Lan Nguyen Date: 29 March 2019

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1. Introduction

1.1. The importance of financial analysts to capital markets

Financial analysts are known as sophisticated information intermediaries who enhance capital market efficiency. Their main role is to process financial information and provide financial forecasts and recommendations to the market. Previous studies (Chan, Jegadeesh and Lakonishok, 1996; Mikhail, Walther and Willis, 2003; Zhang, 2006) have shown that financial analyst forecasts and recommendations reflect news in earnings announcements. In addition, there is evidence that analysts also integrate information not related to earnings into their forecasts (Stickel, 1993; Burgstahler and Eames, 2003; Bratten et al., 2016).

Given the important role of financial analysts, other market participants do rely on analyst forecasts and recommendations to make investment decision. For example, Barber et al. (2001) and Barth and Hutton (2004) find that investors can generate abnormal returns if they structure their trading strategies based on analyst recommendations/forecasts. At the same time, Ikenberry and Ramnath (2002) and Elgers, Lo and Pfeiffer (2003) document that market underreaction to financial news is partially explained by analyst underreaction to such news. Therefore, the efficiency of analysts in processing information (or their forecasting performance) is strongly related to the efficiency of the whole capital market.

Much evidence has also shown that the market is selective in utilizing analyst forecasts and recommendations based on analyst forecasting performance. For example, market participants exhibit stronger reaction to forecasts issued by superior, more reputed, and more experienced analysts (Mikhail, Walther and Willis, 2004; Sorescu and

Subrahmanyam, 2006). In addition, the market can also recognize several factors that explain analyst forecast errors (Mikhail, Walther and Willis, 1997; Clement and Tse, 2003; Chan and Hameed, 2006, Bradley, Gokkaya and Liu, 2017) and forecast biases (Michaely and Womack, 1999; Gleason and Lee, 2003; Barber, Lehavy and Trueman, 2007) to adjust their reaction accordingly.

Given the important role of financial analysts to capital markets, there has been a lot of research in this area. In the next sections of the introduction, I will provide a brief review of the literature and highlight the gaps that still exist in the literature. Finally, I will present a summary of the three studies in my PhD thesis, which focus on investigating unexamined factors that affect financial analyst forecasting performance using different analyst career events as quasi-natural experiments.

1.2. A brief literature review

There has been an enormous number of studies in the financial analyst area, most of which focus on examining the determinants of analyst forecasting performance.

The most popular and important measure of analyst forecasting performance is forecast accuracy, which is the deviation of analyst forecast from the actual earnings per share. Dating back to Mikhail et al. (1997), it is documented that firm-specific experience can enhance analyst forecast accuracy. A later study by Clement (1999) supports this view and suggests that brokerage firm size and analyst work load can also explain the accuracy in analyst forecasts. Jacob, Lys and Neale (1999) add that forecast frequency, or the number of forecast revisions issued within a forecasting period, can enhance forecast accuracy. Another view by Brown (2001), however, shows that past forecast accuracy is better in explaining future accuracy compared to a model that utilized multiple analyst characteristics to identify superior analysts. Clement, Koonce and Lopez (2007) also find that the impact of several analyst characteristics, including brokerage firm size, analyst years of experience, and the number of stocks in analyst portfolio, on forecast accuracy disappears once they control for analysts' innate ability. Similar results are documented by Bradley et al. (2017) after they control for the related industry experience that analysts have gained before they start working as a financial analyst.

Another aspect of analyst performance is herding behavior in analyst forecasts, or the tendency of financial analysts to avoid issuing forecasts that are distinctly different from other analysts following the same stock. Herding is important to detect as it implies that analysts simply revise their forecasts to mimic others instead of fully reflecting their private information (Trueman, 1994; Hong, Kubik and Solomon, 2000). This leads to increased forecast errors (Clement and Tse, 2005), reduced forecast timeliness (Hong et al., 2000), and causes news releases to have a longer lasting impact on the market as one piece of news is reflected repeatedly in a series of forecasts (Welch, 2000). In other words, herding behavior affect the informativeness of analyst forecasts, which can subsequently undermine market efficiency.

There are several studies that investigate the explanations for analyst herding behavior. For example, Stickel (1992), Trueman (1994), and Clement and Tse (2005) find that analysts who are less certain about their ability to predict earnings tend to follow other analysts' forecasts instead of issuing innovative forecasts. Graham (1999) adds that if both an analyst and their employer are uncertain about the analyst's ability, the analyst can send a positive signal about their ability by herding. Hong et al. (2000) and Clement and Tse (2005) find that inexperienced analysts, who are less certain about their job security compared to experienced analysts, tend to display herding behavior to minimize their chance of being fired. Clarke and Subramanian (2006) document a U-shaped relationship between analyst performance and herding behavior. They find that top outperforming (underperforming) analysts who have very low (high) employment risk are less likely to herd in their forecast. More recently, Nolte, Nolte and Vasios (2014) document that during periods of banking stress when job security is low, analysts are more likely to imitate others.

Recently, studies in the financial analyst literature also focus on examining analyst forecast optimism or the tendency of analysts to issue forecasts that give more optimistic predictions than other analysts. Since forecast optimism implies analysts' deviation from their fair judgement, there is strong evidence that it is negatively associated with analyst forecast accuracy (Hong and Kubik, 2003; Cowen, Groysberg and Healy, 2006).

So far, there are two main factors that have been examined as explanations for analyst forecast optimism. The first explanation is analyst work incentives. For example, Hong and Kubik (2003) find that analysts who issue more optimistic forecasts tend to have better career outcomes. They argue that brokerage firms reward relatively optimistic analysts to promote their underwriting business and generate more trading commissions. Cowen et al. (2006), however, find that analysts working for full-service banks with underwriting services issue less optimistic forecasts compared to those who work for non-underwriter banks. Their results suggest that trading commission is an important factor that explains analyst forecast optimism. Chan, Karceski and Lakonishok (2007) document that analysts lower their earnings forecasts before the announcement date to generate positive earnings surprise for the firms. The bias is more pronounced when analysts have a stronger desire to win investment banking clients.

The second factor that explains forecast optimism are analyst career concerns. For example, Ke and Yu (2006) show that analysts tend to issue optimistic forecasts at the beginning of a forecasting period and pessimistic forecasts before the earnings announcement to please firm management to get access to private information. This allows them to issue more accurate forecasts and avoid being fired. Another study by Horton, Serafeim, and Wu (2017) investigates banking analysts and report evidence that banking analysts adjust their forecast optimism during the year to please a bank that could be their future employers, which also leads to favorable career outcomes.

1.3. Gaps in the literature

Despite the wealth of research that has examined analyst forecasting performance, there still exist a number of areas that have not been fully examined. In this section, I focus on two of these that are directly related to my thesis. The first is a methodological issue relating to endogeneity concerns and the second is a gap in the empirical literature that has yet to fully explore the impact that life events have on analyst forecasting performance.

1.3.1. Endogeneity problems

Though previous studies investigate several factors to explain financial analyst performance, the findings are sometimes mixed. This is possibly due to the problems of endogeneity causing bias to both the sign and significance of regression coefficients. Endogeneity problems arise due to three main causes: self-selection bias, reverse-causality and omitted variables. This results in a correlation between an explanatory variable and the error term, violating one assumption of Ordinary Least Squared (OLS) regressions. As a result, the regression coefficients can be biased and inconsistent, leading to spurious results (Woolridge, 2012).

In studies on the performance of analysts, two primary endogeneity concerns are usually raised. The first is a reverse causality relationship between the dependent and independent variables. This happens when it is hard to determine the direction of causality between the two variables. For example, Clement (1999) find that larger brokerage firm size, longer years of experience, and lighter workload can lead to analysts' better forecasting performance. However, it can also be argued that analysts who are performing well are more likely to work for larges firm, to survive for a longer time in the brokerage industry, and to negotiate for less workload. Second, endogeneity problems may arise due to self-selection bias, in which the studied sample is biased to a specific group of observations. The study by Mikhail et al. (1997) is a typical example of self-selection. In this study, the authors restrict their sample to analysts who have 32 continuous quarters of forecast for the same company, excluding all the analysts who switch the tracking company or who leave the industry. This requirement, therefore, biases the sample to the group of well-performing analysts.

One methodology that has arisen in popularity from research in accounting and finance (Hong and Kacperczyk, 2010; Irani and Oesch, 2013; Derrien and Kecskés, 2013; Chen, Harford and Lin, 2015; Irani and Oesch, 2016) is the use of quasi-natural experiments that takes advantage of examining the impact of exogenous shocks on the variables of interest. A quasi-natural experiment is an empirical study that utilizes an event as an exogenous shock to an independent variable and examines the impact of this exogenous shock on the dependent variable. The difference between a natural experiment and a quasi-natural experiment is the degree of randomization. While a natural experiment involves actual randomization, a quasi-natural experiment is "patterned after randomized experiments", which then requires the use of a difference-in-differences (DiD) approach that provides a before-after comparison between the treatment and control groups of observations (DiNardo, 2010). Given the advantage of this methodology in dealing with endogeneity problems, I aim to utilize quasi-natural experiments to provide more robust results than the extant literature on analyst performance has provided. This is a departure from the standard empirical analysis that has previously examined analyst behavior, with few exceptions to the rule (Hong and Kacperczyk, 2010; Irani and Oesch, 2013; Derrien and Kecskés, 2013; Chen et al., 2015; Irani and Oesch, 2016).

1.3.2. The impact of life events on analyst forecasting performance

Although a large amount of work has been done in exploring factors that explain analyst forecasting performance, prior research has paid little attention to the impact that life events have on analyst performance. In the seminal work by Holmes and Rahe (1967), the authors provide a list of 61 life events (seven of which are work related events) and an estimation of a social readjustment score for each event. Since then, several studies (Bhagat, 1983; Ivancevich, 1986; Pugh, Skarlicki and Passell, 2003; Georgellis, Lange and Tabvuma; 2012) have been done to investigate the impact of different life events on individual work performance.

Recent studies the financial analyst literature start directing their attention to explore how analysts respond to certain exogenous events, however, little attention has been paid to the impact of life events on analyst forecasting performance. For example, Bourveau and Law (2016) find that analysts who work in Louisiana during the arrival of Hurricane Katrina show more pessimism in their subsequent forecasts. This impact lasts for 12 to 18 months after the natural event. Antoniou, Kumar and Maligkris (2016) also document more pessimism in analyst forecasts among those who locate near terrorist attacks. This pessimism still persists one year after the attacks. The most related study in the financial analyst literature that mentions analyst personal life event is the paper by Wu and Zang (2009), which examines the factors that affect analyst career outcomes (i.e. get promoted, be retained in the merged firm, or leave the merged firm) following a merger of their employer with another brokerage firm. This study, however, does not explore how this career event affect analysts' subsequent performance.

My thesis utilizes different analyst career events as quasi-natural experiments to study the determinants of analyst forecasting performance, from the change to analyst work arrangement or an employment change following a brokerage M&A, to a job loss following a brokerage firm closure. This research approach allows my study to fill the gap in the literature by examining how analyst work performance is affected by important career events. It also opens an avenue for future research that focuses on the impact of important life events on financial analyst forecasting performance.

1.4. Summary of the three studies

As aforementioned, I aim to utilize different career events of analysts as quasi-natural experiments to study the unexamined determinants of analyst forecasting performance. Specifically, my idea is to examine brokerage firm mergers and acquisitions (M&As) and closures, in which I investigate different life events that are associated with financial analyst careers. Based on this idea, I conduct three studies for my thesis. First, I focus on the impact of job change on analyst herding behavior among those analysts whose decision to change jobs is triggered by the M&A event of their former employer. Second, I examine how analyst forecast accuracy is affected by a change in their work specialization caused by the rearrangement of workload among analysts following an M&A between two brokerage firms. Finally, I examine how analyst forecast optimism is affected by a previous job loss resulting from their brokerage firm closure. I provide further details of each of these three studies below.

1.4.1. Study 1: The impact of employment change on analyst herding behavior

This study examines how employment changes affect analyst herding behavior. This research question is motivated by the findings from the career transitions literature that newcomers will need extra time and effort to adapt to their new workplace environment (Brett, Feldman and Weingart, 1990; Miller and Jablin, 1991; Saks, Uggerslev and Fassina, 2007; Bauer et al., 2007). This includes, for example, the need to learn different operational processes (Pinder and Schroeder, 1987; Huckman and Pisano, 2006) and to build new social networks within the firm (Bauer et al., 2007) in order to rebuild the nontransferable human capital that is lost when an analyst moves to a new employer (Groysberg, Lee and Nanda, 2008). In doing so, analysts will have less time to focus on tracking stocks within their portfolio and will therefore be more likely to adopt time-saving strategies, such as herding, when making forecasts.

In addition, the career transitions literature also shows that unfamiliarity with a new work environment also leads to individuals experiencing a greater level of uncertainty (Pinder and Schroeder, 1987; Feldman and Brett, 1983; Brett et al., 1990; Bauer et al., 2007; Saks et al., 2007). This itself can lead to herding behavior. For example, much evidence has shown that analysts herd when they are uncertain about their own ability (Stickel, 1992; Trueman, 1994; Clement and Tse, 2005) or when they are concerned about their relative performance against peers (Hong et al., 2000; Clement and Tse, 2005; Nolte et al., 2014). This, together with the time constraints faced by analysts when they start working in a new environment, leads to my research question of whether an employment change can increase the likelihood that analysts herd in their forecasts.

To answer this question, I first utilize a difference-in-difference (DiD) approach with a treatment sample of 312,242 annual earnings forecasts during the period from 2005 to 2016. Next, to minimize endogeneity concerns, I also perform quasi-natural experiments by focusing on analysts who change job following brokerage firm M&As. My results show that analysts exhibit strong herding behavior following an employment change. Specifically, they have a greater tendency to imitate other analysts' earnings forecasts. Also, relative to their peers, they are slower in issuing forecasts and, as a result, issue revisions less frequently. This has a consequential negative effect on the market impact of their forecasts. My study, therefore, raises a significant human resource question on how brokerage firms should support employees who have recently switched jobs.

1.4.2. Study 2: The heterogeneous impact of work specialization on analyst performance

This study examines the impact that work specialization has on the performance of superior and inferior analysts. Dating back to Clement (1999) and Jacob et al. (1999), work specialization has been identified as one of the key factors that promote analyst forecast accuracy. Similarly, Piotroski and Roulstone (2004) and Chan and Hameed (2006) find that analysts are able to identify the common industry component of each firm-specific news event, which they then utilize to make inferences on other stocks within the same industry. This means that the more stocks an analyst follows within the same industry, the more opportunity they will have to facilitate the transfer of intra-industry information. In contrast, other research finds no systematic relationship between analyst forecast accuracy and how many industries the stocks that they cover are in (Mikhail et al., 1997; Clement et al., 2007; Kim, Lobo and Song, 2011; Bradley et al., 2017). Rather, they argue other factors, such as the innate ability of superior analysts (Clement et al., 2007) can explain differences in analyst performance.

Given the above fact that analysts play an important role in disseminating industry-relevant information to the market, and that we should expect superior analysts to do a better job at this due to their innate ability to benefit from task-specific knowledge (Clement et al., 2007), I conduct my second PhD study to test for the heterogeneous impact that work specialization has on analyst forecast accuracy. This can potentially explain the mixed results within the extant literature, as one cohort of analysts (i.e. superior analysts) benefit from specialization while another cohort (i.e. inferior analysts) do not.

I utilize the Herfindahl-Hirschman Index (*HHI*) to measure how concentrated the stocks that an analyst follows are within a limited number of industries (i.e. work specialization). I first generate panel regression results using the full sample of 535,203 analyst earnings forecasts during the period from 2005 to 2016. After this, and to deal with endogeneity concerns, I conduct quasi-natural experiments by utilizing brokerage firm M&As to capture changes to the work specialization of analysts who continue to work in the merged firms after the M&A events. My results show that the forecast accuracy of superior analysts improves when their coverage is more concentrated within a few industries. However, there is no evidence of an equivalent improvement for inferior analysts. My findings, therefore, have implications for how brokerage firms allocate coverage to analysts with different abilities.

1.4.3. Study 3: The impact of job loss on analyst forecast pessimism

This study examines whether a previous job loss can lead to analyst forecast pessimism. While the extant literature mostly focuses on explaining analyst forecast optimism (Hong and Kubik, 2003; Cowen et al., 2006; Horton et al., 2017), little has been known about the factors that can lead to forecast pessimism and the implications of it. Empirical evidence has shown that analyst forecast optimism is associated with a reduction in forecast accuracy (Hong and Kubik, 2003; Cowen et al., 2006). However, I conjecture that both forecast optimism and pessimism can have adverse impact on analyst forecasting performance since they both indicate a diversion of analyst forecasts from their fair judgement. Therefore, it is equally important to study analyst forecast pessimism.

At the same time, previous studies (Cohn, 1978; Donovan and Oddy, 1982; Pugh et al., 2003; Waters, 2007) from the career transitions literature document that job loss can cause several psychological issues to the displaced employees including a reduction in self-esteem, anxiety, and other symptoms of depression due to the change in the social status of the displaced employees. In addition, one obvious causal effect from a reduction in self-esteem is an increased pessimistic outlook. There already exists strong evidence of a positive relationship between self-esteem and optimism/pessimism (Mäkikangas, Kinnunen and Feldt, 2004; Heinonen, Räikkönen and Keltikangas-Järvinen, 2005; Lyubomirsky, Tkach and DiMatteo, 2006). This leads to my research question asking whether financial analysts who have previously lost their job will be pessimistic when re-employed and how this affects their work performance.

In this study, I utilize brokerage firm closures as quasi-natural experiments. Specifically, I focus on analysts who lose job following their brokerage firm closures and subsequently move to another firm during the period from 2004 to 2016. I find that individuals who have recently experienced a job loss tend to issue more pessimistic forecasts compared to both their peers and the actual earnings. Importantly, this leads to a decline in their forecast accuracy in their new job. My findings, therefore, raise an important question on how brokerage firms should support their new employees who have recently experienced a job loss to avoid any negative impact it might have on their performance.

2. The impact of employment change on analyst herding behavior

2.1. Introduction

The paper in this Chapter examines how employment changes affect the tendency of financial analysts to avoid issuing forecasts that are distinctly different from other analysts following the same stock (i.e. herding behavior). My investigation is motivated by observations from the career transitions literature that find newcomers will expend time and effort to adapt and assimilate within their new workplace environment (Brett et al., 1990; Miller and Jablin, 1991; Saks et al., 2007; Bauer et al., 2007). This includes, for example, the need to learn different operational processes (Pinder and Schroeder, 1987; Huckman and Pisano, 2006) and to build new social networks within the firm (Bauer et al., 2007) in order to rebuild the nontransferable human capital that is lost when an analyst moves to a new employer (Groysberg et al., 2008). In doing so, analysts will have less time to focus on tracking stocks within their portfolio, and will therefore be more likely to adopt time-saving strategies, such as herding, when making forecasts.

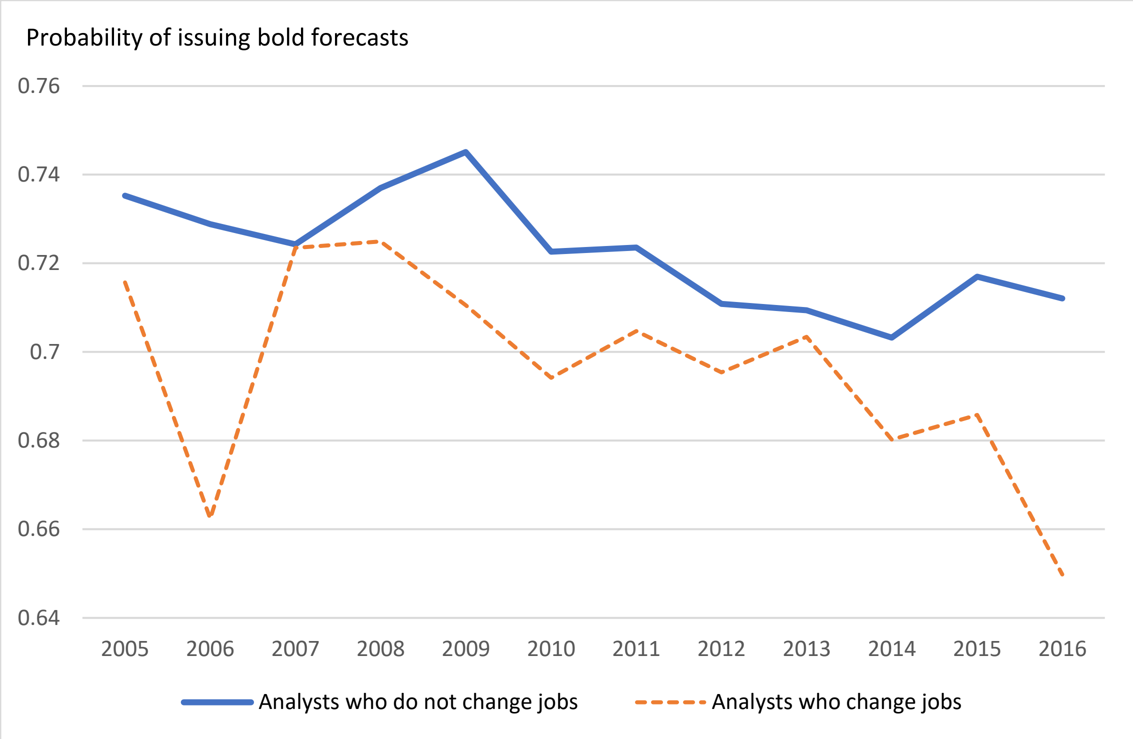
Interrelated with this, the career transitions literature highlights that unfamiliarity with a new work environment also leads to individuals experiencing a greater level of uncertainty (Pinder and Schroeder, 1987; Feldman and Brett, 1983; Brett et al., 1990; Bauer et al., 2007; Saks et al., 2007). This itself can lead to herding behavior. Analysts, for example, have been shown to herd when they are uncertain about their own ability (Stickel, 1992; Trueman, 1994; Clement and Tse, 2005) or when they are concerned about their relative performance against peers (Hong et al., 2000; Clement and Tse, 2005; Nolte et al., 2014). This uncertainty, plus the additional time constraints placed on newcomers in becoming familiar with their new environment, leads me to hypothesize that employment change increases the likelihood that analysts will herd.

Herding is important to detect as it has a significant market impact. It is inefficient and implies that analysts simply revise their forecasts to mimic others instead of fully reflecting their private information (Trueman, 1994; Hong et al., 2000). This leads to increased forecast errors (Clement and Tse, 2005) and causes news releases to have a longer lasting impact on the market as one piece of news is reflected repeatedly in a series of forecasts (Welch, 2000). Since analysts act as information intermediaries who enhance market efficiency, and given that in any single year over the past decade almost 10% of all analysts change jobs, any market effect of herding behavior from analysts switching jobs is potentially significant.[1]

To provide some anecdotal evidence in support of my primary hypothesis, Figure 2.1 shows the relationship between analyst job change and herding behavior. I use a sample of 312,242 annual earnings forecasts. The data is extracted from the Institutional Brokers' Estimate System (I/B/E/S) database between 2005 and 2016 and the figure compares the probability to issue *Bold* forecasts for analysts that change jobs within a particular year with those of analysts that do not change jobs in that year. Following Clement and Tse (2005) and Gleason and Lee (2003), I define *Bold* forecasts as forecasts that deviate from both the consensus and the analyst’s most recent forecast for the same stock. The figure illustratively shows that, on average, analysts who experience a job change consistently exhibit a lower probability of issuing bold forecasts (i.e. more herding) compared to those who remain in their job.

[1] This is based on forecasts that are recorded in the Institutional Brokers’ Estimate System (I/B/E/S) database from 2005 to 2016.

Figure 2.1: Analyst job change and the probability of issuing bold forecasts.



This figure shows the difference in herding behavior between analysts who change jobs in any given year, and those who do not, from 2005 to 2016. The comparison is based on the average probability of analysts to issue *Bold* forecasts (forecasts that deviate from both the consensus and the analysts' previous forecast for the same stock).

There are, of course, problems with inferring too much from this figure. There are several reasons why analysts change jobs, inclusive of the fact that it can be endogenously related to analysts herding in the first place. In particular, analysts who tend to herd may be more likely to change jobs. Therefore, any observable herding witnessed after analysts change jobs may simply be a function of the herding behavior they previously exhibited. In addition, analysts who change jobs may, for instance, be given different stocks to follow, leading to a potential rise in herding behavior due to the unfamiliarity with the stocks they must track, rather than it being due to the change in job itself. Other factors, including analyst resources and experience, could also potentially explain why herding becomes more prevalent after a job change.

To minimize the effect that the above factors can have in influencing the results, I utilize three different identification strategies, on top of my baseline results, to construct three types of treatment and control samples to test my hypothesis. The first strategy is to utilize a difference-in-difference (DiD) approach to compare the herding behavior of forecasts for a specific stock from a particular analyst that changes their job (my treatment sample) with forecasts for the same stock from an analyst that exhibits similar herding behavior but does not experience any job change (my control sample). I also match to account for differences in the workload that the analyst experiences in the new job, as well as differences in resources available between the analyst's prior and new employer.

I examine the herding behavior between the treatment and control samples of the above strategy from three different perspectives. The first measure, *Bold*, is used to capture herding behavior from a pricing perspective. I then use a measure to gauge how quick analysts are at posting their forecasts. We would expect an analyst that is exhibiting herding behavior to prefer to wait until other analysts have posted their forecasts in order to determine what forecast they will make. My second measure, *Speed*, identifies whether an analyst is timely in issuing their first forecast for a stock relative to all other analysts that are tracking the same stock. Finally, I follow Hong et al. (2000) and use *Frequency* to determine if analysts are more or less likely to provide forecast revisions for a stock.

As an alternative to matching analysts based on their *ex ante* level of herding behavior, my second approach is to create a treatment and control set following Hong and Kacperczyk (2010), who match based on the likelihood that analysts will herd. This is achieved by matching several analyst and stock characteristics between the treatment and control group of forecasts. These characteristics include the resources of the brokerage firm the analyst works for, analyst experience, analyst coverage of the stock, the annualized stock return, and the book-to-market value of the stock. Finally, to avoid any selection bias arising from matching one treatment forecast with only one control forecast, my third identification strategy involves examining the results generated from forming portfolios of control forecasts based on the above characteristics to test whether there is a difference between my treatment forecasts and a comparable portfolio of control forecasts.

Regardless of how I construct my treatment and control samples, my results show that herding is prevalent for analysts that change jobs. My baseline results from my DiD analysis using the simplest treatment and control group split (of those analysts who switch jobs and those that do not) shows that the probability of an analyst who has recently changed jobs issuing a *Bold* forecast is reduced by 8.7%. In addition, the timeliness of earnings forecasts from these analysts declines, such that their *Speed* in issuing the first forecast for any given stock in the fiscal year reduces by 16%. This has a knock-on effect on how often these analysts post revisions to their forecasts. I show that these analysts will tend to wait until most of the other analysts following a stock have posted their forecasts, implying the need for them to revise their own forecasts to match others becomes less important. The *Frequency* of revisions declines by 8.2%.

Next, I investigate whether analyst herding varies with the degree of familiarity analysts are likely to encounter in their new job. I find that analysts who move to a new firm together with colleagues from their former firm, thus providing some familiarity surrounding who they work with, show less herding behavior compared to those who are alone in moving to a new firm. In addition, I perform several sub-sampling analyses to examine whether my results are driven by a group of atypical analysts that are unduly influencing my results. I find that analysts who undergo a job change will exhibit more herding behavior regardless of whether the analyst (i) has higher or lower forecast accuracy; (ii) shows more or less herding behavior prior to a job change; (iii) takes a longer time to find a new job; and (iv) moves to larger or smaller brokerage firms. Furthermore, my results remain consistent when I account for analysts who reappear many times in the sample, and when I account for stocks with higher forecasting complexity (i.e. stocks with low analyst coverage or stocks of large firms). Furthermore, I re-run my analyses using my treatment and control portfolios of analyst forecasts where the average herding estimate is used for each of my three herding measures. The results remain the same.

I also perform a test using a quasi-natural experiment. Wu and Zang (2009) show that turnover is higher among analysts with high forecasting abilities following brokerage M&As. And as analysts with high forecasting performance are not those that herd (Clement and Tse, 2005), I will likely reduce the probability that the reason analysts are changing jobs is due to their *ex ante* herding behavior if I restrict my analyses to analysts that switch jobs due to an M&A. Also, to reduce the risk that there are other endogenous factors which lead analysts to change jobs following an M&A, I utilize a method of estimating the average treatment effect through a two-stage regression procedure (Wooldridge, 2002 p.614-621). In the first-stage, I regress the treatment effect on two exogenous covariates plus all other control variables.

The two covariates I utilize are based on the findings of Wu and Zang (2009) that show analyst turnover following an M&A is higher among the target analysts and analysts who have a direct competitor analyst following similar stocks in the counterpart firm. Therefore, the first covariate I use is a dummy variable that identifies whether the analyst is from the target firm. It is exogeneous since analysts have no power in determining whether they belong to the target or to the acquirer firm. The second covariate is a dummy variable that identifies whether the analyst has a direct competitor in the counterpart firm. It is also exogeneous for similar reasons as with the first covariate, namely that the analyst has no control over whether they have a direct competitor in the counterpart firm.[2] From the first-stage regression results, I generate a predicted series of the treatment effect (i.e. the average treatment effect). I then use this predicted series as the regressor in my DiD models in the second stage, replacing the treatment dummy. Again, my results are consistent to my baseline results, demonstrating the robustness of my findings.

Finally, I document that herding after an employment change leads to both a statistically and economically significant impact of analyst forecasts on the market. On average, the two-day cumulative market-adjusted return surrounding a forecast announcement from an analyst that has recently changed jobs is 7.7% less than before they changed jobs.

My study contributes to the literature in several ways. First, while previous studies document that herding can be linked to analyst forecasting ability and experience (Stickel, 1992; Trueman, 1994; Graham, 1999; Hong et al., 2000; Clement and Tse, 2005), no one has examined analyst job changes as a source of herding behavior. Given that 10% of analysts change jobs each year, the impact of employment changes on the performance of the intermediary function that analysts serve within the market can be potentially significant.

My second contribution is to highlight the value of the career transitions literature to the financial analyst literature. Specifically, I emphasize that job changes lead newcomers to contend with unfamiliar environments (Katz, 1980; Klein and Weaver, 2000), which encompasses the need to build new social networks (Brett et al., 1990; Bauer et al., 2007; Saks et al., 2007) and deal with differences in operational processes (Pinder and Schroeder, 1987; Huckman and Pisano, 2006), all of which take both time

[2] A competing analyst from a counterpart firm is an analyst whose portfolio is at least 50% similar to the studied analyst.

and effort and place constraints on how analysts produce their forecasts. In addition, the uncertainty caused by unfamiliarity of the new workplace (Morrison, 1993; Ashforth, Sluss and Saks, 2007) can, itself, be a source of herding.

My study provides an ideal setting to examine the above behavior as performance can be tracked over time and for a large cohort of individuals. In finding significant evidence of herding behavior following a job change, I also suggest that there is a substantial human resource management implication from my findings. In particular, I highlight the need for brokerage firms to adopt appropriate newcomer organizational socialization strategies (see Saks et al., 2007) to manage the unfamiliarity that arises from employment changes in order to enhance the quality of analyst forecasts.

Finally, I contribute to the literature which examines the intermediary function that analysts serve in disseminating information into capital markets (Chung and Jo, 1996; Hong et al., 2000). I show changing jobs can affect the efficacy of analyst forecasts, and given the sizable number of analysts who change jobs in any given year, that the impact on the market at-large is substantial and warrants further research with respect to how career events influence analyst performance.

The remainder of this study is structured as follows. Section 2.2 provides a literature review of the career transitions literature plus hypothesis development, while section 2.3 outlines my data and methodology. In section 2.4 I present my empirical results and discuss the main findings and in section 2.5 I provide additional robustness checks. Section 2.6 contains a summary discussion and conclusion.

2.2. Literature review and hypothesis development

A key area of research that the career transitions literature focuses on concerns itself with organizational socialization – how newcomers deal with a new workplace environment. Katz (1980, p.88) refers to organizational socialization as the "introductory events and activities by which individuals come to know and make sense out of their newfound work experiences". It is viewed as the way in which newcomers acquire new attitudes, behavior and thought processes to function in their new work environment (Klein and Weaver, 2000) and examines how well individuals assimilate within their workplace, as well as the consequential effects the transition can have on, for example, employee performance (Ashforth et al., 2007; Saks and Gruman, 2014).

Newcomers are faced with a number of sources of unfamiliarity even if the activity profile for the new job is identical to what the individual was required to do previously. One main source is the need to contend with a new social network. When working for a new firm, newcomers will need to interact with a different set of colleagues and managers, which will require them not only to work out where they fit into the social structure of the new work environment, but how to utilize the new social network to perform in the job. This is commonly achieved by developing information seeking strategies, primarily in the form of seeking feedback from new co-workers and supervisors (Brett et al., 1990; Bauer et al., 2007; Saks et al., 2007). This strategy requires both effort and time.

In addition to being surrounded by unfamiliar co-workers and managers, Pinder and Schroeder (1987) highlight that individuals also encounter dissimilarities in the processes and tools that are used in the new workplace to execute the role. This includes new operational procedures and the need to familiarize oneself with how to access and utilize the tools necessary to complete the job. Huckman and Pisano (2006), for example, find that the operational assets available in one organization will be different to another, and how familiar an individual is with these assets, which are organization-specific, can affect performance.

All of the above factors are relevant within the context of the financial analyst industry. Groysberg et al. (2008) provide evidence of this. They examine the performance of star analysts when they change jobs and find that these analysts can only bring a part of their human capital when they move to a new brokerage firm. They argue that there is a non-transferable component of an analyst's human capital that is brokage firm-specific and is attributable to an analysts' familiarity with the specific operational procedures and internal network of their former employer. When they move to a new employer, they are unable to transfer this component of their human capital. Therefore, analysts who join a new brokage house will undergo a transition period, where they will need to acquaint themselves with a new social network, as well as differences in operational processes, in order to re-build part of the human capital they left behind with their former employer.

This leads me to link, for two interrelated reasons, analyst employment change with herding behavior. First, since analyst efforts will be partly consumed in expending time on information seeking strategies (Miller and Jablin, 1991) to build a new social network within the firm (Bauer et al., 2007) and contend with differences in operational processes (Pinder and Schroeder, 1987; Huckman and Pisano, 2006), less time will be available for analysts to complete other work-related tasks. In particular, in attempting to re-build their lost non-transferable human capital (Groysberg et al., 2008), analysts will have less time to devote to tracking their portfolio of stocks, forcing them to take short-cuts and develop less time-consuming strategies to complete their work, such as relying on peer forecasts (i.e. display herding behavior).

Second, in following the career transitions literature, the desire of the newcomer to acquire information is based on removing the uncertainty they experience in their new job (Morrison, 1993; Ashforth et al., 2007). I postulate that this uncertainty, caused from being unfamiliar with the new workplace environment, can also be a source of herding behavior. Indeed, herding behavior is a common instinct for most social animals, including humans, when facing duress (Hamilton, 1971; Raafat, Chater and Frith, 2009). Financial analysts are also known to show herding tendencies when they are faced with uncertainty. For example, Stickel (1992), Trueman (1994) and Clement and Tse (2005) all find that analysts herd when they are uncertain about their own ability and reputation, while Hong et al. (2000), Clement and Tse (2005), and Nolte et al. (2014) show that analysts herd when they are concerned about their relative performance against their peers.

Taken together, the above two arguments lead to my main hypothesis that the unfamiliarity arising from an employment change will lead to herding behavior in analyst forecasts:

H2.1: Analysts show more herding behavior after they experience an employment change.

To test this hypothesis, I examine analyst herding behavior from three different perspectives, providing me with three sub-hypotheses to test. I first focus on the probability of analysts issuing a bold forecast (i.e. a forecast that diverts from both the consensus and the analyst's most recent forecast for the same stock). According to Clement and Tse (2005) and Hong et al. (2000), when analysts herd, they are more likely to issue forecasts that are closer to the average forecast of other analysts for the same stock, and less likely to issue bold forecasts. Therefore, I expect that after analysts experience a job change, the probability that they issue bold forecasts will also decline:

H2.1a: The probability that analysts issue a bold forecast declines after they experience an employment change.

Another aspect of analyst herding behavior is related to forecast timeliness (i.e. how quickly analysts issue their forecasts). This is an important measure in its own right, as Cooper, Day and Lewis (2001) conclude that analyst performance rankings based on forecast timeliness are even more informative than rankings based on analyst forecast accuracy or trading volume. If analysts are herding, then they will want to wait until the majority of analysts have issued their forecasts so that they can learn from the crowd. In addition, my primary hypothesis is partly premised upon newcomers having less time to spend on tracking stocks as they develop familiarity with their new workplace environment, and therefore are less likely to be timely in the forecasts that they make. As such, forecast timeliness should be negatively associated with herding behavior. This leads to my second sub-hypothesis:

H2.1b: Analyst forecast timeliness declines after they experience an employment change.

Finally, I also study the frequency with which analysts revise their forecasts for a stock within a forecast period. There is mixed evidence regarding the link between analyst herding behavior and how frequently they provide revisions. On the one hand, Hong et al. (2000) argue that when analysts herd, they tend to issue more forecast revisions to accommodate other analysts' opinions. On the other hand, Clement and Tse (2005) and Jegadeesh and Kim (2009) find that herding analysts issue less forecast revisions since they are less likely to update new stock relevant information into their forecasts. My hypothesis, however, is premised on a slightly different argument. First, my argument for analysts herding after a job change is based on them having less time to properly track the stocks in their portfolio. This would imply less time to consider making revisions to their forecasts. Second, given that I suspect analysts who herd are more likely to post their first forecasts after the majority of other analysts have (H2.1b), I argue that this diminishes the need for them to make revisions in the light of other forecasts. Therefore, contingent on H2.1b being true, there should be a corresponding decline in the frequency of analysts revising their forecasts following an employment change:

H2.1c: Analyst forecast revision frequency declines after they experience an employment change.

2.3. Data and methodology

I collect data on annual earnings per share forecasts from analysts between 2005 and 2016 from the Institutional Brokers' Estimate System (I/B/E/S) database. My period of analysis starts from 2005 so that I only examine analyst forecasts after Global Settlement was introduced, which can potentially affect analyst forecasting behavior.[3]

For my econometric model, I employ three different measures to capture herding behavior. The first measure, $Bold_{ijt}$, adopted from Clement and Tse (2005), is a dummy variable which is equal to one if the forecast for stock i issued by analyst j in forecast period t is either greater than both the pre-revision consensus and the analyst's most recent forecast for stock i, or less than both the pre-revision consensus and the analyst's most recent forecast for stock i. Otherwise, it is equal to zero. I calculate the pre-revision consensus as the average of the most recent forecasts for stock i made by other analysts excluding analyst j during the same forecast period. I also require at least three forecasts to construct the pre-revision consensus and to avoid the effects of reiteration, I only use

[3] This is an enforcement agreement reached in 2003 that requires the physical and operational separation between the investment banking and research departments of brokerage firms to mitigate the potential of bias forecasts for an investment banking client.

the analyst's most recent forecast for each stock in their tracking portfolio prior to the end of each forecast period (Hong and Kacperczyk, 2010). This results in my final sample of 312,242 forecasts.

I utilize $Speed_{ijt}$ as my measure of forecast timeliness (i.e. how quick analysts issue their forecasts). I create a normalized timeliness measure by first ranking all analysts covering the same stock within one forecast period based on the order they issue their first forecasts. The first analyst that issues a forecast for the stock receives the lowest *Rank*. I then estimate $Speed_{ijt}$ using Equation (2.1) below, where the denominator is the *Number of analysts* who issue forecasts for the same stock in one forecast period. A higher value of $Speed_{ijt}$ indicates more forecast timeliness. The variable has a range between 0 and 100, with 100 indicative of the analyst being ranked first.

$$Speed_{ijt}=100 - \left[\frac{Rank\text{-}1}{Number\ of\ analysts\text{-}1}\right] \times 100 \qquad (2.1)$$

My final measure, $Frequency_{ijt}$, is derived from Hong et al. (2000). This is the number of forecast revisions issued by analyst *j* for stock *i* in forecast period *t* (Rev_{ijt}) minus the average number of forecast revisions issued by all analysts for the same stock within the same forecast period ($\overline{Rev_{ijt}}$).

$$Frequency_{ijt}=Rev_{ijt} - \overline{Rev_{ijt}} \qquad (2.2)$$

As for my main independent variable of interest I employ $Move_{ijt}$, a dummy variable that is equal to one if the forecast is issued by an analyst who experiences an employment change in year *t*, and zero otherwise.[4] In regard to my control variable set, I utilize a number of variables to account for analyst proficiency in their work. This

[4] I identify analysts who change jobs based on when the analyst experiences a change in their broker ID across two consecutive years.

includes brokerage firm size (*$Size_{kt}$*) to control for analyst resources, analyst years of general experience (*Gen $Exper_{jt}$*), analyst years of experience with a particular industry (*SIC $Exper_{jt}$*), and years of experience with the stock (*Stock $Exper_{jt}$*). I also control for the analysts' workload using the number of stocks (*$Stocks_{jt}$*) and the number of industries they cover (*$Industries_{jt}$*). Finally, I control for several stock characteristics to account for the complexity of forecasting the stock itself, which includes the number of analysts following the stock (*$Coverage_{it}$*), the stock's firm size (*$Lnsize_{it}$*), the stock's return and variance of return (*$Retann_{it}$* and *$Sigma_{it}$*), the stock's book-to-market ratio and profitability ratio (*$Lnbm_{it}$* and *$Profitability_{it}$*), the stock's return-on-equity ratio and its variance (*ROE_{it}* and *Var ROE_{it}*), and whether the stock is included in the S&P500 index (*$SP500_{it}$*). The above data is collected from a number of sources. I obtain data on stock returns from the CRSP database; data on stock fundamentals from the Compustat database; and data on brokerage firms and financial analysts from the I/B/E/S database. Appendix A provides a detailed description of all the variables.

My basic regression model to examine analyst herding behavior after a job change is:

$$Herd_{ijt} = \alpha + \beta_1 Move_{ijt} + \gamma' X_{ijkt} + \varepsilon_{ijt} \qquad (2.3)$$

In this model, each of my herding measures, represented by *Herd,* is regressed against *Move* and a vector *X*, representing my control variables. The coefficient β_1 represents the impact that a job change has on analyst herding behavior.[5]

The above model does not, however, account for any fixed effects that may be related to the brokerage firms, analysts, and stocks. To accommodate this, my main results

[5] I utilize panel OLS regressions when the dependent variable is continuous, and panel logistic regressions when the dependent variable is discrete (i.e. a dummy variable).

are derived from a difference-in-differences (DiD) regression approach by comparing the herding behavior of analysts that change their job (my treatment group) with those who do not experience any job change during the same event window (my control group). I follow Hong and Kacperczyk (2010) and use a two-year window around the analyst job change (i.e. one year before and one year after). In order to observe the change in the herding level of individual forecasts, I only look at forecasts for stocks that appear in an analyst's portfolio both before and after a job change. This reduces the risk of capturing analyst herding due to being assigned new stocks. In addition, I only focus on forecasts issued for the forecast periods that are closest to the analyst job change,[6] and I strictly require that all treated analysts experience no other job change across the event window. Doing so reduces the size of my sample to 37,692 earnings forecasts.

As for my independent variables, I employ *Treat*, a dummy variable that is equal to one if the forecast belongs to my treatment sample and zero if it belongs to the control sample, and *Post*, a dummy variable that is equal to one if the forecast is issued after the analyst job change and zero if it is before the job change.

My DiD regression model is therefore:

$$Herd_{ijt}=\alpha + \beta_1 Treat_{ijt} + \beta_2 Post_{ijt} + \beta_3 Treat_{ijt} \times Post_{ijt} + \gamma' X_{ijkt} + \varepsilon_{ijt} \qquad (2.4)$$

Here, each of my herding measures is regressed against the *Treat* and *Post* dummies, plus their interaction, and a vector *X* of control variables. The coefficient of the interaction term represents the impact that a job change has on analyst herding behavior.

[6] I identify the time of an analyst job change as the period from the date of the analyst's last forecast issued under the former brokerage firm ID to the date of her first forecast under the new brokerage firm ID.

2.4. Empirical results on analyst herding behavior after a job change

2.4.1. Summary statistics and preliminary regression results

Table 2.1 shows the summary statistics of my variables split between my treatment group (of forecasts from analysts who change jobs) and my control group (of forecasts from analysts that do not change jobs) before any matching has taken place and during the period before the treatment group of analysts have switched jobs. I find in relation to my herding measures that my treatment group of analysts are significantly more likely, at the 1% level, to issue bold forecasts (*Bold*), and more timely in issuing their forecasts (*Speed*).

My statistics also show that analysts in my treatment sample are more experienced (*Gen Exper*, *SIC Exper*, *Stock Exper*) and tend to have a heavier workload (*Stocks*, *Industries*) compared to my control group of analysts. The former group also work for smaller firms (*Size*) compared to the latter. I also find analysts who switch jobs tend to cover stocks where there is greater analyst coverage (*Coverage*), have lower returns (*ROE*), lower risk (*Sigma*, *Var ROE*), lower book-to-market ratio (*Lnbm*), and are more likely to be stocks from the S&P500 index (*SP500*).

Table 2.1: Summary statistics of the variables.

Variables	*Unit of measurement*	*Treatment sample (Analysts who change jobs)*			*Control sample (Analysts who do not change jobs)*			*Diff. in means*
		Mean	*Median*	*StDev*	*Mean*	*Median*	*StDev*	
Dependent variables								
$Bold_{ij}$	Dummy	0.7068	1	0.4553	0.6267	1	0.4837	0.0801***
$Speed_{ij}$	NA	54.6539	56.0488	30.3619	52.7624	52.9412	32.5090	1.8915***
$Frequency_{ij}$	Revision	0.0336	0	2.3735	0.0066	0	2.5702	0.0270
Control variables								
Gen $exper_j$	Year	6.1019	6	3.6540	4.4587	3	3.7740	1.6432***
SIC $exper_j$	Year	5.3948	5	3.5449	3.8200	3	3.7121	1.5748***
Stock $exper_j$	Year	3.5258	3	3.0721	1.6054	0	2.0401	1.9204***
$Stocks_j$	Stock	17.0757	16	7.3906	12.3095	12	9.1230	4.7662***
$Industries_j$	Industry	3.9047	3	2.0107	3.3731	3	2.2416	0.5316***
$Size_k$	Analyst	59.0931	40	54.9183	71.9802	46	69.7122	-12.8871***
$Coverage_i$	Analyst	18.9950	18	11.1461	15.3566	13	11.3030	3.6384***
$Lnsize_i$	NA	8.3671	8.3823	1.9320	8.3722	8.3388	2.0960	-0.0051
$Sigma_i$	%	39.9910	34.2377	23.0640	40.4972	34.7500	21.8737	-0.5062
$Retann_i$	%	10.9786	12.8242	45.2699	12.5179	13.1628	45.7828	-1.5393**
$Lnbm_i$	NA	-0.8835	-0.8444	0.9256	-0.6909	-0.7472	1.2052	-0.1926***
ROE_i	NA	0.0865	0.1992	3.0231	0.6285	0.2179	12.4814	-0.5420***
Var_ROE_i	%	0.8246	0.0154	3.8860	1.8832	0.0220	6.2963	-1.0586***
$Profitability_i$	NA	0.0191	0.0756	2.9774	0.0636	0.0749	0.2344	-0.0445
$SP500_i$	Dummy	0.3041	0	0.4600	0.1619	0	0.3684	0.1422***

This table presents the summary statistics of my variables for the treatment and control samples during the period before a job change. Appendix A provides a detailed description of the variables. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

In Table 2.2, I run some preliminary regressions using the above treatment and control samples. For brevity, I only show the coefficient results for the control variables in Panel A, which are for my panel regressions (using Equation 2.3). Panel B shows the results for the DiD regressions (using Equation 2.4). The results in both panels are consistent with each other and support my hypothesis that analysts tend to herd more after they move to a new job.

To provide some economic significance to these results, while focusing my attention on the DiD regressions of Panel B, I find that the coefficient for the interaction term (*Treat* x *Post)* is -0.2830 in Column (1). As this value is from a logistic regression, I estimate the marginal effect of this impact on *Bold* forecasts when all other variables are calibrated to their mean values. Doing so reveals that the probability of analysts issuing a bold forecast after changing jobs reduces from 70.7% to 64.5% (a proportional decline of 8.7%).[7]

In Column (2), that focuses on *Speed*, the coefficient for the interaction term is -8.7653. Given that the mean value of *Speed* for my treatment sample prior to the job change is 54.6539 (see Table 2.1), it implies that analyst timeliness, on average, declines by 16% after they experience a job change. Also, as *Speed* now drops below the value of 50, these analysts are effectively slower in posting their forecasts than the majority of analysts following the same stock. To provide further evidence of this I re-run my regression using a simple binary measure to capture the probability of an analyst being in the slowest third of analysts to post forecasts. I find that the probability of analysts who have recently changed jobs being in the slowest third jumps from 25% to 45% after their job change.

[7] The calculation is based on the average value of *Bold* for my treatment sample prior to the job change (70.7% - see Table 2.1).

Finally, the number of forecast revisions for a stock, measured by *Frequency*, also reduces. Column (3) shows it decreases by 0.27 revisions, synonymous with an 8.2% decline.[8] I attribute this decline to the fact that analysts who have changed jobs are more likely to post their first forecasts after the majority of other analysts have, removing the need to update their revisions in the light of other forecasts.[9]

Taken together, these results indicate that analysts reduce their forecast boldness, timeliness and frequency, after they change their job. In other words, they show greater herding behavior for the year proceeding a job change.[10]

Panel C of Table 2.2 reports regression results when I examine aggregated herding behavior at the analyst level. The above DiD regressions only examine forecasts for stocks that appear in an analyst's portfolio both before and after an employment change. This means any stocks that the analyst drops after the job change, and new stocks that they are assigned by the new firm, are not accounted for. To address this issue, I aggregate and average each herding measure across the forecasts of all stocks in the analyst portfolio and re-run my regressions at the analyst level.[11] I utilize Equation (2.4) for my regressions but now must exclude stock-level control variables. The results align with my previous findings although the coefficient for the interaction term in Column (3) is no longer statistically significant. Overall, these results allow me to conclude that analysts show, on average, stronger herding behavior across the stocks in their portfolio after an employment change.

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[8] This is when compared with the average number of forecast revisions for my treatment sample prior to the job change (3.3 revisions).
[9] Providing support for this explanation I find that the probability of analysts, who have changed jobs, making a revision is higher than average if their forecasts are within the first third of posted forecasts. It is statistically significant at the 1% level, but not economically significant as the probability increases only by 0.5%.
[10] My tabulated results are based on using robust standard errors, but also hold if I cluster them by analyst.
[11] My results remain qualitatively the same if I aggregate by median values.

Table 2.2: The impact of employment change on analyst herding behavior.

Panel A: Basic regressions

VARIABLES	(1) $Bold_{ij}$	(2) $Speed_{ij}$	(3) $Frequency_j$
$Move_{ij}$	-0.1098***	-8.4628***	-0.3390***
	(0.0116)	(0.1622)	(0.0127)
$Gen\ exper_j$	0.0025	0.3598***	0.0052*
	(0.0023)	(0.0332)	(0.0029)
$SIC\ exper_j$	0.0071***	0.2303***	0.0119***
	(0.0024)	(0.0354)	(0.0031)
$Stock\ exper_j$	0.0097***	2.0132***	0.0814***
	(0.0019)	(0.0290)	(0.0027)
$Stocks_j$	0.0010*	0.1676***	0.0222***
	(0.0006)	(0.0093)	(0.0008)
$Industries_j$	0.0207***	-0.1702***	-0.0363***
	(0.0024)	(0.0369)	(0.0029)
$Size_k$	-0.0004***	0.0367***	0.0028***
	(0.0001)	(0.0011)	(0.0001)
$Coverage_i$	-0.0006	0.0596***	0.0037***
	(0.0006)	(0.0086)	(0.0009)
$Lnsize_i$	-0.0092**	-0.9000***	-0.0582***
	(0.0040)	(0.0608)	(0.0062)
$Sigma_i$	0.0004	0.0313***	0.0011***
	(0.0003)	(0.0040)	(0.0003)
$Retann_i$	0.0004***	-0.0018	-0.0003**
	(0.0001)	(0.0016)	(0.0001)
$Lnbm_i$	-0.0276***	0.0462	0.0275***
	(0.0058)	(0.0854)	(0.0074)
ROE_i	0.0004	0.0013	0.0002
	(0.0008)	(0.0140)	(0.0013)
Var_ROE_i	-0.0055***	0.0901***	0.0036*
	(0.0013)	(0.0204)	(0.0019)
$Profitability_i$	0.0758**	-0.0820	-0.0055
	(0.0348)	(0.2946)	(0.0227)
$SP500_i$	0.0055	-1.2677***	-0.0983***
	(0.0126)	(0.1860)	(0.0225)
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Regression model	Logistic panel	Panel OLS	Panel OLS
Observations	312,242	312,242	312,242

Table 2.2 (continued)

Panel B: DiD regressions

VARIABLES	(1) *Bold$_{ij}$*	(2) *Speed$_{ij}$*	(3) *Frequency$_{j}$*
Treat	0.3442***	1.2513***	0.0235
	(0.0345)	(0.4621)	(0.0361)
Post	0.1968***	-1.5982***	0.2566***
	(0.0322)	(0.2848)	(0.0334)
Treat×Post	-0.2830***	-8.7653***	-0.2715***
	(0.0461)	(0.5294)	(0.0443)
Control variables	Yes	Yes	Yes
Regression model	Logistic panel	Panel OLS	Panel OLS
Observations	37,692	37,692	37,692

Panel C: DiD regressions at the analyst level

VARIABLES	(1) *Bold$_{ij}$*	(2) *Speed$_{ij}$*	(3) *Frequency$_{j}$*
Treat	0.0093**	2.1884***	-0.3406***
	(0.0038)	(0.3216)	(0.0353)
Post	-0.0586***	-4.4330***	-0.0530***
	(0.0020)	(0.0945)	(0.0074)
Treat×Post	-0.0611***	-9.6278***	0.0125
	(0.0056)	(0.4255)	(0.0413)
Control variables	Yes	Yes	Yes
Regression model	Logistic panel	Panel OLS	Panel OLS
Observations	48,391	48,391	48,391

This table reports regression results testing the impact that a job change has on analyst herding behavior. Panel A reports regression results when utilizing Equation (2.3) and Panel B reports DiD regression results when applying Equation (2.4). Panel C presents DiD regression results at the analyst level. Columns (1) to (3) show the results for each of the three different herding measures being used as the dependent variable. Appendix A provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

2.4.2. Analyses using refined matching techniques

The above results do not account for differences in the characteristics of the analysts between the treatment and control samples. This may be driving my results that analysts who change jobs subsequently tend to herd more. To address this, I apply three different approaches to match treatment forecasts with comparable control forecasts and then rerun my analyses.

First, using the data during the period prior to the analyst job change, I match each of my treatment forecast with a control forecast issued for the same stock by an analyst who does not change jobs yet exhibits similar herding behavior to the treatment analyst. Specifically, I match based on propensity scores estimated from logit regressions where the dependent variable is *Treat* and the covariates include the number of stocks an analyst covers (*Stocks*) and one of the herding measures.[12] The first covariate ensures that the analysts who issue the matched forecasts have similar workload so that they do not differ in their tendency to herd, as analysts with a heavier workload tend to herd more in their forecasts (Clement and Tse, 2005). I then use one of the three herding measures as the second covariate to make sure that the matched forecasts come from analysts with similar herding characteristics. In addition, in order to control for the potential difference in resources a brokerage firm can provide analysts in making forecasts, I require that both the treatment and control forecasts are issued by brokerage firms of similar size. Specifically, I split brokerage firms into terciles based on the number of employees each firm has, and require that the matching of analyst forecasts occurs between analysts from firms with the same tercile ranking.[13] I further require the control forecast to be within 30 days of the treatment forecast of the same stock to mitigate any change to the information environment surrounding the stock that may affect analyst forecasting performance. This whole process yields three separate pairs of treatment and control groups (i.e. one for each herding measure).

[12] I use a standard caliper of 0.1 for matching propensity scores.
[13] I follow the literature and use tercile splits. My results still hold when I use alternative splits.

The results from the matching process is reported in Panel A of Table 2.3, where the summary statistics, as well as difference-in-mean tests, of the variables that are matched are displayed. The results show, as I would want, that there is no statistical difference between any of the covariates used in each the corresponding treatment and control groups. The DiD regression results are presented in Panel B and reveal that all of the coefficients for the interaction terms are significant and hold the right sign. It is noteworthy that these DiD regressions are conducted on a substantially smaller subset of observations in order to meet the strict matching criteria that has been imposed.

Table 2.3: Analyses where the treatment and control samples are matched by stock and analyst herding behavior.

Panel A: Summary statistics of the matching criteria

		Treatment sample			*Control sample*			*p-value for diff. in means test*
		Mean	*Median*	*Stdev*	*Mean*	*Median*	*Stdev*	
Pair 1	$Bold_{ij}$	0.7094	1	0.4542	0.6960	1	0.4601	0.3658
	$Stocks_j$	15.1066	15	6.1287	14.8505	15	6.1343	0.1967
Pair 2	$Speed_{ij}$	56.1491	57.1429	26.3006	55.0778	55.5556	26.6585	0.2596
	$Stocks_j$	15.0376	15	5.9906	14.7949	15	6.0516	0.2618
Pair 3	$Frequency_{ij}$	-0.0321	-0.0385	1.6328	0.0020	0	1.6556	0.6385
	$Stocks_j$	15.0074	15	5.9857	14.7703	15	6.0585	0.3720

Panel B: DiD regression results

	(1)	(2)	(3)
VARIABLES	$Bold_{ij}$	$Speed_{ij}$	$Frequency_{ij}$
Treat	0.2366**	0.0002	-0.0558
	(0.1006)	(1.2107)	(0.0961)
Post	0.1661*	-1.6813**	0.2606**
	(0.0993)	(0.7737)	(0.1078)
Treat×Post	-0.3106**	-12.8889***	-0.3860***
	(0.1363)	(1.4805)	(0.1416)
Control variables	Yes	Yes	Yes
Regression model	Logistic panel	Panel OLS	Panel OLS
Observations	4,809	3,930	2,564

This table reports the results of DiD regressions (Equation 2.4) to test the impact that a job change has on analyst herding behavior when I compare the treatment sample with a matched control sample of forecasts for the same stock and from analysts with similar herding behavior (based on a nearest neighbor match). Columns (1) to (3) show the results for the models utilizing three different herding measures. Appendix A provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

Next, I follow Hong and Kacperczyk (2010) and match treatment and control forecasts on the likelihood that analysts will herd, rather than base it on *ex ante* herding behavior. To enable this, I use several analyst and stock characteristics to match treatment forecasts with suitable control forecasts. Similar to the previous matching strategy, I require that both the treatment and control forecasts are issued by brokerage firms of the same *Size* tercile ranking to control for the resources that analysts are provided. Also, both forecasts must be within 30 days of each other. I then proceed to match additional characteristics based on propensity scores estimated from logit regressions where the dependent variable is *Treat* and the covariates include the years of general experience that each analyst has (*Gen Exper*), the return of the stock being forecasted (*Retann*), the log of the book-to-market ratio of the stock (*Lnbm*), and analyst coverage of the stock (*Coverage*). The results in Panel A of Table 2.4 show that for all the variables I use in my propensity score matching process there are no statistical differences between my treatment and matched control samples. The resulting DiD regressions reported in Panel B of Table 2.4 also show consistent results to those of Tables 2.2 and 2.3.

Table 2.4: Analyses where the treatment and control samples are matched by brokerage firm, analyst and stock characteristics.

Panel A: Summary statistics of the matching criteria

	Treatment sample			*Control sample*			*p-value for diff. in*
	Mean	*Median*	*Stdev*	*Mean*	*Median*	*Stdev*	*means test*
$Retann_i$	13.3534	13.2923	30.0295	14.0283	14.9257	29.4574	0.4164
Gen_exper_j	5.4763	5	3.7837	5.4089	5	3.6621	0.4101
$Coverage_i$	16.3473	15	9.0562	16.0633	15	9.3499	0.1606
$Lnbm_i$	-0.8326	-0.7946	0.7618	-0.8329	-0.8050	0.7431	0.9893
$Quin\_rank_k$	2.9708	3	0.1685	2.9708	3	0.1685	1.0000

Panel B: DiD Regression results

	(1)	(2)	(3)
VARIABLES	$Bold_{ij}$	$Speed_{ij}$	$Frequency_j$
Treat	0.4727***	-0.9732	0.6808***
	(0.0695)	(0.9417)	(0.0690)
Post	0.2671***	-0.9311*	0.4416***
	(0.0646)	(0.5398)	(0.0803)
Treat×Post	-0.2916***	-5.2671***	-0.6973***
	(0.0937)	(1.0525)	(0.1014)
Control variables	Yes	Yes	Yes
Regression model	Logistic panel	Panel OLS	Panel OLS
Observations	9,226	9,226	9,226

This table reports the results of DiD regressions (Equation 2.4) to test the impact that a job change has on analyst herding behavior when I compare the treatment sample with a matched control sample of forecasts using different characteristics of brokerage firms, analysts, and stocks (based on a propensity score match). Columns (1) to (3) show the results for the models utilizing three different herding measures. Appendix A provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

I also use a method where treatment forecasts are paired with a benchmark portfolio constructed from forecasts contained in the control group. The advantage of doing this is that it reduces selection bias that can arise when only one control forecast is used to match with each treatment forecast. I construct benchmark portfolios using a procedure employed by Fama and French (1993) when constructing their size and book-to-market portfolios. Specifically, I sort all forecasts within each year into terciles according to the size of the brokerage firm that issues the forecast (*Size*). Then, I repeat the sorting process using *Gen Exper*, *Retann*, *Lnbm*, and *Coverage*. All forecasts belonging to the same tercile for all the sorting criteria forms that benchmark portfolio. This process results in 243 (or 3^5) benchmark portfolios for each year. I then proceed to match each of the treatment forecasts with one benchmark portfolio that the treatment forecast belongs to.

Using the benchmark specification, I construct the benchmark-adjusted DiD estimation for my variables of interest using the following equation:

$$BDiD_{ij} = \left(T_{post} - T_{pre}\right) - (\overline{BC}_{post} - \overline{BC}_{pre}) \tag{2.5}$$

where the first component $\left(T_{post} - T_{pre}\right)$ is the difference in the herding measure of forecasts for stock *i* issued by analyst *j* in my treatment sample before and after a job change; and the second component $(\overline{BC}_{post} - \overline{BC}_{pre})$ is the difference in the average value of the herding measure for the corresponding benchmark portfolio.

I perform univariate tests for the significance of the benchmark-adjusted DiD estimation of each herding measure and report the results in Table 2.5. The results are, again, consistent with my earlier findings, although the magnitude of the impact on herding is larger than my baseline regression results. This is due to the change in herding behavior among my control sample being deflated by taking averages across my benchmark portfolios.

Table 2.5: Analyses where the treatment forecasts are matched with a benchmark portfolio of control forecasts by brokerage firm, analyst and stock characteristics.

	Number of observations	*BDiD estimation*
$Bold_{ij}$	7817	-0.0581***
$Speed_{ij}$	7817	-9.0282 ***
$Frequency_{ij}$	7817	-0.3589***

This table reports Benchmark DiD (BDiD) univariate test results to test the impact that a job change has on analyst herding behavior when I compare the treatment sample with a matched benchmark portfolio of control forecasts using different characteristics of brokerage firms, analysts, and stocks (based on portfolio sorting technique). Appendix A provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

2.4.3. Market implications

Given that analysts who have recently changed jobs will likely provide less informative forecasts if they are herding more, the market reaction to these forecasts should be less. To test this, I compare the cumulative abnormal returns of stocks around analyst forecast announcement dates based on whether the analyst has recently changed jobs (my treatment group) or not (my control group). To perform the test, the dependent variable in Equation (2.3) becomes the absolute value of the two-day market-adjusted cumulative daily returns, CAR_{ijt}, from the day of, to the day after, the analyst forecast date for stock *i* of forecast period *t*:

$$CAR_{ijt}=\left|\left(Stock\ return_{ijt}\text{-}Market\ return_t\right)+\left(Stock\ return_{ijt+1}\text{-}Market\ return_{t+1}\right)\right| \quad (2.6)$$

In Table 2.6, I show the results from using both value and equally weighted market indices plus results from using the S&P 500 index.[14] I find a significant reduction in *CAR* for forecasts coming from analysts that have recently changed jobs, regardless of the proxies I use for market returns. For example, in Column (1), the results from using the value weighted market index show a reduction of 0.2601% in *CAR*, significant at the one

[14] Stock returns and market returns are obtained from the CRSP database.

percent level. Given that the median value for the two-day *CAR* is 2.45% before an employment change, this reduction is equivalent to a decline of 10.6%. The significant reduction in *CAR* is also present if I exclude from the regression forecasts that overlap with other analyst forecasts plus forecasts that overlap with EPS disclosure events. The two-day *CAR* becomes 2.26%, representing a decline of 7.7%. Overall, my findings are consistent with the previous studies that document weaker market reactions to herding forecasts (Gleason and Lee, 2003; Jegadeesh and Kim, 2009), although in my case it is a result of analysts switching jobs.

Table 2.6: Market reactions to analyst forecasts after an employment change.

VARIABLES	(1) CAR_{ij} *Value weighted index*	(2) CAR_{ij} *Equally weighted index*	(3) CAR_{ij} *S&P500*
Treat	1.8416***	1.8319***	1.8384***
	(0.0580)	(0.0579)	(0.0582)
Post	0.0071	0.0128	0.0060
	(0.0360)	(0.0359)	(0.0362)
Treat×Post	-0.2601***	-0.2536***	-0.2615***
	(0.0778)	(0.0777)	(0.0779)
Control variables	Yes	Yes	Yes
Regression model	Panel OLS	Panel OLS	Panel OLS
Observations	35,827	35,827	35,827

This table reports the results of DiD regressions using Equation (2.6) where the dependent variable is the two-day cumulative abnormal returns surrounding analyst earnings forecasts after an employment change. Columns (1) to (3) show the regression results when I utilize the value weighted market index, equally weighted market index, and the S&P500 index, respectively, as a proxy for market return. Appendix A provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

2.5. Additional analyses

2.5.1. Subsampling

My first subsampling analysis aims to provide evidence that analyst herding behavior following a job change is moderated by the degree of unfamiliarity that analysts encounter in their new job. I examine two subsamples of analyst forecasts with the first containing forecasts issued by analysts who move to a new firm together with a group of at least two other colleagues from the former firm, and the second subsample contains forecasts from those who move alone to the new firm. I expect that analysts who move as a group encounter more familiarity in their new work environment, as at least they will be working with some colleagues that are known to them and so part of their social network is transferred with them to the new firm. This should lead to them exhibiting less herding behavior following their job change. The results reported in Table 2.7 support for this. I find that the coefficients for the interaction term in the regressions focusing on *Speed* and *Frequency* are no longer significant (Columns 2 and 3) for the subsample of forecasts from analysts who move in a group. Chi-squared tests also show that these two coefficients are significantly different from the corresponding coefficients obtained from the subsample of analyst forecasts who move alone (Columns 5 and 6).[15] However, I find that there is no significant difference in the impact of job change on *Bold* between these two subsamples. These results suggest that the benefit of analysts having at least part of their social network transfer with them significantly improves timeliness and the ability to post revisions.

[15] χ^2 statistic = 125.36 (*p*-value=0.00) for the test for the difference in coefficients between Columns (2) and (5). χ^2 statistic = 20.29 (*p*-value=0.00) for the test between Columns (3) and (6).

Table 2.7: Subsample analyses based on whether the analysts change jobs alone or together with a group.

	Move as a group			*Move individually*		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	$Bold_{ij}$	$Speed_{ij}$	$Frequency_{ij}$	$Bold_{ij}$	$Speed_{ij}$	$Frequency_{ij}$
Treat	0.2979***	-0.1037	0.3160***	0.3419***	1.9134***	-0.0881**
	(0.0564)	(0.7309)	(0.0562)	(0.0373)	(0.4928)	(0.0388)
Post	0.1839***	-1.8416***	0.1937***	0.1931***	-1.5787***	0.2555***
	(0.0332)	(0.2800)	(0.0340)	(0.0324)	(0.2813)	(0.0335)
Treat×Post	-0.3353***	0.0778	0.0287	-0.2546***	-11.5572***	-0.3377***
	(0.0763)	(0.9294)	(0.0667)	(0.0502)	(0.5841)	(0.0475)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Regression model	Logistic panel	Panel OLS	Panel OLS	Logistic panel	Panel OLS	Panel OLS
Observations	22,692	22,692	22,692	33,061	33,061	33,061

This table reports the results of DiD regressions (Equation 2.4). The first subsample contains forecasts by analysts who have at least two other colleagues moving from the former brokerage firm to the same new firm within the same year. And the second subsample contains forecasts by analysts who are the only one to move to the new brokerage firm. Appendix A provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

Next, I address an argument that my prior results showing the increase in herding behavior after analysts move to a new brokerage firm is driven by a small subset of analysts who experience a significant rise in their career concerns, rather than the results being representative of the whole treatment sample. I address this issue by examining different subsamples of analyst forecasts based on the attributes associated with the analysts and their process of finding a new job.

In Panel A of Table 2.8, I examine earnings forecasts from analysts who belong to the top 30% of most accurate analysts (i.e. analysts with the lowest forecast errors) across the whole industry during the one-year period prior to the job change and those who belong to the bottom 30% (i.e. analysts with the highest forecast errors). I measure forecast errors as the absolute difference between the analyst forecast and the actual EPS, adjusted for the mean forecast errors across all other analysts following the same stock within a fiscal year. It may be that it is the poor analysts, for example, that will herd more after changing jobs. However, the results show that herding behavior after a job change can be found in both subsamples regardless of the analyst's *ex ante* forecast accuracy.

In Panel B of Table 2.8, I test whether all analysts show stronger herding behavior after a job change regardless of their *ex ante* herding level. It may be, for example, that it is analysts who already display herding behavior before they switch jobs that are driving the results. To examine this, I first subsample forecasts from analysts who belong to the top 30% of analysts who show the greatest tendency to issue *Bold* forecasts across the whole industry prior to a job change; and those who belong to the bottom 30%. Interestingly, the tabulated results in Columns (1) and (4) reveal that it is those analysts who previously issued *Bold* forecasts that are more likely to herd following a job change and those who previously had a tendency to herd refrain from it. However, the results from using *Frequency* and *Speed* indicate that herding is present in both subsamples.

Table 2.8: Subsample analyses based on analyst past performance and past herding behavior.

Panel A: Analysts who have higher – low forecast accuracy						
	High accuracy			***Low accuracy***		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	$Bold_{ij}$	$Speed_{ij}$	$Frequency_{ij}$	$Bold_{ij}$	$Speed_{ij}$	$Frequency_{ij}$
Treat	0.3314***	2.0121**	0.0994	0.3227***	1.3984	0.1058
	(0.0683)	(0.9389)	(0.0700)	(0.0657)	(0.8872)	(0.0779)
Post	0.2181***	-0.7591	0.2115***	0.2292***	-1.8456***	0.3544***
	(0.0635)	(0.5592)	(0.0590)	(0.0614)	(0.5628)	(0.0791)
Treat×Post	-0.3302***	-6.3049***	-0.2715***	-0.2171**	-11.2539***	-0.4147***
	(0.0917)	(1.0663)	(0.0869)	(0.0877)	(1.0309)	(0.1002)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Regression model	Logistic panel	Panel OLS	Panel OLS	Logistic panel	Panel OLS	Panel OLS
Observations	9,565	9,565	9,565	10,441	10,441	10,441
Panel B: Analysts who are more likely to issue bold forecasts and those who are less likely to issue bold forecasts						
	More bold			***Less bold***		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	$Bold_{ij}$	$Speed_{ij}$	$Frequency_{ij}$	$Bold_{ij}$	$Speed_{ij}$	$Frequency_{ij}$
Treat	0.6061***	1.5417*	0.0181	0.1008	2.5498**	0.2123**
	(0.0738)	(0.9031)	(0.0587)	(0.0776)	(1.1156)	(0.1026)
Post	0.1074*	-2.1767***	0.2568***	0.2801***	-1.1502*	0.4072***
	(0.0647)	(0.5689)	(0.0529)	(0.0696)	(0.6375)	(0.1054)
Treat×Post	-0.6682***	-11.2792***	-0.1970***	0.1820*	-4.7795***	-0.4377***
	(0.0965)	(1.0434)	(0.0713)	(0.1030)	(1.2053)	(0.1363)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Regression model	Logistic panel	Panel OLS	Panel OLS	Logistic panel	Panel OLS	Panel OLS
Observations	10,018	10,018	10,018	6,957	6,957	6,957

This table reports the results of DiD regressions (Equation 2.4) to test the impact that a job change has on analyst herding behavior across different subsamples of analyst forecasts. Panel A shows the results for forecasts by analysts who have high forecast accuracy and those who have low accuracy (measured by forecast errors) across the whole industry during the one-year period before an analyst job change. Forecast error is measured as the absolute difference between the analyst forecast and the actual EPS, adjusted for the mean forecast errors across all other analysts following the same stock within a fiscal year. Panel B shows the results for forecasts by analysts who are prone to issue bold forecasts and those who are not during the one-year period before analysts change jobs (measured by Bold). Columns (1) to (3) and (4) to (6) show the results for the models utilizing three different herding measures. Appendix A provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

In Panel A of Table 2.9, I compare analysts who belong to the top 30% in the treatment sample for the time it takes them to find a new job against those who belong to the bottom 30%. It can be that those analysts who take longer to find employment will be more insecure and therefore will herd more. I measure the time it takes an analyst to get a new job as the number of days between them posting their last forecast with their former employer, to the first forecast they make with their new brokerage firm. In Panel B, I compare those who move to a larger firm (with respect to the number of employed analysts) compared to their former employer against those who move to a smaller firm. This is to account for the possibility that those who move to a larger/smaller firm may be under differing degrees of pressure to perform, leading to differences in herding behavior. However, the coefficients of the interaction terms in Panels A and B predominantly remain significant in all the sample splits and hold the expected signs.

Table 2.9: Subsample analyses based on the process and outcome of finding a new job.

Panel A: Analysts who need a shorter time to get a new job and those who need a longer time						
	Short time to find a new job			***Longer time to find a new job***		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	*Bold$_{ij}$*	*Speed$_{ij}$*	*Frequency$_{ij}$*	*Bold$_{ij}$*	*Speed$_{ij}$*	*Frequency$_{ij}$*
Treat	0.2418***	0.3463	0.3308***	0.3913***	1.3542**	-0.4135***
	(0.0547)	(0.7195)	(0.0580)	(0.0517)	(0.6503)	(0.0495)
Post	0.1837***	-1.9321***	0.1862***	0.1815***	-1.6331***	0.2304***
	(0.0329)	(0.2793)	(0.0339)	(0.0332)	(0.2781)	(0.0340)
Treat×Post	-0.2514***	-4.4121***	-0.1530**	-0.4001***	-9.3804***	-0.0213
	(0.0744)	(0.9356)	(0.0670)	(0.0701)	(0.8624)	(0.0580)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Regression model	Logistic panel	Panel OLS	Panel OLS	Logistic panel	Panel OLS	Panel OLS
Observations	23,044	23,044	23,044	23,899	23,899	23,899

Panel B: Analysts who move to a larger firm and those who move to a smaller firm						
	Move to a larger firm			***Move to a smaller firm***		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	*Bold$_{ij}$*	*Speed$_{ij}$*	*Frequency$_{ij}$*	*Bold$_{ij}$*	*Speed$_{ij}$*	*Frequency$_{ij}$*
Treat	0.3665***	-0.2452	0.2850***	0.2977***	2.3066***	-0.1912***
	(0.0653)	(0.8369)	(0.0676)	(0.0551)	(0.7094)	(0.0547)
Post	0.1827***	-1.7113***	0.1841***	0.1822***	-1.6404***	0.2099***
	(0.0334)	(0.2810)	(0.0341)	(0.0334)	(0.2785)	(0.0339)
Treat×Post	-0.2981***	-3.8996***	-0.2821***	-0.1690**	-11.8584***	-0.4628***
	(0.0883)	(1.0610)	(0.0765)	(0.0763)	(0.9413)	(0.0655)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Regression model	Logistic panel	Panel OLS	Panel OLS	Logistic panel	Panel OLS	Panel OLS
Observations	21,489	21,489	21,489	22,953	22,953	22,953

This table reports the results of DiD regressions (Equation 2.4) to test the impact that a job change has on analyst herding behavior across different subsamples of analyst forecasts. Panel A shows the results for forecasts by analysts who take a shorter time to find a new job and those who need a longer time (measured by the number of days from their last forecast for the former broker till their first forecast for the new broker). Panel A shows the results for forecasts by analysts who move to a firm with a higher decile ranking, in terms of firm size, and those who move to a lower ranked firm (measured by *Size*). Columns (1) to (3) and (4) to (6) show the results for the models utilizing three different herding measures. Appendix A provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

Next, I account for the fact that some analysts cover a large portfolio of stocks, leading to their forecasts appearing multiple times in the treatment sample. To ensure the forecasting behavior of these analysts are not driving the whole of my results, I exclude forecasts issued by analysts who belong to the top 10% of my sample in terms of portfolio size and rerun my regressions. The results, reported in Panel A of Table 2.10, remain consistent with my main findings.

I also consider if my results are being influenced by forecasts of stocks where analyst coverage is low. For example, if there are three analysts covering one stock, the consensus is driven by only two analysts (as the studied analyst is excluded). This consensus can be very sensitive to the forecasts of either analyst. Therefore, I rerun my regressions but exclude forecasts for stocks covered by three or less analysts from my sample. The results, reported in Panel B of Table 2.10, continue to be consistent with my prior results.

Finally, I examine the possibility that analysts will have a greater tendency to herd for stocks of large firms as those stocks are associated with less disperse information (Nolte et al., 2014). To account for this, I exclude forecasts for stocks belonging to the top 10% of the largest firms (in terms of total assets) in my sample and rerun the regressions. The results in Panel C of Table 2.10 show that all my main conclusions still hold.

Table 2.10: Further robustness tests for the impact of employment change on analyst herding.

Panel A: DiD regression results after excluding forecasts issued by analysts with large portfolios

VARIABLES	(1) *Bold$_{ij}$*	(2) *Speed$_{ij}$*	(3) *Frequency$_j$*
Treat	0.3442***	1.3251***	0.0298
	(0.0355)	(0.4701)	(0.0365)
Post	0.1968***	-1.6331***	0.2487***
	(0.0324)	(0.2814)	(0.0334)
Treat×Post	-0.2772***	-8.9152***	-0.2753***
	(0.0478)	(0.5486)	(0.0454)
Control variables	Yes	Yes	Yes
Regression model	Logistic panel	Panel OLS	Panel OLS
Observations	35,631	35,631	35,631

Panel B: DiD regression results after excluding forecasts for stocks with low analyst coverage

VARIABLES	(1) *Bold$_{ij}$*	(2) *Speed$_{ij}$*	(3) *Frequency$_j$*
Treat	0.3769***	1.5445***	0.0369
	(0.0349)	(0.4649)	(0.0371)
Post	0.1990***	-1.5805***	0.2622***
	(0.0327)	(0.2898)	(0.0347)
Treat×Post	-0.2805***	-8.8722***	-0.2806***
	(0.0467)	(0.5318)	(0.0457)
Control variables	Yes	Yes	Yes
Regression model	Logistic panel	Panel OLS	Panel OLS
Observations	36,499	36,499	36,499

Table 2.10 (continued)

Panel C: DiD regression results after excluding forecasts for stocks of large firms

VARIABLES	(1) *Bold$_{ij}$*	(2) *Speed$_{ij}$*	(3) *Frequency$_j$*
Treat	0.3234***	1.2748***	0.0187
	(0.0356)	(0.4788)	(0.0367)
Post	0.1947***	-1.5831***	0.2537***
	(0.0322)	(0.2838)	(0.0334)
Treat×Post	-0.2744***	-9.0544***	-0.2681***
	(0.0476)	(0.5522)	(0.0453)
Control variables	Yes	Yes	Yes
Regression model	Logistic panel	Panel OLS	Panel OLS
Observations	35,646	35,646	35,646

This table reports the results of DiD regressions (Equation 2.4) for my further robustness test for the impact that a job change has on analyst herding behavior. Panel A shows the results when I exclude from my treatment sample forecasts issued by analysts who belong to the top 10% in terms of *Stocks*. Panel B shows the results when I exclude forecasts for stocks with *Coverage* less than or equal to three analysts. Panel C reports the results when I exclude forecasts for stocks that belong to the top 10% in terms of *Size*. Columns (1) to (3) show the results for the models utilizing three different herding measures. Appendix A provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

2.5.2. Analyses using Quasi-natural experiments and the Average treatment effect

I adopt brokerage M&As as my quasi-natural experiments and focus on a treatment sample of analysts who change jobs when their brokerage firm undergoes an M&A. According to Wu and Zang (2009), turnover is higher among analysts with high forecasting abilities following brokerage M&As. And as analysts with high forecasting performance are not those that herd (Clement and Tse, 2005), I am reducing in my sample the likelihood that the reason analysts are changing jobs as a result of an M&A is due to their *ex ante* herding behavior.

I collect data on broker M&As between 2005 and 2016 from the SDC Mergers and Acquisition database. I follow the method to identify broker M&As by Wu and Zang (2009) and require that the target's four-digit Standard Industrial Classification (SIC) codes are either 6211 (investment banks and brokerage firms) or 6282 (independent research firms), whereas the acquirers belong to either the two-digit SIC code 60 (commercial banks), 62 (securities firms), or 63 (insurance companies). In addition, I only examine completed M&As of which the acquirers wholly own the targets after the transactions. This is to make sure I am capturing M&As where a restructure of the merged brokerage firm has taken place.

I manually match target and acquirer broker names with brokerage house IDs (name abbreviations) from the Institutional Brokers' Estimate System (I/B/E/S) database. To confirm my match, I also require that the target IDs disappear from the database and a large proportion of analysts from the target firms change their broker IDs to the acquirer IDs after the effective date of the M&A.

To supplement the M&A list from the SDC Mergers and Acquisition database, which only covers listed firms, I also try to identify M&As involving non-listed firms by looking for broker IDs that disappear in the I/B/E/S database during the studied period. For those broker IDs which disappear, I investigate whether a large proportion of their employees move to one new brokerage house. This may indicate there is an M&A between the two firms. I confirm this by manually searching for M&A news in Factiva which matches with the broker IDs. In total, my sample consists of 25 M&As involving 256 analysts who get a new job after the M&A,[16] and their 1,825 forecasts before and 1,825 forecasts after the M&A.

For the DiD regressions, I use a two-year window around the M&A date. However, I include a cooling-off period from six months before to six months after the event to avoid any changes to analyst forecasting behavior caused by M&A news and to account for the fact that the date analysts change their job can happen a few months before

[16] I identify this group of analysts as those who change their broker ID after an M&A, and where the new brokerage ID is not the same as the ID of the merged firm.

or after the M&A effective date. My initial control sample contains all forecasts issued by analysts who are not involved in M&As and do not change their broker ID during the event window. In Panel A of Table 2.11 I show the results from running DiD regressions on this sample. The results remain consistent with my main findings. In Panel B of Table 2.11, I rerun the regressions using a propensity score matched control sample based on the characteristics of the analyst and the stock as I previously did for Table 2.4. Finally, in Panel C of Table 2.11, I adopt the portfolio matching technique to pair one treatment forecast with a comparable portfolio of control forecasts (as in Table 2.5).[17] In all cases, my findings remain robust.

[17] I could not perform matching using analysts with similar herding behavior and following the same stock due to a lack of sufficient observations.

Table 2.11: DiD analyses using an M&A treatment sample.

Panel A: DiD regression results using an M&A treatment sample and an unmatched control sample of forecasts

VARIABLES	(1) *Bold$_{ij}$*	(2) *Speed$_{ij}$*	(3) *Frequency$_{j}$*
Treat	0.1890**	4.0024***	-0.3844***
	(0.0823)	(0.9712)	(0.0979)
Post	-0.0740*	-0.8590*	0.0459
	(0.0403)	(0.4554)	(0.0560)
Treat×Post	-0.2159**	-9.2497***	-0.3942***
	(0.1047)	(1.2455)	(0.1033)
Control variables	Yes	Yes	Yes
Deal FE	Yes	Yes	Yes
Regression model	Logistic panel	Panel OLS	Panel OLS
Observations	15,313	15,313	15,313

Panel B: DiD regression results using an M&A treatment sample and a matched control sample based on the characteristics of brokerage firms, analysts, and stocks

VARIABLES	(1) *Bold$_{ij}$*	(2) *Speed$_{ij}$*	(3) *Frequency$_{j}$*
Treat	0.0667	2.1252*	-0.4387***
	(0.1086)	(1.2833)	(0.1588)
Post	0.0184	1.3914	0.4613*
	(0.1058)	(1.1387)	(0.2492)
Treat×Post	-0.2572*	-11.0740***	-0.7529***
	(0.1466)	(1.6430)	(0.2548)
Control variables	Yes	Yes	Yes
Deal FE	Yes	Yes	Yes
Regression model	Logistic panel	Panel OLS	Panel OLS
Observations	3,690	3,690	3,690

Panel C: DiD analyses using benchmark portfolios of control forecasts based on the characteristics of brokerage firms, analysts, and stocks

	Number of observations	*BDiD estimation*
Bold$_{ij}$	1103	-0.0547***
Speed$_{ij}$	1103	-18.6707***
Frequency$_{ij}$	1103	-0.5644***

This table reports the results of DiD regressions (Equation 2.4) to test the impact that a job change has on analyst herding behavior when I utilize brokerage firm M&A as a quasi-natural experiment. Panel A shows the results when I compare my M&A treatment sample against an unmatched control sample of forecasts. Panel B shows the results when I utilize a matched control sample of forecasts regarding different characteristics of brokerage firms, analysts, and stocks (based on a propensity score match). Panel C shows Benchmark DiD (BDiD) univariate test results when I use a matched benchmark portfolio of control forecasts regarding different characteristics of brokerage firms, analysts, and stocks. Columns (1) to (3) show the results for the models utilizing three different herding measures. Appendix A provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

In Table 2.12, I address the concern that the increase in analysts herding after a job change is due to the M&A itself, and is not necessarily caused by the uncertainty that analysts face in their new job. To do this I employ forecasts issued by analysts who undergo the same M&A but are retained to work in the merged firms as my new control sample and rerun the DiD regressions. This method allows me to account for the unobserved impact of the M&A on analyst herding behavior.

I also attempt to minimize the risk that there still may be other unobservable endogenous factors that lead to analysts changing their jobs after an M&A by utilizing a method of estimating the average treatment effect through a two-stage regression procedure (Wooldridge, 2002 p.614-621). According to Wooldridge (2002), this method is more efficient when applied to binary endogenous variables (such as my treatment effect) compared to the usual instrumental variable regression method. In the first-stage, I regress the treatment effect (*Treat*) on two exogenous covariates plus all other control variables. The first covariate identifies whether the analyst is from the target firm (*Target*) and the second identifies whether the analyst has a direct competitor in the counterpart firm (*Compete*). I adopt these covariates based on the findings of Wu and Zang (2009) that show analyst turnover following an M&A is higher among the target analysts and those having a direct competitor in the counterpart firm. At the same time, both covariates are exogenous since analysts cannot decide whether they belong to the target or the acquirer firm, and whether they have a direct competitor. From the first-stage regression results, I generate a predicted series of the treatment effect (i.e. the average treatment effect). This predicted series is free of endogeneity and is used as the regressor in my DiD models in the second stage, replacing the treatment dummy.

My first-stage regression results show that *Treat* is positively associated with both *Target* and *Compete*, and significant at the 1% level. This further supports my use of the two covariates. The results of the second-stage regressions align with my main findings in terms of the sign and significance of the coefficients of the interaction term, although I document the magnitude of these coefficients are lower than my reported baseline results.

Table 2.12: Analyses utilizing the estimation of the average treatment effect.

VARIABLES	(1) *Treat* *First-stage*	(2) $Bold_{ij}$ *Second-stage*	(3) $Speed_{ij}$ *Second-stage*	(4) $Frequency_{ij}$ *Second-stage*
$Target_{jt}$	2.0965***			
	(0.0630)			
$Compete_{jt}$	1.1979***			
	(0.1028)			
Pr(Treat)		0.0220	0.5662	0.1642***
		(0.0300)	(0.4029)	(0.0310)
Post		-0.2321***	-1.8220**	-0.1162**
		(0.0717)	(0.8515)	(0.0515)
Pr(Treat)×Post		-0.0441*	-1.9874***	-0.1177***
		(0.0265)	(0.3239)	(0.0199)
Control variables	Yes	Yes	Yes	Yes
Deal FE	Yes	Yes	Yes	Yes
Regression model	Logistic panel	Logistic panel	Panel OLS	Panel OLS
Observations	10,152	10,152	10,152	10,152

This table reports the results of two-stage regressions to test the impact that a job change has on analyst herding behavior when I compare an M&A treatment sample against a control sample of forecasts issued by analysts who are retained to work in the merged firm after an M&A. Column (1) shows the first-stage regression to estimate the average treatment effect. Columns (2) to (4) are second-stage regressions showing the results of DiD regressions (Equation 2.4) utilizing three different herding measures. Appendix A provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

2.6. Conclusion

This study examines how an employment change affects analyst herding behavior. I find that analysts who switch jobs are significantly less likely to issue bold forecasts, more likely to be slow at posting their forecasts and consequently revise their forecasts less often. Additionally, the observed herding behavior of analysts who have changed jobs also significantly reduces the market impact of their forecasts. Taken together, I show that analyst employment change can be a significant source of herding behavior.

This finding raises an important human resource question in relation to how employers should support analysts who have recently switched jobs. Although I am unable to observe this with my dataset, I expect that where employers facilitate a better transition from one workplace environment to another the tendency for analysts to herd will be reduced. The quicker an analyst familiarizes themselves with their new surroundings, the less likely their performance will suffer. Indeed, I find some evidence to support this in that simply having familiar faces in the new workplace improves the timeliness of their forecasts. Finally, and more generally, my research opens an avenue for future research on the impact that other career events may have on analyst behavior and performance.

3. The heterogeneous impact of work specialization on analyst performance

3.1. Introduction

The paper in this chapter addresses the question: is there a benefit for analysts to specialize in their work? Dating back to Clement (1999) and Jacob et al. (1999), work specialization has been identified as one of the key factors in explaining analysts' forecasting performance. While the aforementioned research finds that analysts benefit when their coverage of stocks is not spread too widely across multiple industries, other research finds no systematic relationship between analyst forecast accuracy and how many industries the stocks that they cover are in (Mikhail et al., 1997; Clement et al., 2007; Kim et al., 2011; Bradley et al., 2017). Rather, they argue other factors, such as the innate ability of superior analysts (Clement et al., 2007) can explain differences in analyst performance.

While the above papers highlight characteristics that explain differences in analyst performance, a separate stream of literature, examining the types of information analysts impound into markets, finds that they play a crucial role in incorporating industry-specific information into stock prices (Piotroski and Roulstone, 2004; Chan and Hameed, 2006). They find that analysts are able to identify the common industry component of each firm-specific news event, which they then utilize to make inferences on other stocks within the same industry. An implication of this being that the more stocks an analyst follows within the same industry, the more opportunity they will have to facilitate the transfer of intra-industry information.

Given the above fact that analysts are an important conduit in disseminating industry-relevant information to the market, and that we should expect superior analysts to do a better job at this due to their innate ability to benefit from task-specific knowledge (Clement et al., 2007), I hypothesize that the benefit of concentrating coverage to a limited number of industries will be pronounced for superior analysts. Conversely, inferior analysts will not be able to benefit from work specialization to the same degree. This heterogeneous impact that work specialization has on analyst performance can also potentially explain the mixed results within the extant literature, as one cohort of analysts (i.e. superior analysts) benefit from specialization while another cohort (i.e. inferior analysts) does not.

My hypothesis requires me to be able to capture how concentrated an analyst's workload is across different industries. While prior studies utilize a count variable to capture industry coverage, I employ a measure of the concentration of stocks each analyst covers across the industries that these stocks are in. I achieve this by applying the Herfindahl-Hirschman Index (*HHI*) to measure how concentrated the stocks that an analyst follows are within a limited number of industries (i.e. work specialization). While a naïve industry count could show, for example, that an analyst's stock coverage crosses over three industries, it can be that all of the individual stocks covered, bar two, are in one of those industries, implying that the analyst's overall workload is still highly specialized to a single industry. My *HHI* accounts for this and will therefore be better able to capture the degree of work specialization there is within an analyst's portfolio.

To test my hypothesis the preliminary analyses consists of generating panel regression results using the full sample of 535,203 analyst earnings forecasts during the period from 2005 to 2016 obtained from the Institutional Brokers' Estimate System (I/B/E/S) database. After this, and to deal with endogeneity concerns, I conduct a quasi-natural experiment following the lead of Hong and Kacperczyk (2010) and use M&As between brokerage firms to capture changes to the work specialization of those analysts who continue to work in the merged firms after an M&A. Using a difference-in-differences (DiD) regression approach, I compare the change in forecasting performance of analysts that have experienced a change in their work specialization through an M&A (my treatment group) with those that have not gone through an M&A (my control group), and then between superior and inferior analysts within the treatment group. In addition, to ensure I have accounted for analyst fixed effects, stock fixed effects and year fixed effects, I repeat the above procedure for treatment forecasts that are then matched with a comparable portfolio of control forecasts, leaving changes in analysts' work specialization due to M&As as the only primary factor that can affect analysts' performance.

Finally, I conduct a number of robustness tests in recognition of the fact that changes to analysts' work specialization caused by brokerage M&As may not be completely exogenous in eliminating all confounding factors that can affect both analysts' work specialization and forecasting performance. My robustness tests include sub-sampling my data based on the findings of Wu and Zang (2009) who examine what type of analysts are more or less likely to remain following an M&A. I also utilize alternative measures for analyst forecasting performance and work specialization, utilizing alternative cut-offs to classify superior and inferior analysts, accounting for forecasts that come from teams of analysts, and performing the analyses when aggregating all variables at the analyst level. In all cases, my conclusions still hold.

The regression results from both the unmatched and matched samples provide similar outcomes. When comparing my M&A treatment sample with the matched control sample I show that an increase of one standard deviation in analyst work specialization

(*HHI*) leads to superior analysts becoming 77% more accurate compared to the average analyst covering the same stock (i.e. the industry consensus).[18] This is a substantial improvement given that, prior to the M&A, an average superior analyst in my sample is 30% more accurate than the industry consensus. In contrast, I find no significant impact of work specialization on inferior analysts. These findings suggest we should no longer treat all analysts the same when assessing how their performance is affected by work specialization.

My study, to the best of my knowledge, is the first to examine the impact that work specialization has on the performance of superior and inferior analysts. By doing so I complement the studies of Clement (1999), Jacob et al. (1999), and Clement et al. (2007) who focus on one specific aspect of analyst work complexity (i.e. workload).

My findings also provide an explanation for the mixed results in the literature studying the average effect of industry concentration on analysts' performance, as I show it is a specific cohort of analysts that benefit from work specialization. By introducing the Herfindahl-Hirschman Index (*HHI*) to measure industry concentration I also provide a more refined measure to capture how specialized the workload of an analyst is within a limited number of industries.

Based on my findings, brokerage firms should consider allocating different types of work to fit with the skill-sets of superior and inferior analysts to effectively enhance their forecasting performance. In particular, I provide evidence supporting the view that superior analysts should specialize, whereas there is no evidence to suggest inferior analysts also benefit from concentrating their coverage to fewer industries.

The remainder of this study is structured into five sections. Section 3.2 is the

[18] A one standard deviation increase in *HHI* in my sample represents a 40.4% increase in the industry concentration of an analyst's portfolio.

hypotheses development. Section 3.3 reports preliminary results when I utilize a complete sample of analyst forecasts during the studied period. In section 3.4 I present my empirical results when restricting my analyses to the M&A sample, and in section 3.5 I provide my robustness tests. Section 3.6 contains my conclusion.

3.2. Hypothesis development

The literature focusing on the aggregate impact of work complexity on analysts' performance provides inconclusive results. For example, Clement (1999) and Jacob et al. (1999) find that the number of industries covered by an analyst is negatively associated with analysts' forecast accuracy. Accordingly, knowing the number of firms and industries followed by an analyst may provide sufficient information to investors to predict economically meaningful differences in analyst forecast accuracy. As indicated by Clement (1999), the ability to identify a small systematic difference in forecast accuracy among the analysts can provide significant benefits to large institutional investors.

While research continues to find other factors that can explain analyst performance, including the advantage of being a local analyst (Bae, Stulz and Tan, 2008) and the type of work experience analysts have before joining the brokerage industry (Bradley et al., 2017), further evidence of the impact that industry coverage has on performance is weak. Specifically, Mikhail et al. (1997) find little support for the positive relationship between forecast accuracy and industry concentration. Also, Kim et al. (2011) show that there is no relationship after controlling for the timing of analyst forecasts and Bolliger (2004) finds no evidence that the relationship holds for European analysts. Additionally, Clement et al. (2007) find that the impact of the number of covered industries has on analysts' forecast accuracy disappears after controlling for analysts' innate ability.

Separate to the above literature, there is research that suggests analysts provide more industry/market wide information, as opposed to firm-specific information, to both the domestic US market (Piotroski and Roulstone, 2004) and international markets (Chan and Hameed, 2006; Fernandes and Ferreira, 2008; Kim and Shi, 2012). Analysts seem to be able to extract core industry information from public news events to make meaningful inferences about other firms' future earnings within the same industry. This indicates that the primary information production activity of analysts facilitates intra-industry information transfer.

Complimentary to the above findings, Clement et al. (2007) find that analysts with high innate ability can apply task-specific knowledge to improve their current forecasting performance, whereas analysts with low innate ability cannot. I expect this is also true when applied to utilizing intra-industry information to price different stocks in the same sector. This leads me to conjecture that superior analysts will be better able to take advantage of tracking stocks within the same industry. I therefore hypothesize that if there is an increase in the level of work specialization (a.k.a. industry concentration), it is the superior analysts who should experience an improvement in their forecasting accuracy:

H3.1: An increase in the level of work specialization leads to a positive impact on the performance of superior analysts

3.3. Analyses based on all analyst forecasts

3.3.1. Data and methodology

I collect data on annual earnings per share forecasts from analysts between 2005 and 2016 from the Institutional Brokers' Estimate System (I/B/E/S) database. My period of analysis starts from 2005 so that I only examine analyst forecasts after Global Settlement was introduced, which can potentially affect analyst forecasting behavior.[19] Also, I limit my analyses to one-year ahead annual EPS forecasts. In addition, to avoid the effects of reiteration, I follow Hong and Kacperczyk (2010) to only use the most recent analyst forecast for each stock in their tracking portfolio prior to the end of a forecast period. This leads to a sample of 535,203 forecasts.

For my econometric model, I follow Clement (1999) and Bradley et al. (2017) to use the proportional mean absolute forecast error ($PMAFE_{i,j,t}$) to capture analyst performance. Specifically:

$$PMAFE_{i,j,t} = \frac{AFE_{i,j,t} - \overline{AFE_{i,t}}}{\overline{AFE_{i,t}}} \tag{3.1}$$

where $AFE_{i,j,t}$ is the absolute forecast error for analyst j's forecast for stock i within forecast period t. $\overline{AFE_{i,t}}$ is the mean absolute forecast error across all analysts issuing forecasts for stock i in forecast period t. To ensure that the estimation of $\overline{AFE_{i,t}}$ is meaningful, I require that there are at least three analysts covering stock i to construct this variable. A negative value of PMAFE suggests the forecast is more accurate than the firm average, whereas a positive value of PMAFE suggests the opposite.[20] This results in my

[19] This is an enforcement agreement reached in 2003 that requires the physical and operational separation between the investment banking and research departments of brokerage firms to mitigate the potential of biased forecasts for investment banking clients.

[20] I follow Hong and Kacperczyk (2010) to winsorize this variable by 2.5% in each tail to address the outlier issue caused by I/B/E/S coding errors.

reduced sample of 467,588 forecasts from 47,726 analyst-year observations (3,826 firm-year observations).

As for my main independent variable of interest, I employ the Herfindahl-Hirschman Index ($HHI_{i,j,t}$) to capture analyst work specialization. It is calculated as:

$$HHI_{i,j,t} = \sum_{k=1}^{n} S_k^{\,2} \tag{3.2}$$

where n is the number of industries (identified by two-digit SIC codes) that analyst j covers, and S_k is the proportion of stocks in analyst j's portfolio that belong to industry k. The Herfindahl-Hirschman Index was originally used as a measure of market concentration to capture whether market share is concentrated within a small number of firms within one industry (Hirschman, 1945; Herfindahl, 1950). Other uses of the index include measuring competition in elections (Stigler, 1972), inequality of income (Owen, Ryan and Weatherston, 2007), the level of industry specialization within a firm (Gompers, Kovner and Lerner, 2009), individual task specialization (Narayanan, Balasubramanian and Swaminathan, 2009), and attention diversification (Boydstun, Bevan and Thomas, 2014). I believe *HHI* is also a suitable measure to capture analyst work specialization. For example, consider two analysts covering the same number of industries, but one has a large proportion of stocks in their portfolio belonging to one industry whereas the other has an equal stock allocation across industries. Obviously, the level of work specialization of the first analyst will be higher than the second analyst, which cannot be captured if I simply look at the number of industries they cover. However, since *HHI* accounts for both the number of industries assigned to the analyst and the proportion of stocks in the analyst portfolio that belongs to each industry, it can efficiently capture the differing levels of specialization between these two analysts.

I also employ two 'ability' dummies to classify analysts into superior and inferior analysts. I define $Superior_{j,t}$ to be equal to one if analyst *j* is ranked within the top 20% of all analysts within the brokerage industry in year *t*, and zero otherwise. I also adopt $Inferior_{j,t}$, which equals to one if analyst *j* is ranked within the bottom 20% during year *t*, and zero otherwise. For each analyst, I calculate an average value for *PMAFE* across all stocks in their portfolio and use this as the ranking criteria.

With regards to my control variables, I utilize a number of variables that can affect analyst forecasting performance based on the prior literature. These include brokerage firm size ($Size_{j,t}$) to control for analyst resources and $Experience_{j,t}$ to control for analyst years of general experience (Clement, 1999). I also control for analyst workload measured by the number of stocks the analyst covers in year *t* ($Workload_{j,t}$), the number of stocks that are new to the analyst portfolio in year *t* ($New\ stocks_{j,t}$), and whether the stock belongs to the S&P500 index in year *t* ($SP500_{i,t}$). These three variables account for the complexity of an analyst's tracking portfolio with respect to the analyst total workload, the difficulty experienced when forecasting new stocks, and the availability of stock information, respectively.[21] Next, I control for the number of days from when the analyst makes a forecast until the end of the forecast period ($Horizon_{i,j,t}$). I use this measure to account for the fact that the closer a forecast is to the forecast period end date, the more information is available to analysts to base the forecast on (Kim et al., 2011). Finally, based on the work of Kim et al. (2011), I account for the number of forecast revisions an analyst issues for a stock within a year ($Revisions_{i,j,t}$). Appendix B outlines how each of these variables is calculated in detail.

[21] I do not control for the number of industries an analyst covers as it is directly represented in the calculation of *HHI*.

My preliminary regression model to examine the different impact that work specialization has on superior and inferior analysts is:

$$PMAFE = \alpha + \beta_1 HHI + \beta_2 HHI \times Superior + \beta_3 HHI \times Inferior + \beta_4 Superior + \beta_5 Inferior + \gamma' X + \varepsilon \quad (3.3)$$

In this model, my measure of analyst performance (*PMAFE*) is regressed against *HHI* plus its interactions with *Superior* and *Inferior*, and a vector *X*, representing my control variables. The coefficient β_1 represents the impact that *HHI* has on analyst forecast errors. β_2 and β_3 indicate the different impact that *HHI* has on superior and inferior analysts.

3.3.2. Summary statistics and preliminary regression results

Table 3.1 presents the summary statistics plus correlation matrix of all variables across my complete sample of analyst forecasts. The mean forecast error (*PMAFE*) in my sample is 1.1229, with the average forecast being issued 77 days before the financial year end date (*Horizon*) and with 25.25% of forecasts being for S&P500 stocks (*SP500*). The table also shows that, on average, an analyst issuing forecasts in my sample works for a brokerage firm employing 66 analysts (*Size*). Also, of the 10 stocks (*Workload*) that the average analyst covers, 2 are likely to be newly assigned for the year (*New stocks*). The average analyst also issues 3 forecast revisions per stock each year (*Revisions*).

Figure 3.1 graphs the proportion of analysts by their *HHI* value and by the number of industries that they follow.[22] While the largest cluster of analysts, representing 44% of my sample, follow only one industry with an *HHI* = 1, another 34% of analysts cover two

[22] Also, the squared bins that are not shaded are indicative of combinations of *HHI* and *Industry* that are populated by less than 1% of analysts (for a total of 5% of my sample).

to three industries, with their *HHI* varying between 0.3333 and 0.9524. This range also incorporates the mean and median *HHI* of my entire sample (0.7114 and 0.7734, respectively). The remaining 22% of analysts cover a dispersed range of stocks across four or more industries. This latter group have an average *HHI* of 0.4.

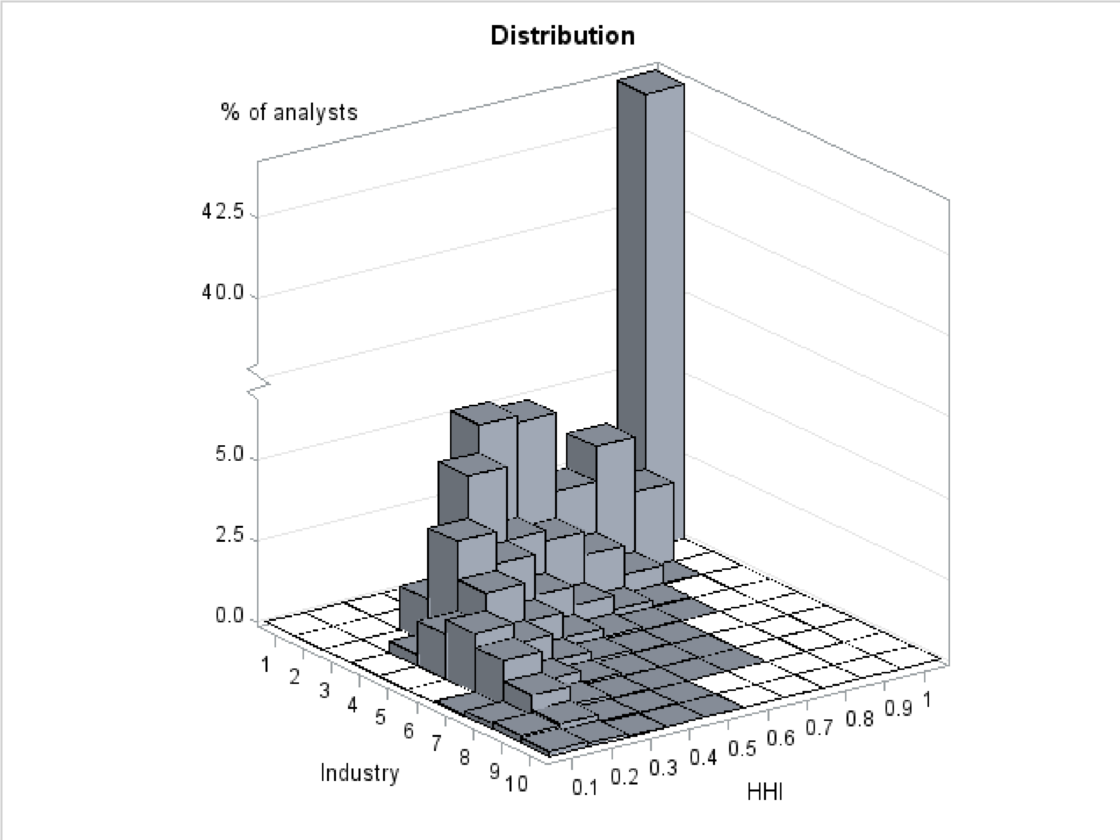
The correlation matrix shows that analyst forecast errors (*PMAFE*) are positively correlated with analyst work specialization (*HHI*). At the same time, *PMAFE* is negatively correlated with analyst *Workload*, the number of *New stocks* in the analyst portfolio, analyst *Experience*, and the number of *Revisions* the analyst issues for the stock being forecasted. *PMAFE* is positively correlated with the dummy variable identifying an *S&P500* stock, and the forecast *Horizon*.

Table 3.1: Summary statistics and correlation matrix for the complete sample of analyst forecasts.

	Summary statistics			***Correlation matrix***								
	Mean	*Med.*	*Std.*	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
(1) PMAFE	1.1229	0.0044	3.2165	1.0000								
(2) HHI	0.7114	0.7734	0.2947	0.0027*	1.0000							
(3) Size	65.7225	38	67.5630	0.0010	0.0256***	1.0000						
(4) Workload	9.8691	8	8.4582	-0.0130***	-0.2091***	0.0692***	1.0000					
(5) New stocks	2.4531	1	3.4724	-0.0035**	-0.1229***	0.0451***	0.5892***	1.0000				
(6) SP500	0.2525	0	0.4344	0.0236***	-0.0245***	0.0787***	0.0558***	0.0061***	1.0000			
(7) Horizon	77.0111	58	85.3517	0.2009***	-0.0005	-0.0502***	-0.0427***	-0.0546***	-0.0246***	1.0000		
(8) Experience	8.4719	5	8.7824	-0.0215***	-0.0933***	0.0322***	0.2742***	0.0771***	0.1074***	-0.0234***	1.0000	
(9) Revisions	3.1874	3	3.6277	-0.0660***	0.0290***	0.0717***	0.0500***	0.0196***	0.0901***	-0.3502***	0.0558***	1.0000

This table reports the summary statistics and correlation matrix of all the variables across my sample containing 467,588 earnings forecasts from 47,726 analyst-year observations (3,826 firm-year observations). Appendix B provides a detailed description of the variables. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

Figure 3.1: The distribution of the number of industries and work specialization of analysts.



This figure shows the distribution of analysts during the period of 2005 to 2016, in terms of the number of industries they cover and the value of the analysts' work specialization, measured by the Herfindahl-Hirschman Index (*HHI*). The value of each variable is divided into deciles, resulting in 100 bins of value. The vertical axis shows the percentage of analysts belonging to each bin. For illustrative purposes, I exclude from the histogram analysts in my sample who cover more than 10 industries. Also, the squared bins that are not shaded are indicative of a combination of *HHI* and *Industry* that are populated by less than 1% of analysts (for a total of 5% of my sample).

My regression results, utilizing Equation (3.3), on the complete sample of analyst forecasts are reported in Table 3.2. Column (1) shows the results when I regress *PMAFE* against my variable of interest *HHI*, the dummy identifying *Superior* analysts, the interaction between *HHI* and *Superior*, and all control variables. In Column (2), I use the same model as in Column (1) but replace *Superior* by *Inferior*. In Column (3), I include both 'ability' dummies, plus their interactions with *HHI* in the model. I include year fixed effects and brokerage firm fixed effects in all three regressions.

In Column (1), the interaction coefficient for *HHI*×*Superior* is -0.3884, significant at a one percent level. This indicates superior analysts tend to show more improvement in forecasting performance compared to other analysts given an increase in work specialization. In contrast, the coefficient for *HHI*×*Inferior* in Column (2) is positive and significant at 0.9177. This means, relative to other analysts, inferior analysts see a decline in their performance when the level of work specialization increases.

Focusing on Column (3), I find that the sum of the coefficients for *HHI* and *HHI*×*Superior* is -0.2455, significant at the one percent level.[23] This means, given an increase of one standard deviation in *HHI* (i.e. 0.2936 – see Table 3.1), the *PMAFE* of a superior analyst will reduce by 0.07. When comparing this with the average *PMAFE* of -0.2671 across forecasts by superior analysts, this suggests a 26% improvement in their performance. In contrast, I find that *HHI* has an adverse impact on the performance of inferior analysts. The sum of the coefficients for *HHI* and *HHI*×*Inferior* is 0.6771, significant at a one percent level (F-stat = 150.87). This means when an inferior analyst experiences a one standard deviation increase in *HHI*, analyst *PMAFE* will increase by 0.20. Given that the average *PMAFE* across forecasts by inferior analysts is 2.7521, this suggests a 7% decline in their performance. Overall, the results in Table 3.2 support my hypothesis that work specialization has a greater, positive impact on the performance of superior analysts relative to inferior analysts.

[23] The F-test statistic for the significance of the sum of the estimated coefficients for *HHI* (-0.1207) and the interaction of *HHI* with *Superior* (-0.1248) is equal to 201.22.

Table 3.2: Regression results using the full sample of analyst forecasts.

VARIABLES	(1) *PMAFE*	(2) *PMAFE*	(3) *PMAFE*
HHI	0.1208***	-0.2106***	-0.1207***
	(0.0211)	(0.0169)	(0.0185)
Superior	-1.0895***		-0.9154***
	(0.0176)		(0.0160)
HHI×Superior	-0.3884***		-0.1248***
	(0.0257)		(0.0226)
Inferior		1.1748***	1.1325***
		(0.0376)	(0.0378)
HHI×Inferior		0.9177***	0.7978***
		(0.0564)	(0.0566)
Size	-0.0004	-0.0006**	-0.0007**
	(0.0003)	(0.0003)	(0.0003)
Workload	-0.0065***	-0.0038***	-0.0078***
	(0.0008)	(0.0008)	(0.0008)
New stocks	0.0039***	0.0057***	0.0047***
	(0.0013)	(0.0013)	(0.0013)
SP500	0.1374***	0.1104***	0.1108***
	(0.0125)	(0.0121)	(0.0120)
Horizon	0.0081***	0.0072***	0.0069***
	(0.0001)	(0.0001)	(0.0001)
Experience	-0.0024***	-0.0012**	-0.0020***
	(0.0006)	(0.0006)	(0.0006)
Revisions	0.0055***	0.0052***	0.0032*
	(0.0019)	(0.0019)	(0.0019)
Year FE	Yes	Yes	Yes
Broker FE	Yes	Yes	Yes
Observations	467,588	467,588	467,588

This table reports the results of panel regressions (Equation 3.3) on the complete sample of analyst forecasts during the studied period. Appendix B provides a detailed description of the variables. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

3.4. Analyses using brokerage M&As as a quasi-natural experiment

3.4.1. Data and methodology

One issue with the above panel regressions is that they may suffer from endogeneity problems. One can argue, for instance, that superior analysts can have more power in negotiating for a higher level of specialization in their work. Therefore, it is uncertain whether work specialization results in a better performance for superior analysts, or the improvement in performance allows analysts to negotiate for more work specialization. This reverse causality problem can lead to an estimation bias.

In an attempt to reduce endogeneity concerns, I next focus on a subset of forecasts issued by analysts who experience a change to their work specialization after their brokerage firm has gone through an M&A. I posit that when the two brokerage firms are merged, there can be substantial changes to the work arrangement among analysts from the two counterpart firms, leading to changes to the level of work specialization for all involved analysts. Since an M&A between two brokerage firms is neither within the control of individual analysts nor easily anticipated by the analysts, it can help remove potential endogeneity problems. I consider some other endogeneity issues later as well.

I collect data on broker M&As between 2005 and 2016 from the SDC Mergers and Acquisition database. Following Wu and Zang (2009), I identify broker M&As by restricting my sample to M&As in which the targets' four-digit Standard Industrial Classification (SIC) codes are either 6211 (including investment banks and brokerage firms) or 6282 (including independent research firms). I also require that the acquirers belong to the three two-digit SIC codes including 60 (commercial banks), 62 (securities firms), and 63 (insurance companies). In addition, I only examine completed M&As of which the targets are 100% owned by the acquirers after the transaction. This is to make sure that the two counterparty firms entirely merged into one entity after the M&As.

I then proceed to manually match target and acquirer names with brokerage house abbreviations (IDs) from the Institutional Brokers' Estimate System (I/B/E/S) Database. This is also the source of my analysts' earnings forecasts. To make sure that the names are correctly matched, I require the targets' IDs to disappear from the database after the M&A effective date. In addition, I require that analysts from the targets change their broker IDs to the acquirers' IDs after the merger. This results in a sample of 21 M&As with 806 retained analysts (approximately 66% of all analysts involved in the M&A). Panel A of Table 3.3 documents my process of M&A sample selection with the number of M&As dropped after each filter.

I follow Hong and Kacperczyk (2010) and use a two-year window around the M&A dates. However, I differ from them by including a cooling-off period from six months before to six months after the event to avoid any changes to analyst forecasting abilities caused by M&A news and to account for the fact that some analysts can depart from the merged firm during this period. To be able to observe the change in the accuracy of forecasts for individual stocks across the event window, I only look at forecasts for stocks that appear in the retained analysts' portfolio both before and after an M&A. Also, I require that the forecasts are issued on the closest date to the cooling-off period. This results in my reduced sample of 585 analysts from 21 M&As, with 5,816 forecasts before and after the M&As.

One potential concern with the above setup is that the merged firms may adjust the level of work specialization for the retained analysts based on their past performance, implying that changes in work specialization is still endogenous to the outcomes of the M&A. However, this is something that I can check. The statistics in Panel B of Table 3.3 show that 274 analysts in my M&A sample experience an increase in *HHI*, with an average increase of 0.0990. This is compared to 283 analysts who see a decline in HHI, with an average reduction of -0.1126. Importantly, my test results show that there is no significant difference between the average forecast error ($\overline{PMAFE}$) among analysts who experience an increase in *HHI* and those who see a decrease in *HHI* (-0.0278 and -0.0398, respectively). This means analysts who see an increase or a decrease in *HHI* following an M&A are equally accurate. In addition, the number of superior analysts, as a proportion of the total number of analysts that experience an increase or a decrease in *HHI* is not significantly different (15.9% and 16.8%, respectively). Likewise, this is also true of inferior analysts (35.7% and 30.1%, respectively). Overall, Panel B shows no evidence that the change to analyst work specialization after an M&A is dependent on analyst prior performance. This is most likely due to the firms not being able to control the substantial shock M&As cause to analyst workload from a combination of the retained analysts having to cover a number of stocks from those analysts that have left the firm (which unlikely would be pre-planned), to needing to follow new stocks, as well as drop those that might already be covered by the counterpart brokerage firm.

Table 3.3: Summary of the M&A sample.

Panel A: Sample selection procedure

Data from CRSP	*Number of M&A*
All M&As between U.S. targets and U.S. acquirers between 1st Jan 2005 and 31st Dec 2016	**109,789**
Less uncompleted M&As	17,489
Less M&As in which targets are not 100% owned by acquirers after transactions	14,108
Less M&As with targets' primary SIC not being 6282 (including investment banks and brokerage firms) and 6211 (including independent research firms)	76,955
Less M&As with acquirers' primary SIC not being 60 (commercial banks), 62 (securities firms), and 63 (insurance companies)	394
Less M&As not matched with the I/B/E/S database	822
Final sample	**21**

Panel B: The decision of firms to change analysts' work specialization (HHI) after M&As

	Increase in HHI	*Decrease in HHI*	*Difference*
Number of analysts	274	283	-9
Average change in HHI post-M&A ($\overline{\Delta HHI}$)	0.0990	-0.1126	0.2117***
Mean forecast errors pre-M&A ($\overline{PMAFE}$)	-0.0278	-0.0398	0.0120
% of analysts as Superior	15.8845	16.7832	-0.8987
% of analysts as Inferior	35.7401	30.0699	5.6701

This table provides a summary of my M&A sample. Panel A describes the sample selection procedure. In Panel B, I report a summary of the change to analyst work specialization after the M&As, then perform tests for the difference in the *ex-ante* performance of analysts who see an increase versus a decrease in work specialization following an M&A. Appendix B provides a detailed description of the variables. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

I adopt a difference-in-differences (DiD) regression approach in which I compare changes to my treatment sample with changes to a control sample of analyst forecasts. My treatment sample includes forecasts issued by analysts involved in the M&As and are retained in the merged entities. My control sample contains all forecasts issued by analysts who are not involved in the M&As. However, I do exclude forecasts issued by analysts who change their broker IDs during the event window from the control sample to make sure that any changes in forecast accuracy observed in the control sample is not due to analysts' job departure. This results in my final control sample of 156,179 earnings forecasts from 24,404 analyst-year observations (1,946 firm-year observations).

For my DiD regression, I estimate DiD for each variable and utilize the DiD estimations as the regressors instead of using the variables themselves as in Equation (3.3). This is done by contrasting the change in the observed variable from a treatment sample (*T*), before (*pre-M&A*) and after (*post-M&A*) an event, with the average change observed in a control sample (*C*):

$$DiD_{i,j}=\left(T_{post\text{-}M\&A} - T_{pre\text{-}M\&A}\right) - \left(C_{post\text{-}M\&A} - C_{pre\text{-}M\&A}\right) \qquad (3.4)$$

In my DiD regression model, all analyst fixed effects and stock fixed effects will be differenced away and will not appear in the model, these also includes the two 'ability' dummies. The variable *Experience* will not appear in this model either since the change in *Experience* (i.e. one year) is the same across all analysts. My final DiD regression model is:

$$DiD.\,PMAFE=\alpha + \beta_1 DiD.HHI + \beta_2 DiD.HHI\times Superior + \beta_3 DiD.HHI\times Inferior + \gamma' DiD.X + \varepsilon \qquad (3.5)$$

In this model, I regress the DiD estimation of analyst performance (*DiD.PMAFE*) against the DiD estimation of work specialization (*DiD.HHI*) plus its interactions with

Superior/Inferior, and a vector *DiD.X*, representing the DiD estimation of my control variables. The coefficients β_2 and β_3 indicate the different impact that a shock to *HHI* has on superior and inferior analysts.

3.4.2. Summary statistics and regression results

Panel A of Table 3.4 reports the summary statistics for all the variables across my treatment and control samples of analyst forecasts. The statistics show that, compared to the control forecasts, my treatment forecasts are more accurate (*PMAFE*), are issued closer to the financial year end date (*Horizon*), and are less likely to cover an S&P500 stock (*SP500*). I also find analysts issuing the treated forecasts have a higher level of work specialization (*HHI*), work for larger brokerage firms (*Size*), cover more stocks (*Workload*), have more new stocks in their portfolio (*New stocks*), and are more experienced (*Experience*).

My DiD regression results, utilizing Equation (3.5), are reported in Panel B of Table 3.4. Column (1) shows my results when I only include the interaction of *DiD.HHI* with *Superior* in the model. In Column (2), I only include the interaction of *DiD.HHI* with *Inferior*. Then in Column (3), I include the interactions of *DiD.HHI* with both 'ability' dummies. I also account for year fixed effects and M&A deal fixed effects in all regressions.

The results in all three regressions are consistent in showing that superior analysts can benefit from work specialization, whereas the impact is not significant for inferior analysts. Focusing on the results in Column (3), I find that the coefficient for *DiD.HHI*×*Superior* is negative and significant at a five percent level, indicating that superior analysts show more improvement than an average analyst when there is an increase in work specialization. The coefficient for *DiD.HHI×Inferior* is, however, not significant. When I consider the total impact of a change to *HHI* on the two groups of analysts, I find that the sum of the coefficients for *DiD.HHI* and *DiD.HHI×Superior* is 1.7659 (*F*-stat=5.78, *p*-value=0.02). This is equivalent to a reduction of 0.5 in the *PMAFE* of a superior analyst when they experience an increase of one standard deviation in *HHI*. In contrast, the total impact is not significant for inferior analysts. Again, these findings further support my hypothesis that superior analysts benefit more from an increase in work specialization compared to inferior analysts.[24]

[24] My main findings still hold when I cluster standard errors by analyst, or analyst and M&A deals, or analyst and year.

Table 3.4: DiD analyses using the M&A sample and an unmatched control sample.

Panel A: Summary statistics of the treatment and control sample of forecasts prior to the M&As

	Treatment sample			***Control sample***			
	Mean	*Median*	*Std.*	*Mean*	*Median*	*Std.*	*Diff. in means*
PMAFE	0.6031	-0.1007	2.4933	0.7527	-0.0698	2.7180	-0.1496***
HHI	0.6723	0.6676	0.2919	0.6457	0.6235	0.2939	0.02676**
Size	112.5426	67	99.7793	56.4441	32	56.9813	56.0985***
Workload	15.3353	16	7.7007	13.3115	13	8.3194	2.0238***
New stocks	3.8765	3	3.9060	3.5533	3	3.8121	0.3232**
SP500	0.2860	0	0.4519	0.2990	0	0.4578	-0.0130**
Horizon	45.4001	56	56.6442	47.7708	56	59.2703	-2.3702***
Experience	13.7044	14	7.9943	12.1897	11	8.7340	1.5147***
Revisions	3.5871	3	2.4811	3.5501	3	4.1788	0.0370

Panel B: Regression results when comparing the M&A sample and an unmatched control sample

	(1)	(2)	(3)
VARIABLES	*DiD.PMAFE*	*DiD.PMAFE*	*DiD.PMAFE*
DiD.HHI	0.3771	-0.0149	0.3806
	(0.4705)	(0.3911)	(0.4442)
DiD.HHI×Superior	-2.1430**		-2.1465**
	(0.8633)		(0.8457)
DiD.HHI×Inferior		0.3864	-0.0227
		(1.8663)	(1.8779)
Control variables	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Deal FE	Yes	Yes	Yes
Observations	5,816	5,816	5,816

This table reports the test results when examining the M&A sample and an unmatched control sample of forecasts. Panel A shows the summary statistics of forecasts in the M&A sample and a control sample during the period prior to the M&A. My M&A sample contains 5,816 forecasts before and after the M&As from 585 analysts in 21 M&As. The control sample contains of 156,179 earnings forecasts from 24,404 analyst-year observations (1,946 firm-year observations). Panel B shows the results of DiD regressions (Equation 3.5) to compare a treatment sample of forecasts issued by analysts who experience an M&A and are retained in the merged firm with an unmatched control sample of forecasts issued by analysts who do not experience an M&A. Robust standard errors are reported in parentheses for all regressions. Appendix B provides a detailed description of the variables. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

One issue with the above treatment and control samples is that they may not share the same characteristics (see Panel A of Table 3.4), which can affect the results of my regressions. To address this issue, I proceed to construct a matched control sample that is comparable to my treatment sample of analyst forecasts. I follow the method used by Hong and Kacperckyk (2010) and match each treatment forecast with one benchmark portfolio of control forecasts based on pre-M&A characteristics. I first rank all forecasts within each event window into terciles according to the average forecast error of analysts who issue the forecasts ($\overline{PMAFE}$). Then, I repeat the ranking process using *HHI*, *Size*, and *Experience*. All forecasts belonging to the same tercile for all the ranking criteria forms one benchmark portfolio. This process results in 81 (3^4) benchmark portfolios for each M&A event. I proceed to match each treatment forecast with one benchmark portfolio that the treatment forecast belongs to.

I then estimate the benchmark DiD for each variable by contrasting the change in the observed variable from a treatment sample (*T*), before (*pre-M&A*) and after (*post-M&A*) an event, with the average change observed in the matched benchmark portfolio of control forecasts (*BC*).

$$BDiD_{i,j}=\left(T_{post\text{-}M\&A}-T_{pre\text{-}M\&A}\right)-\left(BC_{post\text{-}M\&A}-BC_{pre\text{-}M\&A}\right) \tag{3.6}$$

The benchmark DiD estimations (Equation 3.6) of the variables are now used as the regressors in Equation (3.5). The results reported in Table 3.5 are similar to my main findings. I find that a change in *HHI* has a significant total impact of -1.6208 on superior analyst forecast errors (*F*-stat=5.52, *p*-value=0.02). This suggests a reduction of 0.47 in the *PMAFE* of a superior analyst when she has a one standard deviation increase in work specialization.

Table 3.5: Benchmark DiD analyses using the M&A sample and a matched control sample.

VARIABLES	(1) *BDiD.PMAFE*	(2) *BDiD.PMAFE*	(3) *BDiD.PMAFE*
BDiD.HHI	0.4112	-0.0352	0.3201
	(0.4461)	(0.3708)	(0.4223)
BDiD.HHI×Superior	-2.0334**		-1.9409**
	(0.8155)		(0.7990)
BDiD.HHI×Inferior		0.9459	0.5847
		(1.7312)	(1.7422)
Control variables	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Deal FE	Yes	Yes	Yes
Observations	5,816	5,816	5,816

This table reports the test results when examining the M&A sample and a matched control sample. Each treatment forecast is matched with one portfolio of control forecasts issued by analysts having similar *PMAFE, HHI, Size,* and *Experience* characteristics. The results are from DiD regressions (Equation 3.5). Appendix B provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

3.5. Robustness tests

While I find no evidence that the re-assignment of work specialization following an M&A is related to past analyst performance (Panel B of Table 3.3), I nevertheless conduct a robustness test to further limit the impact that past performance can have on determining who is retained. Wu and Zang (2009) examine the characteristics of those analysts who are more/less likely to depart. They find that there are several factors associated with the retention of an analyst that are not related to analyst performance. In particular, analysts from the acquiring firms are more likely to stay in the merged firms. Also, analysts who have no direct competitor have a higher chance to be retained.[25] Based on this, I rerun my analysis on a subset of forecasts that are (i) issued by analysts from the acquirer firms, and (ii) do not have a direct competitor. This group of analysts will

[25] A direct competitor is defined by Wu and Zang (2009) as another analyst in the counterpart firm whose portfolio is at least 50% similar to the studied analyst.

have a higher chance of being retained for reasons unrelated to past performance. The statistics in Panel A of Table 3.6 confirm this. I find that analysts from the acquirer firms are 39.72% more likely to be retained in the merged entity following an M&A. I also find that the probability of retention for analysts who have no direct competitor are 22.03% higher compared to those having at least one competitor. For analysts coming from the acquiring firm that also have no competitor, the chance of being retained is 55.87% higher compared to target analysts having competitors.

Panel B of Table 3.6 shows the results from rerunning my DiD regressions (Equation 3.5) using this subsample of treatment forecasts. The results are consistent with my main findings. For example, in Column (3), the coefficient for *BDiD.HHI×Superior* is negative and significant at a five percent level, suggesting that superior analysts show more improvement than other analysts when their work specialization increases. At the same time, I document the total impact of a change in *HHI* on superior analyst forecast errors is -2.0190 (*F*-stat=5.20, *p*-value=0.02). This is equivalent to a reduction of 0.59 in forecast error given an increase of one standard deviation in work specialization. In contrast, I find that the impact of *HHI* on inferior analyst performance remains insignificant.

Table 3.6: Regression results using a treatment sample of forecasts from analysts who are more likely to be retained in the merged firm.

Panel A: Probability of retention across different analyst groups

	(1) First group		*(2) Second group*		
	Obs.	*Average prob. of retention*	*Obs.*	*Average prob. of retention*	*Diff. in prob.*
(1) From acquirer vs. (2) From target	815	0.9780	231	0.5108	0.3972***
(1) Have no competitor vs. (2) Having at least one competitor	969	0.8369	77	0.6104	0.2203***
(1) From acquirer & having no competitor vs. (2) From target & having at least one competitor	778	0.9087	40	0.3500	0.5587***

Panel B: Regression results using a sample of forecasts from analysts who have a higher probability of retention after an M&A.

VARIABLES	(1) *BDiD.PMAFE*	(2) *BDiD.PMAFE*	(3) *BDiD.PMAFE*
BDiD.HHI	0.4842	0.0645	0.4696
	(0.5154)	(0.4288)	(0.4819)
BDiD.HHI×Superior	-2.5035**		-2.4886**
	(1.0244)		(1.0075)
BDiD.HHI×Inferior		0.4948	0.0841
		(1.8876)	(1.9006)
Control variables	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Deal FE	Yes	Yes	Yes
Observations	4,683	4,683	4,683

This table reports results from a sample of forecasts issued by analysts who are more likely to be retained in the merged firms following an M&A. In Panel A, I examine the probability of retention across different groups of analysts. Panel B reports the results of DiD regressions (Equation 3.5) when examining a treatment sample of forecasts issued by analysts who are from the acquirer firm and have no direct competitor in the target firm compared to a matched control sample. A direct competitor is another analyst whose portfolio is at least 50% similar to the studied analyst. Appendix B provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

One issue with using *PMAFE* as a measure of analyst performance is that the standard deviation for this variable is high. This is potentially caused by low value of $\overline{AFE}_{ijt}$ in the denominator of the equation. Therefore, it is possible that my results are driven by outliers. To address this problem, I employ an alternative measure of analyst forecasting performance (*FA*) as suggested by Hong and Kubik (2003) and Clement and Tse (2005).

$$FA=100 - \left[\frac{Rank - 1}{Number\ of\ analysts - 1}\right] \times 100 \quad (3.7)$$

I first sort all analyst forecasts covering one stock within one forecast period using their *PMAFE* to obtain a *Rank*. The most accurate forecast (lowest *PMAFE*) receives the lowest rank. *Number of analysts* is the number of analysts who issue forecasts for the same stock in one forecast period. *FA* is therefore a measure of forecast accuracy as the more accurate forecast receives a higher value.

I then rerun my DiD regression, utilizing Equation (3.5), with the benchmark DiD estimation of *FA* (*BDiD.FA*) across the event window as my new dependent variable. The results in Panel A of Table 3.7 are consistent with my main results. I find, as I expect, that the coefficient for *BDiD.HHI×Superior* is positive and significant at the five percent level, whereas the coefficient for *BDiD.HHI×Inferior* remains insignificant. The total impact of a change in *HHI* on analyst accuracy (the sum of the coefficient for *BDiD.HHI* and *BDiD.HHI×Superior*) is 18.8481 (*F*-stat=3.94, *p*-value=0.05). Given a one standard deviation increase in *HHI*, this is equivalent to a jump of almost one place in the ranking if I consider that there are, on average, 17 analysts covering one stock. Whereas for inferior analysts, the total impact is not significant.

Next, I utilize an alternative measure for analyst work specialization (*Entropy*) to make sure that my results are not biased by one measurement of my variable of interest:

$$Entropy=-\sum_{k=1}^{n} S_k \times lnS_k \quad (3.8)$$

where *n* is the number of industries (identified by two-digit SIC code) that analyst *j* cover, S_k is the proportion of stocks in the analyst portfolio allocated to industry *k*. *Entropy* is a measure of dispersion, and has been previously used to measure industrial diversification within a firm (Jacquemin and Berry, 1979; Palepu, 1985; Raghunathan, 1995), geographic diversification (Vachani, 1991), and market competition (Horowitz and Horowitz, 1968; Nawrocki and Carter, 2010). Within my study, the higher value for *Entropy* indicates less work specialization. As *HHI* is a nominalized measurement (having values from 0 to 1), it is insensitive to any change near the maximum and minimum values of specialization (Boydstun et al., 2014). The value of *Entropy*, however, moves in a wider range and therefore minimizes this problem. At the same time, the use of *Entropy* allows me to test the impact of work specialization on analyst performance in both directions, when specialization increases or decreases.

I rerun my DiD regression, utilizing Equation (3.5), with the benchmark DiD estimation of *Entropy* (*BDiD.Entropy*) as the variable of interest. The results in Panel B of Table 3.7 show that with an increase in *Entropy*, there is a significant increase in the forecast error of superior analysts, whilst there is no significant impact on the performance of inferior analysts. These results are consistent with my main findings showing that work specialization affects superior, but not inferior, analysts.

Table 3.7: Regression results using alternative measures for the variables of interest.

Panel A: Regression results using an alternative measure for analyst forecast error

VARIABLES	(1) *ΔFA*	(2) *ΔFA*	(3) *ΔFA*
BDiD.HHI	-6.8606	-2.2891	-7.0257
	(4.4110)	(4.3302)	(4.7710)
BDiD.HHI×Superior	25.7062**		25.8738**
	(10.3330)		(10.4860)
BDiD.HHI×Inferior		-3.7552	1.0596
		(11.8196)	(11.9978)
Control variables	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Deal FE	Yes	Yes	Yes
Observations	5,816	5,816	5,816

Panel B: Regression results using an alternative measure for analyst work specialization

VARIABLES	(1) *BDiD.PMAFE*	(2) *BDiD.PMAFE*	(3) *BDiD.PMAFE*
BDiD.Entropy	-0.2851	-0.0243	-0.1771
	(0.2264)	(0.1962)	(0.2194)
BDiD.Entropy×Superior	1.1091***		0.9964**
	(0.4300)		(0.4241)
BDiD.Entropy×Inferior		-0.8303	-0.6733
		(0.8189)	(0.8243)
Control variables	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Deal FE	Yes	Yes	Yes
Observations	5,816	5,816	5,816

This table reports the test results when using alternative measures for the variables of interest, on the M&A sample and a matched control sample. Panel A documents the results of DiD regressions (Equation 3.5) using an alternative measure of analyst forecast accuracy (*FA*). Panel B documents the regressions results when using an alternative measure for analyst work specialization (*Entropy*). Appendix B provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

Another concern is that my results can be biased due to how I classify analysts as being either inferior or superior. To mitigate this concern, I rerun my results using a 30% cut-off, and then a 10% cut-off to identify superior and inferior analysts. The results are reported in Panels A and B of Table 3.8, respectively. My conclusion remains the same regardless of the cut-off I use for my classification. As one would expect, I also notice that the total impact of a change to *HHI* on superior analysts' performance becomes less significant when I use the wider 30% cut-off (the total impact of -0.9547 in Column (3) of Panel B, *F*-stat=3.06, *p*-value=0.08). This is reasonable since I utilize a more relaxed way to classify analysts, leading to the impact of a change in *HHI* on this group of superior analysts to be less pronounced.

Table 3.8: Regression results using different cut-offs to classify superior and inferior analysts.

Panel A: Classification of superior and inferior analysts using a 10% threshold

	(1)	(2)	(3)
VARIABLES	*BDiD.PMAFE*	*BDiD.PMAFE*	*BDiD.PMAFE*
BDiD.HHI	0.2436	-0.0649	0.0907
	(0.4163)	(0.3737)	(0.3960)
BDiD.HHI×Superior10	-2.1882**		-2.0364**
	(0.9705)		(0.9588)
BDiD.HHI×Inferior10		2.0774	1.9258
		(2.4555)	(2.4579)
Control variables	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Deal FE	Yes	Yes	Yes
Observations	5,816	5,816	5,816

Panel B: Classification of superior and inferior analysts using a 30% threshold

	(1)	(2)	(3)
VARIABLES	*BDiD.PMAFE*	*BDiD.PMAFE*	*BDiD.PMAFE*
BDiD.HHI	0.4811	0.0485	0.5862
	(0.5007)	(0.3554)	(0.4606)
BDiD.HHI×Superior30	-1.4343*		-1.5409**
	(0.7393)		(0.7097)
BDiD.HHI×Inferior30		0.1891	-0.3592
		(1.3558)	(1.3887)
Control variables	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Deal FE	Yes	Yes	Yes
Observations	5,816	5,816	5,816

This table reports the test results when using different cut-offs for the classification of superior and inferior analysts, on the M&A sample and a matched control sample. Panel A documents the results of DiD regressions (Equation 3.5) when I use a cut-off of 10% to classify superior and inferior analysts. Panel B documents the regression results when I use a cut-off of 30% to classify analysts. Appendix B provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

In Table 3.9, I perform two additional robustness tests to control for other potential confounding factors that can affect my results. In Panel A, I try to account for forecasts by teams of analysts since I cannot observe the change to the performance of individual analysts in a team. I identify teams of analysts as analyst codes that cover more than 25 stocks, then remove forecasts by those analyst codes from my treatment sample and rerun the regressions.[26] In Panel B, I report the regression results when examining the aggregated forecast error at an analyst level. My main analyses only focus on forecasts for stocks that appear in an analyst portfolio both before and after the M&A. This means I do not account for any stocks that the analyst drops after the M&A, and new stocks that are assigned by the merged firm. To address this issue, I aggregate forecast errors across all stocks in an analyst portfolio to get a forecast error score for each analyst, before and after the M&A. The benchmark DiD estimation of the aggregated forecast error ($BDiD.\overline{PMAFE}$) is now used as the dependent variable for my regressions. I also utilize Equation (3.5) for my regressions but exclude all forecast-level control variables. In both tests, the results align with my main findings.

[26] The results are also robust if I use a cut-off of 20 stocks or 30 stocks.

Table 3.9: Further robustness tests on the impact of work specialization on analyst performance.

Panel A: Regression results when forecasts by teams of analysts are excluded

VARIABLES	(1) *BDiD.PMAFE*	(2) *BDiD.PMAFE*	(3) *BDiD.PMAFE*
BDiD.HHI	0.4228	-0.0396	0.3288
	(0.4685)	(0.3824)	(0.4374)
BDiD.HHI×Superior	-2.0468**		-1.9493**
	(0.8573)		(0.8376)
BDiD.HHI×Inferior		0.9509	0.5804
		(1.7757)	(1.7873)
Control variables	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Deal FE	Yes	Yes	Yes
Observations	5,115	5,115	5,115

Panel B: Regression results when all variables are measured at the analyst-level

VARIABLES	(1) $BDiD.\overline{PMAFE}$	(2) $BDiD.\overline{PMAFE}$	(3) $BDiD.\overline{PMAFE}$
BDiD.HHI	0.5402	0.1202	0.3421
	(0.3519)	(0.3614)	(0.3796)
BDiD.HHI×Superior	-2.2479***		-2.0555**
	(0.8296)		(0.8350)
BDiD.HHI×Inferior		1.4952	1.2701
		(1.0321)	(1.0343)
Control variables	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Deal FE	Yes	Yes	Yes
Observations	585	585	585

Panel A documents the results of DiD regressions (Equation 3.5) when excluding forecasts issued by teams of analysts from the sample. I identify teams of analysts from analyst codes that cover more than 25 stocks in their portfolios. Panel B shows the regression results when all variables are measured at the analyst-level. Appendix B provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

3.6. Conclusion

Using initially a large panel data set and, subsequently, broker M&As as a quasi-natural experiment, I examine the impact of work specialization (how concentrated the stocks an analyst tracks are across industries) on the forecasting performance of superior and inferior analysts. My main findings suggest that the impact that work specialization has on forecast accuracy is significantly different between these two groups of analysts. I find superior analysts can benefit from an increase in specialization in their portfolio, while I do not find evidence of inferior analysts significantly benefiting. My findings are consistent across several robustness tests.

I contribute to the literature on financial analysts by showing that there is a heterogeneous impact of work specialization on analysts. While the prior literature has provided mixed results from examining the average effect that work specialization has on analyst performance, I show that it is necessary to consider how the impact may vary across analysts with differing abilities. Specifically, that it is the superior analysts that can take advantage of concentrating the portfolio of stocks that they track.

4. Job loss and analyst forecast pessimism

4.1. Introduction

The paper in this chapter examines whether the experience of a recent job loss leads to analysts issuing less optimistic forecasts (i.e. forecast optimism) and, rather, more pessimistic recommendations. Research that examines potential bias in analyst forecasts tends to show that they have a predilection to issue optimistic forecasts. In particular, the extant literature has consistently shown that career concerns encourage forecast optimism. Examples of this include the pressure to please investment banking clients (Dugar and Nathan, 1995; Lin and McNichols, 1998; Michaely and Womack, 1999), the desire to have favorable career outcomes (Hong and Kubik, 2003), the incentive to generate trading commissions (Jackson, 2005; Cowen et al., 2006), and the pressure from institutional clients to support their stock positions (Mola and Guidolin, 2009; Firth et al., 2012; Gu, Li and Yang, 2012). There is, however, only limited cases where forecast pessimism has been displayed. Some evidence of this comes from Ke and Yu (2006), Hilary and Hsu (2013) and Horton et al. (2017), who find that analysts try to please firm managers by adjusting their forecasts downward before an earnings announcement date so that firms can more easily beat analysts' latest forecasts.

In this paper, I examine a behavioral reason for expecting analysts to post pessimistic forecasts, and the resulting implication it has on forecast accuracy. Specifically, I examine the performance of analysts who are rehired after losing their prior job as a result of a brokerage firm closure. I expect that the experience of a recent job loss, that is not directly related to their own performance, will lead to analysts issuing less optimistic forecasts and, rather, more pessimistic recommendations when rehired. This assertion is based on evidence originating from the career transition literature that shows the experience of a job loss has a considerable impact on the mental disposition of employees, even in the case where they are immediately rehired and experience no period of unemployment (Latack et al., 1995; Brand, 2015). Feelings of low self-esteem and anxiety are not uncommon, which occur due to the change in their social status, however brief, and the need to find employment elsewhere (Cohn, 1978; Donovan and Oddy, 1982; Pugh et al., 2003; Waters, 2007).

The career transition literature also establishes that a job loss will continue to affect the attitude and self-esteem of employees after they get rehired (Cohn, 1978; Leana and Feldman, 1995; Waters, 2007). I base my hypothesis on these observations and that there also exists a strong relationship between feelings of self-esteem and individuals having a personal disposition towards being optimistic/pessimistic (Mäkikangas et al., 2004; Lyubomirsky et al., 2006). My hypothesis is that financial analysts who have recently experienced a job loss are more likely to have lower self-esteem and a correspondingly more pessimistic outlook of their environment. I expect this will carry through to how they analyze firms and make recommendations. Specifically, they are more likely to issue pessimistic forecasts when re-employed, as their general mental frame of mind will tend to be more negative.

My choice of focusing on analysts who lose their jobs specifically due to a brokerage closure is based on the need to account for endogeneity and selection bias. For example, it is not uncommon for someone to be let go due to their under-performance, and it is this under-performance that may be driving the observation of pessimistic forecasts. In selecting my sample, it is therefore important that I minimize the role that the analyst's specific characteristics contribute to losing their job. To deal with this, I follow Hamilton et al. (1993), Leana and Feldman (1995), and Gowan and Gatewood

(1999) and focus on analysts that lose their jobs due to the employer closing down, as under these conditions the actual behavior of the individual employee is less likely to be directly responsible for their displacement (Brand, 2015). Doing so allows me to conduct quasi-natural experiments where I focus on a treatment group of analysts who experience a brokerage firm closure and subsequently seek employment at another firm. I then study how their tendency to offer either optimistic or pessimistic forecasts changes against differently constructed control groups, to account for other possible explanations and endogeneity concerns that I then also consider.

To capture the potential bias in forecasts, I utilize an established measure that the literature has used to estimate forecast optimism. The measure, as constructed by Cowen et al. (2006), is based on the difference between an analyst's earnings forecast and the average of the most recent forecasts for the same stock made by other analysts, adjusted by the standard deviation of those forecasts. This measure can effectively capture both forecast optimism and pessimism at the same time. For the purpose of this paper, I re-label this measure and call it *Predilection*. While a positive value of *Predilection* indicates that analysts are optimistic in their forecasts, a negative value of *Predilection* indicates forecast pessimism.

My sample period is from 2004 to 2018. I exclude the years before 2004 to mitigate the impact of the Global Analyst Research Settlement (Global Settlement)[27] on analyst forecast optimism. To avoid any confounding effects that can arise from being unemployed for a lengthy period of time, I also limit my attention to analysts that have

[27] This is an enforcement agreement reached in 2003 that requires the physical and operational separation between the investment banking and research departments of brokerage firms to mitigate the potential of biased forecasts being issued for investment banking clients.

experienced a recent job loss and are subsequently rehired within a twelve-month period from when their brokerage firm closes. This implies that I am capturing the impact of a job loss from those analysts that will be least affected by their displacement, relative to those who find it harder to gain re-employment. my final treatment sample contains 13 brokerage firm closures, involving 143 analysts who issued 1,262 forecasts both before and after the respective closure dates.

My baseline multivariate analysis utilizes a difference-in-difference approach (DiD) to compare my treatment group of forecasts from analysts who have recently lost their job with a control group of forecasts from analysts who have not experienced a job loss. To ensure analyst forecasts are not influenced by news of the impending brokerage closure, as well as to account for a 'settling in' period at the new firm when they are rehired, I focus on forecasts outside of a cooling-off period that lasts for six months prior to a firm's closure, and six months after the analyst joins a new firm.[28] I then compare the level of *Predilection* in the treatment group between the last forecast an analyst issues for a stock outside the cooling-off period when working for their former employer and the first forecast that the analyst issues for the same stock in the new brokerage firm after the cooling-off period. The control group is similarly formed from forecasts made during the same periods from analysts that have not experienced a job loss. All forecasts that I examine must also be issued within a twelve-month period either side of the cooling-off periods, although I later consider the impact of examining longer periods in my subsequent analysis.

[28] It is common to utilize a cooling-off period when examining the impact of job loss on employees (see Leana and Feldman, 1995). my results do not qualitatively change if I shorten or lengthen the cooling-off period. I provide some evidence of this in my robustness tests.

My univariate tests document that analysts who have experienced a recent job loss will tend to switch from issuing optimistic forecasts to pessimistic forecasts. *Predilection* changes from being a positive value (0.1295, significant at 5%) for analysts before they lose their job, to a negative value (-0.1533, significant at 1%) for forecasts issued by the displaced analysts when they work for a new firm. my baseline DiD regression results also support this view, with a reduction of more than 100% in forecast optimism among those analysts who have gone through a recent job loss and subsequently get rehired by another brokerage firm. Importantly, I also document that analyst forecast pessimism after a job loss is associated with an increase of up to 54% in analyst forecast error. This implies that forecast pessimism following a job loss can significantly affect the forecasting performance of these analysts.

Although my baseline results are compelling, it is possible that they are being driven by differences between my treatment and control forecasts, including ex ante differences in the level of analyst forecast optimism between the treatment and control groups, plus differences in the characteristics of the stocks that are being followed. To account for this, I proceed to first match each forecast in the treatment sample with a forecast for the same stock from an analyst in the control sample that has a similar level of *Predilection*. Qualitatively, the results from this matching process remain consistent with my baseline results. A similar result is also obtained when I use an alternative matching process and pair treatment and control forecasts using other analyst and stock characteristics. Additionally, the results hold when using alternative measures to capture analyst forecast optimism/pessimism within my DiD regressions.

To add further insight, I examine whether job loss has the same impact on analysts with different personal attributes, how competitive the job market is for analysts and how persistent forecast pessimism is. I first re-run my DiD regressions using subsamples of forecasts issued by analysts who have either high or low forecasting ability, relative to the whole industry. A benefit of conducting this sub-sample analysis is that it also accounts for the possibility that those brokerage firms that close do so because they have a disproportionate number of under-performing analysts. This can influence the prior results. By focusing on analysts with a similar forecasting performance prior to the brokerage closure, I can correct for this potential bias. Secondly, I also split my sample between analysts who have either a limited, or extensive, experience in the brokerage industry. In both cases, my subsample analyses show that the impact of job loss on forecast optimism is more pronounced among analysts who have a higher forecasting ability and/or more years of experience within the brokerage industry. The reason for the latter observation is likely linked to the fact that the experienced analysts will also be older, and research has shown that age is negatively related to how well an individual deals with a job loss (Leana and Feldman, 1990).

My final sub-sample analysis checks to see if my results still hold during a period of increased labor market competition. Peer analysts who do not lose their jobs but see the job market shrink will be inclined to work harder, potentially affecting their forecasting behavior. To check whether this affects my results I split my sample into two periods. The first covers the global financial crisis period and the second captures all other periods. Regardless of the period of examination and the state of the labor market, analysts who have recently experienced a job loss consistently demonstrate a negative *Predilection*.

In addition to the above, in all the sub-sampling analyses I conduct, I show that the effect of issuing pessimistic forecasts is present for the first couple of years of being rehired before dissipating by the third year.

I also consider if there are alternative explanations for the change in analyst *Predilection*, apart from the impact of experiencing a job loss. According to both Bauer et al. (2007) and Saks et al. (2007), individuals can experience unfamiliarity in a new work environment, which in turn can lead to a greater level of uncertainty and subsequently bias forecasts. In addition, the resources available at the new firm may vary, including the amount of support and infrastructure available to the analyst. This can also affect forecasting performance (Clement, 1999). To control for these two items that are related to the new work environment, I re-run my DiD regressions using a control sample of matched forecasts issued during the same period by analysts who job-hop to a new firm with similar resources (measured by the size of the brokerage firm) to show that my baseline results still hold.

Another factor that I consider is whether my results can be driven by feelings of career insecurity which analysts can experience after a job loss. Analyst career concerns can lead to what is termed as an OP pattern where analysts will issue more optimistic (pessimistic) forecasts at the beginning (end) of the fiscal year as it allows for the forecasted stocks to look good, which enhances trading activity and generates more commissions for analysts (Chan et al., 2007; Horton et al., 2017). If my results are due to the experience of a job loss then I should find a decline in the typical OP pattern, as it will suppress the level of optimism exhibited in the first forecast of the fiscal year relative to later forecasts (as the psychological effect of experiencing a job loss dissipates). To test this, I re-sample the data to compare forecasts from my treatment group of analysts that are made at the beginning of the fiscal year with those made at the end of the fiscal year. I use a dummy variable to signify the presence of an OP pattern and run DiD logistic regressions using this measure as my dependent variable. As expected, I find that while the control sample of analysts display an OP pattern, my treatment group show significant evidence of a decline in the OP pattern.

I finish my empirical analyses with some further robustness tests. I re-run my regressions on a subsample which excludes all forecasts for stocks that have less than three analysts following them to make sure that the lack of available information surrounding stocks with low analyst coverage is not affecting my results. I also re-run my regressions on a subsample that excludes forecasts for large firms since analysts have incentives to issue biased forecasts for large firms to boost trading commissions or to win an investment banking client (Horton et al., 2017). Next, I examine a subsample that excludes forecasts issued by analysts with large portfolios. This is to account for the fact that forecasts issued by those analysts appear multiple times in the treatment sample, thereby potentially driving my results. In addition, I also show results generated from aggregating my *Predilection* measure at the analyst level (i.e. I aggregate and average *Predilection* across forecasts of all stocks in the analyst's portfolio). This allows me to not only focus on forecasts for stocks that appear in the analyst portfolio both before and after their job loss, but also consider forecasts for stocks that an analyst drops after a job loss, and any new stocks that they are assigned by their new firm. Finally, I show results for when I extend the cooling-off period to 18 months prior to the brokerage firm closing. By extending the period of time that I examine forecasts before the closure date to one and a half years, I am further minimizing the possibility that my results are contaminated by analysts hearing news of the closure of the firm they work for. In all cases, my main findings remain robust.

My study contributes to the literature on financial analysts by showing that there are factors that can explain the presence of analyst forecast pessimism. While previous studies, including Hong and Kubik (2003), Chan et al. (2007), and Horton et al. (2017), examine analyst career concerns and work incentives as factors that motivate analysts to issue more optimistic forecasts, I find that analysts who have recently experienced a job loss are more inclined to issue pessimistic forecasts when they are rehired. In the same vein as those studies that have investigated the impact that life events have on managerial and investor behavior (Hood et al., 2013; Roussanov and Savor, 2014; Bernile et al., 2017; Shi et al., 2017), I show how financial analysts respond to a life event can also be an important line of study, considering their role as disseminators of financial information to the market.

Second, my study highlights that forecast pessimism can have a significant impact on the accuracy of analyst forecasts in much the same manner that forecast optimism can. I show that analyst forecast pessimism following a job loss is associated with a decline in analyst forecast accuracy. This supplements the findings by both Hong and Kubik (2003) and Cowen et al. (2006), who document that a high level of forecast optimism is negatively associated with analyst forecast accuracy. my results suggest that a reduction in forecast optimism does not necessarily mean an improvement in analyst forecast accuracy if, for example, analysts become too pessimistic. my findings suggest that brokerage firms should adopt policies aimed at supporting newly hired employees who have just gone through a job loss. Such strategies could be instrumental in resolving latent psychological issues that may be affecting their forecasting performance.

My third contribution is that I highlight the value of the career transition literature to help explain the impact of a job loss on the predilection of financial analysts to issue pessimistic forecasts. The career transition literature shows that a job loss can adversely affect the mental health of individuals, and that this can persist even when they are rehired. I show that an individual’s disposition towards optimism is affected by this event, and it is a channel that can partly explain the change in analyst forecasting behavior following a job loss. I also directly contribute to the career transition literature by showing that financial analysts provide a good setting to examine the impact of job loss on performance at the individual level. While the previous studies that focus on the impact of job loss on individuals generally rely on survey data in which individual performance is self-evaluated by the interviewees (Cohn, 1978; Leana and Feldman, 1995; Waters, 2007), I can objectively measure analyst performance by comparing analyst forecasts against actual earnings and/or a consensus forecast.

The remainder of this study is structured as follows. Section 4.2 provides a literature review of the career transition literature plus hypothesis development, while Section 4.3 outlines my data and methodology. In Section 4.4 I present my empirical results, discuss the main findings, and provide additional robustness checks. Section 4.5 contains my conclusion.

4.2. Literature review and hypothesis development

There is a well-established literature that examines how financial analysts tend to provide optimistic forecasts as a result of their career concerns. For example, Dugar and Nathan (1995), Lin and McNichols (1998), and Michaely and Womack (1999) document that underwriter analysts tend to issue more favorable forecasts compared to unaffiliated analysts due to the pressure to please their investment banking clients. Hong and Kubik (2003) find that analysts who issue more optimistic forecasts tend to have better career outcomes as they can promote their firms' underwriting business and generate more trading commissions. Jackson (2005) and Cowen et al. (2006) also show that trading commissions are important factors that explain analyst forecast optimism. Mola and Guidolin (2009), Firth et al. (2012) and Gu et al. (2012) show that analyst recommendations for stocks held by their firm's mutual fund clients are more favorable relative to the consensus. This is due to the pressure to support mutual funds' stock positions in exchange for trading commissions.

The analyst literature does also provide some limited evidence of situations where there is a pessimistic bias in analyst forecasts. For example, Ke and Yu (2006) and Hilary and Hsu (2013) find that analysts adjust their forecasts downward before earnings announcement dates so that firms can more easily beat analysts' latest forecasts. This is to carry favor with firm managers in exchange for private information to enhance analyst forecast accuracy. Horton et al. (2017) also document that banking analysts issue more pessimistic forecasts toward the end of the fiscal year to please banks that could be their future employers, which leads to favorable career outcomes. There is also evidence of analyst forecast pessimism when analysts are faced with an unpleasant condition and/or a traumatic event. For example, Bourveau and Law (2016) document analyst forecast pessimism among those who are affected by Hurricane Katrina. Antoniou et al. (2016) find analysts who locate near a terrorist attack tend to issue more pessimistic forecasts. Dehaan et al. (2017) find evidence that unpleasant weather induces analyst pessimism and delay in response to earnings news. However, the impact of a personal life event on analyst forecast pessimism has yet been investigated.

In this paper I examine how forecast pessimism can arise as a result of a personal experience, in my case, a job loss. Job loss is defined as a life event that occurs when individuals experience an involuntary termination of employment (Latack et al., 1995; Brand, 2015). It ranks in the top quartile, in terms of stress, of impactful life events (Holmes and Rahe, 1967; Paykel et al., 1971). Job loss is different from voluntarily quitting in the sense that it is a career transition that individuals have no control over. While one can lose their job due to performance, it can also occur when individuals are fired or laid off as a result of firms downsizing, restructuring, closing plants, or relocating (Brand, 2015). It is this latter situation that I consider in this paper where employees are laid-off due to no direct fault of their own.

A study by Latack and Dozier (1986) that focuses on managers and professionals, documents that job loss has a considerable impact on displaced employees, even in the case where they are immediately rehired and experience no period of unemployment. Cohn (1978) shows that job loss leads to a decrease in self-satisfaction due to the change individuals experience in their social role. In addition, a job loss entails the need to find new work, which itself can increase stress, even for individuals that are highly employable. Donovan and Oddy (1982) report a rise in depression and anxiety, as well as a decline in self-esteem. Pugh et al. (2003) find that displaced employees tend to also display negative feelings and distrust toward their new employer due to the violation of their psychological contract with their former employer.

Other studies also examine whether the negative consequences of job loss disappear when individuals get rehired. Cohn (1978) finds that individuals continue to suffer from self-dissatisfaction after being re-employed. Leana and Feldman (1995) document some recovery among the re-employed compared to those who remain unemployed following a job loss, however the difference among these two groups is not as strong as they expected. The re-employed report psychological distancing compared to the second group. Likewise, Waters (2007) finds that those who experience a job loss report higher levels of depression even after they get a new job. This can include feelings of pessimism as Mäkikangas et al. (2004) show that both self-esteem and optimism/pessimism are related to the same construct that deals with the ability of people to cope with challenging situations.

Given the above evidence that an experience of a job loss can affect an employee's mental disposition in their new role, I expect that analysts who have suffered from a recent job loss will not only have reduced self-esteem, but will also view the world from a more pessimistic perspective. I expect that this pessimistic attitude will also carry through to how they evaluate firms and provide earnings forecasts, leading to my first hypothesis:

H4.1: Experience of a recent job loss results in analyst forecast pessimism when rehired.

Biased forecasts have performance implications. Specifically, the financial analyst literature examining biased forecasts has shown that they can lead to significant declines in forecast accuracy. In particular, research has shown that forecast optimism leads to a rise in forecast errors (De Bondt and Thaler, 1990; Bulter and Land, 1991; Hong and Kubik, 2003; Cowen et al., 2006). I therefore expect that the forecast pessimism exhibited by those analysts that have experienced a recent job loss will also result in a significant decline in the accuracy of their forecasts. This leads me to my second hypothesis:

H4.2: Forecast pessimism, caused from a recent job loss, reduces analyst forecasting accuracy.

4.3. Data and methodology

4.3.1. Data

I focus on a sample of earnings forecasts issued by analysts who experience a job loss due to the closure of their brokerage firm. This allows me to segregate analyst job loss from other types of analyst job departure. At the same time, this quasi-natural experiment allows me to reduce selection bias in my preliminary sample, since these analysts are laid off when the entire firm closes, regardless of their personal performance and specific characteristics (Brand, 2015).

I collect data on brokerage closures between 2004 and 2016 by looking for broker IDs that disappear in the I/B/E/S database during the period. I then check whether those broker IDs are related specifically to a brokerage closure (as opposed, for example, to a merger) by manually searching for brokerage closure news in Factiva which matches with the broker IDs. As I focus my study on the effect of analysts that experience a recent job loss, I only track analysts who used to work for a closed firm and have then moved to another firm within a 12-month period.[29] For each analyst, I focus on their earnings forecasts for the same stock before and after their job loss to study the change in their forecast *Predilection*. I select only the forecasts that are closest to the closure date (both before and after) while allowing for a six-month cooling-off period on either side of the closure date to avoid the possibility that the forecasts are being influenced by knowledge of the closure event itself and to also allow time for analysts to settle in their new job. As a double-check, I also manually check that there is no news in Factiva to suggest that there was knowledge of an impending brokerage closure outside of the six-month cooling-off period. This whole process results in my final treatment sample consisting of 13 brokerage closures, involving 143 analysts who get a new job after the closure and their 1,262 forecasts before, and 1,262 forecasts after, the closure events.

[29] I identify this group of analysts as those who change their broker ID after a brokerage closure. my results remain the same if I include analysts who find it more difficult to get re-employed after a 12-month period.

4.3.2. Research design

I adopt a difference-in-differences (DiD) regression approach in which I compare changes in my treatment sample with changes in the control sample of analyst forecasts. My initial control sample contains all forecasts issued by analysts who do not experience any job change (i.e. no change in their broker ID) during each closure event window. This results in my control sample containing 97,060 earnings forecasts from 18,210 analyst-year observations.

For my model, $Predilection_{ijt}$ is set as my dependent variable. It is measured as the difference between analyst *j*'s earnings forecast (F_{ijt}) and the average of the most recent forecasts for stock *i* made by other analysts except analyst *j* during the same forecast period (pre-revision consensus - $\overline{F_{ijt}}$); divided by the standard deviation among those forecasts (STD_{it}). I require there are at least three forecasts contributing to the pre-revision consensus. A positive value of *Predilection* indicates that analysts are optimistic in their forecasts, while a negative value of *Predilection* indicates forecast pessimism.

$$Predilection_{ijt}=\frac{F_{ijt}-\overline{F_{ijt}}}{STD_{it}} \tag{4.1}$$

As for my independent variables, I employ $Treat_{ijt}$, a dummy variable that is equal to one if the forecast belongs to my treatment sample and zero if it belongs to the control sample; and $Post_{ijt}$, a dummy variable that is equal to one if the forecast is issued after the brokerage closure and zero if it is before the closure. Following Hong and Kacperczyk (2010) and Horton et al. (2017), I also include a set of control variables that account for brokerage firm size, analyst characteristics, and stock characteristics. Brokerage firm size ($Size_{kt}$) is measured as the number of analysts employed by the firm in a particular year. As for analyst characteristics, I control for the years of analyst general experience ($Gen\ Exper_{jt}$), years of analyst experience following a particular industry ($SIC\ Exper_{jt}$), years of analyst experience following a particular stock (*Stock* $Exper_{jt}$), the number of stocks covered by the analyst (*Tic* $Complex_{jt}$), and the number of industries covered by the analyst (*SIC* $Complex_{jt}$). Stock characteristics that I control for include the number of analysts following the stock ($Coverage_{it}$), log of the total asset value of the firm ($Lnsize_{it}$), stock return ($Retann_{it}$), stock return volatility ($Sigma_{it}$), log of the book to market value of the firm ($Lnbm_{it}$), return on equity of the firm (ROE_{it}), volatility of return on equity using the past ten–year return series (*Var* ROE_{it}), operating income adjusted for asset value of the firm ($Profitability_{it}$), and whether the stock is included in the S&P 500 index ($SP500_{it}$). I obtain data on stock returns and S&P returns from the CRSP database; data on stock fundamentals from the Compustat database; and data on brokerage firms and financial analysts from the I/B/E/S database. Appendix C provides detailed definitions of my control variables.

My regression model to examine analyst forecast optimism after a job loss is:

$$Predilection_{ijt}=\alpha+\beta_1 Treat_{ijt}+\beta_2 Post_{ijt}+\beta_3 Treat_{ijt}\times Post_{ijt}+\gamma' X_{ijkt}+\varepsilon_{ijt} \qquad (4.2)$$

where my dependent variable *Predilection*, is regressed against the $Treat_{ijt}$ and $Post_{ijt}$ dummies, plus their interaction. The coefficient of the interaction term represents the impact that a job loss has on analyst forecast optimism. Vector X_{ijkt} incorporates my control variables. I also include brokerage closure fixed effects.

4.4. Empirical results

4.4.1. Summary statistics

Table 4.1 shows summary statistics of my treatment sample variables for the period before the analysts lose their jobs. Regarding my dependent variable (*Predilection*), there is no significant difference between my treatment and control forecasts. Nonetheless, I document that my treatment and control sample are different in several ways. For example, analysts who issue treatment forecasts are more experienced (*Gen Exper, SIC Exper, Stock Exper*), tend to cover fewer industries (*Industries*), and work for larger firms (*Size*) than those who issue control forecasts. The treatment forecasts are for stocks with more analyst coverage (*Coverage*), larger firm size (*Lnsize*), higher risk (*Sigma, Var ROE*), lower return (*Retann*), and have greater likelihood to be an S&P500 stock (*SP500*) compared to my control sample stocks. These differences can potentially affect my main results and therefore it will be important that I control for them in my tests.

Table 4.1: Summary statistics.

Variables	*Unit of measurement*	***Treatment sample (Forecasts by analysts with a recent job loss)***			***Control sample (Forecasts by analysts with no job loss)***			*Diff. in means*
		Mean	*Median*	*StDev*	*Mean*	*Median*	*StDev*	
Dependent variable								
Predilection	NA	0.1295	0.1912	1.0410	0.0936	0.1449	0.9720	0.0359
Control variables								
Gen exper	Year	20.2448	24	8.1338	12.6314	11	8.9781	7.6134***
SIC exper	Year	10.0233	10	5.3177	6.9573	6	5.3279	3.0659***
Stock exper	Year	8.1714	6	7.8967	4.5331	3	4.8259	3.6382***
Stocks	Stock	17.8299	17	6.9545	17.9340	17	8.7317	-0.1041
Industries	Industry	3.8923	3	1.8912	4.3490	4	2.3707	-0.4568***
Size	Analyst	76.5863	104	40.0608	72.4128	53	63.2670	4.1735*
Coverage	Analyst	20.2521	19	10.5341	19.5295	18	11.4627	0.7226*
Lnsize	NA	8.8173	8.8046	1.7454	8.4966	8.4461	1.9399	0.3207***
Sigma	%	49.5217	43.5470	27.8896	39.7303	35.3631	19.4403	9.7914***
Retann	%	-11.0871	-7.8266	48.7396	11.2392	12.3800	40.5307	-22.3263***
Lnbm	NA	-0.7078	-0.6285	0.9084	-0.7523	-0.7509	0.9134	0.0445
ROE	NA	0.2149	0.2858	1.9669	0.4255	0.2487	5.8987	-0.2106
Var ROE	%	0.3463	0.0122	1.5331	0.4918	0.0153	1.9157	-0.1455**
Profitability	NA	0.0809	0.0786	0.1213	0.0817	0.0809	0.1297	-0.0008
SP500	Dummy	0.4590	0	0.4986	0.3931	0	0.4884	0.0659***

This table presents the summary statistics of my variables for the treatment and control samples during the period before brokerage firm closures occur. Appendix C provides a detailed description of the variables. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

4.4.2. Main findings

I utilize Equation (4.2) to run my preliminary regressions and the results are reported in Table 4.2. Regression 1 in Table 4.2 shows the results when I regress *Predilection* against *Treat*, *Post*, and their interaction. In Regression 2, I include my control variables in the model, and in Regression 3 I also include brokerage closure fixed effects. My results are consistent across all regressions and show a significant reduction in analyst forecast optimism following a job loss. For example, in Regression 3, the coefficient for the interaction term is -0.1320 and significant at the one percent level. This indicates that analysts who recently experience a job loss exhibit a significant reduction in *Predilection* in their forecasts relative to those who do not lose their job during the same event window. On an absolute basis, the change in *Predilection* among my treatment sample after the job loss event is -0.1893, which is the sum of the coefficients for *Post* (-0.0573) and *Treat*×*Post* (-0.1320). When I consider that the average level of *Predilection* for the treatment forecasts before analysts lose their jobs is 0.1295 (see Table 4.1), it indicates that overall *Predilection* has become pessimistic. In other words, these analysts have a *Predilection* to provide optimistic forecasts before they lose their jobs, and after they tend to provide more pessimistic forecasts.[30]

[30] My tabulated results are based on using robust standard errors, but also hold if I cluster them by analyst.

Table 4.2: The impact of job loss on analyst forecast predilection - DiD regression results.

VARIABLES	(1) *Predilection*	(2) *Predilection*	(3) *Predilection*
Treat	0.0361	-0.0153	0.0435
	(0.0365)	(0.0366)	(0.0368)
Post	-0.0753***	-0.0598***	-0.0573***
	(0.0042)	(0.0043)	(0.0043)
Treat×Post	-0.2078***	-0.1359***	-0.1320***
	(0.0477)	(0.0473)	(0.0474)
Gen exper		0.0007*	0.0004
		(0.0003)	(0.0003)
SIC exper		-0.0019***	-0.0009
		(0.0007)	(0.0007)
Stock exper		-0.0002	-0.0004
		(0.0006)	(0.0006)
Stocks		0.0008**	0.0010***
		(0.0003)	(0.0003)
Industries		-0.0017	-0.0010
		(0.0011)	(0.0011)
Size		-0.0001	-0.0001
		(0.0000)	(0.0000)
Coverage		0.0000	-0.0005*
		(0.0003)	(0.0003)
Lnsize		0.0022	0.0075***
		(0.0019)	(0.0019)
Sigma		0.0007***	0.0019***
		(0.0001)	(0.0002)
Retann		-0.0020***	-0.0021***
		(0.0001)	(0.0001)
Lnbm		-0.0012	-0.0067**
		(0.0032)	(0.0032)
ROE		-0.0003	-0.0001
		(0.0004)	(0.0004)
Var ROE		-0.0007**	-0.0010***
		(0.0003)	(0.0003)
Profitability		0.0703***	0.0909***
		(0.0188)	(0.0193)
SP500		0.0136**	0.0138**
		(0.0067)	(0.0067)
Observations	195,679	195,679	195,679
Deal fixed effects	No	No	Yes

This table reports DiD regression results on the treatment sample and an unmatched control sample of earnings forecasts by analysts who do not move to a new job across the event window. The regressions utilize Equation (4.2). Appendix C provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

While the above analysis provides a baseline result, there are two major issues with it. The first is that although I follow analyst forecasts for the same stock both before and after a brokerage closure, I do not match stocks between the treatment and control groups. The stock characteristics themselves may be driving the results. In addition, although Table 4.1 suggests that there is no difference in the overall level of *Predilection* between the treatment and control groups prior to a job loss, differences may still exist once stocks are matched between the two groups. To account for this, I re-run my model after performing a nearest neighbor match to pair each treatment forecast of a stock with one comparable control forecast for the same stock that also has the same level of *Predilection*. In doing so I lose a number of observations as I require that the control forecast *Predilection* must be within one standard deviation from the treatment forecast *Predilection*.[31] The results from Panel A of Table 4.3 shows that my stock-matched treatment and control samples are statistically comparable in terms of their *Predilection* prior to the brokerage closures. I re-run my regressions using Equation (4.2) for my matched samples and report the results in Panel B of Table 4.3. The results are consistent with my preliminary results. Specifically, in Regression 3, the coefficient of the interaction term is -0.1471, significant at the five percent level. This, again, indicates that those analysts that have experienced a recent job loss issue pessimistic forecasts compared to those who have not experienced a recent job loss.

[31] This is the standard deviation of *Predilection* among all forecasts for the same stock within the same forecast period.

Table 4.3: DiD regression results using a matched control sample.

Panel A: Summary statistics of the matching covariate Predilection

	Treatment sample			*Control sample*			*p-value for diff. in means test*
	Mean	*Median*	*Stdev*	*Mean*	*Median*	*Stdev*	
Predilection	0.0869	0.1378	1.0627	0.0602	0.1421	1.0756	0.6029

Panel B: Regression results

VARIABLES	(1) *Predilection*	(2) *Predilection*	(3) *Predilection*
Treat	0.0706	0.0862	0.0929
	(0.0564)	(0.0571)	(0.0585)
Post	-0.1191**	-0.0295	-0.0278
	(0.0516)	(0.0516)	(0.0520)
Treat×Post	-0.1902***	-0.1505**	-0.1471**
	(0.0725)	(0.0713)	(0.0715)
Observations	2,904	2,904	2,904
Control variables	No	Yes	Yes
Deal fixed effects	No	No	Yes

This table reports DiD regression results on the treatment sample and a matched control sample. Specifically, each treatment forecast is matched with a control forecast for the same stock, within the same forecast period, that has the closest level of *Predilection*. Treatment forecasts that cannot be matched within a one standard deviation threshold are discarded from the sample. Panel A shows the summary statistics for the matched covariates in my treatment and matched control samples. In Panel B, I perform DiD regressions utilizing Equation (4.2) to compare my treatment sample against the matched control sample. Appendix C provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

Although the above matching criteria provides closely matched treatment and control samples, it does lead to a substantial decline in the sample size. This can lead to a loss of information from the discarded observations. As an alternative approach, I perform an alternative matching process using an entropy matching technique which allows me to retain all the observations while still having a well-balanced control sample to compare with my treatment sample (Hainmueller, 2012). To perform my entropy match, I follow Hong and Kacperczyk (2010) to match my treatment and control samples based on several brokerage firm, analyst, and stock characteristics that have been linked with analyst forecast optimism. My selected matching covariates include analyst years of experience (*Gen exper*) and the number of stocks in an analyst's portfolio (*Stocks*) to account for analyst characteristics; the number of analysts working in the brokerage firm (*Size*) to account for the size of the brokerage firm; and the number of analysts following the stock (*Coverage*) and log of the firm's total asset value (*Lnsize*) to account for stock characteristics. I then calibrate a unit weight to each control observation, so that the treatment and weighted control samples are comparable in all matching covariates.[32]

Panel A of Table 4.4 shows the summary statistics of the matching covariates in my treatment and weighted control samples where both samples are shown to be statistically comparable in terms of all five covariates. In Panel B of Table 4.4, I show the regression results using Equation (4.2) to compare my treatment sample and the weighted control sample. Consistent with my main findings, the results show a significant decrease in *Predilection* after analysts lose their job. For example, in Regression 3, the coefficient of the interaction term is -0.1227, which is significant at the five percent level. When adding this with the coefficient for *Post* (-0.0552), I get the absolute impact of the job loss on my treatment sample of -0.1779. Compared to the average level of *Predilection* of my treatment sample before a job loss of 0.1295, it suggests *Predilection* declines by more than 100%, leading to a tendency for these analysts to issue pessimistic forecasts.

[32] My results are based on an entropy match that balances the first moment (mean) of the covariates in the two samples. However, the results still hold if I also balance the second moment (variance) and third moment (skewness) of the covariates.

Table 4.4: DiD regression results using a matched control sample using entropy matching.

Panel A. Summary statistics of the matched covariates

	Treatment sample			*Weighted control sample*			*p-value for diff. in means test*
	Mean	*Median*	*Stdev*	*Mean*	*Median*	*Stdev*	
Gen exper	20.2448	24	8.1338	20.2440	5.8015	29.3767	0.9994
Stocks	17.8299	17	6.9545	17.8298	9.6632	18.8417	0.9999
Size	76.5863	104	40.0608	76.5858	32.0942	121.7247	0.9999
Coverage	20.2521	19	10.5341	20.2521	10.3087	25.7550	0.9999
Lnsize	8.8173	8.8046	1.7454	8.8173	4.7504	9.4109	0.9999

Panel B. Regression results

VARIABLES	(1) *Predilection*	(2) *Predilection*	(3) *Predilection*
Treat	0.0332	-0.0218	0.0324
	(0.0366)	(0.0369)	(0.0371)
Post	-0.0794***	-0.0568***	-0.0552***
	(0.0057)	(0.0058)	(0.0058)
Treat×Post	-0.2057***	-0.1254***	-0.1227**
	(0.0485)	(0.0483)	(0.0484)
Observations	195,679	195,679	195,679
Control variables	No	Yes	Yes
Deal fixed effects	No	No	Yes

This table reports DiD regression results on the treatment sample and a weighted control sample constructed using entropy matching. Specifically, I calibrate and set unit weights to my control observations so that the treatment and weighted control samples are comparable across five covariates: *Gen exper, Stocks, Size, Coverage, Lnsize*. Panel A shows the summary statistics for the matched covariates in my treatment and weighted control samples. In Panel B, I perform DiD regressions utilizing Equation (4.2) to compare my treatment sample against the weighted control sample. Appendix C provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

In Table 4.5, I repeat my analysis using Equation (4.2) with alternative measures of analyst forecast optimism to test for the robustness of my baseline results. In Regression 1, I use *Actual Predilection*$_{ijt}$, which is the difference between analyst j's earnings forecast (F_{ijt}) and the actual earnings per share of stock i for the same forecast period (A_{it}), all divided by the standard deviation of the forecasts (STD_{it}):

$$Actual\ Predilection_{ijt}=\frac{F_{ijt}\text{-}\ A_{it}}{STD_{it}} \quad (4.3)$$

This alternative measure allows me to both examine whether analysts are optimistic/pessimistic relative to actual earnings, and at the same time will indicate the analyst deviation from issuing an accurate forecast.

I also test whether the experience of a recent job loss can affect the level of optimism in other types of analyst forecasts. In Regression 2, I examine the impact of job loss on analyst price target forecasts. I follow Cowen et al. (2006) and utilize *Target Predilection*$_{ijt}$ as the dependent variable. It is measured as the difference between analyst j's price target forecast (FP_{ijt}) and the average of the most recent price target forecasts for stock *i* made by other analysts except analyst *j* during the same forecast period (pre-revision consensus - $\overline{FP_{ijt}}$); divided by the standard deviation among those price forecasts ($STDP_{it}$). I also require that there are at least three forecasts contributing to the pre-revision consensus:

$$Target\ Predilection_{ijt} = \frac{FP_{ijt} - \overline{FP_{ijt}}}{STDP_{it}} \qquad (4.4)$$

In Regression 3 of Table 4.5, I examine the impact of job loss on analyst recommendations. I follow Cowen et al. (2006) and utilize *Recommendation*$_{ijt}$ as the dependent variable. *Recommendation* equals 4, 3, 2, 1 and 0 for strong buy, buy, hold, under-perform and sell recommendations, respectively.

Regardless of the measures I use, I still document a significant reduction in analyst optimism. For example, in Regression 1, the coefficient of the interaction term for *Treat* and *Post* is -0.2495, significant at the one percent level. In Regression 2, the coefficient of the interaction term is -0.2460, which is also significant at the one percent level. Finally, in Regression 3, the coefficient of the interaction term is -0.1455, significant at the ten percent level. These results suggest that analysts who recently experience a job loss tend to issue more pessimistic earnings forecasts, more pessimistic price target forecasts, and more negative recommendations compared to those who do not experience any job loss.

Table 4.5: DiD Regressions using alternative measures for analyst forecast predilection.

VARIABLES	(1) *Actual Predilection*	(2) *Target Predilection*	(3) *Recommendation*
Treat	0.0855	0.2082***	0.0348
	(0.0691)	(0.0531)	(0.0835)
Post	0.0633***	-0.1187***	0.2000***
	(0.0099)	(0.0101)	(0.0224)
Treat×Post	-0.2495***	-0.2460***	-0.1455*
	(0.0843)	(0.0536)	(0.0868)
Observations	194,908	29,734	6,792
Control variables	Yes	Yes	Yes
Deal fixed effects	Yes	Yes	Yes

This table reports DiD regression results using alternative measures of analyst forecast predilection. The regressions utilize Equation (4.2). Appendix C provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

4.4.3. Analyst pessimism and forecast accuracy

Previous studies (Walter and Willis, 2009; Hribar and McInnis, 2012) document the impact of investors' collective optimism about stocks on the accuracy of analyst forecasts. Hong and Kubik (2003) and Cowen et al. (2006) also document that a high level of analyst optimism is negatively associated with analyst forecast accuracy. While the evidence in the previous section indicates that analysts become pessimistic in their forecasts following a job loss, I now test whether this forecast pessimism will also lead to a change in analyst forecast accuracy.

I conjecture that, similar to forecast optimism, pessimism in analyst forecasts can lead to a decline in analyst forecast accuracy. To confirm this conjecture, I first perform univariate tests for the change in my measures of predilection before and after a job loss to highlight the magnitude of the change from forecast optimism to pessimism. The results are reported in Panel A of Table 4.6. I find that the level of *Predilection* among the displaced analysts in my sample changes from 0.1295 prior to a job loss to -0.1533 following a job loss, indicative of a switch from forecast optimism to pessimism. In addition, these results also indicate that analyst deviation from the consensus forecast is larger when they become pessimistic following their job loss. I also show the result when using *Actual Predilection*, which compares analyst forecasts against the actual earnings per share announced by the firms. Before the job loss the average value for *Actual Predilection* is 0.2708, which subsequently drops to -0.3118 after the job loss. These results further show that analyst forecasts deviate more from the actual earnings per share when the analysts turn pessimistic after their job loss.

The univariate test results are visualized in Figure 4.1, in which the orange and blue lines represent the change in *Predilection* and *Actual Predilection*, respectively, before and after analysts experience a job loss. I can observe that both lines cross the horizontal axis, implying that analysts turn from being optimistic to being pessimistic following their job loss. In addition, after the job loss, the diversion of both lines from the horizontal axis becomes larger, which means analysts deviate further from the consensus forecast/actual earnings per share.

Next, I test whether the switch from optimism to pessimism in analyst forecasts following a job loss can have a significant impact on forecast accuracy in a multivariate setting. In Panel B of Table 4.6, I utilize Equation (4.2) to run my regressions, however, I use different measures of analyst forecast error as the dependent variable. In Regression 1, my dependent variable is absolute forecast error (FE_{ijt}), measured as the absolute difference between analyst *j*'s earnings forecast and the actual earnings of stock *i* announced by the firm (Hong et al., 2000). In Regression 2, I follow Mikhail et al. (1997) and Hong and Kubik (2003) and use adjusted forecast error ($AdFE_{ijt}$) as the dependent variable. It is calculated as the absolute forecast errors (FE_{ijt}) adjusted by the stock price on the forecast date.

The regression results show that, on average, analysts experience a significant increase in their forecast errors following a job loss. In Regression 1, the coefficient of the interaction term is 8.5480, significant at the one percent level, suggesting that analysts who recently lose their jobs tend to issue less accurate forecasts compared to those who do not leave their brokerage firm across the event window. The absolute impact of job loss on forecast errors is 12.31%.[33] Given that the average value for *FE* among my treatment forecasts prior to analyst job loss is 26.9%, this indicates an increase of 46%, in absolute terms, of the forecast error after analysts lose their jobs. In Regression 2, I document that the coefficient on the interaction term is 0.4484 and the sum of the coefficients for *Post* and *Treat*×*Post* is 0.7611. This indicates an increase of 54% in the adjusted forecast error among displaced analysts given that the average value for *AdFE* prior to job loss is 1.42. These findings suggest that analyst pessimism is associated with a decrease in forecast accuracy.

Overall, I find that both optimism and pessimism indicate a diversion of analyst forecasts from the consensus forecast/actual earnings per share, and the diversion is larger when analysts become pessimistic following a job loss.

[33] This is the sum of the coefficients for *Post* (8.5480) and *Treat*×*Post* (3.7629).

Table 4.6: The impact of a job loss on analyst forecast accuracy.

Panel A: Univariate tests

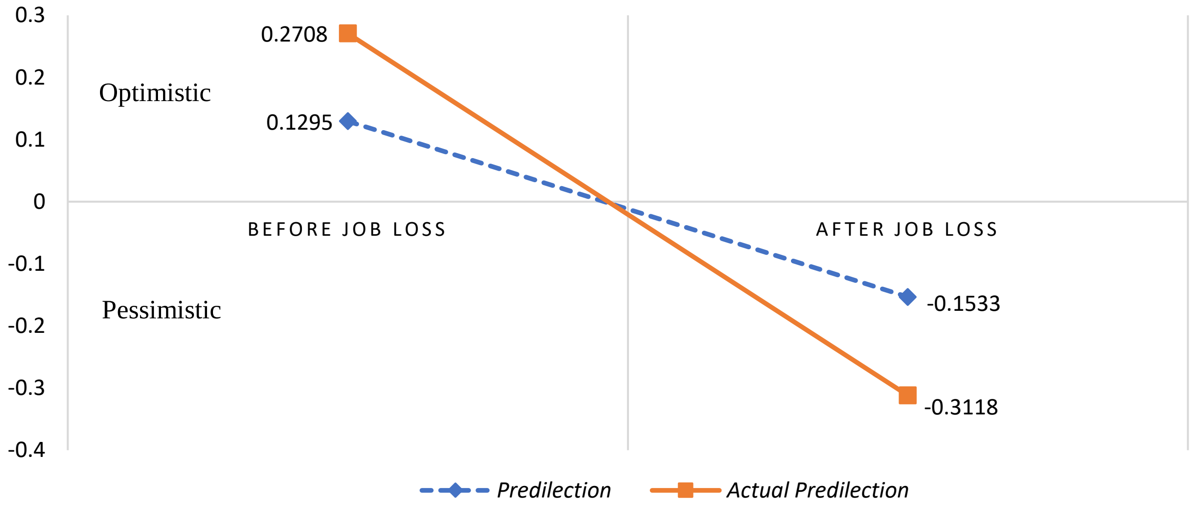
	Before job loss	*After job loss*	*Diff. (After – Before)*
Predilection	0.1295**	-0.1533***	-0.2828***
Actual Predilection	0.2708***	-0.3118***	-0.5827***

Panel B: DiD regressions

	(1)	(2)
VARIABLES	*FE*	*AdFE*
Treat	-4.4935**	-0.1782**
	(1.8811)	(0.0884)
Post	3.7629***	0.3127***
	(0.2237)	(0.0093)
Treat×Post	8.5480***	0.4484***
	(2.7672)	(0.1200)
Observations	189,839	189,839
Control variables	Yes	Yes
Deal fixed effects	Yes	Yes

This table presents the test results for the impact that an experience of a job loss has on analyst forecast accuracy. Panel A shows the results of univariate tests on analyst forecast predilection among my treatment sample, before and after a job loss. Panel B reports DiD regression results, utilizing Equation (4.2), with measures of analyst forecast accuracy as the dependent variable. Appendix C provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

Figure 4.1: The change in analyst forecast Predilection and Actual Predilection before and after a job loss.



This figure shows the change in analyst forecast *Predilection* and *Actual Predilection* before and after a job loss. The vertical axis shows the value of *Predilection/Actual Predilection*. The horizontal axis shows the time before and after analyst job loss. The figure visualizes the univariate test results tabulated in Panel A of Table 4.6. Appendix C provides a detailed description of the variables.

4.4.4. Subsampling analyses and the persistence of the impact from a job loss

While the impact of a recent job loss in driving forecast pessimism holds for the full sample, there can be a variation on the impact it has among different subsamples of forecasts issued by analysts with different characteristics. For example, Leana and Feldman (1990) find that individuals of different ages and levels of education do not necessarily react to a job loss in the same manner. Within the financial analyst literature, Cowen et al. (2006) find that analysts with better forecasting ability and less experience tend to issue less optimistic forecasts. This motivates me to test whether the impact of a job loss on *Predilection* is the same for analysts with different forecasting ability and years of experience.

I first perform univariate tests for the change in *Predilection* before and after a job loss among two subsamples of forecasts issued by superior/inferior analysts who belong to the top and bottom terciles of analysts, based on forecast accuracy, during the year prior to the job loss. I repeat the same tests for two subsamples of forecasts issued by experienced/ inexperienced analysts who belong to the top and bottom terciles of the number of years of experience the analyst has in working in the brokerage industry. The results are reported in Panel A of Table 4.7. I find that *Predilection* does tend to become more negative after the event for both superior and inferior analysts, although the decline in forecast optimism is more pronounced among superior analysts (a decrease of 0.3550 in *Predilection*, significant at the one percent level), relative to inferior analysts (a decrease of 0.2258 for inferior analysts, significant at the ten percent level). I also document that the impact is stronger among experienced analysts (a decrease of 0.3178 in *Predilection*, significant at the one percent level) compared to an insignificant change for inexperienced analysts.

To further confirm these results, I run my DiD regressions on these four subsamples and report the results in Panel B, Table 4.7. Regressions 1 and 2 show the results for my subsamples of forecasts issued by superior/inferior analysts, while Regressions 3 and 4 report the results for experienced/inexperienced analysts. I find that the coefficient of the interaction term for *Treat* and *Post* is negative and statistically significant only for my subsample of forecasts issued by superior/experienced analysts. These results suggest that the impact of job loss on analyst forecast optimism is more negative among superior analysts as well as experienced analysts, which is consistent with my univariate test results in Panel A of Table 4.7. I expect that the reason why superior/experienced analysts are more significantly affected by a job loss is because, in the case of superior analysts, they may be less expecting having to deal with losing a job. In the case of the experienced analysts, they will also be older (as the number of years of experience working as an analyst is directly related to age) and the career transitions literature highlights that this is a factor that can significantly explain how well employees deal with a job loss (Leana and Feldman, 1990). There is also an overlap of those analysts which are either both superior and experienced, or inferior and inexperienced. However, even if I exclude those analysts who intersect both categories, my results remain similar.

Table 4.7 also reports the results from sub-sampling the data based on periods of high and low competition for analyst jobs. At the start of the housing and global financial crisis (GFC), as well as the proceeding recession, a sizable number of brokerage firms closed, leading to a tight labor market for analysts seeking work. Specifically, three brokerage closures occur in 2007, one in 2008 and two in 2010, representing 46% of my sample. Under a tight labor market, employed analysts will be more concerned about their jobs, potentially altering the quality of their forecasts relative to periods of low labor market competition. To examine if this affects my results, I split my samples between a period of time that captures the start of the housing crisis and GFC plus the subsequent recessionary period (2007-2010), and all other years. To ensure I am matching the tighter labor conditions for the analysts that have been let go and then get rehired, and in acknowledgement that over 50% of the analysts in my sample specialize in just one industry, I further restrict my analysis to treatment and control forecasts that are made by analysts that track the top ten 2-digit SIC codes (industries) that have seen the greatest number of analyst redundancies. In doing this I not only account for the tighter labor market conditions for analysts as a whole, but for the particular analysts that have seen their peers, who follow similar stocks, experience the largest number of redundancies. The results, however, tabulated in the last two columns of Table 4.7, show that regardless of labor market conditions, newly rehired analysts who have experienced a recent job loss always show a significant, negative *Predilection*.

Table 4.7: Subsample analyses.

Panel A: Univariate tests

	Predilection Before job loss	*Predilection After job loss*	*Diff. (After – Before)*
Superior analysts	0.1424**	-0.2126***	-0.3550***
Inferior analysts	0.0876	-0.1381	-0.2258*
Experienced analysts	0.1177***	-0.2001***	-0.3178***
Inexperienced analysts	0.1646	-0.0447	-0.2093
High competition period	0.1424***	-0.1897***	-0.3321***
Low competition period	0.0867	-0.1583***	-0.2450***

Panel B: DiD regressions

	(1)	(2)	(3)	(4)	(5)	(6)
SUBSAMPLES	*Superior Analyst*	*Inferior Analyst*	*Experienced Analyst*	*Inexp. Analyst*	*High competition*	*Low competition*
VARIABLES	*Predilection*	*Predilection*	*Predilection*	*Predilection*	*Predilection*	*Predilection*
Treat	0.0737	-0.0471	0.0048	0.0371	0.1529***	0.0196
	(0.0705)	(0.0785)	(0.0458)	(0.1113)	(0.0536)	(0.0675)
Post	-0.0411***	-0.0829***	-0.0703***	-0.0732***	-0.1479***	-0.0670***
	(0.0087)	(0.0088)	(0.0081)	(0.0084)	(0.0179)	(0.0120)
Treat×Post	-0.2252**	-0.0586	-0.1189**	-0.0854	-0.1978***	-0.1760**
	(0.0900)	(0.1051)	(0.0582)	(0.1412)	(0.0656)	(0.0829)
Observations	47,040	52,910	55,877	60,425	13,413	22,036
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Deal fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

This table presents the results for the impact of experiencing a job loss on analyst forecast accuracy using different subsamples of analyst forecasts. Panel A shows the results of univariate tests on analyst forecast predilection among subsamples of forecasts by superior/inferior analysts, experienced/inexperienced analysts, and forecasts issued during high/low periods of job market competition. Panel B reports DiD regression results utilizing Equation (4.2). Superior/Inferior analysts are identified as being ranked in the top/bottom terciles of performers (in terms of forecasting accuracy) during the year prior to the job loss. Experienced/Inexperienced analysts are identified as being part of the top/bottom tercile in terms of years of experience working in the brokerage industry. The high job market competition period is from 2007 to 2010 and the low job market competition period includes the remaining time in my sample. Appendix C provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

While Cohn (1978), Leana and Feldman (1995) and Waters (2007) show that individuals will continue to emotionally suffer from the experience of a job loss after they get re-employed, their evidence of this spans only for a short period of time after the person is rehired. At the same time, Jackson et al. (1983) find that psychological distress, in general, reduces when individuals are rehired. I therefore examine how long the impact of the job loss affects the predilection of analyst forecasts once displaced analysts find a new job. In addition, I utilize subsample analyses to test whether the persistence of this impact is the same among analysts with different characteristics.

First, in Panel A of Table 4.8, I re-run my DiD regression comparing the level of analyst forecast *Predilection* one year before a job loss against the level of *Predilection* two years after the event among four subsamples of forecasts issued by superior/inferior and experienced/inexperienced analysts.[34] Then, in Panel B of Table 4.8, I repeat the tests in Panel A but compare *Predilection* one year before a job loss against *Predilection* three years after the event. I find that in the second year after displaced analysts get a new job, only superior analysts suffer from significant, negative *Predilection* (see Column 1, Panel A of Table 4.8). However, this impact disappears in the third year (see Column 1, Panel B of Table 4.8). For the other five subsamples, I find no significant difference between forecast optimism one year before and two (three) years after an analyst experiences a job loss. Taken together, these results show that while the effect of experiencing a job loss on analyst *Predilection* dissipates for most analysts by the second year of re-employment, it takes a little longer for superior analysts to return to their prior levels of forecast optimism that they exhibited prior to the job loss.

[34] I use the first forecast the analyst makes in the second year of employment with their new employer for each stock that is matched with the pre-event period.

Table 4.8: The longer-term impact of job loss on analyst forecast predilection.

Panel A: DiD regressions – Two and a half years after experiencing a job loss						
	(1)	(2)	(3)	(4)	(5)	(6)
SUBSAMPLES	*Superior Analyst*	*Inferior Analyst*	*Experienced Analyst*	*Inexp. Analyst*	*High competition*	*Low competition*
VARIABLES	*Predilection*	*Predilection*	*Predilection*	*Predilection*	*Predilection*	*Predilection*
Treat	0.0662	0.0090	0.0831**	0.0485	0.0173	0.0191
	(0.0548)	(0.0515)	(0.0327)	(0.0755)	(0.0396)	(0.0530)
Post	0.0026	-0.0030	-0.0135	-0.0385**	-0.0212	-0.1051***
	(0.0137)	(0.0146)	(0.0145)	(0.0182)	(0.0233)	(0.0355)
Treat×Post	-0.1712**	-0.0205	-0.0540	0.1241	-0.0312	0.1107
	(0.0742)	(0.0685)	(0.0463)	(0.1340)	(0.0501)	(0.1027)
Observations	15,527	14,036	17,140	18,042	6,697	3,962
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Deal fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: DiD regressions - Three and a half years after experiencing a job loss						
	(1)	(2)	(3)	(4)	(5)	(6)
SUBSAMPLES	*Superior Analyst*	*Inferior Analyst*	*Experienced Analyst*	*Inexp. Analyst*	*High competition per.*	*Low competition per.*
VARIABLES	*Predilection*	*Predilection*	*Predilection*	*Predilection*	*Predilection*	*Predilection*
Treat	0.0724	0.0713	0.0625	0.2702**	-0.0497	0.2183**
	(0.0600)	(0.0579)	(0.0381)	(0.1262)	(0.0456)	(0.1077)
Post	-0.0244	-0.0031	-0.0116	0.0045	-0.0677**	-0.0901**
	(0.0176)	(0.0198)	(0.0186)	(0.0264)	(0.0268)	(0.0442)
Treat×Post	0.1298	0.0266	0.0388	0.2853	0.0704	0.0547
	(0.0907)	(0.0843)	(0.0554)	(0.2484)	(0.0589)	(0.1444)
Observations	10,714	9,096	12,478	12,093	5,473	1,553
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Deal fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

This table reports DiD regression results examining the longer-term impact of experiencing a job loss on analyst forecast predilection. The regressions utilize Equation (4.2). Panel A and B shows the impact on *Predilection* among different subsamples of analyst forecasts 2.5 and 3.5 years after an analyst experiences a job loss, respectively. Appendix C provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

4.4.5. Alternative explanations

Although I appropriate the experience an analyst has of a recent job loss to explaining subsequent forecast pessimism, there can be other potential explanations. First, according to Bauer et al. (2007) and Saks et al. (2007), individuals can experience unfamiliarity with a new working environment, which leads to a greater level of uncertainty. There is also evidence that uncertainty can lead to biases in analyst forecasts (Hong et al., 2000; Clement and Tse, 2005; Nolte et al., 2014). Therefore, it is possible that the change in the *Predilection* of analyst forecasts documented in my main results is driven by analysts being unfamiliar with the new working environment of the firm that hires them rather than it being related to their recent experience of losing their job. In addition, analysts moving to a new brokerage firm may find that they have a different level of support and infrastructure at the new firm to what they were previously used to. This too can have an impact on the forecasts that they issue and potentially influence the results.

To examine the above issue, I compare my treatment sample of forecasts issued by analysts who experience a job loss and then move to a new firm against a control sample of forecasts issued by those analysts who do not experience a brokerage closure but nonetheless job-hop to work in a new brokerage firm.[35] Importantly, I match treatment and control forecasts such that they originate from analysts that work for similarly-sized brokerage houses, both before and after they move to a new employer. Specifically, analysts must be working in the same quartile-sized brokerage firm, as measured by my variable *Size* (the number of analysts working for a firm). This allows me to control for

[35] I identify this group of analysts in the I/B/E/S database as those who change their broker ID across the event window but are not included in my treatment sample.

the impact that a new working environment may have on the *Predilection* of analyst forecasts. I re-run my regressions using Equation (4.2) and report the results in Panel A of Table 4.9. The results are consistent with my main findings. For example, in Regression 3, the coefficient for the interaction term of *Treat* with *Post* is -0.1353, significant at the one percent level. The absolute impact of experiencing a job loss on *Predilection* among my treatment sample is -0.2129.[36] Compared to the average value of *Predilection* among my treatment sample prior to analysts losing their jobs (0.1295), the results indicate that analyst *Predilection* turns negative only after the experience of a job loss. This result highlights that it is specifically those analysts that experience a recent job loss, and not simply any analyst who switches to a new employer, that subsequently exhibit forecast pessimism.

Secondly, previous studies document a strong link between analyst career concerns and their forecast optimism. For example, Chan et al. (2007) conclude that analysts tend to issue less optimistic forecasts toward the end of the forecast period to generate earnings surprises for the firms they follow to please management. Horton et al. (2017) also document that analysts who desire to work for the firms they follow tend to issue optimistic (pessimistic) forecasts at the beginning (end) of the fiscal period (i.e. the OP pattern) to please firms. While a more optimistic forecast at the beginning of the fiscal year can provide a better outlook about the forecasted stocks, a pessimistic forecast towards the end of the period can create a positive earnings announcement surprise. If my treatment group of analysts are rehired closer to the end of the fiscal year, they may be tempted to issue pessimistic forecasts for this reason, which would be unrelated to how they are dealing with recently losing their previous job. I therefore need to consider when

[36] This is the sum of the coefficients for *Post* (-0.0776) and *Treat*×*Post* (-0.1353).

the forecasts are made in relation to the fiscal year. In addition, and in contrast to the generally observed OP pattern, I conjecture that my treatment group of analysts should exhibit a decline in the OP pattern, as the longer they remain employed at their new job, the impact of their experience from a job loss has on their likelihood of posting pessimistic forecasts will diminish (see Table 4.8).

To test this, I re-sample my data so that for each stock that is matched between my treatment and control groups I capture a forecast for it that the analyst makes at the start, and then at the end, of the fiscal year that is before (for the pre-event period) and after (for the post-event period) the brokerage closure occurs. I then construct a dummy variable, *OP*, that is equal to one whenever I notice that the forecast issued at the start of the fiscal year is higher than the actual earnings for the firm, plus that the last forecast made for the stock in the same fiscal year is lower than the actual earnings; and zero otherwise (Libby et al., 2008).

I utilize Equation (4.2) in a DiD logistic regression framework with *OP* as the dependent variable and report the results in Panel B of Table 4.9. The results show a significant decrease in *OP* among forecasts of analysts who recently experience losing a job relative to the control forecasts. Combining these results with Table 4.8, it supports the view that forecasts issued closer to when the analyst is rehired will be more pessimistic than later forecasts.

Table 4.9: Alternative explanations of analyst predilection following a job loss.

Panel A: DiD regressions using a control sample of forecasts from analysts who change jobs across the event window

VARIABLES	(1) *Predilection*	(2) *Predilection*	(3) *Predilection*
Treat	0.0394	0.0031	0.0997*
	(0.0481)	(0.0514)	(0.0551)
Post	-0.0801***	-0.0776***	-0.0776***
	(0.0231)	(0.0232)	(0.0232)
Treat×Post	-0.2542***	-0.1543**	-0.1353**
	(0.0608)	(0.0627)	(0.0628)
Observations	8,188	8,188	8,188
Control variables	No	Yes	Yes
Deal fixed effects	No	No	Yes

Panel B: DiD logistic regressions examining the impact of career concerns on analyst OP pattern

VARIABLES	(1) *OP*	(2) *OP*	(3) *OP*
Treat	0.0059	-0.1086	0.0407
	(0.0822)	(0.0828)	(0.0852)
Post	-0.1206***	-0.1017***	-0.1223***
	(0.0110)	(0.0113)	(0.0114)
Treat×Post	-0.4319***	-0.3680***	-0.3467***
	(0.1237)	(0.1238)	(0.1255)
Observations	174,550	174,550	174,550
Control variables	No	Yes	Yes
Deal fixed effects	No	No	Yes

This table reports test results examining alternative explanations for the impact of experiencing a job loss on analyst forecast predilection. The regressions utilize Equation (4.2). Panel A presents DiD regression results on the treatment sample and a control sample of earnings forecasts by analysts who experience a job change across the event window. I further require that analysts in both samples move to a new brokerage firm that are in the same quartile ranking as their former employer in terms of the number of employed analysts. Panel B presents logistic regression results that examine the impact of analyst career concerns following a job loss on the analyst optimism – pessimism (OP) pattern. Appendix C provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

In addition to the above two issues, there can be other factors that can potentially distort my main results. For example, if there are less than three analysts covering one stock, there would likely be a lack of available information surrounding the stock, which can result in analyst forecast bias (Das, Levine and Sivaramakrishnan, 1998). Therefore, in Regression 1 of Table 4.10, I report results from excluding all forecasts for stocks with low analyst coverage from my sample. In Regression 2 of Table 4.10, I consider the fact that some analysts cover a large portfolio of stocks, leading to their forecasts appearing multiple times in the treatment sample, thereby potentially disproportionally driving my results. I therefore exclude from my sample any forecasts by analysts who are ranked in the top decile, in terms of the number of stocks they have in their tracking portfolio.

In Regression 3 of Table 4.10, I account for the possibility that analysts might issue biased forecasts for stocks of large firms in order to boost trading commissions or to win an investment banking client (Horton et al., 2017). To deal with this I exclude forecasts for stocks belonging to the top decile of the largest firms (based on market value). In Regression 4, I address the issue that my main regressions only examine forecasts for stocks that appear in an analyst's portfolio both before and after a job loss. This means any stocks that an analyst drops after a job loss, and any new stocks that they are assigned by their new firm, are not considered. Therefore, I aggregate and average analyst *Predilection* across forecasts of all stocks in the analyst portfolio and re-run my regressions at the analyst level. Finally, in Regression 5, I present results for when I extend the cooling-off period to 18 months prior to a brokerage firm closure. Even if no formal announcement has been made, employees may get wind of the firm imminently closing. This is despite me checking for any news during this time period that may allude to this. To err on the side of caution, I therefore extend the cooling-off period to last for a period of one and a half years prior to the closure date to account for the above possibility.

In all cases, my results in Table 4.10 are consistent with my baseline results. In particular, the interaction coefficient between *Treat* and *Post* remains significant at either the one or five percent levels.

Table 4.10: Other robustness tests.

	(1)	(2)	(3)	(4)	(5)
SUBSAMPLES	*Exclude low coverage stocks*	*Exclude analysts with large portfolios*	*Exclude stocks of large firms*	*Aggregate at the analyst level*	*Extend the pre-closure cooling off period to 18m.*
VARIABLES	*Predilection*	*Predilection*	*Predilection*	$\overline{Predilection}$	*Predilection*
Treat	0.0387	0.0259	0.0472	0.0763*	-0.0034
	(0.0370)	(0.0377)	(0.0379)	(0.0393)	(0.0410)
Post	-0.0652***	-0.0678***	-0.0624***	-0.0487***	0.0423***
	(0.0045)	(0.0047)	(0.0047)	(0.0046)	(0.0060)
Treat×Post	-0.1248***	-0.1120**	-0.1308***	-0.2353***	-0.1113**
	(0.0475)	(0.0482)	(0.0500)	(0.0480)	(0.0526)
Observations	190,663	176,733	174,639	36,634	115,167
Control variables	Yes	Yes	Yes	Yes	Yes
Deal fixed effects	Yes	Yes	Yes	Yes	Yes

This table reports the results of other robustness tests on the impact of experiencing a job loss on analyst forecast predilection. The regressions utilize Equation (4.2). Column (1) shows the regression results using a subsample of analyst forecasts for stocks with at least three analysts following. In Column (2), I exclude from my sample any forecasts by analysts who belong to the top 10% of analysts in terms of the number of stocks they have in their tracking portfolio. In Column (3) I exclude forecasts for stocks that belong to the top 10% of the largest firms in my sample based on market value. Column (4) presents DiD regression results at the analyst level. Column (5) reports the regression results when I extend the cooling-off period prior to the closure date to 18 months. Appendix C provides a detailed description of the variables. Robust standard errors are reported in parentheses for all regressions. ***, **, and * represent significance levels of 1%, 5%, and 10%, respectively.

4.5. Conclusion

Utilizing brokerage firm closures as a quasi-natural experiment, I find that analysts who have a recent experience of losing their job are more prone to provide pessimistic forecasts. Importantly, I document that analyst forecast pessimism significantly increases analyst forecast errors. This impact, however, dissipates within a two to three-year period of being rehired.

My study contributes to the literature on financial analysts by examining factors that explain analyst forecast pessimism. By building on the extant career transition literature, I argue and find that financial analysts, as is the case with other types of employees, experience a negative emotional mindset from losing a job, which is not totally resolved when they are rehired, and that this subsequently leads to analyst forecast pessimism.

My findings have implications for brokerage firms as I suggest that they should adopt policies to support newly hired employees who have recently experienced a job loss to ensure they overcome their predilection to issue pessimistic forecasts. my study also suggests an avenue for future research that focuses other important life events that may affect financial analyst forecasting performance. While several studies (Hood et al., 2013; Roussanov and Savor, 2014; Bernile et al., 2017; Shi et al., 2017; Shu, Sulaeman and Yeung, 2017) have investigated the impact of life events on the behavior of firm management and investors, studies on how financial analysts respond to such events can be equally meaningful given the important role they play in facilitating the efficient dissemination of market relevant information.

5. Conclusion

This thesis consists of three studies that utilize different analyst career events as quasi-natural experiments to examine the determinants of analyst forecasting performance. From a methodological perspective, I contribute to the extant literature by utilizing quasi-natural experiments to minimize the endogeneity problems that are prevalent when examining financial analyst performance. From a theoretical perspective, I introduce a number of psychological and career transitions theories to the financial analyst literature by showing that career related life events can have a significant impact on analyst forecasting performance. My findings also have important practical implications since analyst performance is closely linked to the efficiency of capital markets (Ikenberry and Ramnath, 2002; Elgers et al., 2003). My studies, therefore, suggest that brokerage firm management should adopt suitable policies to support financial analysts during important life events to ensure their forecasting performance and subsequently enhance market efficiency.

The remaining of Chapter Five provides a summary of findings and contributions for each study in this thesis, research limitations, and directions for future research.

5.1. Summary of main findings and contributions

The first study presented in Chapter Two investigates the impact of employment change on analyst herding behavior. To ensure the robustness of the main findings, I utilize quasi-natural experiments by focusing on analysts who change job following brokerage firm M&As. My results show that analysts exhibit strong herding behavior following an employment change. Specifically, they are more inclined to issue forecasts that are close to a consensus forecast. Also, relative to their peers, they are slower in issuing forecasts and, as a result, issue forecast revisions less frequently. I also find that the market can recognize this herding behavior and show weaker reaction to forecasts issued by those analysts.

These findings contribute to the financial analyst literature by examining analyst job changes as a source of herding behavior, while previous studies only show that analyst forecasting ability and experience are the two main factors that explain herding (Stickel, 1992; Trueman, 1994; Graham, 1999; Hong et al., 2000; Clement and Tse, 2005). Given a large number of analysts change jobs every year, the impact of employment change on the performance of the intermediary function that analysts serve within the market can be potentially significant.

My findings, therefore, have an important human resource management implication. In particular, I highlight the need for brokerage firms to adopt appropriate socialization strategies for newcomers (see Saks et al., 2007) to manage the unfamiliarity that arises from employment changes in order to enhance the quality of analyst forecasts.

In Chapter Three, my second study examines the heterogeneous impact that work specialization has on superior and inferior analysts. I conduct quasi-natural experiments by utilizing brokerage firm M&As to capture changes to the work specialization of analysts who continue to work in the merged firms after the M&A events. My results show that the forecast accuracy of superior analysts improves when their stock portfolio is more concentrated within a few industries. However, there is no evidence of an equivalent improvement for inferior analysts.

This study, to the best of my knowledge, is the first to examine the different impact that work specialization has on the performance of individual analysts. My findings, therefore, provide an explanation for the mixed results in the literature studying the average effect of industry concentration on analysts' performance (Clement ,1999; Jacob et al., 1999; Clement et al., 2007), as I show that only superior analysts can benefit from work specialization. In addition, while prior studies utilize a naïve industry count to capture industry coverage, my study introduces the Herfindahl-Hirschman Index (*HHI*) to measure industry concentration. This is a more refined measure to capture how specialized the workload of an analyst since it accounts for both the number of industries assigned to the analyst and the proportion of stocks in the analyst portfolio that belongs to each industry.

Based on my findings, brokerage firms should consider allocating different types of work to fit with the skill-sets of superior and inferior analysts to effectively optimize their forecasting performance. Specifically, my findings support the view that superior analysts should concentrate within fewer industries, whereas there is no evidence that inferior analysts also benefit from industry specialization.

Finally, in Chapter Four, I utilize brokerage firm closures as quasi-natural experiments to examine a recent job loss due to firm closures as a channel to explain analyst forecast pessimism when they get rehired. I find that individuals who have recently experienced a job loss tend to issue more pessimistic forecasts compared to both their peers and the actual earnings. Importantly, my study provides evidence that analyst forecast pessimism following a job loss leads to a decline in analyst forecast accuracy when the analysts work in the new firm.

While previous studies, including Hong and Kubik (2003), Chan et al. (2007), and Horton et al. (2017), focus on examining analyst forecast optimism and its impact on analyst performance, my study contributes to the literature by showing that there are also factors that explain analyst forecast pessimism. In particular, I find that an experience of a recent job loss can lead to analyst forecast pessimism and that forecast pessimism can have a significant impact on the accuracy of analyst forecasts in much the same manner that forecast optimism can.

My findings, therefore, suggests that brokerage firms should adopt policies to support newly hired employees who have recently gone through a job loss. Such strategies could be crucial in resolving latent psychological issues and ensuring the quality of analyst forecasts.

5.2. Limitations

The first limitation of this thesis is primarily related to the limitations of the data source that I utilize (i.e. the I/B/E/S database). Although the database provides an extensive source of data on analyst forecasts, it is based on the voluntarily reported data from individual analysts. Therefore, the database itself is exposed to selection bias as analysts can decide whether they disclose their information, or which forecasts they would disclose. In addition, to observe the change to analyst career as well as its timing, I track the change in their brokerage firm IDs and the time that analysts issue their forecasts under the new brokerage firm IDs. This method, however, can result in errors as it also depends on the decision of analysts to report their information. Another issue with the database is that analyst characteristics, particularly demographic factors, are not disclosed. Therefore, my thesis cannot consider the moderation and/or mediation roles of those factors on the impact of life events on analyst forecasting performance.

My thesis also suffers from the limitations of the quasi-natural experiment methodology. As mentioned in Chapter One, a quasi-natural experiment is different from a natural experiment in terms of the degree of randomization. While a natural experiment involves actual randomization, a quasi-natural experiment is “patterned after randomized experiments" (DiNardo, 2010). Therefore, there can potentially be criticism about the events I utilize as quasi-natural experiments in my studies. For example, one may argue that the decision to retain or let go analysts following a brokerage M&A is related to their relative performance compared to peers. Similarly, it is arguable that job loss due to brokerage firm closures can be explained by employee poor performance, which triggers the closure of the firms. Therefore, these career events may not be completely random. Apparently, much of these issues can be resolved when using a difference-in-differences approach in conjunction with the quasi-natural experiments, which explains the methodological approach that I took. At the same time, I have conducted several robustness tests in each study to ensure this problem with the randomization of quasi-natural experiments does not fundamentally affect my main findings. However, this latent issue does not allow my thesis to fully address endogeneity problems.

5.3. Directions for future research

Given the significant impact that life events have on analysts forecasting performance, future studies on financial analysts could investigate how analysts respond to other important life events. These can include fully exogenous events such as a personal accident or sudden illness, death of family member(s), or career related events of a spouse. Although this research direction requires the collection of additional data apart from the available database on analyst forecasts (for example I/B/E/S), it can provide further information on analyst characteristics to supplement the analyst forecast database. At the same time, such exogenous events would, at least, not suffer from endogeneity problems.

Another research direction could focus on evaluating the effectiveness of various policies from policy makers as well as brokerage firm management to support financial analysts during the period surrounding their important life events. The outcomes of this research direction would provide valuable information to policy makers and firm management in the process of adopting suitable policies to enhance the performance of financial analysts. At the same time, it will also help enhance capital market efficiency, which partially relies on the efficiency of financial analysts.

Future studies should also examine the extent to which capital markets recognize and respond to the change in analyst performance due to important life events. This research direction could also involve suggesting and investigating the effectiveness of various trading strategies during the period that analysts experience these significant life events.

Appendix A

Variable definitions – Chapter 2

This appendix provides a detailed description of the construction of all the variables used in Chapter 2.

Variable	Unit	Definition
DEPENDENT VARIABLES		
$Bold_{ijt}$	Dummy	=1 if the forecast for stock *i* issued by analyst *j* in forecast period *t* is: (1) greater than both the pre-revision consensus* (i.e. the average of the most recent forecasts for stock *i* made by other analysts except analyst *j* during the same forecast period) and the analyst's most recent forecast; or (2) less than both the pre-revision consensus and the analyst's most recent forecast. =0 otherwise ** I require there are at least three forecasts contributing to the pre-revision consensus.*
CAR_{ijt}	%	The absolute value of the two-day cumulative market-adjusted daily returns from the day of, to the day after, the analyst forecast date for stock *i* for forecast period *t*.
$Frequency_{ijt}$	Revision	Number of forecast revisions issued by analyst *j* for for stock *i* in forecast period *t* minus the average number of forecast revisions issued by all analysts for the same stock within the same forecast period.
$Speed_{ijt}$	Dummy	The subtraction of 100 by analyst *j*'s ranking over the number of analysts following stock *i* times 100. Analyst ranking is the order of analysts in issuing their first forecasts for a stock within a forecast period, with the first analyst receiving the lowest rank.
INDEPENDENT VARIABLES OF INTEREST		
$Move_{ijt}$	Dummy	=1 if the forecast for stock *i* is issued by analyst *j* who experiences an employment change in year *t* =0 otherwise.
$Post_{ijt}$	Dummy	=1 if the forecast for stock *i* issued by analyst *j* in forecast period *t* is after the event date =0 otherwise.
$Treat_{ijt}$	Dummy	=1 if the forecast for stock *i* issued by analyst *j* in forecast period *t* belongs to the treatment group =0 otherwise.
CONTROL VARIABLES		
$Coverage_{it}$	Analyst	Number of analysts across the whole industry following stock *i* in forecast period *t*.
$Gen\ Exper_{jt}$	Year	Number of years from the first forecast of analyst *j*.
$Industries_{jt}$	Industry	Number of industries covered by analyst *j* in forecast period *t*.

$Lnbm_{it}$	NA	Log of the book to market value of firm *i* in forecast period *t.*
$Lnsize_{it}$	NA	Log of the total asset value of firm *i* in forecast period *t.*
$Profitability_{it}$	NA	Operating income over book value of assets of firm *i* in year *t.*
$Retann_{it}$	%	Annualized average monthly returns of stock *i* in year *t.*
ROE_{it}	NA	Annual return on equity (*ROE*) ratio of firm *i* in year *t.*
$SIC\ Exper_{jt}$	Year	Number of years from analyst *j*'s first forecast for a stock within one two-digit SIC code.
$Sigma_{it}$	%	Annualized daily return volatility of stock *i* in year *t.*
$Size_{kt}$	Analyst	Number of analysts employed by brokerage firm *k* in year *t.*
$SP500_{it}$	Dummy	=1 if stock *i* is included in the S&P 500 index in year *t*
		=0 otherwise.
$Stock\ Exper_{jt}$	Year	Number of years from analyst *j*'s first forecast for stock *i.*
$Stocks_{jt}$	Stock	Number of stocks covered by analyst *j* in forecast period *t.*
$Var\ ROE_{it}$	%	The variance of the residuals from an *AR(1)* model for stock *i*'s *ROE* using the past ten-year series of the company's annual *ROEs.*

Appendix B

Variable definitions – Chapter 3

This appendix provides a detailed description of the construction of all the variables used in Chapter 3.

Variable	Unit	Definition
DEPENDENT VARIABLES		
$FA_{i,j,t}$	NA	A measure of analyst forecast accuracy. It is measured as the subtraction of 100 by analyst *j*'s ranking over the number of analysts following the same stock times 100.
$PMAFE_{i,j,t}$	NA	A measure of analyst forecast error. It is the difference between analyst *j*'s absolute forecast error for stock *i* in year *t* and the mean absolute forecast error across all analysts following stock *i* in the same year, divided by the mean absolute forecast error. * I require that there are at least three analysts covering stock *i* in year *t* to construct this variable.
INDEPENDENT VARIABLES OF INTEREST		
$Entropy_{j,t}$	NA	A measure of work diversification that is equal to the negative value of the sum of the multiplication between the proportion of stocks within each industry that analyst *j* covers in year *t* and the natural log value of the proportion.
$HHI_{j,t}$	NA	A measure of work specialization that is equal to the sum of the squared proportion of stocks within each industry that analyst *j* covers in year *t*.
$Inferior_{j,t}$	Dummy	A dummy variable that is equal to one if analyst *j* is ranked within the bottom 20% of all analysts within the brokerage industry in year *t*, and zero otherwise. For the M&A sample, this variable is identified using the analyst performance in the year prior to the M&A.
$Inferior10_{j,t}$	Dummy	Similar to *Inferior* except that it is equal to one if analyst *j* is ranked within the bottom 10% of all analysts within the brokerage industry in year *t*.
$Inferior30_{j,t}$	Dummy	Similar to *Inferior* except that it is equal to one if analyst *j* is ranked within the bottom 30% of all analysts within the brokerage industry in year *t*.
$Superior_{j,t}$	Dummy	A dummy variable that is equal to one if analyst *j* is ranked within the top 20% of all analysts within the brokerage industry in year *t*, and zero otherwise. For the M&A sample, this variable is identified using the analyst performance in the year prior to the M&A.
$Superior10_{j,t}$	Dummy	Similar to *Superior* except that it is equal to one if analyst *j* is ranked within the top 10% of all analysts within the brokerage industry in year *t*.

Superior30$_{j,t}$	Dummy	Similar to *Superior* except that it is equal to one if analyst *j* is ranked within the top 30% of all analysts within the brokerage industry in year *t*.
CONTROL VARIABLES		
Experience$_{j,t}$	Year	The number of years analyst *j* works in the brokerage industry till year *t*.
Horizon$_{i,j,t}$	Day	The number of days from analyst *j* forecast for stock *i* in year *t* till the end of the forecast period.
New stocks$_{j,t}$	Stock	The number of stocks that analyst *j* issues forecasts for the first time in year *t*.
Revisions$_{i,j,t}$	Revision	The number of forecast revisions that analyst *j* issues for stock *i* in year *t*.
Size$_{j,t}$	Analyst	The number of analysts employed by the brokerage firm that analyst *j* works for in year *t*.
SP500$_{i,t}$	Dummy	A dummy variable that is equal to one if stock *i* in year *t* belongs to the S&P500 index, and zero otherwise.
Workload$_{j,t}$	Stock	The number of stocks follow by analyst *j* in year *t*.

Appendix C

Variable definitions – Chapter 4

This appendix provides a detailed description of the construction of all the variables used in Chapter 4.

Variable	Unit	Definition
DEPENDENT VARIABLES		
$AdFE_{ijt}$	NA	The absolute difference between analyst *j*'s earnings forecast and the actual earnings of stock *i* released by the firm, adjusted by the stock price on the forecast date.
FE_{ijt}	Dollar	The absolute difference between analyst *j*'s earnings forecast and the actual earnings of stock *i* released by the firm.
$Predilection_{ijt}$	NA	The difference between analyst *j*'s earnings forecast and the average of the most recent forecasts for stock *i* made by other analysts except analyst *j* during the same forecast period (pre-revision consensus); divided by the standard deviation among those forecasts. ** I require that there are at least three forecasts contributing to the pre-revision consensus.*
$Actual\ Predilection_{ijt}$	NA	The difference between analyst *j*'s earnings forecast and the actual earnings of stock *i* released by the firm; divided by the standard deviation among all earnings forecasts for stock *i* during the same forecast period.
$Target\ Predilection_{ijt}$	NA	The difference between analyst *j*'s price target forecast and the average of the most recent price target forecasts for stock *i* made by other analysts except analyst *j* during the same forecast period (pre-revision consensus); divided by the standard deviation among those forecasts. ** I require that there are at least three forecasts contributing to the pre-revision consensus.*
OP_{ijt}	Dummy	=1 if (1) the first forecast for stock *i* issued by analyst *j* in forecast period *t* is higher than the actual earnings, and (2) the last forecast for stock *i* issued by analyst *j* in forecast period *t* is lower than the actual earnings =0 otherwise ** I require that the first forecast is issued before the mid-fiscal-year date and the last forecast is after the mid-fiscal-year date.*
$Recommendation_{ijt}$	NA	It equals 4, 3, 2, 1 and 0 if the recommendation for stock *i* issued by analyst *j* in forecast period *t* is strong buy, buy, hold, under-perform and sell, respectively.

INDEPENDENT VARIABLES OF INTEREST		
$Post_{ijt}$	Dummy	=1 if the forecast for stock *i* issued by analyst *j* in forecast period *t* is after the event date =0 otherwise.
$Treat_{ijt}$	Dummy	=1 if the forecast for stock *i* issued by analyst *j* in forecast period *t* belongs to the treatment group =0 otherwise.
CONTROL VARIABLES		
$Coverage_{it}$	Analyst	Number of analysts across the whole industry following stock *i* in forecast period *t*.
$Gen\ Exper_{jt}$	Year	Number of years from the first forecast of analyst *j*.
$Industries_{jt}$	Industry	Number of industries covered by analyst *j* in forecast period *t*.
$Lnbm_{it}$	NA	Log of the book to market value of firm *i* in forecast period *t*.
$Lnsize_{it}$	NA	Log of the total asset value of firm *i* in forecast period *t*.
$Profitability_{it}$	NA	Operating income over book value of assets of firm *i* in year *t*.
$Retann_{it}$	%	Annualized average monthly returns of stock *i* in year *t*.
ROE_{it}	NA	Annual return on equity (*ROE*) of firm *i* in year *t*.
$SIC\ Exper_{jt}$	Year	Number of years from analyst *j*'s first forecast for a stock within a specific two-digit SIC code.
$Sigma_{it}$	%	Annualized daily return volatility of stock *i* in year *t*.
$Size_{kt}$	Analyst	Number of analysts employed by brokerage firm *k* in year *t*.
$SP500_{it}$	Dummy	=1 if stock *i* is included in the S&P 500 index in year *t* =0 otherwise.
$Stock\ Exper_{jt}$	Year	Number of years from analyst *j*'s first forecast for stock *i*.
$Stocks_{jt}$	Stock	Number of stocks covered by analyst *j* in forecast period *t*.
$Var\ ROE_{it}$	%	The variance of the residuals from an *AR(1)* model for stock *i*'s *ROE* using the past ten-year series of the company's annual *ROEs*.

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