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Three essays on financial analysts' performance

Andreea MORARU-ARFIRE

UNIVERSITÉ DE NEUCHÂTEL

FACULTÉ DES SCIENCES ÉCONOMIQUES

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La doyenne

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Executive Summary

This dissertation is composed of three chapters. The first chapter explores the importance of previously identified factors in explaining the variation in analysts' earnings forecast error. As earnings forecasts are the main input in determining price targets and consequently stock recommendations, much of the process through which analysts process their input remains in a so-called "black box". This study attempts to shed light on these inputs. First, it reveals that forecast errors are stable over time, and analysts do not efficiently integrate past information in their forecasts. Second, analysts do not factor in expectations related to the macroeconomic conditions for the underlying forecast horizon. Analysts overreact (underreact) to positive (negative) macroeconomic expectations on both GDP and consumer sentiment index. Third, this study decomposes analysts' forecast errors variance by observable characteristics and fixed effects. Importantly, the analysis shows that there is an unobserved, time-invariant component related to the firm-analyst dimension that explains much of the variance in the forecast errors. This component is not yet captured by the existing observable characteristics which, at date, have a trifling effect on their own in explaining the variation in analysts' forecast error.

In the second chapter, I investigate the role of financial reporting frequency in analysts' earnings forecasts. I addresses two questions. First, does mandatory quarterly reporting benefit financial analysts in decreasing their earnings forecast error and dispersion? Second, to what extent common accounting standards increase the convergence of analysts' information set for firms with different reporting frequencies? I find little support to the claim that regulation forcing firms to issue more frequent financial information benefits financial analysts. Compared to a control sample of semiannual reporting firms in the European market, analysts issuing earnings forecasts for firms with mandatory quarterly frequency experience higher forecast error and dispersion. When firms are mandated to report not only on a quarterly frequency, but also under International Financial Reporting Standards (IFRS), analysts' both forecast error and dispersion decrease. However, while IFRS does benefit analysts by increasing the quality of their

information set in absolute terms, they do not wipe out the relative noise associated with mandatory quarterly statements.

The third chapter focuses on how financial analysts adapt to the passage of regulations aiming at limiting conflicts of interest in the investment banking industry. This last chapter investigates analysts' price targets and recommendations, and unravels a new form of conflicts of interest. Specifically, it investigates whether affiliated brokers issue unfavourable ratings on their clients' competitors in the product market (rivals). The findings document an important gap between ratings for affiliated and rival firms. Specifically, brokers issue persistently higher ratings on firms with which they are affiliated compared to their rivals. Importantly, the Sarbanes-Oxley Act and the related financial regulations aiming at curbing the conflicts of interests had no significant impact in reducing this gap. As such, affiliated brokers continue to indirectly favour their clients. This form of conflict was devoid of adequate attention in prior research. Furthermore, investors are unaware of the existence of such conflict in the short-run.

Key words: financial analysts, earnings forecasts error, reporting frequency, financial statements, price targets, stock recommendations, Sarbanes-Oxley act, conflicts of interest, investment banking.

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"You businessmen kept predicting that we'd perish, but we haven't."

-Ayn Rand, Atlas Shrugged. 1996. *50th Anniversary Edition, Signet, New York, NY,* p.903

Introduction

In financial markets, investors generally rely on the experts - and particularly on financial analysts – to obtain information on what the future may hold. Accurate predictions are paramount for dealing in a world fraught with uncertainty, since they improve the decision making process and help optimizing resource allocations. What equities should one invest in over the long run? Which sectors will outperform? How will the market react to the next regulatory initiatives? The role of financial analysts is to provide investors the answers to these, and other related topics.[1] Sell-side financial analysts have the expertise and resources to value companies, research and issue professional reports on firms' operations, and provide an investment rating to their clients.

The research interest on financial analysts has grown exponentially over the last three decades. While in the early years the academic literature tackled analysts' expectations to investigate market efficiency, it later explored the investment value of their output. Along this way, the spectrum of analysis has extended from solely earnings forecasts to stock recommendations and, eventually, price targets. This work has certainly increased our understanding on analysts' relevance and how they operate in financial markets as intrinsic agents.

Nevertheless, we still lack a great deal of knowledge in two particular areas. The first one relies on analysts' input in producing forecasts. Despite hundreds of studies, the academic research has made limited breakthrough in what constitutes the analysts' toolbox for valuing firms. What are the key determinants of analysts' forecast errors? Do analysts efficiently integrate past information in their forecasts? The process through which analysts forecast earnings remains by and large a "black-box" and warrants further investigation.

The second strand of research concerns the role of financial regulations on analysts' output. As the crisis of 2008 set back the financial industry in a state of ill-repute, financial regulators responded with an unprecedented growth in reforms to increase confidence in capital markets. To illustrate, the number of

[1] See, e.g., "14 Predictions for 2016 from the brightest minds in finance", *Bloomberg*, November 20, 2015; "Analysts' top sectors and stock picks for 2016", *CNBC*, December 31, 2015.

regulatory restrictions issued by the U.S. Security and Exchange Commission increased by 19% from 1997 to 2012.[2] Financial regulations have expanded on a global scale and show no signs of abating. For example, in 2014, financial regulators issued on average 155 alerts per day concerning worldwide updates, policy notes, briefings and enforcement of financial rules[3]. These regulatory initiatives most likely changed the way analysts interact with firms, mix private and public information, and conduct their research. Do analysts issue more accurate forecasts? Do price targets and stock recommendations better reflect stocks' expected performance? How these regulations affect analysts' role as intermediaries in financial markets? Did they have unintended consequences?

This dissertation aims to contribute to these lines of inquiry. The first essay examines the determinants of analysts' forecast errors. In a variance decomposition model, this study checks to what extent the determinants previously identified in the literature are important in the cross-section and time-series variation of analysts' forecast errors. The analysis reveals that the importance of the existing determinants is dismal, and that forecast errors are rather stable over time. Firm and analyst fixed effects explain much of the variance in analysts' forecast errors, suggesting that there is an important, unobserved firm and analyst related component in the forecast error, not yet captured by the existing variables. This time-invariant component reveals that much of what constitutes analysts' forecast error is currently left unmeasured. As such, the inherent firm-analyst dimension plays a critical role in explaining the forecast error. This first essay complements recent findings showing that private and public communication between analyst and management constitutes one of the most valuable sources of information for the earnings forecasts.

The second essay investigates whether financial analysts benefit from the mandatory quarterly financial reporting in Europe. Specifically, do firms with mandated quarterly reports and semiannual reports differ in terms of analysts' annual earnings forecast error, dispersion and following? On one hand, if mandated quarterly reports decrease information asymmetry, the quality of analysts' information

[2] Source: Mercatus Center, RegData, http://regdata.org.

[3] The corresponding numbers for 2013 and 2012 were 103 and 68 respectively. Source: "The peak of regulatory change may be some way off ", *Thomson Reuters Regulatory Intelligence*, March 16, 2015.

environment should increase. On the other hand, forcing firms to issue financial statements too frequently may constrain the management to focus extensively on the short-term performance in reporting noisy information, and analysts should experience larger forecast errors for such firms. I find support for the latter case. Specifically, mandatory quarterly reports are associated with greater forecast error and dispersion. These results are consistent with recent evidence showing that mandated quarterly reporters exhibit higher real activities manipulation compared to semiannual reporters. In a second step of the analysis, I examine whether the mandatory adoption of International Financial Reporting Standards (IFRS) increases the convergence of accounting information among firms with different reporting frequencies. Consistent with extant evidence, analysts' errors decrease significantly for the entire universe of firms after IFRS. At the same time, the positive gap between quarterly and semiannual reporters in terms of analysts' forecast error and dispersion continues to exist. As such, while IFRS does benefit analysts by decreasing forecast error and dispersion in absolute terms, they do not wipe out the pernicious relative noise associated with mandatory quarterly statements.

The third essay examines how the Sarbanes-Oxley Act (SOX501) has affected analysts' conflicts of interest. While the focus of previous research is on the brokers' positive bias for their clients, this study investigates whether brokers are unfavorable towards their clients' rivals in the product market. Two questions are addressed. First, do brokers provide significantly lower ratings for rival than for affiliated firms? Second, are investors aware of this form of conflicts? The results suggest that brokers comply with the regulation and issue less optimistic ratings. Nevertheless, brokers continue to keep their clients on significantly higher ratings than their clients' rivals. In fact, brokers switched to a downward scale for both affiliated and rival firms, maintaining the rating gap in the aftermath of the regulation. This gap is identified in both price targets and stock recommendations. To go further, the study reveals that investors are unaware of brokers' negative bias on rival firms, both before and after the passage of SOX501. This last essay uncovers a form of conflicts of interest in the investment banking industry that was not addressed so far. To the extent that conflicts of interest were mitigated, it finds evidence that brokers continue to favor their clients by following an alternative strategy without breaching the rules.

Overall, these three essays shed light on what ultimately drives analysts' forecast errors and how regulations affect their output. This thesis also narrows the scope of the future research in what I believe it remains an exciting and promising direction to better understand the behavior of these agents. In this respect, important challenges await further investigation. What are the steps to forecast bottom-line earnings and what information analysts factor in along the process? Does a common set of standards such as IFRS facilitate this process?

In the next years, the academic research can contribute in several significant ways to bring us closer to a better understanding of how financial analysts operate. First, we need to assess accuracy with more rigor and much more exact measurement tools. Consider the questions at the beginning of this section. What does the *long run* mean: two-year, five-year, ten-year, or an indefinite horizon? What does *outperform* exactly stand for: beating the sector, or beating the market? As long as we do not develop tools to quantitatively assess analysts' accuracy, relative judgements will not help much. Second, the progress in technology should undoubtedly facilitate our work in providing us with an array of available data resources and measurement tools. Until recently, the bottom-line stock recommendations had the lion's share of attention in research. However, the last years provided a fruitful opportunity to investigate the entire content of analysts' reports through text analysis and tone measurement. To better advance in our collective knowledge, integrating these tools in our future research remains essential. Finally, I believe we need as a community to increase our interest in interdisciplinary research. For example, parallel work in cognitive biases and neuroscience could provide fruitful insights on whether, and how financial analysts form and use their priors. As Tetlock and Brown (2015) state it: "Beliefs are hypotheses to be tested, not treasures to be guarded."[4] We ought to abide by the same principle in our profession.

[4] Source: Tetlock, P. E., and D., Gardner. 2015. Superforecasting: The art and science of prediction. *Crown Publishers, New York, NY*, p.127.

Chapter 1: Decomposing Analysts' Forecast Error: What are the Key Factors?

(In collaboration with Michel Dubois and Žana Grigaliūniene)

1.1. Introduction

Over the last decades, the academic research on financial analysts has undoubtedly increased our understanding on a broad swath of subjects related to these agents. Central to this literature is the notion that financial analysts are important information intermediaries and investors react significantly to the dissemination of their output (e.g., earnings forecasts, price targets and stock recommendations). Moreover, financial analysts' earnings forecasts are used in stock valuation models to compute proxies for the cost of capital in accounting and finance research. While much of the research focuses on the analysts' output, little is known at date on the process through which they forecast earnings. The quality of analysts' forecasts, e.g., the errors they are prone to, stems directly from the input they use in the process. As such, knowing what determines these errors warrants further investigation. What are the key determinants of analysts' forecast errors? Do analysts persist in their errors? Are errors mostly driven by analysts' bias (i.e., ex-ante information), or firm-unrelated, unexpected changes in macroeconomic conditions (i.e. ex-post information)? How can we come to a better understanding of what ultimately drives analysts to commit these forecast errors? The goal of this chapter is to answer these questions.

We begin by reviewing the literature on analysts' earnings forecasts over the 25 years and retain a list of commonly identified determinants of forecast errors. Typically, these variables relate to the observable factors at the time of the forecast, and pertain to four categories: forecast, analyst, firm, or institutionally related characteristics. We extend the list of variables by two other factors. First, we compute the unexpected changes in the future macroeconomic conditions at the time of the earnings forecasts. Specifically, we compute the surprise over the same horizon in the aggregated output (i.e., the difference between GDP forecasted and realized values) and consumer sentiment (i.e., the difference between

Michigan's Consumer Sentiment Index at the time of the earnings forecast and at the end of the year). Since this information is observable ex-post, but predictable ex-ante, our motivation is to examine the interplay between this type of information and the one available at the time of the earnings forecast. Importantly, adding these factors sheds light on the degree to which analysts integrate firm-unrelated information into their forecasts. Second, we add the prior forecast error to account for the dynamics of the series.

In the first part of the analysis, we follow Arellano and Bond (1991) and run a system GMM model to quantitatively assess the magnitude of analysts' forecast errors association with the set of variables mentioned above. Two findings emerge from this exercise. First, the effect of prior forecast errors is rather economically weak. Specifically, while analysts do persist in their errors by an estimate of 0.05% (statistically significant at the 5% level), the dynamic remains low. Second, the unexpected changes in the macroeconomic conditions are strongly associated with analysts' earnings forecasts errors. This association includes firm's sensitivity to the macroeconomic conditions (i.e., either GDP or consumer sentiment) and reveals that the more exposed the firm is to the economy, the more analysts overreact (underreact) to optimistic (pessimistic) forecasts of the macroeconomic conditions. In economic terms, one standard deviation in the GDP input factor for example is equivalent to an increase of 1.3 USD based on the median per-share assets in our sample.

In the second step, we appraise the relative importance of each factor. We perform a variance decomposition of analysts' forecast errors and check to what extent these factors are important in the cross-section and time-series variation of analysts' forecast errors. We find that the importance of the existing determinants is dismal. Specifically, the adjusted R-square from a baseline model with the entire set of measurable variables is 2.4%. Confirming our previous findings, the time dimension is rather weak in explaining the forecast error variance. Specifically, a model with both observable characteristics and time fixed effects has an explanatory power of 4%., with the time dimension mostly orthogonal to these variables.

When we augment the baseline model with firm (analyst) fixed effects, the adjusted R-square is 34% (19%), revealing that there is an important, unobserved firm and analyst related component in the forecast error not yet captured by the existing variables. This time-invariant component shows that much of what constitutes analysts' forecast error is currently left unexplained. Importantly, when we add both firm and analyst fixed effects adjusted R-square increases to 61%. This value is higher than the additive adjusted R-squares from the two models with separate firm and analyst fixed effects, suggesting that the inherent firm-analyst dimension plays a critical role in explaining the forecast error.

The main takeaway of this chapter is that the firm-analyst relationship is at the core of the process of forecasting earnings. We build on the existing gap in the literature called by Schipper (1991), Ramnath, Rock and Shane (2008), and Bradshaw (2011) in narrowing the scope of the "black-box" process through which analysts forecast earnings. Our study complements the recent results in Soltes (2014) and Brown, Call, Clement and Sharp (2015), among others, that show that private and public communication between analyst and management constitutes one of the most valuable sources of information input for the forecasts. Specifically, we quantitatively assess the relative importance of this relationship in an extended array of factors.

The rest of this chapter is structured as follows. Section 1.2 provides the literature review. Section 1.3 describes the data. In Section 1.4 we investigate the dynamics of earnings forecast error. Section 1.5 examines the variance decomposition of the error and Section 1.6 concludes.

1.2. Background and related literature

Prior research identifies a myriad of factors associated with analysts' earnings forecasts errors. However, it is important to note that these factors are, by construction, directly related to the timing of the forecast; i.e., the ex-ante information available to the analyst at the forecast date. While we can currently measure the analyst's bias in terms of her available information set, two factors warrant further attention. First, the literature is silent as to what extent analysts' forecast error is associated with ex-post information up until the forecast's realization. To wit, how is the error related to the inherent analysts' forecast ability,

and to unexpected events unfolding after the forecast date? Second, the dynamic structure of the forecast errors was little explored; i.e., to what extent current financial analysts' earnings forecast errors related to past earnings forecast errors? We lay out each of these factors below.

1.2.1. Bias in earnings forecasts

Several characteristics identified as significant in explaining analysts' forecast error paved the way to a multitude of studies that repeatedly used these characteristics in the last two decades of research in the field. We classify these characteristics in four categories: analyst-, firm-, forecast- and institutional-related.

1.2.1.1. Analyst characteristics

Starting with Clement (1999), a large body of literature documents analyst's experience in producing forecasts as one of the most important variable associated with earnings forecast error. Clement, Koonce and Lopez (2007), Clement, Hales and Xue (2011), and Walter and Willis (2013) among others, find that analyst's experience in the financial analysis industry – computed as the number of years the analyst was active in producing earnings forecasts – is negatively related to analyst's error. In addition, Brown and Mohd (2003), Chen and Martin (2011) and Hugon, Kumar and Lin (2015) document that analysts' forecast error is indirectly associated with their firm-specific experience – computed as the number of years the analyst was active in producing earnings forecast for the firm. The results of these studies demonstrate that analysts improve their forecasting skills by practice.

Analyst's portfolio complexity is another characteristic associated with forecast error. While the above studies find a positive relation between the number of firms and industries followed by an analyst and her forecast error, Call, Chen and Tong (2009) suggest that the analyst's complexity task is also important. Specifically, earnings forecast errors are smaller when accompanied by cash flow forecasts. Analysts' resources are as well important in producing unbiased forecasts. Clement (1999), Brown (2001), and Malloy (2005) among others find that working for a large size broker – measured as the number of

analysts employed – is negatively associated with analysts' forecast errors. In a nutshell, analysts' informational advantages at large brokerage houses, as well as their specialization in covering the firm are directly related to accurate forecasts.

1.2.1.2. Firm characteristics

Analysts issue forecasts based on their expectations on firm-related performance. Not surprisingly, a good knowledge of firm characteristics is directly related to unbiased forecasts. Firm size is among the main characteristics associated with analyst's earnings forecast error. Bowen, Davis and Matsumoto (2002), Das, Levine and Sivaramakrishan (1998), Land and Lundholm (1996), Barron, Kile and O'Keefe (1999), Gul, Hutchinson and Lai (2013), Herman, Hope and Thomas (2008), Barron, Byard, Kile and Riedl (2002), Teoh and Wong (2002), and Simpson (2010) measure size as firm's market capitalization and find a negative association with forecast error. Lys, Gilbert and Soo (1995), Chen, Krishnan and Sami (2015), and Espahbodi, Espahbodi and Espahbodi (2015) measure firm size as total assets and document a similar relation. This finding could have two interpretations. First, large firms are likely to be held by large institutional investors – and thus brokerage houses – which drives analysts to behold more information on those firms. Second, since large firms are covered by more analysts – as information production costs are lower – the informational advantage flows to the entire set of followers. In line with this hypothesis, Alford and Berger (1999), Chen and Martin (2011), Mikhail, Walter and Willis (1997), and Becchetti, Ciciretti and Giovanelli (2013) argue that the number of analysts following a firm is negatively associated with the earnings forecast error.

The forecast error is also deemed to rely on the complexity of firm's operations. For example, Barron et al. (2002) and Kross and Suk (2012) find that firm's research and development expenses are directly associated with analysts' forecast error. In addition, He and Tian (2013) show that firms covered by a larger number of analysts invest less in long-term innovative projects. Chen et al. (2015) argue that forecast error is positively related to firm's intangible assets, proxied by the goodwill balance on total assets. Bradshaw, Richardson and Sloan (2001), Teoh and Wong (2002), Hugon et al. (2015) find that

firm's total accruals are directly associated with analysts' forecast error, since the former are difficult to predict. Firm's sales are as well important inputs in accurately predicting earnings. Datta, Iskandar-Datta and Sharma (2011) find that firm's industry-related market share is negatively related to analysts' forecast error. On the other hand, the error increases with firm's international diversification (Duru and Reeb, 2002) and with the number of reported business segments (Herman et al., 2008).

Another strand of literature controls for analysts' bias by including firm's market-to-book ratio. Barron et al. (2002), and Espahbodi et al. (2015) find that analysts' forecast error is negatively associated with firm's market-to-book ratio, arguing that high-growth companies have more information available due to a greater number of analysts following. At the same time, Chen and Martin (2011) argue that analysts' forecast error is positively associated with firm's market-to-book ratio.

The recent literature explores the informational role of firm's accounting numbers in relation with macroeconomic conditions, which are an important determinant of corporate earnings. Firms' cyclicality is an important feature in empirical studies exploring earnings forecasts. Assuming that some firms are more sensitive to macroeconomic conditions, it is important to investigate discrepancies in earnings forecasts for firms that exhibit cyclical characteristics. Hann, Ogneva and Sapriza (2012) find that analysts under-react to bad macroeconomic news at the aggregate level, and this relationship varies with firms' characteristics. Specifically, the authors sort firms into groups according their size and exposure to macroeconomic shocks, and document that analysts under-react less for smaller and less cyclical firms. Hugon et al. (2015) also suggest that financial analysts underreact to macroeconomic news. They also find that analysts working for brokers that issue more accurate macroeconomic forecasts issue also more accurate earnings forecasts. Hutton, Lee and Shu (2012) explore the information advantage of both management and financial analysts' earnings forecasts focusing on firm's cyclicality. Their results point out that financial analysts' information advantage is stronger when firms vary with macroeconomic factors.

As the GDP growth is related to market sentiment, the firm's sensitivity to macroeconomic activity can also be measured by its exposure to the market sentiment. As such, similar to GDP growth, financial

analysts are likely inefficient in incorporating changes in market sentiment in their earnings forecasts. In line with this conjecture, Bergman and Roychowdhury (2008) find that market sentiment, measured by the Michigan Consumer Sentiment Index, and GDP growth are positively related to financial analysts' forecast errors. Livnat and Petrovits (2008) find that upward forecast revisions to extreme good news are not statistically different during low and high sentiment periods, suggesting that analysts are generally optimistic and do not follow sentiment in revising their forecasts. In a nutshell, although firm's earnings depend on both firm's fundamentals and macroeconomic conditions, financial analysts tend to underweight information that is not firm specific.

1.2.1.3. Forecast characteristics

While previous literature finds that individual analyst and firm characteristics are associated with earnings forecast error, it also suggests that the interrelation among these characteristics is not innocuous for accurately predicting firm's earnings. Mikhail et al. (1997), Brown and Mohd (2003), Clement et al. (2011), and Becchetti et al. (2013) among others, find that the frequency of analyst's revisions in the underlying firm's forecast is negatively related with forecast error, suggesting that analysts update – and improve – their forecasts as new information is available. On the contrary, earnings forecast errors are associated with the forecast horizon, i.e. the distance between the forecast release date and the fiscal year end; see, e.g., Agrawal, Chadna and Chen (2006); Chen and Martin (2011); Duru and Reeb (2002). More recently, Baum, Bowers and Mohanram (2015) show that analysts' forecast timing in competing for the same stock coverage is important in providing accurate forecasts. Specifically, the authors compute the leader-follower ratio based on Cooper, Day and Lewis (2001) and find that information leadership is positively correlated with forecast error.

Financial analysts' incentives in gaining underwriting mandates at investment banks and their conflicts of interest have been explored extensively in the literature. Malmendier and Shantikumar (2014) show that affiliated analysts do not distort earnings forecasts, as opposed to recommendations, since they are disseminated predominantly to large institutional investors. At the same time, Malloy (2005) argue that

forecast error is smaller for affiliated analysts employed by banks serving as lead underwriter for the firm's initial public offering. Similarly, Chen and Martin (2011) find that bank-affiliated analysts gain an informational advantage with lending relationship. Specifically, the forecast error decreases after a loan initiation with the affiliated bank.

1.2.1.4. Institutional environment

As firms do not operate in an isolated environment, their performance and the underlying analysts' forecasts are prone to institutional changes. Several studies investigate how regulating analysts' dissemination of private information affects their forecast errors. For instance, Kross and Suk (2012) find that Regulation Fair Disclosure (RegFD) is associated with more forecast revisions and higher forecast errors. To wit, RegFD levels the playing field by requiring firms to disclose information to all interested parties instead of a small number of analysts. Agrawal et al. (2006) also show that in the aftermath of RegFD, analysts' forecast errors are higher, suggesting a reduction in selective guidance.

In an attempt to restore confidence in public US markets, the was enacted in July 2002. According to the Sarbanes-Oxley Act (SOX) requirements, public firms have to disclose auditors' internal control reports. Kim, Song and Zhang (2009) suggest that analysts' forecast error decrease in the aftermath of these reports' release. Consequently, effective internal controls improve the quality of analysts' earnings forecasts.

1.2.2. Unexpected changes in macroeconomic factors

Prior literature shows that analysts' forecast errors are associated with several biases. However, little is known on analysts' ability to integrate expectations at the macroeconomic level. To the extent that analysts fail to account for this information, one can expect that forecast errors are related to unexpected changes in macroeconomic variables.

There are at least two reasons why financial analysts should integrate in their forecasts expectations at the aggregate level of the economy. First, macroeconomic forecasts translate the extent to which

individual firms are expected to evolve as an aggregate.[1] The more cyclical the firm, the more these forecasts are likely to serve as an important input for the analysts' forecasts. Linking earnings forecast with macroeconomic forecasts, Hann et al. (2012) investigate whether the two sets of forecasts reflect macroeconomic information. The authors regress each forecast on the counterpart's forecast revisions that reflects the unexpected component in macroeconomic news. They find that both sets of forecasts contain a common component of the macroeconomic conditions.

Second, macroeconomic forecasts are likely to be less biased and more objective than individual firms' forecasts. To illustrate, Hann et al. (2012) support previous findings in Darrough and Russell (2002) that top-down forecasters are less optimistic compared to the bottom-up forecasters, because the former have less economic incentives concerns[2].

A handful of studies link macroeconomic indicators and sentiment in explaining variation in forecast errors. Qian (2009) shows that market sentiment (proxied by S&P100 index put-call ratio and CBOE equity put-call ratio) has a significant impact on average forecast errors. Specifically, optimism in firm-level earnings forecasts rises with sentiment when extra-reward concerns dominate and decreases or even disappears when reputation concerns dominate. At the same time, macroeconomic conditions represented by GDP growth have no effect on forecast errors. These results suggest that the macroeconomic information on GDP growth has been already subsumed in earnings forecasts. Milani (2011) finds that market sentiment predicts future economic activity, with sentiment shocks explaining to a large extent the US output fluctuations at business cycle horizons. This relationship between sentiment and future economic activity suggests using both forces in explaining analysts' ability to incorporate their unexpected components in earnings forecasts.

[1] While prior research explores how economic forces affect aggregate earnings forecasts (see, e.g., Daniel and Mande, 1994), recent studies explore the reverse causality; see, e.g., Basu, Markov and Shivakumar (2010), and Konchitchki and Patatoukas (2014).

[2] Bottom-up forecasters follow individual companies and issue earnings forecasts, recommendations and price targets, whereas top-down forecasters or market strategists predict earnings of various company aggregates (i.e. S&P500 or DJIA).

1.2.3. Forecast error dynamics

Several studies explore the dynamics of analysts' forecast errors. In terms of explanatory power, Brown (2001) argues that past analyst-firm forecast error (i.e., one period lag) explains current analyst-firm forecast error as well as a more complex model incorporating analyst characteristics. In terms of direction, if financial analysts' efficiently integrate past information in their current forecasts (i.e., they learn from past forecasts), then the relation between past and current forecast error should be strictly negative. Alternatively, if there is a persistence effect, the relation between past and current forecast error should be strictly positive. The empirical evidence pertains to the latter case. To illustrate, Ali, Klein and Rosenfeld (1992), Nutt, Easterwood and Easterwood (1999), Clement et al. (2007) and Call et al. (2009) find a significant positive relation between past and current analysts' forecast errors.

From the perspective of obtaining unbiased estimators for past forecast error, the above studies do not fully account for the dynamic panel data structure of the series. Specifically, the models are estimated either with OLS or by imposing a within-group transformation for fixed effects, which do not eliminate the dynamic panel bias, i.e. the autoregressive structure of the forecast error in this case; see, e.g., Roodman (2009). One way to overcome this bias is to use the Arrelano-Bond estimation method based on the generalized method of moments (GMM). In light of the recent developments in estimating dynamic panel data models, it remains an empirical question whether analysts persist in their forecast errors, and if yes, to what extent.

1.3. Data and sample selection

We use I/B/E/S US detail file to collect individual analyst annual earnings forecasts and realized earnings for the period January 1997 to December 2014. We start the sample in 1997 to have sufficient data in terms of macroeconomic forecasts and realized values. Financial firms and utilities (SIG codes 01 and 11 respectively) are excluded. We retain firms with fiscal year ending in December 31st to align fiscal with calendar years for which we have macroeconomic forecasts. Imposing this criterion ensures cross-

sectional comparisons.[3] The timing of the earnings forecasts is depicted in Figure 1.1. We first retain earnings forecasts at the beginning of the fiscal year issued after the release dates of the prior fiscal year realized earnings and the corresponding GDP. This condition imposes that information on prior earnings and macroeconomic output is available to all analysts at the same time. Second, we restrict the forecasts to the period that spams before the release of the first fiscal quarter realized earnings or GDP. The above timeframe of annual forecasts provides a sufficient period of computing unexpected macroeconomic changes starting from the annual earnings forecast's issue until the end of the year.

Furthermore, we retain the last available forecast in the above timeframe per firm-analyst-year and drop forecasts issued by team analysts. We work with unadjusted data and use adjustment factors to allow time-series comparisons. We also transform all data in US dollars and require non-missing data on firm's price and accounting information from CRSP and Compustat. Finally, we retain the firms for which we can compute the one-year lag forecast error and winsorize the dependent variable at 1% on both tails. Table 1.A.1 provides details on the sample selection process.

[Insert Figure 1.1 about here]

To identify analysts' conflicts of interest, we use SDC to extract lead and co-managers for IPO, SEO, debt issues and M&A deals in the last three years from the forecast's announce date. We link the data to I/B/E/S by the firm's parent's CUSIP and hand matching of brokers' names. Macroeconomic data comes from two sources. First, we extract quarterly and annual GDP growth and the corresponding consensus forecasts from the Survey of Professional Forecasters (SPF) available at the Federal Reserve Bank of Philadelphia.[4] Second, to account for expected and realized consumer sentiment at the end of the year, we use the monthly Consumer Sentiment Index (CSI) available at the University of Michigan.[5]

[3] Forecasts corresponding to firms with fiscal year end in December 31st represent 82% of the initial sample. Moreover, imposing this filter does not change the distribution of observations by industries during each fiscal quarter.
[4] Available at https://www.philadelphiafed.org/research-and-data/real-time-center/survey-of-professional-forecasters/data-files/ngdp.
[5] Available at http://www.sca.isr.umich.edu/tables.html

1.4. The role of the forecast error components

In this section, we investigate quantitatively the importance of the existing determinants of analysts' forecast error.

1.4.1. Model specification

We use the following autoregressive panel model.

$$FERROR_{a,i,t} = b_0 + b_1 FERROR_{a,i,t-1} + b_2' Dec(\hat{}^{Y}_{i,t}) \times UCH^{Y}_{t} + b_3' Bias_{a,i,t} + \dagger_{a,i} + £_{a,i,t} \quad (1)$$

where $Y = \{GDP; CSI\}$, subscripts *a, i, t, T* stand for analyst, firm, date of the forecast and the fiscal year end respectively, $\dagger_{a,i}$ are firm-analyst fixed effects, and $«_{a,i,t} \equiv \dagger_{a,i} + £_{a,i,t}$ is the decomposition of the error term.

The dependent variable is the analyst's forecast error computed as $FERROR^{T}_{a,i,t} = (Forecast^{T}_{a,i,t} - Actual^{T}_{i}) \big/ TAS^{T-1}_{i}$, i.e. the difference between the analyst's forecast and actual earnings, scaled by total assets per share at the beginning of the fiscal year.[6] By construction, a positive (negative) value of the forecast error implies an optimistic (less optimistic) forecast.

In line with the breakdown of the forecast error in Section 2, Eq. (1) splits the dependent variable in three measurable variables: the past error $(FERROR_{a,i,t-1})$, the unexpected change in macroeconomic conditions $(Decile\hat{}^{Y}_{i,t} \times UCH^{Y}_{t})$, and bias $(Bias_{a,i,t})$. We detail the construction of each component below.

1.4.1.1. Past forecast error

We include the last year's forecast error per firm-analyst to account for the dynamic structure of the forecast error series. If analysts learn from (are persistent in) their past errors, then b_1 should be significantly negative (positive).

[6] In unreported analysis we use alternatively price as deflator instead of assets per share. The results, available upon request, do not change.

1.4.1.2. Unexpected changes in macroeconomic variables

The magnitude of the relation between forecast error and unexpected changes in macroeconomic variables should depend on the magnitude of the underlying firm's sensitivity to macroeconomic conditions. We capture the firm's sensitivity to GDP (i.e., macroeconomic output) and CSI (i.e., sentiment to macroeconomic conditions) with the parameters $\hat{}^{GDP}$ and $\hat{}^{CSI}$ respectively. For each firm-year in Eq. (1), we use 16-prior quarters of available data to compute the current firm's exposure (see, e.g., DeFranco, Kothari and Verdi, 2009) and estimate the following model.

$$\Delta ROA_{i,q} = \sim_i + {}^{Y}_{i}\Delta Y_q + \Delta {}^{Y}_{i}\Delta Y_q \times DNEG_q + £_{i,q} \quad (2)$$

where subscript *q* corresponds to the quarter of the year *t* when the forecast on firm *i* is issued (i.e. no later than Q3). Δ*Y* denotes the absolute change in the macroeconomic growth from *q-1* to *q*, and *Y* is defined previously.

The dependent variable in Eq. (2) is the firm's absolute change in return on assets from *q-1* to *q*.

$ROA_{i,q} = IBT_{i,q} / TA_{i,q}$, where *IBT* and *TA* denote income before extraordinary items and total assets respectively. *DNEG* is a dummy variable that equals one if $\Delta Y_q < \Delta Y_{q-1}$.[7] Finally, to limit the error in variable bias, we form deciles based on the values of ${}^{Y}_{i}$.[8, 9]

We measure the unexpected change in macroeconomic variables as follows.

$$UCH_t^{GDP} = SPF_{q,t} - g_t^{GDP} \quad (3)$$

$$UCH_t^{CSI} = g_{m,t}^{CSI} - g_t^{CSI} \quad (4)$$

In Eq. (3), $SPF_{q,t}$ represents the SPF forecast for GDP growth in year *t* issued in quarter *q*. We retain the most recent SPF annual forecast available at the time of the earnings forecast announce date;[10] g_t^{GDP}

[7] The slope of Y is not statistically different if we do not include the interaction term.

[8] Since we are interested in the magnitude, rather than the sign, of the firm's exposure to macroeconomic aggregates, we work with absolute values. Results are not sensitive to this choice.

[9] While our objective is to capture firm's sensitivity to these aggregates by the betas, we consider also the R-square of Eq. (2) as a measure for the firm's correlation with the aggregate (see, e.g., Hugon et al., 2015). As such, we consider the two measures simultaneously by constructing a 10 x 10 matrix based on the deciles of the two series. The results are robust to this alternative.

stands for annual percentage GDP growth in year t. In Eq. (4), $g_{m,t}^{CSI}$ denotes the CSI percentage change between the month *m* and the end of the year. Similar to the GDP measure, we retain the most recent CSI monthly value available at the time of the earnings forecast announce date; g_t^{CSI} represents the annual CSI percentage change in year *t*. The signed unexpected error on macroeconomic conditions implies the following interpretation of b_2. A positive value denotes analyst's overreaction to optimistic macroeconomic predictions (i.e., when $UCH > 0$), or alternatively underreaction to pessimistic macroeconomic predictions (i.e., when $UCH < 0$). A negative value denotes analyst's underreaction to optimistic macroeconomic predictions, and alternatively overreaction to pessimistic macroeconomic predictions. In all the above, the analysts fail to integrate in their current earnings forecast the unexpected changes in macroeconomic conditions by the end of the fiscal year. Contrarily, if b_2 is not statistically different form zero, the earnings forecasts already contain this information and analysts are efficient in processing the current macroeconomic predictions.

For robustness, we consider two additional measures for aggregated earnings. The first is the COMPUSTAT universe of firms at quarter *q*, computed as the cumulative product between the firms' income before extraordinary items and the number of outstanding shares, divided by the sum of the firms' total assets. The second is the I/B/E/S universe of firms at quarter *q*, computed as the cumulative product between the firms' EPS (i.e street earnings) and the number of outstanding shares, divided by the sum of the firms' total assets. For the latter measure, we impose minimum 12 (instead of 16)-prior quarters due to missing data. Both measures are similar to GDP and CSI.

1.4.1.3. Bias

To control for the analyst's bias, we consider a list of variables measured at the different dimensions of our unit of observation (i.e., analyst-firm-time). For analyst characteristics, we consider analyst's general and firm experience, the size of analyst's employer (in terms of the number of active analysts in the last

[10] By construction, the earnings forecasts are issued no later than the first month of Q2 (i.e. April) in our sample. As such, the corresponding SPF forecast for year t is issued either in Q2, Q1, or Q4 of the prior year.

365 days), and the analyst's portfolio complexity in terms of the number of firms and number of 2-digit SIG codes industries followed in the last 365 days from the earnings forecast's announce date. For firm characteristics, we consider firm's market capitalization, number of analysts following the firm, firm's goodwill balance (as percentage of total assets), R&D expense (as percentage of total operating expenses), total accruals, and firm's market-to-book ratio.[11] We retain forecast age, the frequency of the analyst-firm forecasts in the last 365 days, the leader-follower ratio and an indicator variable for conflicts of interests as characteristics of firm-analyst dimension. Finally, to account for the regulatory environment, we consider two indicator variables for the passage of RegFD and SOX respectively.

1.4.2. Descriptive statistics

Table 1 reports the summary statistics of the dependent variable, the variables corresponding to bias and the unexpected changes in macroeconomic conditions. The forecast error is on median closer to zero for the entire sample, and ranges from -17% to 36% (percentage as of per-share total assets). In terms of analyst characteristics, the average analyst in our sample has a general (firm-related) experience of 9 (5) years, while the most mature analysts are active for 33 (31) years. Analysts range from specialists (following one firm or industry) to generalists (following a maximum of 203 firms or 11 industries). However, the average analyst portfolio is constituted of 17 firms and two industries, in line with the idea that analysts rather specialize by industry. In our sample, the brokerage houses employ on median 44 analysts, with the largest ones having 370 active analysts during the year.

Concerning the firm characteristics, the average firm in our sample is followed by 17 analysts during the year and has a market capitalization of 2'281 million USD. The sample is skewed towards firms with low ratio of R&D expenses and goodwill balance, with mean values of 0.07% and 0.11% respectively. The mean (median) total accruals represents -0.07% (-0.06%) of total assets. In line with previous studies, this negative value reflects the firms' amortization and depreciation. For example, Bradshaw, Richardson

[11] In unreported results, we also include the HHI of the firm's sales by business segments (Source : Compustat Segment files). Since we lack data to compute this measure for around 20% of the observations and our findings do not change qualitatively in the restricted sample, we present the results without this variable.

and Sloan (2008) find a mean (median) value for the same variable of -0.07% (-0.04%). In terms of current market valuation, the firms in our sample have a median market-to-book ratio of two.

The annual frequency of earnings forecasts per firm-analyst ranges from one to 96. On average, an analyst issues five annual forecasts for one firm over the year. In terms of horizon, analysts issue forecasts at an average distance of 292 days to the end of the fiscal year. A leader-follower ratio greater than one reveals that analysts are rather leaders, i.e. they issue earnings forecasts ahead and at large distance in time compared to their peers. The sample statistics is highly skewed with a mean of the ratio of 2.9 and a median of 0.8 with an interquartile range of 1.72.

Similar to earnings forecast errors, the macroeconomic error corresponding to both GDP and CSI growth is on average zero over the sample timeframe. We observe that, in terms of magnitude, the unexpected change (i.e., the error) in GDP growth is around four-fold smaller than the one corresponding to the CSI change. Not surprisingly, consumers are less efficient at grasping the macroeconomic state than professional GDP forecasters.

[Insert Table 1.1 about here]

In Table 1.2 we present the sample correlations by characteristics class. Panel A reports the correlation of the variables related to analyst characteristics. We note that forecast errors decrease with both measures of analyst experience. While firm and general experience are highly correlated, the magnitude of the correlation between the forecast error series with the former is almost three-fold higher than with the latter. The error is positively related with the analyst's portfolio complexity in terms of number of industries followed. Conversely, the forecast error decreases with broker size, in line with the idea that larger brokers provide more informational resources for analysts. In Panel B, the forecast error is negatively correlated with both firm size and analyst following. In line with Bhushan (1989), we observe that the two latter variables are strongly correlated, showing that larger firms are covered by more analysts than smaller firms; see Brennan and Hughes (1991). We include in the firm characteristics class the firm's exposure to macroeconomic variables, i.e., to GDP and CSI. As this variable is computed with the information available to the analyst at the time of her forecast, we are interested to see how it relates to her

earnings forecast accuracy. We observe that analysts' forecast error is correlated with firm's exposure to macroeconomic variables, suggesting that firms that are more exposed to macroeconomic fluctuations (i.e., both cyclical and countercyclical firms) are more difficult to forecast.

[Insert Table 1.2 about here]

Panels C and D display two interesting characteristics for our sample. First, there is virtually zero correlation between earnings forecast error and conflicts of interest. This finding is in line with the conjecture that analysts are not prone to bias earnings forecasts that are disseminated to more sophisticated investors, as opposed to recommendations ; see, e.g., Malmendier and Shantikumar (2014). Second, forecast errors are negatively correlated with the passage of SOX and RegFD. There is strong evidence of a decrease in analysts' optimism in earnings forecasts – one of the objective of the above laws. Finally, we draw the correlation of earnings forecast error with the macroeconomic errors in Panel E. There is a positive correlation between unexpected changes in both GDP and CSI measures and earnings forecasts. This finding demonstrates that analysts do not efficiently integrate the current macroeconomic expectations when they forecast earnings. Specifically, analysts overreact (underreact) to optimistic (pessimistic) current macroeconomic expectations.

1.4.3. Dynamic panel data estimators

At this stage of the analysis, we are interested to check whether the dynamics of the forecast error series imposes a bias in estimating the parameters corresponding to each of our independent variables. To do so, we estimate Eq. (1) with GMM for dynamic panel data; see, e.g., Arellano and Bond (1991) and Arellano and Bover (1995). This technique became increasingly popular in the last years, as it allows to obtain unbiased estimators under the assumptions of heteroscedasticity and autocorrelation within individuals (i.e., earnings forecast by an analyst *a* on a firm *i*), together with the use of fixed effects; see, e.g., Blundell and Bond (1998), and Roodman (2009).

To avoid multicollinearity and instrument over-identification, we reduce the set of individual variables by eliminating *GEXP*, *NFOLLOW*, and *REGFD*. These variables are highly correlated with *FEXP*, *SIZE*

and *SOX* respectively, and their correlation with forecast error is weaker than the latter three. Note also that due to the inclusion of year dummies, *SOX* is dropped from the model. We return to the entire set of variables in examining their contribution when decomposing the earnings' forecast variance in the following section.

[Insert Table 1.3 about here]

Table 1.3 provides the results. In line with previous studies, pooled OLS specifications in column (1) and (2) show that analysts are persistent in their errors; see, e.g., Brown (2001). The corresponding coefficient b_1 is equal to 0.17 and statistically significant at the 1% level. However, this estimation procedure gives a coefficient b_1 that is upward biased; see Nickell (1981). The opposite happens in column (3) when the analyst-firm fixed effect is introduced. The coefficient b_1 (equal to -0.19) is downward biased. In column (4), the GMM estimation shows a positive association between current and past forecast error. The coefficient b_1 equals 0.05, it is unbiased and statistically significant at the 5% level.[12] As such, we argue that the past earnings forecast error is not the essential factor at play in explaining the current analysts' errors.

Irrespective of the specification, there is a significant positive association between analysts' forecast error and unexpected changes in macroeconomic conditions. Since we multiply the latter variable by firm's exposure to macroeconomic conditions, the interpretation of this positive association is the following. The more exposed the firm is to the economy, the more analysts overreact (underreact) to optimistic (pessimistic) forecasts of the macroeconomic conditions. In economic terms, one standard deviation of the unexpected change in GDP (CSI) x firm's current exposure to GDP (CSI) is equivalent to an increase of 0.06% (0.15%) in analysts' earnings forecast error, or 1.3 (3.2) USD based on the median of 20.3 USD total assets per share in our sample. For comparison, a similar analysis yields to a five cents impact corresponding to prior forecast error[13].

[12] See, e.g., Roodman (2009) for a detailed comparing analysis among the above estimations.

[13] When the exposure of the firm is computed with respect to aggregate earnings, we find similar results. They are available upon request.

Brown (2001) argues that the past earnings forecast error explain the current earnings forecast error inasmuch as a more complex model including firm and analyst characteristics. In light of this conjecture, we run Eq. (1) only with prior forecast error as the right-hand side variable and compare the adjusted R-square in the first three specifications. The adjusted R-square is 2.8% in the simple model. It almost doubles when adding the remaining determinants with year dummies. Moreover, the value almost triples when we add up firm-analyst fixed effects. Taking into consideration the weak magnitude of the prior forecast error, it appears that the dynamics of the series is not the main factor in explaining the current forecast error. On the other hand, the firm-analyst relationship seems to be a core factor. However, it remains unclear what is the individual importance of each existing determinant, and their relative contribution as a whole in explaining the forecast error.

1.5. Variance decomposition of analysts' forecast error

To understand the variation in analysts' forecast error corresponding to each factor, we estimate the following model.

$$FERROR_{a,i,t} = b_0 + \sum_{f=1}^{n} b_f Factor^{f}_{a,i,t} + \alpha_a + \beta_i + \gamma_t + \varepsilon_{a,i,t} \quad (5)$$

where n is the number of variables identified in Model (1), and $\alpha_a, \beta_i, \gamma_t$ denote analyst, firm, and year fixed effects.

Our objective is to identify to what extent the existing factors capture the variation in forecast error. As we noted in the previous section that the dynamics of the series is weak, we focus the analysis on the static specification. We begin by performing the analysis of variance of forecast error per each factor and fixed effect respectively in Eq. (5). Next, we turn to the relative importance of each determinant in the whole model.[14]

[14] Since the baseline sample contains 8'163 analysts, it is infeasible to perform the variance decomposition on the entire panel. To solve this problem, we randomly split the sample to 5% and perform the analysis on this subsample. We repeat the process 100 times and average the results to minimize sampling error; see, e.g., Lemmon, Roberts and Zender (2008) for a similar approach.

1.5.1. One-way analysis

We report the adjusted R-square of the model for each factor in Table 1.4. The first observation from the one-way analysis of variance reveals that the existing determinants have a dismally low contribution in explaining individually the variation in earnings forecast errors. Among the 21 characteristics in Panels A to E, we find that the strongest ones are related to firm characteristics. To illustrate, firm's size (number of analysts following) explain as much as 1.4% (respectively 0.97%) of the total variance in earnings forecast errors. Firm's exposure to macroeconomic conditions in GDP (consumer sentiment) follows and explains 0.97% (respectively 0.45%) of the variation. The third strongest variable is SOX, with a ratio of total variation explained of 0.51%.

[Insert Table 1.4 about here]

The second observation we draw from Table 1.4 is that firm (analyst) fixed effects explain individually 33% (17%) of the variance in forecast errors. On the other hand, year fixed effects fall well short in explaining the forecast error variance. Indeed, compared to the firm and analyst fixed effects, year fixed effects explain individually 2% of variation. As the dynamics of the earnings forecast error appears rather low and the errors seem stable over time, this low ratio is not surprising. As such, analysts' earnings forecast error varies more between- as opposed to within- the firm-analyst dimension. In other words, the error varies more across firm-analyst pairs than within firm-analyst pairs over time. Figure 1.2 visualizes graphically the comparison of the adjusted R-square among the entire set of factors.

[Insert Figure1. 2 about here]

1.5.2. Multivariate analysis

Table 1.5 reports the results on the variance decomposition for the entire set of factors in model (5). To grasp the relative importance of each factor, we proceed as follows. We compute for each estimate the fraction of the total Type III partial sum of squares as the partial sum of squares of the respective factor divided by the aggregated partial sum of squares of all the factors plus the residual in the model.

In column (1), we observe that the existing determinants of the forecast error explain together 2.37% of the variance. Importantly, the analysis confirms the pattern observed in the prior sub-section, where almost half of the above section is represented by the firm size factor (1.18%). In addition to the variables related to bias, the unexpected changes in macroeconomic conditions (multiplied by firm's exposure) explain 0.14% in the variance of the earnings forecast error. When we augment specification (1) with firm fixed effects, the adjusted R-square spikes to 34%. Compared to the one-way model with firm fixed effects, that has an adjusted R-square of 33%, this finding suggest that the observable characteristics have an important firm-related component, since their importance diminishes with the introduction of firm fixed effects. The same remark holds when we augment specification (1) with analyst fixed effects. Nevertheless, the explanatory power of the analyst-related unobserved factor is now 19%, almost half of the one with firm fixed effects.

[Insert Table 1.5 about here]

In column (4), we add time fixed effects to the observable factors. In line with the previous findings, we observe that the time dimension is rather weak in explaining the earnings forecast error variance, and the model has an explanatory power of 4%. At the same time, the observable characteristics are mostly orthogonal to the year fixed effects, since their importance does not change significantly compared to column (1).

Taken together, these results reveal that there is an important, unobserved firm and analyst related component in the forecast error not yet captured by the existing variables. This time-invariant component suggests that much of what constitutes analysts' earnings forecast error is still left unexplained currently. To go further, in column (5) we add both firm and analyst fixed effects to the baseline model from column (1). This last specification has an adjusted R-square of 61%. Importantly, the explanatory power of the model with both firm and analyst fixed effects is higher than these models combined (i.e., 53% when considering the one-way analysis). Not only this model has the highest explanatory power, but shows that the inherent firm-analyst dimension, over and above the firm and analyst individual dimensions, plays a

key role in explaining the forecast error. We provide the graphical comparison of these models in Figure 1.3.

[Insert Figure 1.3 about here]

There are at least three implications to this finding. The first one is that the public and private communication channels between analysts and management is potentially one of the most valuable sources of information for forecasting earnings. For example, Soltes (2014) shows that over and above firm-related news, private communication with management is key for the analysts in forming their forecasts. Moreover, Brown et al. (2015) argue that analysts' private communication with management is a more useful input than analysts' own research. The second implication - and closely related to the first - is that the global regulatory wave trying to limit the spreading of private communication might have leveled the playing field among analysts and investors, but at the same time might have deprive the markets of adjusting to new information more timely. The third implication is related to Hilary and Hsu (2013) who find that analysts displaying "more consistent forecast errors have greater ability to affect prices, and that this effect is larger than that of stated accuracy". Since earnings forecast accuracy strongly depends on the firm-analyst couple, and is time invariant, what really matters for forecast is its consistency.

1.6. Conclusions

This chapter investigates the determinants of analysts' earnings forecast errors. First, we reveal that forecast errors are stable over time, with weak dynamics throughout our sample period of 17 years. Specifically, prior earnings forecast errors are weak determinants of current earnings forecast errors, and analysts do not seem to efficiently integrate past information in their forecasts.

Second, we find that analysts do not factor in expectations related to the macroeconomic conditions for the underlying forecast horizon. Analysts overreact (underreact) to positive (negative) macroeconomic expectations on both GDP and consumer sentiment index. This ex-post information (i.e., unexpected

changes in macroeconomic conditions) is quantitatively as important as the ex-ante information available at the time of the forecasts (i.e., analysts' bias).

Third, we decompose analysts' earnings forecast errors variance by observable characteristics and fixed effects. Importantly, we find that there is an unobserved, time-invariant component related to the firm-analyst dimension that explains much of the variance in the earnings forecast errors. This component is not yet captured by the existing observable characteristics which, at date, have a trifling effect on their own in explaining the variation in analysts' forecast error.

As earnings forecasts are the main input in determining price targets and consequently stock recommendations, much of the process through which analysts process their input remains in a so-called "black box". This study attempts to shed light on these inputs. While we show that the firm-analyst relationship is at the core of the variation in forecast errors, we expect the process through which analysts forecast earnings to gain additional currency from academic research in the next years. Particularly, future work on this area should bring us closer to a more complete understanding on the sources, and the measurement of this relationship.

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Appendix

Table 1.A.1: Sample selection

This table presents the sample construction. All control variables are defined in Table 1.A.2.

Description	Observations
Initial sample	346,861
Less	
EPS forecasts released after the Q1 EPS announcement date or Q1 realized GDP release date	(123,066)
EPS forecasts excluded by I/B/E/S due to inconsistency with the majority rule of EPS computation	(9,379)
Multiple analysts per EPS forecast/firm/broker/date	(10)
Multiple brokers per EPS forecast/firm/analyst/date	(139)
Multiple EPS forecasts per firm/analyst/broker/date	(672)
Multiple EPS forecasts per firm/analyst/broker/year (keep the most recent)	(53,505)
Team analysts	(1,224)
EPS forecast and actual values with no adjustment factors available	(197)
Records with no correponding data in CRSP/COMPUSTAT	(3,336)
Records with no correponding exchange rate data	(43)
Unavailable data on control variables:	
SIZE	(104)
R&D & *TACC*	(1,311)
MTB	(13,822)
$Dec(\widehat{\ }^{GDP}), Dec(\widehat{\ }^{CSI})$	(30,902)
Baseline sample	109,151
Available data on prior year forecast error	43,243
No. firms	2,284
No. analysts	4,788
No.years	17

Table 1.A.2: Variables definition and source

Variable	Name	Definition	Source
$FERROR_{a,i,t}$	Forecast error	Difference between analysts *a*'s annual EPS forecast issued for firm *i* during the fiscal year *t* and the actual annual EPS, scaled by firm *i*'s total assets per share at the beginning of fiscal year *t*.	I/B/E/S CRSP/COMPUSTAT
$ActualEPS_{i,t}$	Realized earnings per share	Realized annual earnings per share for firm *i* in fiscal year *t.*	I/B/E/S
$ForecastEPS_{a,i,t}$	Forecasted earnings per share	Forecasted annual earnings per share by analyst *a* for firm *i* for fiscal year *t.*	I/B/E/S
$TAS_{i,t}$	Per-share total assets	Total assets divided by the number of common shares of firm *i* at the beginning of fiscal year *t*.	COMPUSTAT
$TA_{i,t}$	Total assets	Total assets of firm *i* for fiscal year *t*.	COMPUSTAT
$FERROR_{a,i,t-1}$	Lagged forecast error	Annual EPS forecast error of analyst *a* for firm *i* in the previous fiscal year.	I/B/E/S CRSP/COMPUSTAT
$FEXP_{a,i,t}$	Firm experience	Number of years (including *t*) for which analyst *a* provides annual EPS forecasts for firm *i* in the database.	I/B/E/S
$GEXP_{a,t}$	General experience	Number of years (including *t*) for which analyst *a* provides annual EPS forecasts in the database.	I/B/E/S
$NFIRMS_{a,t}$	Portfolio complexity: firms	Number of firms followed by analyst *a* in the last 12 months from the EPS announce date.	I/B/E/S
$NIND_{a,t}$	Portfolio complexity: industries	Number of two-digit SIG codes industries followed by analyst *a* in the last 12 months from the EPS announce date.	I/B/E/S
$BSIZE_{a,t}$	Broker size	Number of analysts working for analyst's *a* broker in the last 12 months from the EPS forecast announce date.	I/B/E/S
$NFOLLOW_{i,t}$	Firm coverage	Number of analysts issuing annual EPS forecasts for firm *i* in the last 12 months from the EPS forecast announce date.	I/B/E/S
$SIZE_{i,t}$	Market capitalization	Natural logarithm of firm *i* market capitalization at the beginning of fiscal year *t*.	CRSP/COMPUSTAT
$R\&D_{i,t}$	Research & development expense	Percentage of R&D expense in total operating expenses for firm *i* at the beginning of fiscal year *t*.	COMPUSTAT
$GDW_{i,t}$	Goodwill	Percentage of goodwill in total assets for firm *i* at the beginning of fiscal year *t*.	COMPUSTAT
$TACC_{i,t}$	Total accruals	Income before extraordinary items minus total cash flow from operations scaled by total assets for firm *i* at the beginning of fiscal year *t*.	COMPUSTAT
$MTB_{i,t}$	Market-to-book	Market-to-book ratio for firm *i* at the beginning of fiscal year *t*.	CRSP/COMPUSTAT

(continued)

Table 1.A.2
(continued)

Variable	Name	Definition	Source
$Dec(\widehat{\beta}^{GDP}_{i,t})$	Firm's sensitivity to GDP	Decile beta of the regression of quarterly change in return on assets for firm *i* on the quarterly change in realized GDP growth, run over the last 16 quarters from the EPS forecast announce date.	COMPUSTAT, Federal Reserve Bank of Philadelphia
$Dec(\widehat{\beta}^{CSI}_{i,t})$	Firm's sensitivity to sentiment	Decile beta of the regression of quarterly change in return on assets for firm *i* on the quarterly change in Consumer Sentiment Index (CSI) growth, run over the last 16 quarters from the EPS forecast announce date.	COMPUSTAT, Michigan Consumer Sentiment Index
$ROA_{i,q}$	Return on assets	Income before extraordinary items scaled by total assets for firm *i* at the beginning of fiscal quarter *q*.	COMPUSTAT
$IBT_{i,q}$	Income before extraordinary items	Income before extraordinary items for firm *i* at the beginning of fiscal quarter *q*.	COMPUSTAT
$DNEG_{q}$	Negative growth in GDP (CSI)	Dummy variable equal to one if the GDP (CSI) growth in quarter *q* is smaller than the GDP (CSI) growth in the previous quarter, zero otherwise.	Federal Reserve Bank of Philadelphia
$FREQ_{a,i,t}$	Forecast frequency	Number of annual EPS forecasts analyst *a* issues for firm *i* in the last 12 months from the EPS forecast announce date.	I/B/E/S
$FAGE_{a,i,t}$		Number of days between the day of the EPS forecast provided by analyst *a* for firm *i* and the end of the fiscal year *t*.	I/B/E/S
$LFR_{a,i,t}$	Leader-follower ratio	Ratio between the cumulative sum of the days between the current forecast and the two most recent preceding forecasts by different analysts on firm *i* and the cumulative sum of the days between the current forecast and the next two forecasts by different analysts on firm *i*.	I/B/E/S
$CI_{a,i,t}$	Conflicts of interest	Dummy variable equal to one if the analyst *a*'s employer had an investment business deal (IPO, SEO, M&A or debt raising) with firm *i* in the last three years from the EPS forecast announce date, zero otherwise.	I/B/E/S SDC
SOX_{t}	Sarbanes-Oxley Act	Dummy variable equal to one if the EPS forecast is issued after the passage of Sarbanes-Oxley Act, zero otherwise.	I/B/E/S
$REGFD_{t}$	Regulation Fair Disclosure	Dummy variable equal to one if the EPS forecast is issued after the passage of Regulation Fair Disclosure, zero otherwise.	I/B/E/S
$FAGE_{a,i,t}$		Number of days between the day of the EPS forecast provided by analyst *a* for firm *i* and the end of the fiscal year *t*.	I/B/E/S
$UCH^{GDP}_{i,t}$	Unexpected change in GDP growth	Difference between the latest SPF GDP forecast growth available before the EPS forecast announce date and the realized GDP growth in year *t*.	Federal Reserve Bank of Philadelphia
$UCH^{CSI}_{i,t}$	Unexpected change in sentiment	Difference between the latest CSI growth available before the EPS forecast announce date and the CSI growth in year *t*.	Michigan Consumer Sentiment Index

Table 1.1: Descriptive statistics

This table reports the summary statistics of the dependent and control variables. To preserve space, we do not report the binary and discrete series here. All variables are defined in Table 1.A.2.

Variable	Min.	Mean	Median	Max.	St. Dev.
FERROR	-0.173	0.008	0.001	0.360	0.050
Analyst characteristics					
FEXP	1	4.690	4	31	3.600
GEXP	1	8.536	7	33	5.665
NFIRM	1	17.140	16	203	9.694
NIND	1	2.362	2	11	1.462
BSIZE	1	63.657	44	370	60.249
Firm characteristics					
NFOLLOW	1	16.700	15	56	10.314
SIZE	14.864	21.548	21.484	26.961	1.719
R&D	0	0.071	0	0.999	0.153
GDW	0	0.114	0.050	0.810	0.148
TACC	-0.998	-0.066	-0.055	0.905	0.089
MTB	0.267	3.471	2.195	46.857	4.765
Forecast characteristics					
FREQ	1	5.397	5	96	3.187
FAGE	246	291.860	292	337	25.339
LFR	0.012	2.861	0.843	66	7.546
Unexpected changes in macroeconomic variables					
UCH^{GDP}	-0.036	-0.002	-0.002	0.042	0.014
UCH^{CSI}	-0.156	0.006	0.002	0.171	0.060

Table 1.2: Sample correlations

This table presents sample correlation for analyst's forecast error and control variable within each class of characteristics. In each panel, the lower-side (upper-side) of the matrix diagonal denotes Pearson (Spearman) correlations. * represents significance at the 5% level or better. All variables are defined in Table 1.A.2.

Panel A. Analyst characteristics

	FERROR	*FEXP*	*GEXP*	*NFIRM*	*NIND*	*BSIZE*
FERROR	1	-0.0226*	-0.0102*	-0.0016	0.0260*	-0.0272*
FEXP	-0.0389*	1	0.6273*	0.2943*	0.0570*	0.0806*
GEXP	-0.0160*	0.6116*	1	0.3910*	0.1427*	0.0754*
NFIRM	-0.0094*	0.2093*	0.2899*	1	0.2825*	0.1003*
NIND	0.0329*	0.0331*	0.1221*	0.3452*	1	-0.1051*
BSIZE	-0.0341*	0.0766*	0.0605*	0.0252*	-0.1184*	1

Panel B. Firm characteristics

	FERROR	*NFOLLOW*	*SIZE*	*R&D*	*GDW*	*TACC*	*MTB*	$Dec(\widehat{\ }^{GDP})$	$Dec(\widehat{\ }^{CSI})$
FERROR	1	-0.0634*	-0.0828*	-0.0309*	-0.0591*	0.0663*	-0.0271*	0.0173*	0.0526*
NFOLLOW	-0.0794*	1	0.7331*	-0.0754*	-0.0101*	-0.1214*	0.0247*	-0.0592*	-0.1040*
SIZE	-0.1184*	0.6910*	1	-0.0253*	0.1210*	0.0640*	0.1962*	-0.1703*	-0.1895*
R&D	0.0039	-0.0582*	-0.0987*	1	0.0651*	0.0861*	0.3364*	0.1598*	0.1641*
GDW	-0.0251*	-0.0540*	0.0676*	-0.0756*	1	0.1529*	-0.0481*	-0.1188*	-0.2258*
TACC	0.0154*	-0.0759*	0.0886*	-0.0147*	0.0837*	1	0.0492*	-0.1186*	-0.1094*
MTB	0.0149*	-0.0128*	0.1029*	0.2338*	-0.0805*	-0.0162*	1	0.0866*	0.1018*
$Dec(\widehat{\ }^{GDP})$	0.0533*	-0.0458*	-0.1692*	0.2307*	-0.1243*	-0.1380*	0.1068*	1	0.4213*
$Dec(\widehat{\ }^{CSI})$	0.0815*	-0.0954*	-0.1841*	0.2087*	-0.1978*	-0.1354*	0.1114*	0.4213*	1

Panel C. Forecast characteristics

	FERROR	*FREQ*	*FAGE*	*LFR*	*CI*
FERROR	1	-0.0120*	-0.0323*	0.0188*	0.001
FREQ	-0.0355*	1	-0.2000*	0.0517*	0.0495*
FAGE	-0.0195*	-0.2321*	1	-0.2786*	-0.0334*
LFR	0.0119*	-0.0377*	0.0005	1	0.0246*
CI	0.004	0.0491*	-0.0338*	0.0131*	1

Panel D. Institutional environment

	FERROR	*SOX*	*REGFD*
FERROR	1	-0.1071*	-0.0507*
SOX	-0.0713*	1	0.7684*
REGFD	-0.0364*	0.7684*	1

Panel E. Unexpected changes in macroeconomic variables

	FERROR	UCH^{GDP}	UCH^{CSI}
FERROR	1	0.0230*	0.0143*
UCH^{GDP}	0.0047	1	0.2763*
UCH^{CSI}	0.0295*	0.2210*	1

Table 1.3: Regression results

This table presents the results of regressions examining analysts' forecast error (multiplied by 100). Specifications (1) and (2) include firm-analyst fixed effects. Specification (4) is estimated based on Arrelano-Bond system GMM with firm-analyst fixed effects. The last two rows denote the p-value of a test for first- and second-order serial correlation in the first-differenced residuals, asymptotically distributed as N(0,1) under the null of no serial correlation. Robust standard errors (in parenthesis) are clustered at the firm-analyst level. ***, ** and * denote statistical significance at 1%, 5% and 10% respectively. All variables are defined in Table 1.A.2.

	Pooled OLS		Firm-Analyst FE	GMM
	(1)	(2)	(3)	(4)
$FERROR_{t-1}$	0.1716***	0.1701***	-0.1903***	0.0529**
	(0.0153)	(0.0156)	(0.0169)	(0.0229)
FEXP		-0.0120***	0.0079	-0.0077
		(0.0045)	(0.0871)	(0.0047)
NFIRM		-0.0020	0.0027	-0.0032
		(0.0023)	(0.0077)	(0.0023)
NIND		0.0046	-0.0079	0.0164
		(0.0176)	(0.0461)	(0.0177)
BSIZE		-0.0016***	0.0004	-0.0015***
		(0.0003)	(0.0009)	(0.0003)
SIZE		-0.2271***	0.8368***	-0.2168***
		(0.0166)	(0.0895)	(0.0195)
R&D		-0.5695	2.5773	-1.2127***
		(0.3480)	(2.2872)	(0.3543)
GDW		0.3666**	0.5999	0.2949*
		(0.1696)	(0.5930)	(0.1749)
TACC		1.8553***	2.7953***	1.5854***
		(0.4643)	(0.6196)	(0.4691)
MTB		0.0120	-0.0467**	-0.0045
		(0.0091)	(0.0196)	(0.0094)
FREQ		-0.0082	-0.0091	-0.0065
		(0.0058)	(0.0105)	(0.0060)
FAGE		-0.0064***	-0.0016	-0.0049***
		(0.0009)	(0.0011)	(0.0008)
LFR		0.0014	-0.0008	0.0002
		(0.0030)	(0.0032)	(0.0027)
CI		0.1363*	-0.0124	0.1084
		(0.0741)	(0.1168)	(0.0727)
$UCH^{GDP} \times Dec(\widehat{\ }^{GDP}_{i,t})$		0.8297**	1.5316***	0.7264**
		(0.4058)	(0.4333)	(0.3674)
$UCH^{CSI} \times Dec(\widehat{\ }^{CSI}_{i,t})$		0.5018***	0.2456*	0.4423***
		(0.1184)	(0.1369)	(0.1133)
Const.	0.4698***	7.7090***	-17.1474***	7.1237***
	(0.0206)	(0.4977)	(2.3028)	(0.5413)
Year dummies	No	Yes	Yes	Yes
Obs.	43,423	43,423	43,423	43,423
Adj. R-sq.	2.8%	6.4%	9.4%	
p-val. Arellano-Bond test for AR(1)				0.00
p-val. Arellano-Bond test for AR(2)				0.21

Table 1.4: Analysts' forecast error variance decomposition (one-way)

This table presents the variance decomposition of analysts' forecast error per each observable variable and fixed effects separately. * denotes statistical significance of the factor at the 5% level or better. All variables are defined in Table 1.A.2.

Variable	Adj. R-sq.
Panel A. Analyst characteristics	
*FEXP**	0.15%
*BSIZE**	0.12%
*NIND**	0.11%
*GEXP**	0.02%
*NFIRM**	0.01%
Panel B. Firm characteristics	
*SIZE**	1.40%
$Dec(\widehat{\ }^{CSI})$ *	0.97%
*NFOLLOW**	0.63%
$Dec(\widehat{\ }^{GDP})$ *	0.45%
*GDW**	0.06%
*TACC**	0.02%
*MTB**	0.00%
R&D	0.00%
Panel C. Forecast characteristics	
*FREQ**	0.12%
*FAGE**	0.04%
CI	0.00%
*LFR**	0.00%
Panel D. Institutional environment	
*SOX**	0.51%
*REGFD**	0.13%
Panel E. Unexpected changes in macroeconomic variables	
UCH^{CSI} *	0.09%
UCH^{GDP}	0.00%
Panel F. Fixed effects	
Firm FE*	32.85%
Analyst FE*	17.42%
Year FE*	2.15%

Table 1.5: Variance decomposition by observable characteristics and fixed effects

This table reports the variance decomposition of analysts' forecast error for the entire set of observable characteristics and fixed effects. We compute the Type III partial sum of squares for each variable and then normalize each estimate by the sum across all factors plus the residual. For example, in model (2), 33.73% of the variance in forecast error is captured by unobservable firm fixed effects. * denotes statistical significance of the factor at the 5% level or better. All variables are defined in Table 1.A.2.

	(1)	(2)	(3)	(4)	(5)
FEXP	0.01%*	0.00%	0.01%	0.01%*	0.01%
NFIRM	0.01%*	0.00%	0.02%	0.02%*	0.01%
NIND	0.04%*	0.00%	0.02%	0.03%*	0.01%
BSIZE	0.04%*	0.00%	0.01%	0.04%*	0.01%
SIZE	1.18%*	0.37%*	0.26%*	1.25%*	0.14%*
R&D	0.01%*	0.00%	0.06%	0.01%*	0.05%
GDW	0.00%	0.00%	0.02%	0.00%	0.01%
TACC	0.05%*	0.19%*	0.06%	0.02%*	0.08%
MTB	0.07%*	0.05%*	0.06%*	0.06%*	0.04%
FREQ	0.00%	0.00%	0.01%	0.00%	0.01%
FAGE	0.17%*	0.01%*	0.06%	0.14%*	0.01%
LFR	0.00%	0.00%	0.01%	0.00%	0.01%
SOX	0.38%*	0.29%*	0.08%	0.07%*	0.07%*
CI	0.01%*	0.00%	0.02%	0.01%*	0.01%
$UCH^{GDP} \times Dec(\widehat{\ }^{GDP}_{i,t})$	0.03%*	0.04%*	0.05%	0.00%	0.05%*
$UCH^{CSI} \times Dec(\widehat{\ }^{CSI}_{i,t})$	0.11%*	0.00%*	0.06%	0.06%*	0.01%
All observables	2.09%	0.95%	0.80%	1.71%	0.51%
Firm FE		33.73%*			57.32%
Analyst FE			60.09%		31.45%
Year FE				1.63%*	
Adj. R-sq.	2.37%	33.74%	19.01%	3.98%	60.88%

Figure 1.1: Timing of EPS forecast
This figure depicts the interval period considered for the EPS forecasts in the analysis. FYE denotes the fiscal year end for year t.

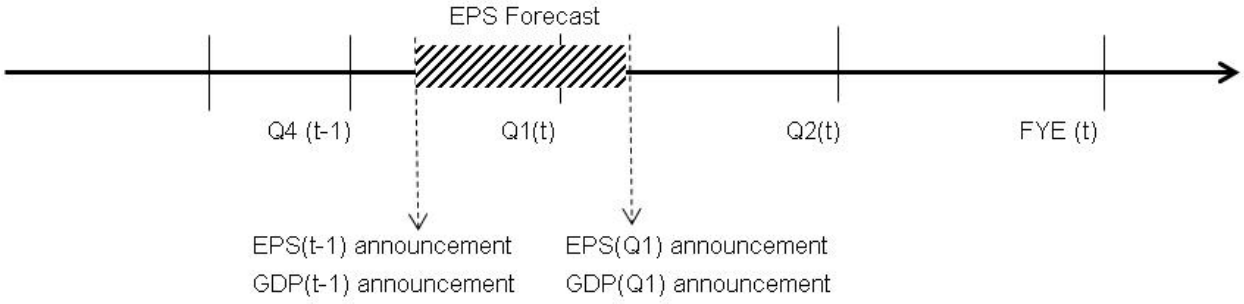


Figure 1.2: Variance decomposition by observable characteristics and fixed effects (one-way)
This figure reports the adjusted R-square from a variance decomposition model of analysts' forecast error on each observable and fixed effect separately. All variables are defined in Table 1.A.2.

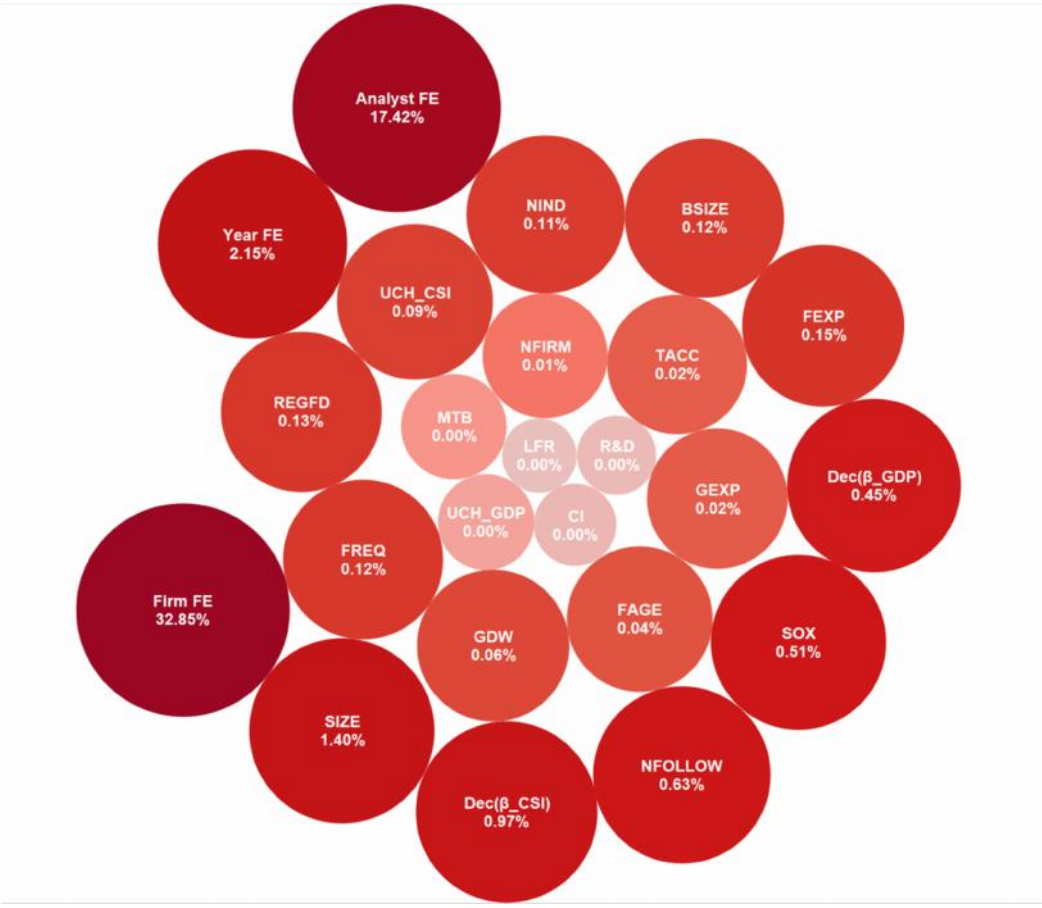
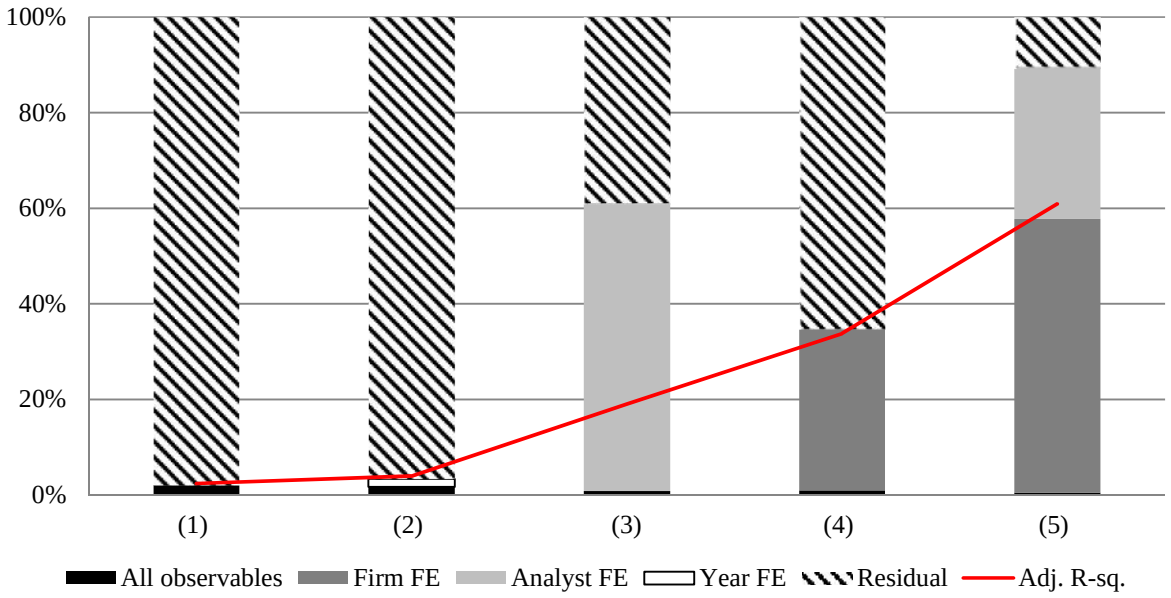


Figure 1.3: Variance decomposition by observable characteristics and fixed effects
This figure presents the percentage of analysts' forecast error variance decomposition in a model including the entire set of observable characteristics and fixed effects. We compute the Type III partial sum of squares for all observable characteristics and each fixed effect and then normalize each estimate by the sum across all factors plus the residual, so that we force each column to sum to 100%. The red line represents the adjusted R-square relative to each specification. All variables are defined in Table 1.A.2.



Chapter 2: Quality versus Quantity: The Role of Reporting Frequency on Financial Analysts' Information Environment

2.1. Introduction

The wide range of financial scandals that emerged in the last decades have set the grounds for a sweeping demand for more information. While disclosure and transparency became the prevalent tone in financial regulation, the real consequences of disclosure regulation remains largely unexplored. Firms listed on the European markets have to follow a mandatory quarterly or semiannual reporting frequency, depending on each country's requirement. More recently, arguing that more frequent information would level the playing field among investors, the European countries enacted the disclosure of interim management statements in 2004 for firms that were not already reporting on a quarterly basis. In November 2013, in stark contrast with its initial objective, the European Commission abolished the requirement to release quarterly information on the grounds that too frequent release of financial statements throughout the fiscal year conducts to management short-termism.

This study investigates whether, and to what extent, financial analysts - a pivotal group of users of financial statements - benefit from the mandatory quarterly financial reporting in Europe. Specifically, I address two questions. First, do firms with mandated quarterly reports and semiannual reports differ in terms of analysts' annual earnings forecast, dispersion and following? Second, to what extent the mandatory adoption of International Financial Reporting Standards (IFRS) increases the convergence of accounting information among firms with different reporting frequency regimes?

On one hand, if mandated quarterly reports decrease information asymmetry among the users of financial statements, the quality of analysts' information environment should increase after the first and third quarter earnings releases, since more information becomes available for these firms. On the other hand, forcing firms to issue financial statements too frequently may constrain the management to focus extensively on the short-term, at the expense of long term performance. Consequently, the required

additional reports may bring more noise than information, and analysts should experience larger earnings forecast errors and higher disagreement for such firms. I find support for the latter case. Among 14 European countries, the difference in the annual earnings forecast error (dispersion) between mandatory quarterly and semiannual reporting firms is from 0.70 (0.41) percentage points (pp) to 1.52 (0.43) pp (statistically significant at 5% and 1% (1%) level respectively).

In terms of analyst following, during the last fiscal quarter, mandatory quarterly reporting is associated with more analyst coverage. To illustrate, during the last three months of the year, quarterly reporting firms are followed by 19% more analysts than semiannual reporting firms (difference statistically significant at 5% level). Importantly, analysts are likely to increase coverage on quarterly mandatory reporters as long as these firms operate in a strong enforcement regime and thus credibly commit to more frequent mandatory disclosure. However, while mandatory quarterly reports attract more analyst following, they are nevertheless associated with greater forecast error and dispersion. These results are consistent with Ernstberger, Link and Vogler (2011) who show that, for a similar sample of European firms, mandated quarterly reporters exhibit higher real activities manipulation compared to semiannual reporters in years in which a firm just meets, or beats past years' earnings or the zero benchmark.

Assessing the efficiency of mandatory quarterly disclosure in isolation is tenuous because one needs to account for the concomitant regulatory reforms addressed to achieve financial convergence. Specifically, the adoption of IFRS in 2005 mandates a common accounting framework for the European listed firms, while leaving the issue of the reporting frequency at the national discretion (IAS 34.1). As such, I go one step further and check whether financial analysts account for the adoption of IFRS under different reporting frequency. The interaction between mandated IFRS and several frequency reporting regimes provides a natural framework to investigate the relative importance of accounting standards.

Two key findings emerge from this analysis. First, consistent with extant evidence, analysts' errors decrease significantly for the entire universe of firms and throughout all quarters of the fiscal year after the mandated IFRS. Importantly, once they report under IFRS, firms with mandated quarterly financial statements experience a decrease (an increase) in the forecast error and dispersion (analyst coverage). For

example, analysts improve their forecast accuracy by 0.48 pp (p-value < 0.05), and by 0.72 pp (p-value = 0.00) in the first, and respectively the fourth quarter. Second, the positive difference between quarterly and semiannual reporters in terms of analysts' forecast error and dispersion continues to exist under the mandatory IFRS. From the perspective of analysts' information set, these results suggest that mandated common accounting standards dominate mandated quarterly reporting. These results are robust to several specifications and after controlling for firm differences in the quality of institutional and accounting environment. As such, while IFRS does benefit analysts by decreasing (increasing) forecast error and dispersion (coverage) in absolute terms, they do not wipe out the pernicious relative noise associated with mandatory quarterly statements.

This paper contributes to three streams of research. First, it increases our understanding of what types of disclosure firms should provide, and which ones are useful and credible. This subject is highlighted by Dumontier and Raffournier (2002) as it remains largely unexplored at the European level. Second, I add to the debate on whether accounting standards play a beneficial role in "stabilizing expectations" in financial markets when information is incomplete; see Healy and Palepu (2001); Leuz and Wysocki (2008). Third, this study answers to Ramnath, Rock and Shane (2008) call for more empirical research on the effects of institutional and regulatory environment on analysts' output.

The remainder of this chapter is organized as follows. Section 2.2 describes the milestones in the financial harmonization process in Europe. Section 2.3 presents the literature review and the hypotheses development. In Section 2.4 I provide the details on the methodology. Section 2.5 outlays and discusses the results. Section 2.6 provides the robustness tests and Section 2.7 concludes.

2.2. The harmonization process of financial reporting in Europe

2.2.1. The historical context

In mid-2000, the European Commission set the grounds for a common financial reporting strategy, with the major goal to enhance the comparability of companies' financial statements:

"To achieve this objective, the Union requires common financial reporting standards – standards that are transparent, fully understood, properly audited and effectively enforced. Only with such standards will there be potential to allow the EU securities markets to grow from its present level of around half the size of the US capital markets. [..] Similarly, periodic financial reporting also has a crucial role to ensure transparency, provide safeguards for investors and contribute to the overall stability of markets." (European Commission, 2000)

[Insert Figure 2.1 about here]

In the pressure to whittle down corporate differences in financial reporting, the next years followed with the adoption of several directives. In 2001, the Commission required semiannual reports to be published within no more than four months after the end of the six-month period.[1] IFRS were formally passed into law in 2002 with mandatory adoption for all European listed companies with accounting periods starting on or after January 1st, 2005.[2] One year later, the Market Abuse directive was enacted to prohibit the use of inside information and disclosure of such information to privileged parties.[3] In 2004, the Transparency Directive mandated interim management statements for all companies starting from January 20, 2007. More specifically, all companies that were not publishing already quarterly financial statements (i.e., all semiannual reporting companies), were mandated to publish during the first and third fiscal quarter a management statement containing "an explanation of the material events and transactions that have taken place in the (six-month) period and their impact on the financial position", as well as "a general description of the financial position and performance". Additionally, the directive required that annual financial statements remained publicly available for at least five years and reduced the timeliness of semiannual financial reports to two months after the end of the six-month period[4]. Figure 2.1 details the timeline of the harmonization process described above.

[1] See Directive 2001/34/EC, Article 72.
[2] See Regulation (EC) No 1606/2002.
[3] See Directive 2003/6/EC.
[4] See Directive 2004/109/EC, Articles 4, 5 and 6.

2.2.2. Recent developments

Countries such as the UK and Denmark, with a tradition in semiannual reporting, drastically disfavor mandating more frequent financial reporting, arguing that these may impose unnecessary administrative burdens for companies and may short-term manager myopia over long-term firm performance.[5] As such, the political debate at the European level over a common reporting frequency converged into a compromise of interim management statements requirement for all Member States starting with 2007. In 2010, the Commission conducted a series of public consultations on the effectiveness of such requirement, whose conclusions were included one year later into a proposal for amending the Transparency Directive. In stark contrast with the initial goal of increasing frequency reporting among all European listed firms, the proposal affirms the need for investors to refocus on long-term, rather than short-term firm performance, and suggests abandoning the quest of harmonization in terms of quarterly reporting.[6] Finally, in November 2013, these new amendments were adopted with required implementation by November 2015:

"Those obligations [to publish interim management statements or quarterly financial reports] encourage short-term performance and discourage long-term investment. In order to encourage sustainable value creation and long-term oriented investment strategy, it is essential to reduce short-term pressure on issuers and give investors an incentive to adopt a longer term vision. The requirement to publish interim management statements should therefore be abolished. Member States should not be allowed to impose in their national legislation the requirement to publish periodic financial information on a more frequent basis than annual financial reports and half-yearly financial reports." (European Commission, 2013)

These recent amendments not only abolish any type of mandatory quarterly reporting, but also extend the period of publishing the semiannual financial statements from two to three months "to provide more flexibility". On the other hand, annual financial statements have to remain available to the public for an extended period of at least ten years. The harmonization process of financial reporting does not stop however with these measures. According to the above Directive, by January 1st, 2020, annual financial

[5] See, e.g., the Kay Review (2012).
[6] See Proposal for a Directive of the European Parliament and of the Council amending Directive 2004/109/EC.

statements will be prepared in a single electronic reporting format to make "reporting easier and facilitate accessibility, analysis and comparability".[7]

2.3. Literature review and hypotheses development

2.3.1. Mandatory interim financial statements

The quality of the financial reporting environment is a prerequisite for firms to disclose relevant, timely and sufficient information. On one hand, the users of financial statements should benefit from increased reporting frequency in better disseminating a firm's current and future prospects. Moreover, a frequent reporting regime should impose greater discipline on the firm's investment by curbing incentives to invest in negative net present value projects (Gigler, Kanodia, Sapra and Venugopalan, 2014). On the other hand, as long as the costs to prepare and disclose such information are high, the quality of reporting may diverge from this benefit (Wagenhofer, 2014). Current evidence pertaining to each approach indicates that this subject warrants further investigation.[8] Fu, Kraft and Zhang (2012) show that higher reporting frequency reduces information asymmetry and the cost of equity. Consistent with this conjecture, Botosan and Harris (2000) find that firms with quarterly segment disclosures are associated with more analysts following.

Rahman, Tay, Ong and Cai (2007) document that quarterly reporting is associated with analyst following, but also with higher price volatility. There is evidence that the quality of such disclosure is also important. Specifically, Lang and Lundholm (1996) find that firms with more informative disclosure policies have larger analyst following, more accurate and less dispersed earnings forecasts. The authors argue that although firms that disclose more information have a larger pool of potential investors, direct contact with the company remains a primary source of information for analysts. DeFond, Hung and Trezevant (2007) suggest that earnings announcements are more informative in countries with higher earnings quality. They also show that firms with more frequent interim reports have less informative

[7] See Directive 2013/109/EC, Amendment (4) and paragraph (26).

[8] See Healy and Palepu (2001), Beyer, Cohen, Lys and Walther (2010) and Leuz and Wysocki (2008) for detailed reviews and suggestions for future research on financial reporting.

annual earnings announcements, since the earnings information is likely to be already impounded in prices.

From the perspective of regulating the frequency of financial disclosure, Brown and Pinello (2007) document that mandated quarterly reporting provides managers with more opportunities to manage earnings upward, compared to annual independent audited financial statements. At the same time, management decreases the magnitude of downward guidance during the fiscal year. Likewise, Mendenhall and Nichols (1988) show that managers delay bad news to the end of the year. Notably, the authors find that analysts' quarterly earnings forecasts are higher in the fourth quarter than in the first three quarters of the fiscal year. Butler, Kraft and Weiss (2007) investigate the earnings timeliness conditional on the firm's reporting frequency. They analyze US firms that either disclose voluntarily quarterly reports or are mandated by the SEC to do so and find little evidence in the timeliness between semiannual and quarterly reporters.

While empirical research on the above topic is abundant in US, a handful of studies try to pin down the effects of interim financial reporting in Europe. In a comparative study, Mensah and Werner (2008) find that quarterly reporting in the US and Canada accentuates stock price volatility compared to markets in the UK and Australia. Cuijpers and Peek (2010) conjecture that voluntary increases in reporting frequency in Denmark, the Netherlands and the UK decrease information asymmetry among investors, increase share liquidity and stimulate trading. Similarly, Alves and Teixeira Dos Santos (2008) test the incremental information content of Portuguese firms between 1994 and 2004 and document both price and trading reactions to unaudited first and third quarter. This evidence holds both before and after 1999, when quarterly reports became mandatory. In addition, audited mandatory quarterly statements spur more significant price reactions than the unaudited ones, consistent with the view that market accounts not only for the amount, but also for the quality of the information released. Von Eije and Megginson (2008) find that financial reporting frequency for 15 European countries between 1989 and 2005 is associated with higher payout, suggesting that firms with more frequent reporting are more likely to forego overinvestment and return cash to shareholders.

Based on the recent regulatory reforms, Link (2012) examines to what extent European firms required to report quarterly financial statements would switch to semiannual reporting, provided such requirement would be abolished. The author contends that, with the exception of firms listed in Greece and Italy, the majority of firms would follow a semiannual reporting regime, if free to choose the reporting interval. At the same time, Ernstberger et al. (2011) find that quarterly reporters exhibit higher level of managers' use of real activities manipulation than semiannual reporters. These results support the view that frequent financial reporting leads managers to focus on short-term performance at the expense of long-term performance; see, e.g., Graham, Harvey and Rajgopal (2005). There is also evidence that financial analysts attach a great importance to earnings figures, while their interpretation of accounting data is rather perfunctory (Dumontier and Raffournier, 2002). At date, however, no study investigates the impact of the frequency reporting on analysts' information dissemination in Europe. The question of whether increased reporting frequency benefits financial intermediaries is all the more important in light of the ongoing process of regulatory reforms. The first null hypothesis is stated as follows.

Hypothesis 2.1. *The quality of analysts' information environment is not associated with mandatory quarterly financial statements.*

The evidence gathered so far suggests that high frequency reporting is not a necessary and sufficient condition for high quality information. At the same time, assessing the efficiency of mandatory quarterly disclosure in isolation is tenuous in light of the concomitant regulatory reforms to achieve financial convergence in Europe. Specifically, the adoption of IFRS in 2005 mandates a common accounting framework for the European listed firms, irrespective of their reporting frequency.

2.3.2. Mandatory IFRS adoption

The current literature on the IFRS adoption argues that firms reduce their information asymmetry, improve their information quality, and enhance their transparency and comparability; see, e.g., Barth, Landsman and Lang (2008), Landsman, Maydew and Thornock (2012), Hung and Subramanyam (2007), Barth, Landsman, Lang and Williams (2012), Brochet, Jagolinzer and Riedl (2013). These benefits fall

also on financial analysts. Horton, Serafeim and Serafeim (2013) find that after IFRS adoption, earnings forecast accuracy and the quality of firms' information environment increase significantly for mandatory adopters in 46 countries. Moreover, the effect is stronger for analysts with portfolios that move from local (or multiple GAAP) to IFRS. Tan, Wang and Welker (2011) argue that IFRS adoption attracts more foreign analysts and improves foreign analysts' earnings forecast accuracy. Jiao, Koning, Mertens and Roosenboom (2012) document an increase in earnings forecast accuracy and analyst agreement after the switch to IFRS in 19 European countries. The inclusion of more relevant information enables analysts not only to provide more timely estimates, but also to increasingly form similar earnings expectations. Indeed, Dargenidou and McLeay (2010) indicate that cross-country variation with earnings forecasts becomes immaterial under the mandatory IFRS adoption in Europe. Indeed, analysts' forecast errors increase as long as a firm uses atypical accounting method choices compared to its peers (Bradshaw, Miller and Serafeim, 2009). From this perspective, the use of a common set of standards should imply greater comparability and higher analysts' accuracy.

In spite of worldwide political efforts to provide globalized financial markets with a single set of accounting standards, uneven implementation among countries curtails the ability to assess the quality of such standards. Specifically, a large body of literature suggests that IFRS adoption conducts to capital market benefits conditional on legal enforcement and corporate incentives for transparency; see, e.g., Daske, Hail, Leuz and Verdi (2008), Christensen, Hail and Leuz (2013), Ball, Robin and Wu (2003), Soderstrom and Sun (2007). Indeed, Filip and Raffournier (2013) suggest that despite common accounting standards, value relevance of earnings in Europe continues to depend heavily on country characteristics. The properties of analysts' earnings forecasts are similarly prone to differences in institutional backgrounds. Preiato, Brown and Tarca (2013) compare several enforcement indicators in 37 countries that mandate IFRS. The authors find that legal and accounting enforcement are associated with greater analysts' accuracy and less dispersion. Likewise, Byard, Li and Yu (2011) and Bae, Tan and Welker (2008) show that mandated IFRS decreases analysts' forecast error and dispersion in countries with both strong legal enforcement and domestic accounting standards that differed significantly from IFRS. Using a

sample of German firms, Glaum, Baetge, Grothe and Oberdorster (2013) separate the effects of changes in disclosure quality from other effects of IFRS introduction to test the changes in analysts' accuracy. They find that increased quality of company disclosure contributes to increased forecast accuracy.

The interaction between mandated IFRS and several frequency reporting regimes in Europe provides a natural framework to investigate to what extent common reporting frequency and common accounting standards dominate one another. As such, I check whether financial analysts account for the adoption of IFRS under different frequency reporting. Specifically, I hypothesize the following.

Hypothesis 2.2. *The effect of IFRS on the quality of analysts' information environment is unrelated to firms' reporting frequency.*

Examining the interaction between mandated IFRS and the reporting frequency is important for at least three reasons. First, it is unclear to what extent the frequency of accounting information is conditional on the accounting standards used to communicate this information. Second, to properly assess the effects of IFRS on analyst's accuracy, the amount (i.e., the frequency) of information released by the firm has to be constant. Third, the process through which financial reporting and disclosure regulations are designed and implemented in Europe lacks of substantive research to properly assess its current efficiency. The two hypotheses fill this gap by building on the dynamics of such regulations.

2.4. Data and methodology

2.4.1. Sample construction

I start by collecting annual earnings forecasts and realized earnings from IBES International Historical Detail files for fiscal years ending between January 1st, 2000 and December 31st, 2010 for all non-financial firms listed in 16 European countries. I choose the above timeframe for three reasons. First, the implementation of European financial convergence by the formal adoption of the Euro and the financial harmonization process begins in early 2000. Second, it allows me to account for a majority of countries that, before or starting 2000, mandate quarterly financial statements. Third, it provides a wide-ranging window around 2005 to analyze the effects of the mandatory IFRS adoption. The initial list of countries

consists of Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, the Netherlands, Norway, Portugal, Spain, Sweden, and UK. These countries were at the core of the Lamfalussy process initiated in 2000 for harmonizing the financial service industry in the EU. I include Norway since it is a member of the European Economic Area and participates in the EU's internal market.

Accounting standards, earnings reporting frequency, fiscal year end date, total assets and the number of outstanding shares are from Worldscope. I match IBES and Worldscope data via firm's IBES ticker and fiscal year. The Worldscope fields are displayed in the Table 2.A.2. I follow Tan et. al (2011) to classify firms under the IFRS or local accounting standard regime. Concerning the earnings reporting frequency field, Worldscope mentions that "if there is no data on this field, a quarterly reporting frequency can be assumed". For these missing observations, I extract also the frequency reporting field on a quarterly format, and identify mismatches where the available reporting frequency in this field is either semiannual or annual. Therefore, I assume a quarterly reporting frequency only when the Worldscope earnings reporting frequency field extracted in quarterly format reports as such.

To identify the country frequency reporting regime, I use various sources at each Member State level. Quarterly reporting may be mandated by the national securities market regulatory authority, by the securities market operator, or for different markets segments within a listing exchange. Specifically, all firms listed in Greece, Finland, Italy, Norway, Portugal, Spain and Sweden, and only firms listed in the Prime market segment in Austria and Germany are required to issue quarterly financial reports.[9] I obtain the constituents of these market segments from Worldscope. The rest of the countries - Belgium, Denmark, France, Ireland, Netherlands, and UK - follow a semiannual reporting regime.[10]

Several data adjustments and filters are performed. First, I replace 31 EPS actual values by restatements available in IBES. Analysts' EPS forecasts excluded by IBES due to differences in the analyst's and the underlying firm's accounting methods are discarded. Also, I use the IBES currency

[9] Note that Italy, Spain and Norway abolished the requirement of quarterly financial statements once they adopted the Transparency Directive in 2009, 2007 and 2008 respectively. Nevertheless, the majority of firms did not switch to a semiannual regime and continued to follow voluntarily quarterly financial reporting.

[10] See Link (2012) for a detailed overview of each country's interim disclosure regulation and the corresponding securities and stock market regulators.

exchange rate file to translate all values in EUR. Following Payne and Thomas (2003) and Baber and Kang (2002), I use the IBES unadjusted files and account for stock splits to obtain adjusted data accordingly. Acker and Duck (2009) report inconsistencies in the earnings announcement dates between Thomson Reuters's IBES and Worldscope database. In the same line, I identify differences in the fiscal year end date between these two databases for four firms in my sample, and I drop the corresponding observations. Next, I eliminate firms that cross-list in US (sponsored ADRs)[11] (330) and in more than one country in a fiscal year (7). Due to inconsistency, I drop firms that are based in more than one country in a fiscal year according to IBES (28), as well as firms that report on a semiannual basis in mandatory quarterly reporting regimes (26). I next eliminate firms that report under US GAAP (74). To control for self-selection bias, I eliminate firms that voluntarily report under IFRS before 2005 (453), as well as firms in semiannual reporting countries that voluntarily report on a quarterly basis (334). Finally, I require that a firm appears in the sample at least one fiscal year both before and after IFRS and run my analysis on earnings forecasts released during the last two months of each fiscal quarter. Table 2.A.1 provides the details on the final sample selection.

Table 2.1 depicts the distribution of firm-year observations by country. Overall, the final sample consists of 10'931 firm-years with 30% (70%) of the observations under mandatory quarterly (semiannual) reporting frequency.[12]

[Insert Table 2.1 about here]

2.4.2. Empirical design

The focus of my analysis is to investigate to what extent financial analysts benefit from increased reporting frequency and harmonization of accounting standards. For each firm, I use three proxies for analysts' information environment. First, for each firm-analyst-year-quarter, I compute analysts forecast

[11] I use BNY-Mellon and JPMorgan ADR online databases and match firms by their US ISIN and underlying ISIN respectively.

[12] There are two potential concerns for which results may be driven by the sample of UK firms. First, they represent 47% of the sample observations. Second, since a large amount of financial analysts are most likely located in UK and cover several European countries, the forecast error for UK firms can be smaller provided their geographical proximity. In subsequent tests, I eliminate these firms. Although the sample size reduces considerably, the inferences do not change.

error (*FError*) as $\left|\text{ForecastEPS}_{a,i,q,t} - \text{ActualEPS}_{i,t}\right| / \text{APS}_{i,t}$, where *ForecastEPS* is analyst *a*'s annual earnings forecast for firm *i* for the fiscal year *t* issued during the current quarter *q*, *ActualEPS* is firm's actual annual earnings, and *APS* are firm's total assets per share at the beginning of the fiscal year; see Qian (2009)[13]. Second, for each firm-year-quarter, I measure analysts' forecast dispersion (*Disp*) as $\text{sd}\left(\text{ForecastEPS}_{a,i,q,t}\right) / \text{APS}_{i,t}$, where sd(ForecastEPS) is the standard deviation of analysts' earnings forecasts for firm *i* for the fiscal year *t* issued during the current quarter *q*. For ease of exposition, I multiply both *FError* and *Disp* by 100 and finally winsorize both measures at 1% at both tails. To measure the gradual impact of the information released via interim reports during the fiscal year, I compute *FError* and *Disp* at the end of each quarter *q* (last two months) of the fiscal year, i.e., when the information related to the previous quarter has been already made public. Specifically, I retain the first forecast for an analyst-firm pair issued in each period. Figure 2.2 illustrates the construction of the forecast error over the fiscal year. Third, for each firm-year-quarter, I calculate the number of analysts following the firm (*LogFollow*) as the log of the total number of analysts that cover the firm during the entire fiscal quarter *q*.

[Insert Figure 2.2 about here]

To test my hypotheses, I use the following model.

$$InfoEnv_{a,i,q,t} = \ _0 + \ _1 QTR_i + \ _2 IFRS_{i,t} + \ _3 QTR_i \times IFRS_{i,t} + ¥ Controls_{a,i,t} + £_{a,} \quad (1)$$

where subscripts *a, i, q, t* denote analyst, firm, fiscal quarter and year respectively.

The dependent variable *InfoEnv* is *FError*, *Disp*, or *LogFollow*. The magnitude of the mandatory quarterly reporting is estimated by QTR_i. This variable equals one (zero) if firm *i* publishes mandatory quarterly (semiannual) financial statements in fiscal year *t*. $IFRS_{i,t}$ equals one if firm *i* follows mandatory IFRS in fiscal year *t* and zero otherwise.[14] This variable captures the difference in analysts' information set between the post and pre period of mandatory IFRS adoption. In line with previous studies, I control for a series of analysts and firm characteristics. Specifically, I add analyst's general and firm specific

[13] The results remain qualitatively unchanged when I use alternative deflators such as absolute actual earnings and stock price at the beginning of the fiscal year.

[14] While the vast majority of the firms in my sample adopt mandatorily IFRS starting from 2005, a small number of firms delay the adoption for 2006 and 2007. As such, I add the subscript *i* for the sake of precision.

experience, the number of firms, industries and countries covered by the analyst during the last twelve months, forecast age, broker size, and firm size; see, e.g., Clement (1999), Bolliger (2004) and Sonney (2009). Since the country's interim financial reporting regime is likely to be associated with institutional characteristics, I control for the level of enforcement, the audit quality (Brown, Preiato and Tarca, 2014) [15] and the distance between the local accounting standards and IFRS (Bae et al., 2008).[16] Table 2.A.2 describes the construction of the variables.

I also use an alternative specification where I include country-industry and time fixed effects to control for the time trend and time-invariant heterogeneity within-country across industries, within-industries across countries and across industries-countries. Since QTR_i does not change over time for a given country-industry pair, it gets subsumed by the country-industry fixed effects. Similarly, the year fixed effects capture the $IFRS_{i,t}$ dummy variable. As such, the two indicators are dropped out from Eq. (1) in this alternative specification.[17]

To identify the presence of a fixed effect, I cluster standard errors by one and alternatively more dimensions and compare the standard errors of the estimates; see Gow, Ormazabl and Taylor (2010), Petersen (2009), Gormley and Matsa (2014). Additionally, I assess the magnitude of the correlation between each dimension with the observed covariates. The organization of the dataset is based on the assumption that country characteristics are central to the interpretation of my results. Indeed, since firms are country invariant, the mandatory quarterly reporting regime is fundamentally associated with the country dimension. As such, I control for any other country feature potentially correlated and form pairs

[15] ENFBPT and AUDBPT variables are available for 2002, 2005 and 2008. To insure enough variation in the index during my timeframe period, I distribute the firm-year observations among the values of 2002 (years <2004), 2005 (years between 2004 and 2006) and 2008 (years>2006). The inferences do not change if I alternatively change the scale to values of 2002 (years<2005), 2005 (year=2005) and 2008 (years>2005).

[16] While the first two variables vary by country-year, the latter varies only by country, thus it gets dropped when I simultaneously introduce it into Eq (1). To control for this variable, I later split the sample firms between low and high distance to IFRS.

[17] See Jayaraman and Verdi (2014) for a similar empirical design.

by industry to account for the time invariant sector characteristics. In both specifications, standard errors are adjusted for heteroskedasticity and clustered at the country-industry level.[18]

In Eq. (1), $_1$ captures the effect of publishing mandatory quarterly statements across firms in the pre IFRS period, while $_2$ represents the time series change with the adoption of IFRS for the semiannual reporting firms. In both equations, the coefficient for the second-level interaction, $_3$, captures the difference in the analysts' information set across mandatory quarterly and semiannual reporters in the time series effect of mandatory IFRS adoption. According to the first null hypothesis, there should be no difference in analysts' information set between firms with mandatory quarterly and semiannual reports ($_1 = 0$). In line with the second hypothesis, the mandatory IFRS adoption should have no impact on the difference in analysts' information set between quarterly and semiannual reporters. Hence, $_3$ should be zero.

2.5. Results

2.5.1. Univariate results

Table 2.2, Panel A reports the annual distribution of the firms in the sample according to their reporting frequency. As mentioned in the previous section, the proportion of mandatory quarterly and semiannual reporting firms is on average one to two thirds during the sample period. Since I require firms to be present in the sample before and after IFRS, the number of firms is generally constant throughout the entire period. In line with previous literature, I identify an increasing trend in the number of earnings forecasts. To illustrate, the earnings forecasts increase by 50% from 2000 to 2010. During the fiscal year, the average analyst issues 21 forecasts, while the average firm receives 103 forecasts.

Panel B presents the descriptive statistics of the control variables. The average analyst experience with issuing earnings forecasts is ten years, while the average specific experience with a firm is three years. Not

[18] Barth and Israeli (2013) point out to a similar cross-country research design where the choice of the variable used to cluster standard errors leads to different statistical inferences. While using, as the authors suggest, the most conservative method to account for the dependency in the data, my inferences are not sensitive to this choice.

surprisingly, these numbers are higher compared to prior studies since I use a more recent time period.[19] On a one-year basis, the average analyst covers 13 firms in three industries and two countries. The coverage universe increases slightly compared to studies on earlier periods. For example, on the same basis, an analyst follows seven firms in two industries and two countries during the 1994-2004; see Bolliger (2004). This is a consequence of the increased portfolio complexity, coverage and data availability in recent years. Generally, the forecasts are issued around the seventh month in the fiscal year (i.e., on a distance of 161 days from the fiscal year end). The mean (median) of the analysts employed at the broker level is 110 (53), confirming the skewed distribution of the brokers' size, where a small number of brokers employ a large number of analysts.

[Insert Table 2.2 about here]

The quality of analysts' information environment by reporting frequency is depicted in Table 2.3. I identify a consistent decrease in both forecast error and dispersion for mandatory quarterly and semiannual reporters throughout the fiscal year. For both types of reporters, analysts have the highest (lowest) error during the first (last) half of the fiscal quarter. To illustrate, the average forecast error for mandatory quarterly (semiannual) reporting firms decreases by 47% (34%) between the first and the last fiscal quarter during the entire period. Similar to previous findings, analysts' error is an increasing function of the forecast horizon. The average forecast dispersion for mandatory quarterly (semiannual) reporting firms is the highest in the second quarter and decreases by 9% (7%) by the end of the year.

The gap in forecast error and dispersion between quarterly and semiannual reporting firms is significantly positive (p-value = 0.00) during each quarter. This initial finding suggests that the mandatory quarterly reports are likely to bring more noise in analysts' information set compared to semiannual financial statements. The difference in both measures is also economically significant. To wit, the magnitude of the gap in forecast error goes from 1.3% in Q1 to 0.42% in Q4. Likewise, the difference in analysts' disagreement for quarterly and semiannual firms is between 0.33% in third quarter to 0.45% in the first quarter. Importantly, the positive difference in forecast error between quarterly and semiannual

[19] Sonney (2009) and Bolliger (2004) find an average general (firm) experience of four (two) and respectively two (two) years using a sample period from 1990 to 1999, and from 1994 to 2004 respectively.

reporters decreases considerably along the fiscal year. For example, during the entire period, the gap in forecast error declines by 68% between the start and the end of the year (i.e., 1.3% in the first quarter, 0.8% in the second quarter, 0.7% in the third quarter and 0.4% in the fourth quarter). Similarly, the difference in forecast dispersion decreases as the year end approaches. Consequently, the closer the year end, the smaller the difference in analysts' information set between quarterly and semiannual reporters.

The evidence so far suggests that, while higher reporting frequency is associated with more noisy information, the information set increases consistently towards the end of the year; see Elton, Gruber and Gultekin (1984). Not surprisingly, quarterly reporting firms are followed by more analysts than semiannual firms over the fiscal year (i.e., in three of the four fiscal quarters). Taken together, these first results suggest that, while analysts are more likely to follow firms with more frequent disclosure, they do not manage to provide more accurate, or to agree more, on their forecasts along the fiscal year for quarterly reporting firms (compared to semiannual reporters).

Consistent with extant evidence, the mandatory introduction of IFRS in 2005 decreases analysts' forecast error and dispersion significantly for the entire universe of firms and throughout the four quarters of the fiscal year. To illustrate, for firms reporting semiannually, the forecast error decreases with 0.62 pp (p-value = 0.00) in the first quarter, and with 0.71 pp (p-value = 0.00) in the fourth quarter. Importantly, firms with quarterly reports also experience a drop in their forecast error and dispersion. Accordingly, analysts' forecast dispersion decreases with 0.27 pp (p-value = 0.00), and 0.39 pp (p-value = 0.00) in the first, and respectively the fourth quarter. The mandatory adoption of IFRS attracts more analysts following the firms in the post period. With the exception of the second and fourth quarter for quarterly reporting firms, the number of analysts covering the entire set of firms increases with IFRS over the fiscal year.

[Insert Table 2.3 about here]

Figure 2.3 displays graphically the above findings. Panel A depicts a clear delineation in the forecast errors for quarterly reporting firms from the first to fourth quarter. As documented above, the closer the fiscal year start (end), the higher (lower) the difference in forecast error. The same trend is visible for semiannual reporters, although at a smaller scale. In line with this trend, Panel B suggests that the

dispersion in analysts' forecast is greater for quarterly reporting firms than the one for semiannual reporting firms over the entire period. Panel C exhibits a slight positive difference in the number of analysts following the quarterly and semiannual reporting firms. The mandated IFRS in 2005 decreases both forecast error and dispersion in absolute terms for quarterly and semiannual reporters. Unlike analysts' dispersion that drops significantly from 2005, absolute forecast errors seem to start decreasing before 2005, most likely in anticipation of the common standards adoption by a majority of analysts. In terms of analyst following, mandated quarterly reporters exhibit a continuous increasing trend in all quarters overt the sample period, while the coverage of semiannual reporters increases slightly from 2008.

[Insert Figure 2.3 about here]

2.5.2. Multivariate results

Table 2.4 presents the results of the baseline model. In Panel A, under the local accounting standards, the forecast error for firms mandated to issue quarterly reporting is significantly higher than the one for semiannual reporting firms. To illustrate, the forecast error for mandatory quarterly reporters is with 1.52 pp higher (p-value = 0.01) than the one for firms reporting semiannually during the first quarter. The average association of forecast error and mandatory frequent reporting (*QTR*) is positive during all four quarters of the fiscal year and decreases in both statistical and economic significance throughout the year. For example, the gap decreases from 1.52 pp (significant at 1%) to 0.44 pp (significant at 10%). This decreasing trend throughout the year pertains to previous evidence on a key time-property of quarterly earnings; i.e., the auto-regressive coefficients are positive for the first three lags and exhibit exponential decay; see, e.g., Ragan and Sloan (1998), Bernard and Thomas (1990). According to these authors, stock prices fail to reflect the extent to which firms' earnings series differ from a seasonal random walk. My results suggest that financial analysts succumb as well to this naïve expectations hypothesis. Indeed, the extensive use of managers' fiscal-year expectations when formulating interim cost estimates may drive the higher forecast errors and dispersion for quarterly reporting firms especially at the beginning of the year. This conjecture is further supported by Ernstberger et al. (2011), showing that in years in which a firm just

meets or beats past years' earnings or the zero benchmark, mandated quarterly reporters exhibit higher real activities manipulation compared to semiannual reporters in Europe.

Panel B brings more evidence to this conjecture. Indeed, analysts' forecast dispersion is significantly higher for quarterly mandatory reporters than for semiannual reporters in three out of the four fiscal years. To wit, the difference in analysts' disagreement is 0.43 pp (0.31 pp) in the first (third) quarter. Similar to forecast error, the gap decreases in statistical significance and magnitude throughout the fiscal year. Panel C reports the results of Eq. (1) where the dependent variable is analyst following. The *QTR* estimator is not different from zero during the first three quarters, and it becomes significantly positive during the last fiscal quarter. This finding suggests that mandatory quarterly reporting is associated with more analysts following at the end of the fiscal year. For example, during the last three months of the year, quarterly reporting firms are followed by 19% more analysts than semiannual reporting firms. Taken together, the results on *QTR* estimated coefficient in the three panels reject the first hypothesis. While mandatory quarterly reports attract more analysts following throughout the end of the year, they are nevertheless associated with greater forecast error and dispersion throughout the entire fiscal year. I additionally test whether the higher forecast errors, dispersion and analysts following associated with quarterly reporters in Europe before IFRS persist after 2005. The p-value of the F-test in the second to last row in Table 2.4 is close to zero, confirming the persistence of the gap in the post period.

In line with a large body of evidence, the mandated IFRS is associated with a decrease in analysts' forecast error and dispersion significant at 1% level in all quarters. Importantly, analysts experience an increased accuracy and agreement for both semiannual and quarterly reporters. To wit, the *IFRS* estimated coefficient in Panel A (B) indicates that the average forecast error (dispersion) for semiannual reporting firms decreases by 0.60 (0.30) pp (significant at 1% level) on average over the fiscal year. The corresponding effect for mandated quarterly reporters is a decrease in forecast error (dispersion) by 0.86 (0.32) pp on average during the year. According to the last row in Table 2.4, this effect is highly significant over each fiscal quarter. The adoption of IFRS is also associated with more analysts following, but only during the first quarter of the year (difference significant at 1%).

[Insert Table 2.4 about here]

I further assess the impact of IFRS on the difference in analysts' information environment between quarterly and semiannual reporters documented above. The estimated coefficient of *QTRxIFRS* is not significant over the entire fiscal year for all three proxies. Both specification (1) and (2) support the argument that the convergence of accounting standards in Europe did not change the relative noisy information associated with high frequency reporting. Indeed, the positive difference in the gap of analysts' forecast error and dispersion for quarterly and semiannual reporting firms is not different from zero (Panel A and B). In addition, the adoption of common standards does not change the difference in analysts following between quarterly and semiannual firms (Panel C). Therefore, I do not reject the second hypothesis. These results suggest that, although IFRS increases the quality of analysts' information set in absolute terms, the relative noise associated with mandated quarterly reporting firms persists irrespective of the accounting standard followed.

2.5.3. Differences in the mandated quarterly reporting

The previous results indicate that the quality of analyst's information environment increases on average for both mandatory quarterly and semiannual firms after the adoption of IFRS. Indeed, if the information asymmetry is high, then quarterly financial statements would increase the quality of analysts' information set at least to the one for semiannual firms. Consequently, the distinction between different levels of institutional quality is important.

Table 2.5 lists the six proxies I use for institutional differences for financial reporting. The first two measure the degree of accounting enforcement activity and the quality of public company auditors' working environment assessed by independent enforcement bodies. Brown, Preiato and Tarca (2014) construct the two measures based on surveys at different cut-off dates, and show that they have more explanatory power than the traditional legal enforcement proxies. Additionally, the advantage of using these indexes is that they allow for within country-time variation in the level of institutional setting. The next three proxies are time-invariant country enforcement indexes from Christensen, Hail and Leuz (2013)

and Jackson and Roe (2009) respectively. The last proxy is the distance of firm's local accounting standards to IFRS (Bae, Tan and Welker, 2008). I split the sample of firms in those listed in country-years above (below) the annual median levels of each proxy and run the baseline model simultaneously.

2.5.3.1. Different enforcement levels

Table 2.5, Panels A to E provide the results of the estimations using two country-time variant and three time-invariant country variant enforcement proxies.[20] Two insights emerge from this exercise. First, the higher analysts' forecast error and dispersion for quarterly reporting firms documented above persist under both strong and weak enforcement regimes and analysts do not differentiate between the two. This finding is in line with the baseline results where the same proxy for enforcement is not different from zero, confirming that the adoption of IFRS decreases analysts' forecast error only in countries with strong enforcement regimes.

Second, the difference in enforcement regimes in terms of analysts following is statistically significant for all enforcement proxies. Specifically, mandatory quarterly reporting firms are followed by significantly more (less) analysts than semiannual ones in strong (weak) enforcement regimes. This finding suggests that analysts are willing to invest more resources in increasing their coverage for quarterly reporting firms conditional on the quality of the information disclosed. For example, during the fourth quarter, the number of analysts following quarterly reporting firms is 27% higher (15% smaller) in strong (weak) enforcement countries (significant at 1% level) in Panel A. In addition, while the *IFRS* estimator is not different in strong and weak enforcement regimes for forecast error and dispersion, it is statistically different for analyst following.

In Panel C, the number of analyst following after IFRS is 21% higher (13% lower) in countries that have simultaneously (not) changed their enforcement of financial reporting with the mandated IFRS adoption (significant at 1% level). Importantly, the *QTRxIFRS* estimator is significantly negative (positive) in strong (weak) enforcement countries in all panels.

[20] To conserve space, I report the results of the last quarter (i.e. Q4) in all panels. Results are qualitatively similar for the first three fiscal quarters.

In a nutshell, before IFRS, mandatory quarterly reporting in Europe is associated with more analysts following as long as these countries have strong enforcement regimes. After IFRS, analysts increase their coverage for semiannual reporters, suggesting that under strong enforcement regimes, analysts expect more informational advantage from mandatory common accounting standards. Importantly, while mandating quarterly reporting seems to attract more (less) analyst coverage in strong (weak) enforcement regimes, analysts' forecast error and dispersion are higher for this type of firms irrespective of the enforcement regime.

[Insert Table 2.5 about here]

2.5.3.2. Distance to IFRS

Byard et al. (2011) show that the distance between firm's local accounting standards and IFRS is important in depicting the change in the quality of analysts' information environment with the adoption of IFRS. Table 2.5, Panel F reports the results of the simultaneous regression of low and high distance to IFRS samples. Similar to enforcement proxies, the higher forecast error and dispersion associated with quarterly reporting firms is not related to the firm's distance to IFRS.

On the other hand, the distance to IFRS matters in terms of analysts following. Specifically, before IFRS, analysts cover significantly more (less) quarterly reporting firms when firm's local accounting standards are low (high) distance to IFRS. To illustrate, during the last quarter, analysts following for quarterly reporting firms is 37% higher when firm's accounting standard is similar to IFRS (significant at 1% level). This finding is relevant to assessing the degree of interaction between the amount of information released by firms via interim reports and the quality of accounting standards used to communicate this information. Specifically, analysts are willing to allocate resources to cover more firms with quarterly reports as long as the accounting standards used by these firms do not depart too much from the most familiar accounting principles of the analysts. On average, firms with semiannual reports are followed by 21% (15%) more (less) analysts when firm's local accounting standard distance to IFRS is

low (high). In line with previous research, analysts following increases with the mandated IFRS only when firms' local accounting standards are close to IFRS.

2.6. Robustness tests

2.6.1. Matching sample

The empirical design so far allows me to assess the exogenous relative role of quarterly frequency reporting. Specifically, to avoid a potential self-selection bias, I consider in the main analysis only firms that report quarterly under a mandatory requirement to do so. However, quarterly mandated reporters and semiannual firms may vary in a bundle of characteristics. I run the following matching test. For each earnings forecast issued for a mandatory quarterly reporting firm, I identify the closest semiannual reporting firm during the same fiscal year in the same 2-digit SIG code in terms of firm size. Additionally, I require that the total assets ratio of the control to the treated firm is at least 50% (Yip and Young, 2012). I also require that the forecasts are issued by the same analyst. Table 2.6, Panel A reports the results. Overall, the findings to not change under this alternative test design.

[Insert Table 2.6 about here]

2.6.2. Transparency Directive

A potential concern with the interpretation of my results is the adoption of the Transparency Directive in 2007, which requires semiannual firms to report additional interim management statements in the first and third quarter. Prior research suggests that this measure increases investor attention and firms' news dissemination (Boulland, Degeorge and Ginglinger, 2014). As such, the effect of the IFRS adoption of the decreased forecast error and dispersion for semiannual reporting firms might be in fact attributable to this above directive. To check whether this is the case, I run Eq. (1) on a reduced period of time, i.e. from 2002 to 2007. This six-year window is generally used in several empirical studies to test the effect of mandated IFRS in 2005. The results are reported in Table 2.6, Panel B. The findings are similar to the ones of the baseline sample.

2.6.3. Mandatory versus voluntary samples

In the main analysis, I find that mandatory quarterly reporting is associated with more forecast error and dispersion, and that the mandatory adoption of IFRS increases significantly analysts' forecast accuracy and agreement. The construction of the baseline sample provides an opportunity to analyze the association between analysts' information environment and reporting frequency in several settings. Specifically, under the frequency reporting regime and the accounting standard followed, I can classify firms in a two-by-two framework depending on whether they report mandatorily or voluntarily on a quarterly basis and/or under IFRS. I therefore check whether the two above results hold in another sample.

By keeping fixed the mandatory IFRS adoption, I test whether I obtain the same results by using a sample of voluntary (instead of mandatory) quarterly and semiannual reporting firms. The results are provided in Table 2.7, Panel A. Analysts' forecast error is by 1% higher for voluntary quarterly reporting firms than for semiannual reporters in the first fiscal quarter (significant at 5% level). This finding is consistent with the view that firms choosing to report more frequently are likely to have higher information asymmetries especially at the beginning of the year. However, during the rest of the fiscal year, analysts do not distinct between voluntary quarterly and semiannual reporters in terms of forecast error and dispersion. Compared with the baseline results on mandatory quarterly reporters, these results suggest that voluntary quarterly reporting is not likely to noisy information input for analysts. Moreover, when firms decide to voluntarily report more frequently, analysts following is significantly higher than for semiannual firms. In line with the results on the main sample, mandatory IFRS adoption decreases significantly analysts' forecast error and dispersion. The difference between the gap in mandatory versus semiannual reporting firms before and after IFRS is not different from zero. Taken together, these findings reveal that when firms deliberately choose to report more frequent financial statements, the quality of analysts' information setting does not decrease. Conversely, the mandatory requirement itself of publishing more frequent financial statements throughout the year is associated with higher analysts' forecast error and dispersion.

[Insert Table 2.7 about here]

By keeping fixed the mandatory quarterly reporting, I test whether I find similar results by using a sample of voluntary (instead of mandatory) IFRS adoption firms. Panel B reports the results. The presence of mandatory quarterly firms (compared with semiannual reporters) is associated with a decreasing positive difference in forecast error from 2.1% in first quarter (significant at 5% level) to 0.7% in the third quarter (significant at 10% level). Importantly, there is no significant difference in analysts' information set when firms report voluntarily under IFRS. This result can be associated with previous findings on insignificant impact in terms of firm's information quality with a "label adoption" (i.e., signaling higher financial reporting quality without a corresponding cost to do so). The results in Panel B need to be considered however with caution, since the sample of firms is small.

2.7. Conclusions

This paper investigates the effect of the European financial harmonization process on the quality of analysts' information environment. First, I check whether mandatory quarterly reporting benefits financial analysts. Second, I assess the interaction between financial reporting frequency and accounting standards.

The results of this study are summarized as follows. First, I find little support to the claim that regulation forcing firms to issue more frequent information benefits financial analysts. When firms are mandated to report on a quarterly basis, analysts' forecast error and dispersion are higher compared to a control sample of semiannual reporting firms. Mandatory quarterly reporting is associated with more analysts following as long as firms are located in strong enforcement regimes and their local accounting standards are closer to IFRS. Second, analysts' forecast error and dispersion decrease significantly when firms mandated to report quarterly financial statements are, in addition, mandated to report under IFRS. From the perspective of analysts' output, this result suggests that accounting standards dominate reporting frequency. However, in relative terms, the pernicious effect of mandating more frequent reporting continues to exist under IFRS. Specifically, the positive gap in both forecast error and dispersion between quarterly and semiannual reporters does not change with the mandatory IFRS regime. In a nutshell, while IFRS does benefit analysts by increasing the quality of analysts' information set in absolute terms, they do

not wipe out the relative noise associated with mandatory quarterly statements. These results hold under a series of different specifications and after controlling for alternative measures of firms' institutional environment and accounting quality.

This paper has implications for policymakers and contributes to the assessment of the recent regulatory initiatives in Europe. Examining the interaction between mandated IFRS and the reporting frequency is important, since, at date, it is unclear to what extent the frequency of accounting information is conditional on the accounting standards used to communicate this information. In the context of transient laws, the process through which disclosure regulations are created and implemented deserves more careful empirical support and investigation. This paper aims to contribute towards a more substantive body of research to properly address the efficiency of such regulations. Further empirical research in this respect should shed more light on whether, and under what institutional design, disclosure regulation is likely to benefit both preparers and users of financial statements.

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Appendix

Table 2.A.1: Sample selection

This table describes the sample selection process and the filters applied to the sample of firms used in the baseline analysis.

Description	Firms
Starting sample: Worldscope + IBES, 2000-2010	5'143
Minus	
Firms with different fiscal year end date in Worldscope and IBES	(4)
Missing data on control variables	(354)
Cross-listed firms in US (sponsored ADRs)	(330)
Firms listed in more than one country in a fiscal year	(7)
Firms headquartered in more than one country in a fiscal year	(28)
Semiannual reporting firms in mandatory quarterly reporting countries	(26)
Firms following USGAAP	(75)
Voluntary IFRS reporters	(453)
Voluntary quarterly reporters	(334)
Firms with no observations both before and after IFRS	(2'004)
Firms with no observations during the last two months of the fiscal quarter	(2)
Final sample	1'526

Table 2.A.2: Variables definition and source

This table provides the list of variables used in the models.

Panel A. Variables construction

Variable	Definition	Source
$FError_{a,i,q,t}$	Absolute difference between analysts a's annual EPS forecast issued in quarter q for firm i and fiscal year t and the actual annual EPS, scaled by firm i's total assets per share in fiscal year t.	I/B/E/S Worldscope
$Disp_{i,q,t}$	Standard deviation of EPS forecasts issued in quarter q for firm i, scaled by firm i's total assets per share in fiscal year t.	I/B/E/S Worldscope
$LogFollow_{i,q,t}$	Logarithm of the number of analysts issuing at least one EPS forecast for firm i during quarter q of fiscal year t.	I/B/E/S Worldscope
$ActualEPS_{i,t}$	Realized annual earnings per share for firm i in fiscal year t.	I/B/E/S
$ForecastEPS_{a,i,q,t}$	Forecasted annual earnings per share by analyst a for firm i issued in quarter q for fiscal year t.	I/B/E/S
$APS_{i,t}$	Total assets per share of firm i for fiscal year t.	Worldscope
$TA_{i,t}$	Total assets of firm i for fiscal year t.	Worldscope
$Shares_{i,t}$	Number of common shares outstanding of firm i for fiscal year t.	Worldscope
QTR_i	Dummy variable equal to one if firm i is a mandatory QTR reporter, zero otherwise.	Worldscope
$IFRS_{i,t}$	Dummy variable equal to one if firm i follows mandatory adoption of IFRS during fiscal year t, zero otherwise.	Worldscope
$GEXP_{a,t}$	Number of years since analyst a provides annual EPS forecasts in the database.	I/B/E/S
$FEXP_{a,i,t}$	Number of years since analyst a provides annual EPS forecasts for firm i in the database.	I/B/E/S
$NFIRMS_{a,t}$	Number of firms followed by analyst a in fiscal year t.	I/B/E/S
$NIND_{a,t}$	Number of two-digit SIG codes industries followed by analyst a in fiscal year t.	I/B/E/S
$NCTR_{a,t}$	Number of countries followed by analyst a in fiscal year t.	I/B/E/S
$FAGE_{a,i,t}$	Number of days between the day of the EPS forecast provided by analyst a for firm i and the end of the fiscal year t.	I/B/E/S
$BSIZE_{a,t}$	Logarithm of the number of analysts working for analyst's a broker in fiscal year t.	I/B/E/S
$FSIZE_{i,t}$	Total assets of firm i for fiscal year t in EUR mil.	Worldscope

(*continued*)

Table 2.A.2
(continued)

Variable	Definition	Source
$ENFBPT_{i,t}$	Accounting enforcement activity in country of firm in fiscal year t.	Brown, Preiato, Tarca (2014)
$AUDBPT_{i,t}$	Quality of the auditors' working environment in country of firm in fiscal year t.	Brown, Preiato, Tarca (2014)
$ENFCHL_i$	Indicator variable equal to one if firm i is listed in a country that adopted changes in enforcement of financial reporting with the mandated IFRS, zero otherwise.	Christensen, Hail and Leuz (2013)
$BUDGET_i$	Public enforcement intensity based on securities regulators' budget/GPD level in country where firm i is listed.	Jackson and Roe (2009)
$STAFF_i$	Public enforcement intensity based on securities regulators' staff/population level in country where firm i is listed.	Jackson and Roe (2009)
$IFRSDIST_i$	Distance between the local accounting standard followed by firm *i* and IFRS.	Bae, Tan, Welker (2008)

Panel B. Worldscope data Item	Field
Accounting standard followed	WC07536
Common shares outstanding	WC05301
Date of fiscal period end	WC05350
Earnings reporting frequency	WC05200,WC05200A
Ibes ticker	IBTKR
ISIN	WC06008
Total assets	WC02999

Table 2.1: Distribution of firm-year observations by country

This table presents the number of firm-years observations by country associated to a mandatory quarterly (Panel A) or semiannual (Panel B) reporting frequency regime. A firm-year observation is classified as mandatory quarterly (semiannual) if the country in which the firm is listed in the corresponding year requires the disclosure of financial statements on a quarterly (semiannual) basis.

Country	Firm-years	%
Panel A. Mandatory quarterly reporters		30%
Finland	683	6.2%
Germany (Prime Standard)	158	1.4%
Greece	353	3.2%
Italy	68	0.6%
Norway	456	4.2%
Portugal	113	1.0%
Spain	381	3.5%
Sweden	1'060	9.7%
Panel B. Semi-annual reporters		70%
Belgium	247	2.3%
Denmark	61	0.6%
France	1'695	15.5%
Germany (without Prime Standard)	28	0.3%
Ireland	141	1.3%
Netherlands	400	3.7%
United Kingdom	5'087	46.5%
Total	10'931	100%

Table 2.2: Descriptive statistics

This table reports the descriptive statistics of the dataset. Panel A describes the annual distribution of the firms by their reporting frequency, analysts, and earnings forecasts. Panel B describes the summary statistics of the control variables. The number of firms in the entire sample differs from the sum of quarterly and semiannual firms since two firms in Spain are reporting quarterly (semiannually) before (after) adopting the Transparency Directive in 2007. QTR (SAN) reporters denote the firms reporting under the mandatory quarterly (semiannual) regime. Control variables are defined in Table 2.A.2.

Panel A. Sample distribution

Year	QTR reporters	SAN reporters	No. analysts	No. firms	No. forecasts
2000	209	565	2'291	774	10'416
2001	287	576	2'347	863	11'568
2002	344	608	2'322	952	13'176
2003	343	644	2'167	987	14'573
2004	386	720	2'155	1'106	15'476
2005	382	759	2'184	1'141	14'869
2006	373	829	2'391	1'202	16'220
2007	302	810	2'231	1'112	14'574
2008	225	784	2'190	1'009	15'423
2009	208	712	2'052	920	15'867
2010	210	655	2'109	865	15'582
2000-2010	448	1'080	7'523	1'526	157'744

Panel B. Control variables summary	p25	Mean	Median	p75
General experience (years)	5	10.09	10	15
Firm experience (years)	0	3.08	2	4
Number of firms followed	7	12.56	11	16
Number of industries followed	2	3.36	3	4
Number of countries followed	1	2.32	2	3
Broker size	23	110.31	53	157
Firm size (EUR mil.)	0.31	3.86	0.99	2.90
Forecast age (days)	93	160.62	142	235
Accounting enforcement	9	15.55	16	22
Audit quality	17	23.61	25	32
Enforcement indicator	0	0.58	1	1
Budget	28'851	55'117	45'937	80'902
Staff	7.19	13.72	13.76	19.04
IFRS distance	1	7.39	10	12

Table 2.3: Summary of analysts' information environment by reporting frequency

This table presents the average annual forecast error, average forecast dispersion and average analyst following during each fiscal quarter by reporting frequency. Q1 to Q4 refers to the financial statements corresponding to quarter one to four. QTR (SAN) reporters denote the firms reporting under the mandatory quarterly (semiannual) regime. *FError* is the absolute difference between the analyst's annual earnings forecast and the actual annual earnings, deflated by firm's total assets per share. *Disp* is the standard deviation of annual earnings forecasts, deflated by firm's total assets per share. In each quarter, I retain the first annual earnings forecast for each firm-analyst pair issued in the last two months. *LogFollow* is the logarithm of the number of analysts following the firm during the entire fiscal quarter. *** denote statistical significance at 1 percent (two-tailed t test).

	FError				*Disp*				*LogFollow*			
	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
2000-2010												
QTR	3.40	3.01	2.32	1.79	1.14	1.27	1.12	1.16	1.99	1.77	1.76	1.82
SAN	2.07	2.17	1.66	1.37	0.69	0.87	0.78	0.81	1.88	1.62	1.81	1.61
QTR-SAN	1.33***	0.84***	0.66***	0.42***	0.45***	0.41***	0.33***	0.34***	0.10***	0.15***	-0.05***	0.21***
Pre IFRS												
QTR	3.98	3.49	2.68	2.11	1.29	1.43	1.28	1.35	1.92	1.78	1.74	1.81
SAN	2.42	2.53	1.99	1.75	0.85	1.01	0.97	1.04	1.77	1.60	1.78	1.55
QTR-SAN	1.56***	0.95***	0.70***	0.36***	0.44***	0.42***	0.31***	0.31***	0.15***	0.18***	-0.04***	0.26***
Post IFRS												
QTR	2.92	2.58	1.99	1.47	1.02	1.13	0.97	0.95	2.04	1.77	1.78	1.82
SAN	1.80	1.85	1.39	1.04	0.57	0.75	0.64	0.62	1.97	1.64	1.84	1.66
QTR-SAN	1.12***	0.74***	0.61***	0.43***	0.45***	0.38***	0.33***	0.34***	0.08***	0.13***	-0.06***	0.17***
Post-Pre IFRS												
QTR	-1.06***	-0.90***	-0.69***	-0.64***	-0.27***	-0.30***	-0.31***	-0.39***	0.13***	-0.01	0.04***	0.01
SAN	-0.62***	-0.68***	-0.60***	-0.71***	-0.28***	-0.26***	-0.33***	-0.42***	0.20***	0.05***	0.06***	0.11***

Table 2.4: Baseline results

This table presents the results of Eq. (1). Q1 to Q4 refers to fiscal quarter one to four. *FError* is the absolute difference between the analyst's annual earnings forecast and the actual annual earnings, deflated by firm's total assets per share. *Disp* is the standard deviation of annual earnings forecasts, deflated by firm's total assets per share. In each quarter, I retain the first annual earnings forecast for each firm-analyst pair issued in the last two months. *LogFollow* is the logarithm of the number of analysts following the firm during the entire fiscal quarter. All control variables are defined in Table 2.A.2. Robust standard errors (in italics) are clustered at country-industry level. ***, ** and * denote statistical significance at 1, 5 and 10 percent respectively.

Panel A. Dependent variable: FError								
	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
QTR (i)	1.519***	1.032**	0.701**	0.435*				
	0.518	*0.457*	*0.333*	*0.251*				
IFRS (ii)	-0.476**	-0.576***	-0.608***	-0.715***				
	0.188	*0.189*	*0.147*	*0.131*				
QTRxIFRS (iii)	-0.566	-0.336	-0.145	0.029	-0.496	-0.297	-0.206	-0.053
	0.471	*0.383*	*0.275*	*0.222*	*0.431*	*0.354*	*0.286*	*0.208*
GEXP	-0.014**	-0.011*	-0.007	-0.006	-0.005	0.000	0.000	0.000
	0.007	*0.007*	*0.005*	*0.004*	*0.004*	*0.005*	*0.003*	*0.004*
FEXP	-0.051***	-0.071***	-0.054***	-0.035***	-0.036***	-0.050***	-0.041***	-0.023***
	0.013	*0.015*	*0.010*	*0.008*	*0.011*	*0.013*	*0.008*	*0.006*
LNBSIZE	-0.079**	-0.081**	-0.112***	-0.067*	-0.088***	-0.088**	-0.105***	-0.059*
	0.038	*0.041*	*0.034*	*0.034*	*0.031*	*0.037*	*0.034*	*0.031*
NFIRMS	0.005	0.014**	-0.001	0.001	-0.001	0.007**	-0.003	0.000
	0.007	*0.007*	*0.006*	*0.001*	*0.004*	*0.003*	*0.003*	*0.001*
NIND	-0.056	-0.083**	-0.039	-0.032	-0.023	-0.005	-0.003	0.008
	0.046	*0.040*	*0.031*	*0.023*	*0.025*	*0.022*	*0.02*	*0.013*
NCTR	-0.031	-0.064**	-0.007	-0.005	-0.019	-0.052***	-0.011	-0.009
	0.026	*0.030*	*0.020*	*0.019*	*0.014*	*0.017*	*0.012*	*0.013*
FSIZE	-0.058***	-0.033**	-0.035***	-0.020**	-0.059***	-0.023	-0.034***	-0.014
	0.012	*0.015*	*0.010*	*0.008*	*0.015*	*0.018*	*0.012*	*0.011*
FAGE	0.002	0.005**	0.003	0.002	0.002	0.004**	0.002	0.002
	0.003	*0.002*	*0.002*	*0.002*	*0.003*	*0.002*	*0.002*	*0.002*
ENFBPT	0.006	0.018	0.016	0.016	0.017	0.046*	0.023	0.027
	0.027	*0.024*	*0.017*	*0.013*	*0.032*	*0.027*	*0.016*	*0.018*
Constant	2.661**	2.146***	2.327***	2.022***	3.116***	2.243***	2.589***	2.163***
	1.120	*0.605*	*0.471*	*0.290*	*1.107*	*0.518*	*0.478*	*0.282*
Obs.	27'374	28'277	36'990	29'644	27'374	28'277	36'990	29'644
Adj. R-sq.	6.6%	4.0%	4.1%	3.3%	3.3%	2.6%	2.6%	1.9%
Country-Industry FE	No	No	No	No	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	Yes	Yes	Yes	Yes
p-val (i) + (iii) = 0	0.00	0.02	0.01	0.00				
p-val (ii) + (iii) = 0	0.01	0.00	0.00	0.00				

(continued)

Table 2.4
(continued)

Panel B. Dependent variable: Disp								
	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
QTR (i)	0.430**	0.408**	0.309*	0.243				
	0.166	*0.166*	*0.171*	*0.158*				
IFRS (ii)	-0.255***	-0.239***	-0.313***	-0.383***				
	0.080	*0.085*	*0.078*	*0.088*				
QTRxIFRS (iii)	-0.020	-0.069	-0.002	0.014	-0.007	-0.042	0.044	0.069
	0.123	*0.143*	*0.138*	*0.137*	*0.111*	*0.135*	*0.141*	*0.117*
FSIZE	-0.011**	-0.011**	-0.011***	-0.006	-0.006	-0.007	-0.012***	-0.001
	0.005	*0.005*	*0.004*	*0.006*	*0.009*	*0.006*	*0.004*	*0.008*
ENFBPT	-0.001	-0.002	-0.001	-0.009	0.003	0.013	0.012	0.005
	0.010	*0.010*	*0.010*	*0.009*	*0.008*	*0.010*	*0.008*	*0.010*
Constant	0.918***	1.082***	1.014***	1.201***	0.932***	1.107***	0.964***	1.148***
	0.193	*0.192*	*0.183*	*0.172*	*0.126*	*0.152*	*0.131*	*0.152*
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	25'566	25'757	34'219	26'895	25'566	25'757	34'219	26'895
Adj. R-sq.	7.3%	3.8%	4.0%	3.8%	2.0%	1.3%	2.6%	1.8%
Country-Industry FE	No	No	No	No	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	Yes	Yes	Yes	Yes
p-val (i) + (iii) = 0	0.00	0.01	0.01	0.02				
p-val (ii) + (iii) = 0	0.01	0.01	0.01	0.00				
Panel C. Dependent variable: LogFollow								
	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
QTR (i)	0.063	0.105	-0.110	0.194**				
	0.086	*0.093*	*0.088*	*0.087*				
IFRS (ii)	0.194***	0.042	0.073	0.099				
	0.065	*0.093*	*0.074*	*0.078*				
QTRxIFRS (iii)	-0.028	-0.002	0.024	-0.041	0.056	0.057	0.116**	0.045
	0.073	*0.097*	*0.081*	*0.090*	*0.060*	*0.070*	*0.057*	*0.067*
FSIZE	0.035***	0.022***	0.028***	0.023***	0.043***	0.020*	0.033***	0.025**
	0.006	*0.006*	*0.007*	*0.005*	*0.008*	*0.010*	*0.010*	*0.01*
ENFBPT	-0.012**	-0.009*	-0.010*	-0.007	0.006	0.016*	0.013*	0.015**
	0.006	*0.005*	*0.006*	*0.006*	*0.007*	*0.009*	*0.007*	*0.007*
Constant	1.825***	1.674***	1.854***	1.581***	1.500***	1.351***	1.466***	1.293***
	0.107	*0.112*	*0.114*	*0.115*	*0.081*	*0.144*	*0.112*	*0.122*
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	33'906	36'956	49'147	37'735	33'906	36'956	49'147	37'735
Adj. R-sq.	14.6%	9.9%	9.2%	10.0%	12.4%	6.7%	9.6%	7.9%
Country-Industry FE	No	No	No	No	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	Yes	Yes	Yes	Yes
p-val (i) + (iii) = 0	0.67	0.09	0.22	0.05				
p-val (ii) + (iii) = 0	0.00	0.36	0.04	0.26				

Table 2.5: Differences in country institutional regimes

This table reports the simultaneous results of Eq. (1) under different enforcement regimes. In Panel A (B), ENFBPT (AUDBPT) depicts the degree of accounting enforcement activity (quality of public company auditors' working environment) (Brown, Preiato and Tarca 2014, 19). Strong (Weak) denotes observations where the firm-year index is greater (smaller) than the annual median value. In Panel C, ENFCHL is an indicator of simultaneous changes in enforcement of financial reporting around the mandatory IFRS adoption (Christensen, Hail and Leuz 2013, 153). Yes (No) denotes observations where the country of listing have made (have not made) substantive changes in enforcement. In Panel D (E), BUDGET (STAFF) denotes the level of public enforcement intensity based on securities regulators' budget/GDP (staff/population) level (Jackson and Roe, 2009, 214-215). Strong (Weak) denotes observations where the country index is greater (smaller) than the median value. In Panel F, IFRSDIST reports the distance between the country's local accounting standards and IFRS (Bae, Tan and Welker 2008, 601-602). Low (High) denotes observations where the index is lower (higher) than the median distance value. *FError* is the absolute difference between the analyst's annual earnings forecast and the actual annual earnings, deflated by firm's total assets per share. *Disp* is the standard deviation of annual earnings forecasts, deflated by firm's total assets per share. I retain the first annual earnings forecast for each firm-analyst pair issued in the last two months of the quarter. *LogFollow* is the logarithm of the number of analysts following the firm during the entire fiscal quarter. All control variables are defined in Table 2.A.2. Robust standard errors (in italics) are clustered at country-industry level. ***, ** and * denote statistical significance at 1, 5 and 10 percent respectively.

Variable of interest	*FError*			*Disp*			*LogFollow*		
Panel A. ENFBPT	Strong	Weak	p-val S - W = 0	Strong	Weak	p-val S - W = 0	Strong	Weak	p-val S - W = 0
QTR	0.343***	0.505***	0.74	0.277***	0.111*	0.66	0.268***	-0.145***	0.00
	0.062	*0.105*		*0.031*	*0.065*		*0.016*	*0.026*	
IFRS	-0.698***	-0.196	0.05	-0.381***	-0.606***	0.36	0.123***	-0.540***	0.00
	0.041	*0.183*		*0.022*	*0.118*		*0.011*	*0.045*	
QTRxIFRS	0.197**	-0.423**	0.19	0.110**	0.135	0.94	-0.085***	0.562***	0.00
	0.092	*0.192*		*0.048*	*0.124*		*0.025*	*0.047*	
Control variables	Yes	Yes		Yes	Yes		Yes	Yes	
Obs.	20'942	8'702		18'754	8'141		26'960	10'775	
Adj. R-sq.	3.9%	2.2%		3.4%	2.2%		10.2%	15.4%	
Panel B. AUDBPT	Strong	Weak	p-val S - W = 0	Strong	Weak	p-val S - W = 0	Strong	Weak	p-val S - W = 0
QTR	0.336***	0.431***	0.86	0.398***	0.046	0.35	0.297***	-0.067**	0.02
	0.052	*0.100*		*0.028*	*0.058*		*0.014*	*0.027*	
IFRS	-0.727***	-0.163	0.01	-0.399***	-0.460***	0.76	0.115***	-0.206***	0.06
	0.043	*0.116*		*0.024*	*0.067*		*0.011*	*0.031*	
QTRxIFRS	0.152**	-0.458***	0.20	0.041	0.041	1.00	-0.077***	0.279***	0.09
	0.074	*0.137*		*0.041*	*0.081*		*0.020*	*0.037*	
Control variables	Yes	Yes		Yes	Yes		Yes	Yes	
Obs.	23'385	6'259		21'086	5'809		30'024	7'711	
Adj. R-sq.	4.3%	1.7%		4.8%	3.3%		13.5%	2.1%	

(continued)

Table 2.5
(continued)

Variable of interest	*FError*			*Disp*			*LogFollow*		
Panel C. ENFCHL	Yes	No	p-val Y - N = 0	Yes	No	p-val Y - N = 0	Yes	No	p-val Y - N = 0
QTR	0.253***	0.379***	0.81	0.255***	0.397***	0.65	0.403***	0.022	0.01
	0.071	*0.060*		*0.037*	*0.035*		*0.018*	*0.017*	
IFRS	-0.764***	-0.549***	0.35	-0.454***	-0.353***	0.53	0.214***	-0.134***	0.00
	0.053	*0.063*		*0.029*	*0.036*		*0.013*	*0.017*	
QTRxIFRS	0.106	-0.057	0.71	0.074	-0.073	0.56	-0.226***	0.205***	0.00
	0.094	*0.086*		*0.052*	*0.050*		*0.024*	*0.024*	
Control variables	Yes	Yes		Yes	Yes		Yes	Yes	
Obs.	16'193	13'451		14'423	12'472		20'804	16'931	
Adj. R-sq.	0.04	0.04		0.03	0.05		0.07	0.13	
Panel D. BUGET	Strong	Weak	p-val S - W = 0	Strong	Weak	p-val S - W = 0	Strong	Weak	p-val S - W = 0
QTR	-0.02	0.499***	0.37	0.177***	0.437***	0.43	0.272***	0.047***	0.10
	0.073	*0.064*		*0.039*	*0.036*		*0.020*	*0.016*	
IFRS	-0.767***	-0.516***	0.28	-0.454***	-0.353***	0.52	0.235***	-0.164***	0.00
	0.050	*0.068*		*0.027*	*0.039*		*0.013*	*0.017*	
QTRxIFRS	0.095	-0.041	0.77	0.067	-0.042	0.68	-0.201***	0.222***	0.00
	0.093	*0.091*		*0.051*	*0.052*		*0.026*	*0.023*	
Control variables	Yes	Yes		Yes	Yes		Yes	Yes	
Obs.	15'848	13'796		14'041	12'854		19'680	18'055	
Adj. R-sq.	3.5%	3.3%		3.3%	3.6%		11.6%	7.4%	
Panel E. STAFF	Strong	Weak	p-val S - W = 0	Strong	Weak	p-val S - W = 0	Strong	Weak	p-val S - W = 0
QTR	0.220***	0.391***	0.74	0.262***	0.373***	0.71	0.366***	0.033*	0.01
	0.067	*0.064*		*0.035*	*0.037*		*0.016*	*0.017*	
IFRS	-0.740***	-0.511***	0.34	-0.449***	-0.336***	0.48	0.217***	-0.160***	0.00
	0.051	*0.066*		*0.028*	*0.039*		*0.013*	*0.018*	
QTRxIFRS	0.149*	-0.132	0.53	0.095**	-0.099*	0.43	-0.255***	0.235***	0.00
	0.088	*0.091*		*0.048*	*0.054*		*0.023*	*0.025*	
Control variables	Yes	Yes		Yes	Yes		Yes	Yes	
Obs.	17'446	12'198		15'491	11'404		21'939	15'796	
Adj. R-sq.	3.7%	3.1%		3.6%	3.4%		11.5%	10.0%	

(continued)

Table 2.5
(continued)

Variable of interest	*FError*			*Disp*			*LogFollow*		
Panel F. IFRSDIST	Low	High	p-val L - H = 0	Low	High	p-val L - H = 0	Low	High	p-val L - H = 0
QTR	0.501***	0.209***	0.59	0.336***	0.359***	0.94	0.374***	0.028	0.01
	0.065	*0.063*		*0.036*	*0.035*		*0.017*	*0.017*	
IFRS	-0.736***	-0.587***	0.52	-0.472***	-0.341***	0.40	0.211***	-0.153***	0.00
	0.051	*0.063*		*0.029*	*0.036*		*0.013*	*0.017*	
QTRxIFRS	-0.07	0.055	0.79	0.049	-0.068	0.63	-0.103***	0.099***	0.10
	0.088	*0.088*		*0.050*	*0.051*		*0.023*	*0.024*	
Control variables	Yes	Yes		Yes	Yes		Yes	Yes	
Obs.	17'657	11'987		15'719	11'176		24'270	10'896	
Adj. R-sq.	4.0%	3.4%		3.9%	5.0%		15.1%	20.7%	

Table 2.6: Robustness results

This table reports the baseline model estimates under several robustness tests. In Panel A, I match each mandatory quarterly reporting firm with a semiannual reporting firm. For each mandatory quarterly reporting firm, I identify the closest semiannual reporting firm in terms of size (total assets ratio of the control to treatment firm is at least 50%) if the two firms are in the same 2-digit SIG code during the same fiscal year. For forecast error, I require in addition that the two firms are followed by the same analyst. In Panel B, I restrict the timeframe analysis to 2002 – 2007. Q1 to Q4 refers to fiscal quarter one to four. *FError* is the absolute difference between the analyst's annual earnings forecast and the actual annual earnings, deflated by firm's total assets per share. *Disp* is the standard deviation of annual earnings forecasts, deflated by firm's total assets per share. In each quarter, I retain the first annual earnings forecast for each firm-analyst pair issued in the last two months. *LogFollow* is the logarithm of the number of analysts following the firm during the entire fiscal quarter. All control variables are defined in Table 2.A.2. Robust standard errors (in italics) are clustered at country-industry level. ***, ** and * denote statistical significance at 1, 5 and 10 percent respectively.

	Ferror				*Disp*				*LogFollow*			
	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
Panel A. Matching sample												
QTR (i)	1.431*	1.401**	0.959**	0.451	0.597***	0.500***	0.475***	0.340*	0.188**	0.108	-0.011	0.249***
	0.754	*0.615*	*0.465*	*0.361*	*0.16*	*0.182*	*0.165*	*0.19*	*0.094*	*0.11*	*0.095*	*0.092*
IFRS (ii)	-0.617**	-0.368	-0.492**	-0.811***	-0.223***	-0.218**	-0.277***	-0.381***	0.252**	0.108	0.099	0.156*
	0.307	*0.263*	*0.219*	*0.235*	*0.077*	*0.108*	*0.08*	*0.122*	*0.107*	*0.135*	*0.079*	*0.09*
QTRxIFRS (iii)	-0.591	-0.644	-0.385	0.129	-0.079	-0.114	-0.083	-0.038	-0.136	-0.076	-0.047	-0.115
	0.651	*0.561*	*0.448*	*0.34*	*0.121*	*0.15*	*0.138*	*0.153*	*0.114*	*0.134*	*0.085*	*0.097*
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	7'084	6'664	7'523	6'938	13'699	14'745	17'326	15'618	20'883	23'865	28'685	24'700
Adj. R-sq.	6.3%	4.1%	4.3%	3.0%	7.5%	2.5%	3.8%	2.9%	14.7%	5.7%	4.9%	6.2%
p-val (i) + (iii) = 0	0.12	0.09	0.07	0.02	0.00	0.01	0.00	0.01	0.60	0.74	0.48	0.20
p-val (ii) + (iii) = 0	0.04	0.04	0.03	0.01	0.01	0.01	0.01	0.00	0.03	0.49	0.25	0.44
Panel B. Restricted timeframe (2002 - 2007)												
QTR (i)	1.282***	1.029**	0.631**	0.371	0.393**	0.444***	0.245	0.188	0.132	0.119	-0.089	0.188**
	0.473	*0.437*	*0.305*	*0.235*	*0.183*	*0.168*	*0.175*	*0.180*	*0.084*	*0.093*	*0.099*	*0.092*
IFRS (ii)	-0.631***	-0.609***	-0.651***	-0.631***	-0.366***	-0.239**	-0.409***	-0.351***	0.154***	-0.002	0.021	-0.003
	0.179	*0.185*	*0.165*	*0.144*	*0.092*	*0.104*	*0.073*	*0.111*	*0.046*	*0.079*	*0.071*	*0.067*
QTRxIFRS (iii)	-0.099	-0.077	0.123	0.193	0.140	-0.016	0.210	0.069	-0.207***	-0.088	-0.039	-0.014
	0.382	*0.340*	*0.258*	*0.199*	*0.145*	*0.167*	*0.135*	*0.165*	*0.061*	*0.083*	*0.078*	*0.078*
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	15'271	16'341	21'283	16'835	14'198	14'830	19'602	15'109	18'752	21'247	27'809	21'080
Adj. R-sq.	6.4%	4.3%	4.0%	2.7%	8.3%	4.1%	4.1%	2.8%	15.8%	13.3%	11.8%	12.8%
p-val (i) + (iii) = 0	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.06	0.34	0.63	0.11	0.03
p-val (ii) + (iii) = 0	0.02	0.01	0.01	0.00	0.05	0.04	0.10	0.01	0.27	0.02	0.70	0.70

Table 2.7: Voluntary versus mandatory quarterly and IFRS reporting

This table presents the results of Eq. (1) for different samples of firms. In Panel A, the sample of firms consists of semiannual and voluntary quarterly reporters that mandatorily adopt IFRS. In Panel B, the sample of firms consists of semiannual and mandatory quarterly reporters that voluntarily report under IFRS. to Q4 refers to fiscal quarter one to four. N denotes the number of firms in the sample. *FError* is the absolute difference between the analyst's annual earnings forecast and the actual annual earnings, deflated by firm's total assets per share. *Disp* is the standard deviation of annual earnings forecasts, deflated by firm's total assets per share. In each quarter, I retain the first annual earnings forecast for each firm-analyst pair issued in the last two months. *LogFollow* is the logarithm of the number of analysts following the firm during the entire fiscal quarter. All control variables are defined in Table 2.A.2. Robust standard errors (in italics) are clustered at country-industry level. ***, ** and * denote statistical significance at 1, 5 and 10 percent respectively.

	Ferror				*Disp*				*LogFollow*			
	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
Panel A. Voluntary QTR - Mandatory IFRS (N = 1,190)												
QTR (i)	1.019**	0.471	0.374	0.259	0.259	0.198	0.149	0.195	0.379***	0.509***	0.258**	0.194**
	0.493	*0.330*	*0.275*	*0.274*	*0.181*	*0.135*	*0.133*	*0.165*	*0.114*	*0.126*	*0.118*	*0.087*
IFRS (ii)	-0.443***	-0.615***	-0.604***	-0.708***	-0.219**	-0.230***	-0.322***	-0.356***	0.253***	0.059	0.111	0.099
	0.148	*0.177*	*0.147*	*0.132*	*0.083*	*0.081*	*0.082*	*0.085*	*0.060*	*0.096*	*0.083*	*0.078*
QTRxIFRS (iii)	-0.970*	-0.180	-0.178	-0.109	-0.052	-0.002	0.037	0.001	-0.125	-0.190	-0.083	-0.041
	0.504	*0.436*	*0.326*	*0.321*	*0.211*	*0.234*	*0.192*	*0.224*	*0.132*	*0.149*	*0.106*	*0.090*
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	20'744	21'246	30'208	22'394	19'337	19'131	27'978	20'139	25'268	27'696	40'314	37'735
Adj. R-sq.	3.4%	3.0%	3.1%	3.3%	4.0%	2.0%	3.0%	3.6%	4.9%	6.9%	3.4%	10.0%
p-val (i) + (iii) = 0	0.86	0.35	0.34	0.37	0.03	0.24	0.11	0.15	0.03	0.00	0.02	0.05
p-val (ii) + (iii) = 0	0.01	0.05	0.01	0.01	0.13	0.30	0.11	0.10	0.26	0.20	0.81	0.26
Panel B. Mandatory QTR - Voluntary IFRS (N = 59)												
QTR (i)	2.066**	1.372**	0.688*	0.496	0.568**	0.262	0.337*	0.185	0.190	0.190	0.000	0.243
	0.950	*0.656*	*0.400*	*0.445*	*0.209*	*0.192*	*0.197*	*0.277*	*0.227*	*0.163*	*0.215*	*0.249*
IFRS (ii)	0.272	-0.152	-0.246	0.158	0.200	0.242	0.181	-0.185	0.308*	0.023	0.106	-0.131
	0.788	*0.403*	*0.246*	*0.245*	*0.162*	*0.203*	*0.186*	*0.256*	*0.177*	*0.153*	*0.110*	*0.133*
QTRxIFRS (iii)	-0.940	-1.024	-0.268	-0.415	-0.318	-0.360**	-0.194	-0.051	-0.490**	-0.140	-0.166	-0.009
	1.075	*0.688*	*0.388*	*0.410*	*0.201*	*0.157*	*0.168*	*0.265*	*0.216*	*0.182*	*0.152*	*0.197*
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	1'356	2'002	2'580	2'355	1'298	1'927	2'510	2'264	1'702	2'616	3'408	3'124
Adj. R-sq.	14.4%	11.0%	9.2%	5.9%	24.6%	13.5%	10.3%	4.9%	21.4%	25.3%	26.2%	26.1%
p-val (i) + (iii) = 0	0.05	0.38	0.07	0.81	0.01	0.58	0.36	0.46	0.01	0.71	0.25	0.09
p-val (ii) + (iii) = 0	0.51	0.07	0.13	0.54	0.57	0.48	0.95	0.18	0.18	0.37	0.61	0.26

Figure 2.1: Milestones in the reporting harmonization process in Europe

This figure depicts the main legislative steps to create a common set of accounting standards and frequency reporting for all European public listed firms.

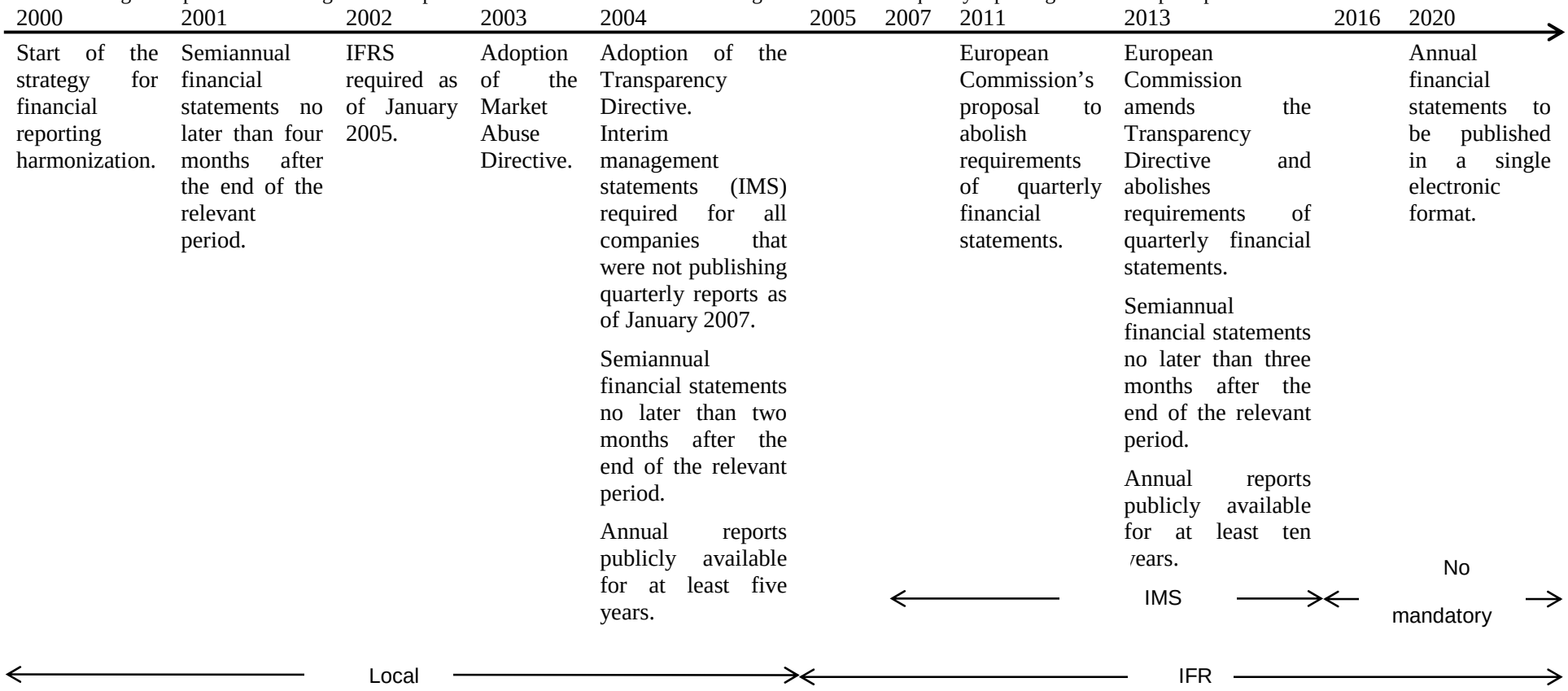


Figure 2.2: Annual forecast error cut-off dates during the fiscal year

This figure illustrates the four periods of the fiscal year in which the annual forecast error is computed. In each quarter, I retain the first annual earnings forecast for each firm-analyst pair issued in the last two months. Q1 to Q4 refers to the financial statements corresponding to quarter one to four. SAN1 refers to the financial statements corresponding to the first six months of the fiscal year.

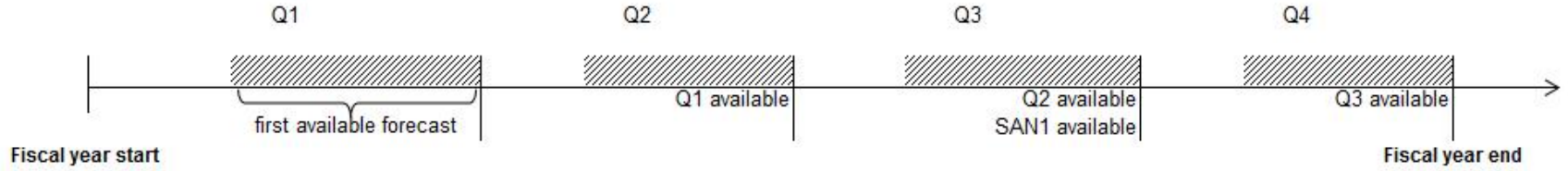
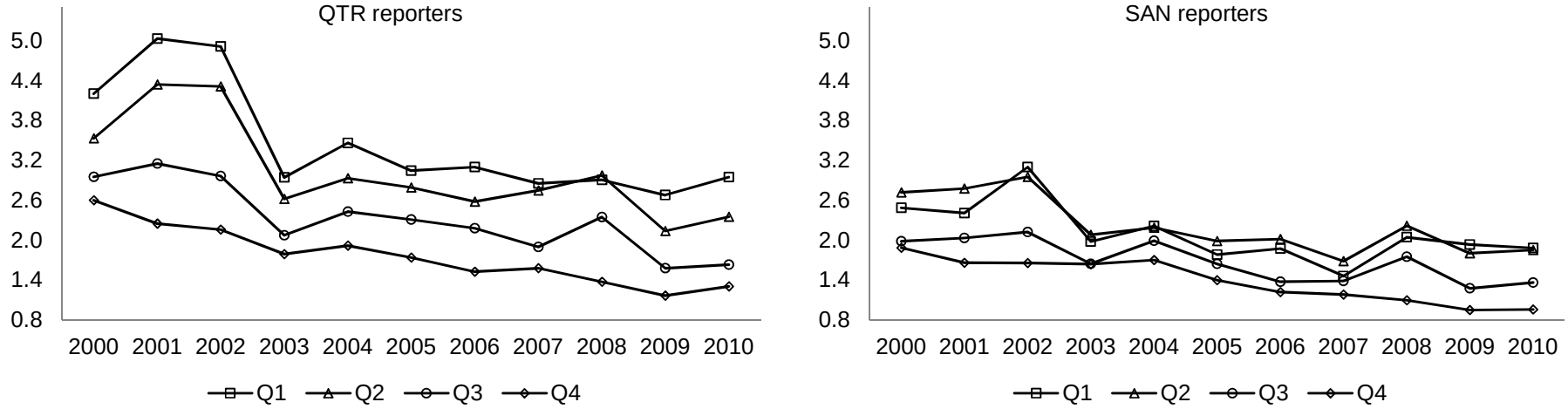


Figure 2.3: Analysts' annual information environment

These graphs present the average annual forecast error (Panel A), forecast dispersion (Panel B) and number of analyst following during each quarter of the fiscal year by type of reporting frequency regime. *FError* is the absolute difference between the analyst's annual earnings forecast and the actual annual earnings, deflated by firm's total assets per share. *Disp* is the standard deviation of annual earnings forecasts, deflated by firm's total assets per share. In each quarter, I retain the first annual earnings forecast for each firm-analyst pair issued in the last two months. *LogFollow* is the logarithm of the number of analysts following the firm during the entire fiscal quarter. QTR (SAN) reporters denote the firms reporting under the mandatory quarterly (semiannual) regime.

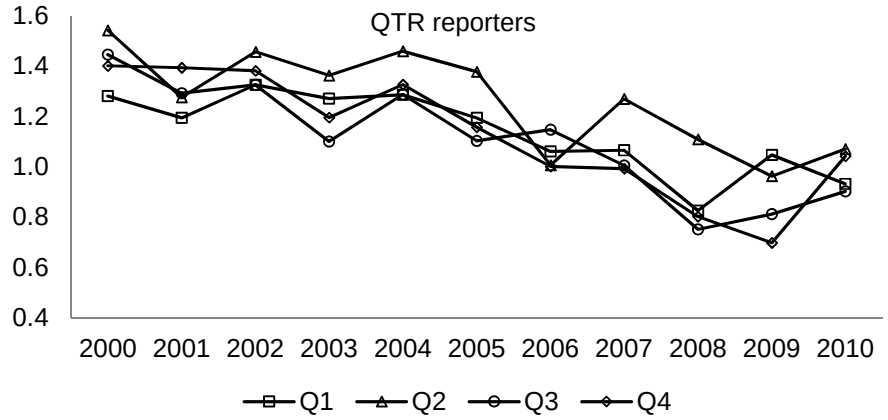
Panel A. *FError*

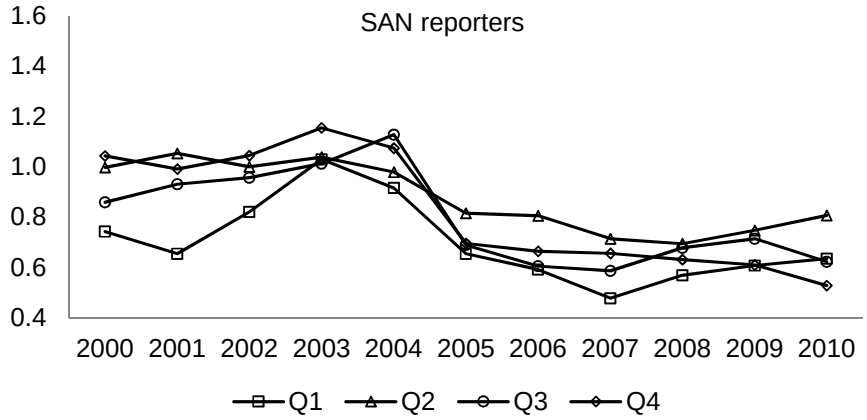


(continued)

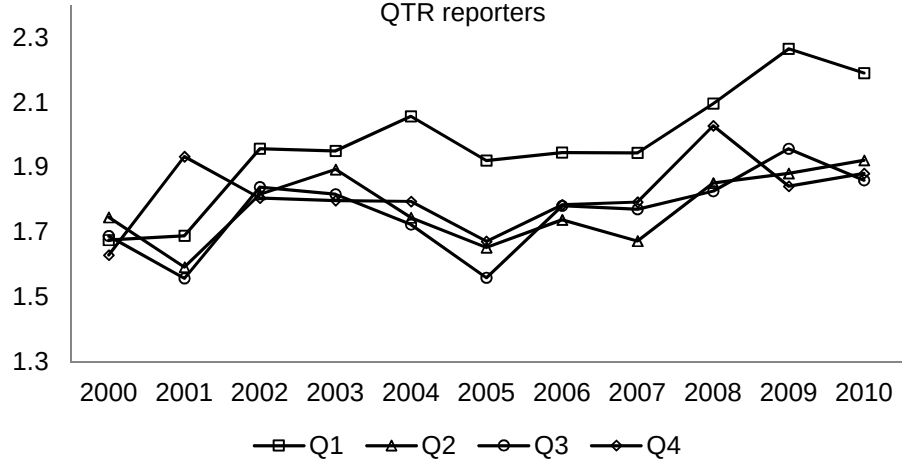
Figure 2.3
(continued)

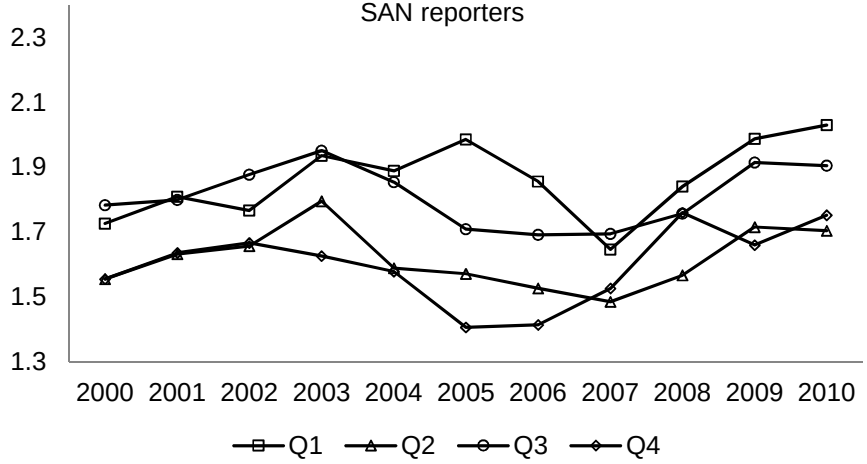
Panel B. *Disp*





Panel C. *LogFollow*





Chapter 3: Hype My Stock or Harm My Rivals? Another View on Analysts' Conflicts of Interest

(In collaboration with Michel Dubois)

3.1. Introduction

In an attempt to whittle down conflicts of interest, a wave of regulatory initiatives emerged in recent years and brought investment banks to the fore. The Sarbanes-Oxley Act (SOX) was enacted in 2002 to restore investors' confidence in the U.S. stock market. More specifically, Section 501(a) of the act relates to the codes of conduct for securities analysts and requires the disclosure of conflicts of interest. NASD (now FINRA) Rule 2711, NYSE Rule 427 and the Global Research Settlement Act (Global Settlement hereafter) followed shortly to address analysts conflicts of interest related to the research and investment banking departments.[1]

The ongoing debate on the real effects of these regulations has triggered widespread attention of both academics and professionals. However, mainstream research has been essentially inward looking into the direct effect of the law on the over-optimism bias for affiliated firms. Moreover, the current research has not incorporated yet a salient point, i.e. the alternative path that affiliated brokers might have followed to continue favoring their clients. Specifically, affiliated brokers could alter their rating policy by issuing more unfavorable recommendations for their client firms' rivals. As such, these clients could still benefit from the discrimination of their rivals within a purely legal environment.[2]

[1] We refer hereafter generically to SOX for the more complex and widespread regulatory initiatives that surged around its enactment.

[2] Evidence based on anecdotal accounts is pertaining to our investigation. In 2003, the French luxury group Louis Vuitton Moet Hennessy (LVMH) sued Morgan Stanley for alleged unfair research from analyst Claire Kent that favored direct competitor Gucci, with which the investment bank was involved at the time in an M&A deal. The star luxury goods analyst downgraded LVMH in July 2002 from outperforming to neutral, while maintaining Gucci on outperforming. In January 2004 the bank was ordered to pay $38 million, which it appealed. Finally, in a joint declaration in 2007, LVMH and Morgan Stanley agreed to resume business relations and settled their five year court battle without any paid compensation to either side. Moreover, the analyst's reputation remained untarnished, since two years after the suit, she ranked second in the Thomson Extel survey of luxury goods analysts. This case illustrates the difficulty to prove financial analysts' wrongdoing, if any, before the court. See, for instance, "Morgan Stanley, LVMH tango in Paris", by Ackman D., Forbes, May 27, 2003; "LVMH battles against Morgan Stanley in court", by Tagliabue, J., New York Times, November 18, 2003.

This paper takes the affiliation relationhip as the nexus of the analysis and treats the brokers' positive bias on affiliated firms with respect to their rivals as the basic building block of conflicts of interest. We ask two questions. First, do brokers provide significantly lower ratings for rival than for affiliated firms? Second, are investors aware of this form of conflicts? These questions are important since they refer to a class of conflicts unaddressed by the current regulation. To the best of our knowledge, no study to date addresses the question of whether affiliated brokers use different strategies to favor their clients, nor whether these innovations involve sidestepping the current regulatory framework.

We investigate these issues by focusing on recommendations and price targets. Consistent with our conjecture, we find that the proportion of favorable (unfavorable) recommendations of affiliated firms is significantly higher (lower) that the one for rival firms both before and after SOX. In line with previous studies, affiliated brokers became more pessimistic on average after the enactment of SOX. Strikingly, after 2002, rival firms have the lowest (highest) proportion of favorable (unfavorable) recommendations. Actually, the proportion of unfavorable recommendations before SOX was 3% for rival, 2% for affiliated and 4% for neutral firms, while this proportion is 15%, 11% and 13% after SOX. A similar pattern is revealed in price targets. This first set of results suggests that, while brokers comply with the regulation and issue less optimistic ratings, they nevertheless continue to keep their clients on significantly higher ratings than their clients' rivals.

To shed more light on this issue, we run a difference-in-differences model and estimate the bias in affiliated and rival ratings, as compared to neutral ones, before and after the enactment of SOX. The relative recommendations for affiliated firms are higher than the ones for rival firms before SOX; this difference is statistically significant at the 1% level. When assessing the impact of SOX, we find that brokers continue to issue significantly higher recommendations and price targets on affiliated firms compared to rival ones after SOX. In fact, brokers switched to a downward scale for both affiliated and rival firms, maintaining the rating gap.

We also check whether investors recognize this new form of bias by analyzing the short-term market reaction to recommendations before, and after SOX. We find that investors discounted the brokers'

affiliation bias before SOX. Specifically, the market reaction for added-to-buy recommendations for affiliated stocks was significantly lower, suggesting that the market recognized brokers' positive bias for their clients. To illustrate, the two-day cumulated market-adjusted abnormal return associated with added-to-buy recommendations for affiliated stocks was 1.2% lower than the one for rival stocks (statistically significant at the 5% level). At the same time, the brokers' bias for rival firms was not recognized in the short-run before SOX (i.e. we do not identify a difference in market reaction between rival and neutral stocks). After 2002, the two-day abnormal return of affiliated firms is not significantly different from the one of rival firms, suggesting that the market accounts for the passage of SOX and expects the conflicts of interest to have disappeared. Nevertheless, investors continue to be unaware of brokers' negative bias on rival firms.

This paper contributes to three streams of research. First, it uncovers a form of conflicts of interest in the investment banking industry that was not addressed so far. Second, similar to Kadan, Madureira, Wang, and Zach (2009), Ertimur, Sunder, and Sunder (2007) and Barber, Lehavy, McNichols, and Trueman (2006), we focus on the central question of whether the regulatory initiatives decreased the positive bias for affiliated stocks. However, we differentiate from these studies by shedding light on the unexpected effects of the recent regulations. To the extent that conflicts of interest were mitigated, we find evidence that brokers continue to favor their clients by following an alternative strategy without breaching the rules. Our analysis centers on the question raised by Mulherin (2007) of whether SOX had unintended consequences. Finally, we explore the effects of the institutional and regulatory environment on analysts' output.

The reminder of this chapter is organized as follows. Section 3.2 presents the hypotheses development in the context of the existing literature. In Section 3.3, we provide details on the sample construction and methodology. Section 3.4 presents and discusses the results. We extend the analysis to the short-term market reaction in Section 3.5. Section 3.6 concludes.

3.2. Prior research and hypotheses development

3.2.1. Conflicts of interest in sell-side equity research

Financial analysts play a key role in the dissemination of information to investors. Their production (i.e. earnings forecasts, price targets and stock recommendations) portends future stock performance and reduces information asymmetry; see, e.g., Barber, Lehavy, McNichols, and Trueman (2001); Boni and Womack (2006); Brav and Lehavy (2003); Kadan, Madureira, Wang, and Zach (2013). Loh and Mian (2006) emphasize the role of future earnings in predicting stock price movements and argue that more accurate earnings forecasts are correlated with more profitable recommendations. Kecskés, Michaely and Womack (2015) find that recommendation changes are more informative to investors when they are based on earnings, instead of discount rate revisions. At the same time, analysts' reports receive the most attention from investors when they incorporate bad news; see, e.g., Asquith, Mikhail, and Au (2005).

From the perspective of accurately predicting market performance, analysts working for investment banks are inherently subject to conflicts of interests. This bias was extensively investigated in prior research. Central to this literature is the notion that the pernicious effects of conflicts of interest in analysts' output fall on investors and distort the market.[3] Lin and McNichols (1998) identify a positive bias in the long-term growth forecasts from affiliated brokers. They find that investors react more negatively to neutral recommendations, consistent with the conjecture that the market interprets neutral ratings from affiliated brokers as downgrades. Likewise, Dechow, Hutton, and Sloan (2000) argue that long-term growth forecasts from affiliated brokers are systematically overoptimistic around equity offerings, and the level of optimism is correlated with their fees. Not only equity offerings, but also M&A deals, are prone to conflicts of interests. Kolasinski and Kothari (2008) note that affiliated brokers, both with the acquirer and the target firm, upgrade their recommendations and publish optimistic reports on the acquirer. Beyond financial analysts, Cain and Dennis (2013) provide evidence that, although valuations in the fairness opinions of target firms issued by the acquirers' advisors exhibit higher positive errors than those provided by the targets' advisors, they contain information useful to investors.

[3] See Mehran and Stulz (2007) for a comprehensive survey on the literature of conflicts of interest in investment banking.

Stock recommendations, by their relative nature, are the most exposed to optimistic bias from affiliated brokers. Michaely and Womack (1999) find that stocks recommended by affiliated brokers perform more poorly that the ones from unaffiliated brokers both before and after the recommendation announcement. Moreover, the market does not recognize this bias. In the same vein, Barber, Lehavy, and Trueman (2007) show that the average daily abnormal return from independent research firms with favorable recommendations exceeds the one from investment banks. Additionally, during the bear market of 2000s, these brokers were highly reluctant to downgrade stocks.

Besides underwriting relations, other factors such as trading fees, clients' sophistication and brokers' reputation are important determinants of the analysts' bias; see Bradshaw (2011). In particular, analysts working for retail brokerage houses are more optimistic than analysts serving only institutional investors, while the optimism is offset by the reputation of the bank and the presence of institutional investors; see, e.g., Cowen, Groysberg, and Healy (2006); Ljungqvist, Marston, Starks, Wei, and Yan, (2007). Indeed, analyst reputation is more effective than sole bank reputation in limiting conflicts of interests; see Fang and Yasuda (2009). Finally, strong legal enforcement and sanctions mitigate the magnitude of conflicts of interest; see, e.g., Dubois, Frésard, and Dumontier (2014).

3.2.2. The Sarbanes-Oxley Act and related regulations

The evidence so far shows that brokers' optimism bias towards their clients is not innocuous since limited objectivity of securities research can hurt issuers and investors alike. This salient feature was integrated into an extensive series of regulations meant to restore the public confidence in the U.S. stock market.[4]

[4] Adopted by the SEC in July 2002, the Sarbanes-Oxley Act required public disclosure of conflicts of interest together with the securities analysts' protection from retaliation of their employers in the event of unfavorable research reports. Similarly, NASD Rule 2711 and NYSE Rule 472 were implemented to improve the objectivity and transparency in the sell-side research. On April 28, 2003 the Global Settlement was enforced as an agreement among the SEC, NASD, the NYSE, the New York State Attorney General, and ten of the largest Wall Street investment banks to settle charges alleging misleading or fraudulent research; see Kadan et al. (2009, pp. 4193-4194) for the detailed requirement of each regulation.

A rich literature has emerged around the overarching question of the efficacy of these regulations.[5] Ertimur et al. (2007) argue, among others, that the profitability of "buy" and "hold" recommendations of affiliated analysts increased following this regulatory wave. Barber et al. (2006) show an increase in the proportion of unfavorable stock recommendations in the aftermath of the NASD Rule 2711, which requires brokers to release their ratings distribution. Likewise, after the passage of these regulations, affiliated analysts are less likely to issue optimistic recommendations compared to independent analysts. As such, optimistic (neutral and pessimistic) recommendations are more (less) informative; see Kadan et al. (2009). On the flip side, Buslepp, Casey, and Huston (2014) argue that the independent research funded by the Global Settlement is of lower quality than the one of investment banks in terms of future performance and market reaction to recommendations.

From another perspective, little is known about the unintended consequences of the regulation on brokers' behavior. One fact that gained increased public attention is the orphaned stock effect, especially for small firms. To the extent that brokers have to allocate high costs to comply with legal requirements, and most importantly, since the funding of research is limited due to its separation from investment banking, many stocks have lost immediate analysts coverage. To illustrate, Morgan Stanley and Merrill Lynch cut the number of North American covered stocks by 26% and 30% respectively as of April 2004.[6] According to Mehran and Stulz (2007, p. 291), the increased costs of producing analyst services result ultimately in a decrease in the efficiency of financial markets, because these costs can adversely affect valuations and make it difficult for companies to finance their growth. However, since there is no legal obligation to cover stocks, there is no wrongdoing in adopting this strategy.

3.2.3. Persistence of conflicts of interest

At the first glance, the aforementioned literature is consistent with a reduction of the optimism bias for affiliated brokers. It is important to stress that conflicts of interests were inadvertently understood as optimistic recommendations on affiliated firms. However, inflating affiliated stock recommendations is

[5] See Mulherin (2007) for an extensive analysis of the costs and benefits of regulating financial markets.
[6] See, for example, "Change comes slowly to Wall Street research", by Lee, M. and Metaxas, J., CNBC, April 26, 2004.

just one outcome of a bundle of possibilities relating to any other type of broker's misbehavior in trying to peddle their clients' stocks and generate commissions. Consistent with this conjecture, Carapeto and Gietzmann (2011) bring into light another type of affiliation relationship in the U.K. practice of the "corporate broker". According to the U.K. Listing Authority, every listed company on the London Stock Exchange must have an appointed broker to advice communication with the stock market on an ongoing basis. The corporate broker has unique access to private information and participates to board meetings. The authors note that, although the Chinese walls were created between the brokerage and investment bank activities, these brokers may easily know whether the management is considering a SEO and thus provide the information to their affiliated underwriters.

More recently, Lee (2013) identifies a new source of conflicts in the parent-subsidiary relationship. Not only analysts at the parent investment bank provide positively biased recommendations for the client firms, but also those employed in the broker's subsidiaries. Similar with extant literature, their recommendations have worse, or no investment value. Malmendier and Shantikumar (2014) find that affiliated analysts strategically distort recommendations since they are disseminated to small investors, as opposed to earnings forecasts, which are more prone to the dissemination among large, sophisticated investors. In fact, Guan, Lu and Wong (2012) document that the reforms targeting the conflicted research decreased the optimism in recommendations of the sanctioned banks, but not in earnings forecasts.

We start from the premise that favoring the affiliated firms by boosting their ratings is only one of the many forms to gain underwriting commissions. Specifically, we investigate whether brokers provide higher ratings for affiliated firms as compared to their rivals.[7] To the extent that affiliated brokers provide less-than-stellar ratings on their clients' rivals, they continue to curry favor with their clients by means of this strategy. Our objective is thus to explore the existence and to commensurate the magnitude of such conflicts of interests that materialize through the brokers' hostile attitude towards their client firms' rivals. We therefore state the first null hypothesis as follows.

[7] Recent findings suggest that peer firms play a central role in a firm's financing decisions through their characteristics and their own financing policies; see Frésard (2010), Leary and Roberts (2014).

Hypothesis 3.1a. *Before SOX, affiliated brokers provided similar ratings for their clients and their clients' rivals.*

To go further, we investigate whether SOX and the related regulations had an effect in mitigating the potential rating gap between affiliated and rival firms. The objective of these laws was to limit the positive bias towards client firms and no other form of conflicts was addressed. While these regulations ostensibly reduced the optimistic bias for affiliated firms, we expect that they inadvertently created a *de facto* situation in which other forms of the same conflict continue to exist. The second null hypothesis is the following.

Hypothesis 3.1b. *The rating gap between affiliated and rival firms vanished after SOX.*

The regulations aiming to decrease conflicts of interest made it apparent that, at the time, the markets were devoid of adequate recognition of such conflicts. To investigate whether this was the case, we ascertain the difference in the short-term market reaction between the affiliated and rival stocks. If the brokers' bias is recognized before SOX, then investors should discount (assign a premium to) the affiliated (rivals) stocks that are added-to-buy accordingly. In the same vein, we should observe the most (least) negative market reaction for added-to-sell recommendations for affiliated (rival) stocks. We therefore test the following null hypothesis.

Hypothesis 3.2a. *Before SOX, there is no difference in investors' reaction to ratings between affiliated and rival firms.*

Investors' reaction to biased recommendations ought a priori to be related with the passing of the regulations. Since the regulations' main objective is to limit such conflicts, we check whether the market accounts accordingly for its effective implementation, i.e., whether investors recognize the limitation of both types of bias. Hence, our fourth hypothesis is as follows.

Hypothesis 3.2b. *There is no difference in the short-term market reaction to ratings between affiliated and rival firms after SOX.*

3.3. Data and methodology

3.3.1. Sample construction

We focus on both stock recommendations and price targets for several reasons. First, recommendations were at the core of the scandals and the regulations put in place to curb these conflicts are mainly centered on recommendations. The Institutional Brokers' Estimate System (I/B/E/S) translates on a single five-point scale a multitude of specific rating scales, which opens the possibility that the relative nature of a recommendation differs from one broker to another; see Kadan et al. (2009, 2013). We integrate price targets to our analysis since recommendations have no mutually-agreed-upon interpretation. As such, price targets allow us to identify the bias of affiliated brokers without a concern for misinterpretation. Moreover, while some studies focus on the performance of price targets, they open the field for the question of whether price targets are subject to conflicts of interest as well; see, e.g., Brav and Lehavy (2003); Bradshaw, Brown and Huang (2013); Gleason, Johnson and Li (2013).

The sample selection process is illustrated in Table 3.A.1. The initial sample consists of all outstanding recommendations and price targets from the I/B/E/S U.S. Historical Detail files for the period January 1996 to December 2008.[8] I/B/E/S started collecting recommendations in October 1993 and the data is scant for the first three years. Price targets enter the database as of February 1999. The data collection stops in 2008 to provide a time balanced sample around the adoption of SOX in 2002.[9]

In May 2002, NASD 2711 Rule imposed the disclosure of the percentage of all securities rated by each broker to whom they would assign a "buy", "hold" or "sell" rating. Consequently, during this year many brokers changed their rating scale and issued recommendations in bulk for the firms they were covering at the moment.[10] Following Loh and Stulz (2011), we count for this structural break for the top 100 largest

[8] Following Ljungqvist, Malloy and Marston (2009a), a recommendation (price target) is outstanding if it has been confirmed by the broker in the I/B/E/S review date field in the last twelve months and has not been stopped by the broker in the I/B/E/S Stopped file.

[9] Ljungqvist et al. (2009a) identify changes in recommendation archives from one I/B/E/S download to another during the 2000-2007 period; subsequently Thomson Reuters had announced that it has solved the problem. We collect the data for both recommendations and price targets as of December 2011 snapshot so that they are not subject to this issue.

[10] We find no evidence of this structural break in price targets.

brokers in terms of total number of recommendations issued during the entire period.[11] The recommendations issued in bulk on the new rating scale are removed from the dataset since they do not bring additional information. We identify 19 brokers that resumed coverage in 2002, with a change from four- or five-point scale to three- or four-point scale. For eight brokers, these days are identical with the ones in Kadan et al. (2009). Three of these brokers change again their rating scale in 2003, 2006 and 2008 respectively. We find additional changes in rating scales in 1997 (one broker), 2001 (one broker), and between 2003 and 2008 (16 brokers). Finally, we reverse the I/B/E/S scale and code recommendations from one (strong sell) to five (strong buy), so that higher values correspond to more positive recommendations.

We perform several adjustments on the price targets sample. First, to preserve a common interpretation across brokers, we keep only the one-year-ahead price targets. These observations count for 98% of the initial sample. Second, we extract current stock prices from CRSP[12] and work with unadjusted data in both I/B/E/S and CRSP. Finally, we compute the price target to current price ratio and winsorize the sample at 1% on both tails.

3.3.1.1. Identification of affiliated recommendations and price targets

Brokers' conflicts of interest are determined with data on IPO, SEO, public debt issues and M&A deals obtained from the Security Data Company Platinum (SDC) database and syndicated loans from LPC's DealScan (LPC). Following Ljungqvist, Marston and Wilhelm (2006), we construct corporate families so that we give credit to a parent firm for any form of investment banking relationship of its subsidiaries. In addition, since many banks were the product of mergers and acquisitions during the sample period, we account for all the bank's predecessors when identifying an investment banking relationship.

[11] In total, these brokers count for 80% of the observations. We check for cases when the broker stopped the totality or the majority of her coverage and resumed the coverage in the subsequent days on a different rating scale. One year post-resumption, we check whether the broker continues to use the new rating scale. If this is the case, we assume that this broker uses the new rating scale starting from the resumption day.

[12] We check previously for any inconsistencies between the firm's currencies reported in I/B/E/S and CRSP on the same day and remove around 0.1% of the observations.

To identify an affiliated recommendation or price target, we proceed as follows. First, we match the firms in I/B/E/S with those from SDC (LPC) by the firm's CUSIP parent (official ticker) at a given date. Second, since each database provides a different form of the brokers' name, we match by hand the brokers' names in SDC and LPC with those from the I/B/E/S Broker's Translation file. When the matching between two names is not unique, we search for additional information on the identity and the type of the broker in the Internet and on Nelson's Directory of Investment Research.[13] In April 2008, I/B/E/S stopped providing the names of the brokers in the database. In our sample, we identify 99 out of 930 distinct brokers for which the names are not available. Though the majority of these I/B/E/S codes could not be matched in SDC or LPC via the Nelson's Directory of Investment Research, these brokers count for less than 2% of the total observations. To assure a maximum consistency in the names matching, we use a restrictive matching procedure, so that we create a link between two databases only when the two names clearly depict the same broker.

Finally, a broker is identified as conflicted at the time of the recommendation or price target issue if she is a lead or co-manager for at least one IPO, SEO, public debt issue, financial advisor for an M&A deal, lender or co-lender in a syndicated loan with the given firm from one year prior to the recommendation or price target announcement date. We impose this interval of time in line with the restrictions of SOX (Sect. 501-b3), which requires the disclosure of any existing conflicts between the issuer and the broker during the one year period preceding the issue date.[14] Kadan et al. (2009) point to the fact that the interests of co-managers are similar with those of lead managers. Ljungqvist, Marston and Wilhelm (2009b) also show that appointments for co-management allow banks to establish relationships with the issuers and gain more mandates in the near future. For these reasons, we consider both lead managers and co-managers as conflicted.

[13] For example, Cantor Fritzgerald is identified as "Cantor Fritgera" in I/B/E/S, "Cantor Fritzgerald Inc" in SDC and "Cantor Fritzgerald Securities" in LPC. Additionally, we identify cases in which, for the same name in I/B/E/S, there is more than one entry in the Nelson's Directory. For instance, Renaissance Capital in I/B/E/S can be linked to either Renaissance Capital LLC (independent research firm) or Renaissance Capital Ltd (investment bank). In such cases, we go one step further and check the analysts' name employed at each house and link them to the analyst's name in I/B/E/S to choose the correct matching firm.

[14] We later use alternative time intervals for defining conflicts of interests from three months prior to the deal initiation to five years after the deal initiation. The results, available upon request, do not change qualitatively.

3.3.1.2. Identification of rival recommendations and price targets

Our objective is to identify recommendations and price targets issued on direct competitors of the affiliated firms. To do so, we use the text-based network industry classification (TNIC) from Hoberg and Phillips data library; see Hoberg and Phillips (2010), Hoberg and Phillips (2015). [15] We proceed in two steps. First, for each affiliated firm, we retain its TNIC competitors on which the affiliated broker issues recommendations or price targets during the affiliation period. Second, we define as rival the firm with the highest pairwise TNIC score with an affiliated firm during the calendar year. To avoid cases with weak similarity scores, we retain TNIC pairwise scores that are above the annual average score of the entire set of competitors. [16] Following this definition, for each affiliated firm, we use a set of rivals that is updated annually.

Two aspects of the baseline definition of rivals deserve attention. First, the set of competitors for a given firm may be different from the broker's perspective compared to the firm's perspective. By construction, the TNIC scores are based on the same level of coarseness as the three-digit SIC codes. We form a set of potential rival firms by retaining all firms covered by the affiliated broker in the same three-digit SIC code as the affiliated firm during the calendar year of the recommendation or price target issue.

Second, the playing field of competition may not imply the product market exclusively. From an investor' perspective, two firms compete in the stock market if they are close substitutes in her portfolio. Consistent with this view, affiliated brokers rely on a set of highly substitutable firms for their customers and only follow a subset of the entire panel of the client's product market competitors. As such, for each affiliated firm-year, we retain as rivals the competitor firms in the same three-digit SIC group with which the affiliated firm has the highest weekly return correlation over the calendar year. [17] This definition allows us to identify both the direct competitors in the product market and the closest substitutes in terms of portfolio management. The choice for the firms with the highest correlation in returns is also based on the

[15] We thank the authors for making this dataset available at http://cwis.usc.edu/projects/industrydata/.

[16] The results are not sensitive to this filter. Similarly, the findings remain robust when we alternatively consider the first five firms with the highest TNIC pairwise score with the affiliated firm (instead of the first one).

[17] We use weekly return correlation to avoid lead/lag effects in stock returns (Lo and MacKinley, 1990). The findings are not sensitive when using a daily frequency.

affiliated brokers' objective. Specifically, we conjecture that brokers indirectly favor affiliated firms by issuing negative ratings on their rivals that an investor could easily replace in her portfolio with the client firm.

3.3.1.3. Final sample

The sample of recommendations and price targets consists of affiliated, rival, neutral, and simultaneously affiliated and rival firms. While a firm can enter in our sample as both affiliated and rival for a certain broker and date, it is not clear to what extent the corresponding recommendations and price targets are biased. Therefore, they are excluded from the sample. We eliminate 1.5% (1.4%) of total recommendations (price targets) by applying this filter.

Finally, for each firm-broker we retain the most recent recommendation or price target during the quarter. The final sample in Table 3.A.1 contains 6% (8%) affiliated firm-broker observations for recommendations (price targets). Rival recommendations (price targets) count for 6% (5%) of the total sample. The rest of the observations are neutral (88% for both samples).

3.3.2. Research design

To test hypotheses 3.1a and 3.1b, we examine the brokers' bias on their relative ratings and estimate the following difference-in-differences model.

$$Y_{b,i,t} = {}_1 AFFILIATED_{b,i,t} + {}_2 RIVAL_{b,i,t} + {}_3 SOX_t + {}_4 AFFILIATED_{b,i,t} \times SOX_t + {}_5 RIVAL_{b,i,t} \times SOX_t + \delta' X_{b,i,t} + \ddot{} _i + £_{b,i,t} \quad (1)$$

where subscripts *b*, *i*, *t* stand for broker, firm, and time respectively.

The dependent variable, $Y_{b,i,t}$, represents the recommendation $(RELREC_{b,i,t})$ or the price target to current price ratio $(RELPT_{b,i,t})$ issued by broker *b* on firm *i* in quarter *t* minus the mean outstanding recommendation (price target to current price ratio) from the rest of the brokers covering firm *i*. To ensure the time matching of recommendations (price targets) among brokers, we follow Ljungqvist et al. (2007)

and keep the most recent recommendations (price targets) per firm-broker over the last three months.[18] We also require at least two other brokers covering the stock when computing the consensus.[19] We focus our analysis at the broker (and not at the analyst) level for two reasons. First, the issuer-bank relationship is the first stage through which conflicts of interest materialize. Second, the financial regulations target primarily the research at the investment banking level.

The magnitude of the conflicts of interest at the firm-broker level is estimated by the *AFFILIATED* dummy. This variable equals one if the broker is a lead manager, co-manager, financial advisor, lender or co-lender for the firm in a least one IPO, SEO, public debt offering, M&A deal or syndicated loan less than one year prior to the recommendation or price target date. The dummy variable *RIVAL* counts for the bias in rival recommendations or price targets. It takes the value of one if the firm has the highest TNIC pairwise similarity score with an affiliated firm of the broker during the same period. Since our sample consists of three groups, the *AFFILIATED* and *RIVAL* dummies capture the difference in the relative rating with the neutral firms. To account for the influence of the U.S. regulations on brokers' bias, we introduce *SOX*, a dummy variable that equals one if the recommendation or price target is issued after the adoption of SOX.

In line with prior studies, the additional variables (*X*) control for other sources of bias in brokers' recommendations and price targets. Since large institutions have more resources to support research and, supposedly, more access to private information, we follow Stickel (1995) and control for the size of the broker based on the number of analysts employed over the past quarter *(BROKERSIZE)*. We include the number of brokers who issued at least one recommendation or price target on the stock over the past quarter to capture the stock's information environment *(NBROKER)*. To account for the fact that brokers may become optimistic about a stock because it is performing well or because of market-wide optimism, we include the stock's market-adjusted return *(PASTFIRMPERF)* and market return *(PASTMKTPERF)* for the preceding six months; see Kadan et al. (2009), Jegadeesh, Kim, Krische and Lee (2004). We also

[18] We consider alternatively the prior six and twelve months in computing the recommendation and price target consensus and obtain similar results.

[19] One potential concern is that with a small number of ratings, the timing of their releases can substantially affect the *relative* nature of our dependent variable. The results are not sensitive to this restriction.

include a dummy variable that equals one if the broker initiates the coverage for the firm *(INITIATION)*. Table 3.A.2 details the construction of the variables.

To account for the time-invariant firm heterogeneity, we use a vector of firm fixed effects $(\ddot{}_i)$ and cluster standard errors by quarter; see Petersen (2009).[20] We use the above specification for the following reasons. First, to identify the presence of a fixed effect, we compute the standard errors of the model by clustering at each of the three dimensions. Additionally, we assess the magnitude of the correlation between each group with the observed covariates. While we cannot reject the hypothesis of the presence of each effect, the correlation between the broker specific effect and the observables is close to zero. This suggests that the estimates from fitting a firm fixed effect model, i.e. a model which assumes that the broker-error component and the variables of interest are uncorrelated, should be preferred to a broker fixed effects model.[21]

3.4. Empirical results

3.4.1. Descriptive evidence

Table 3.1 presents the descriptive statistics on the recommendations and price targets. Panel A reports the distribution of recommendations by year and by rating. Two notable features emerge from the data. Firstly, the proportion of unfavorable recommendations is around 4% before 2002. Consistent with previous findings, this low number corresponds to brokers' general reluctance to issue unfavorable ratings on the covered firms during that period; see, e.g., Barber, Lehavy, McNichols and Trueman (2003); Jegadeesh and Kim (2006). Secondly, the distribution becomes more balanced after 2002 with a decrease (an increase) in the proportion of favorable (unfavorable) recommendations. To illustrate, the percentage

[20] While we use the most conservative method to account for the dependency in the data, our inferences are not sensitive to this choice.

[21] Although no standard methodology has been so far developed for estimating three-way error-component models, this topic has started to gain some attention in recent years; see Andrews, Schank and Upward (2006); Gormley and Matsa (2014). We confirm further the implication of using firm fixed effects by fitting a three-way fixed effects model, which groups all unique firm-broker combinations (i.e. by defining a "spell"; see Andrews et al., 2006). This model eliminates both the unobserved firm- and broker-error components, while clustering standard errors by quarter. The estimates from this model are close to those of the firm fixed effects model with clustered standard errors by quarter, implying that the broker specific component and the covariates are uncorrelated. The results are available upon request.

of favorable (unfavorable) recommendations dropped (increased) from 71% (2%) in 2000 to 40% (14%) in 2008. Simultaneously, the percentage of hold recommendations increased substantially from 27% in 2000 to 46% in 2008, suggesting that brokers became more pessimistic in the aftermath of the regulations enacted around SOX. The distribution of recommendations by year is generally smooth, with a peak in 2002. The number of covered firms increases from 1,710 in 2001 to 2,003 in 2002, implying higher coverage intensity for this year. The fact that we still depict a higher number of recommendations after controlling for resumption in light of the change in rating scales is also directly related to the almost 25% of the top 100 brokers, which did not resume their coverage (in the I/B/E/S Stopped file) when issuing recommendations on different scales. Barber et al. (2006, p. 97) use data from FirstCall and identify a similar peak in the number of recommendations.

[Insert Table 3.1 about here]

The distribution by year of the price target to current price ratio is depicted in Panel B. Brokers map the recommendation ratings on their assessment of the stock's expected return within twelve months; see Kadan et al. (2013), Table 3.1. In other words, favorable and unfavorable recommendations derive from favorable and unfavorable views on the stock's future performance. We classify a price target as favorable (unfavorable) if it belongs to the third (first) tercile of price target to current price ratio over the last three months.[22] This mapping allows us to mirror the price targets to each of the three recommendation categories (i.e. favorable, hold and unfavorable).

The targeted one-year-ahead price increase lies between 0 and 50% in the 1st (3rd) tercile, whereas the mean (median) of the ratio is 1.22 (1.07) for the entire period. Similar to recommendations, we identify a drop in the price target to current price ratio subsequent to 2002. This ratio decreases from 1.76 (1.09) in the 3rd (1st) tercile in 2000 to 1.38 (0.98) between 2003 and 2008. This decrease in price targets reinforces the idea that affiliated brokers became more pessimistic in the post regulation period. Similar to Brav and Lehavy (2003), price targets observations contain fewer covered firms (5,018 versus 5,445), fewer brokers (546 versus 667), and are issued almost three times more frequently compared to recommendations.

[22] We alternatively classify a price target as favorable (unfavorable) if it belongs to the third (first) tercile over the calendar year and obtain similar results.

We detail the distribution of each category of recommendation and price target in Table 3.2. Panel A breaks down the distribution of recommendations for each type of rating. In line with prior studies, we find that affiliated recommendations have the highest percentage of favorable views in all years before 2002; see, e.g., Kadan et al. (2009); Ljungqvist et al. (2007). They account for 71% in the pre-SOX period, whilst rival and neutral firms count for 64% and 63% of favorable recommendations over the same period.

We observe a sharp change after 2002. While the proportion of favorable recommendations drops for all three types of firms, rival firms now have the lowest proportion of favorable recommendations. To illustrate, the proportion equals 40% for affiliated, 35% for rival, and 41% for neutral firms respectively after SOX. This pattern is complemented by the evolution of unfavorable recommendations. As expected, affiliated brokers are the most reluctant to issue negative views on their clients during the entire period. Nevertheless, after 2002, the proportion of unfavorable ratings increases for all types of firms, and brokers issue the highest proportion of unfavorable recommendations on rival firms. Actually, in the periods before and after SOX, the proportion of unfavorable recommendations went from 3% (2%) to 15% (11%) for rival (affiliated) firms.

At the first glance, affiliated brokers have complied with the regulations to come forth with less optimistic research on their clients. Looking more closely, however, the downward switch in ratings for clients went simultaneously with a greater downward switch for rivals. Thus, the fact that affiliated brokers became more pessimistic had no effect on the existing rating gap between client and rival firms.

Panel B depicts the distribution of price targets in the 1st and 3rd tercile. Rival firms have the lowest (highest) proportion of price targets in the 3rd (1st) tercile over the entire period. The proportion of price targets in the 3rd tercile for rival (affiliated) firms decreases from 32% (37%) before SOX to 29% (32%) after SOX. As with recommendations, there exists a constant gap between affiliated and rival price targets, both before and after SOX. This finding suggests that although brokers appear to be mainly concerned with applying the law on their recommendation scales, they nevertheless manifest a similar pattern in price targets by favoring (disfavoring) the affiliated (rival) firms.

[Insert Table 3.2 about here]

3.4.2. Negative ratings on rival stocks

We start by ascertaining the affiliated and rival bias from the baseline specification in Table 3.3. Panel A reports the results for recommendations. In column (1), the coefficient on affiliated firms *(AFFILIATED)* is positive and statistically significant at the 1% level. This result is similar to what has been documented in the literature; see, e.g., Ljungqvist et al. (2007); Kadan et al. (2009). The average effect of rival firms *(RIVAL)* is positive and significant, suggesting that, prior to the enactment of SOX, affiliated brokers issued higher recommendations also for their clients' rivals compared to neutral firms. This positive bias in recommendations for rival firms can be driven from the proximity and the common business characteristics with the affiliated firms. However, the magnitude of the bias is almost half of the one on affiliated firms. More importantly, the relative recommendations for affiliated firms are 0.07 (0.13 - 0.06) higher than the ones for rival firms before SOX. This difference (i.e. $_1 - _2$) is statistically significant (p-value = 0.00). Therefore, we reject Hypothesis 3.1a.

After the adoption of SOX, we notice that the gap between affiliated (0.13 – 0.12 = 0.01) and rival (0.07 – 0.14 = -0.07) recommendations persists. The difference between the two biases (i.e. $(_1 + _4) - (_2 + _5)$) is positive and statistically significant (p-value = 0.03). Within this setting, the gap continues to exist in the aftermath of the regulation; therefore we reject Hypothesis 3.1b. In other words, even if affiliated brokers comply with the regulation by reducing the over-optimism for their clients, they continue to keep their clients on significant higher ratings compared to their rivals. Indeed, the gap between affiliated and rival does not disappear with SOX, since the coefficients on *AFFILIATED x SOX* and *RIVAL x SOX* are not significantly different (p-value = 0.76). Overall, the control variables display signs that are in line with related studies; see, e.g., Ljungqvist et al. (2007); Kadan et al. (2009).

[Insert Table 3.3 about here]

To verify if our results are robust, we run several alternative specifications. In column (2), we require at least one outstanding recommendation per firm-broker both before and after SOX. To reduce heterogeneity among firms and brokers, in column (3) we consider the sample of brokers that simultaneously cover rival, affiliated and neutral firms, and firms that appear at least once as rival,

affiliated, and neutral over the sample period; see Malmendier and Shantikumar (2014). Although the sample size reduces to almost half, the results are consistent with the baseline specification. In column (4), *RIVAL* is defined as the firm in the same three-digit SIC code group as an affiliated that has the highest weekly return correlation with the affiliated over the calendar year. Again, the results are similar to those obtained with the baseline specification.

We run the same specifications on price targets and present the results in Panel B. The findings correspond to our previous results on recommendations. It appears that conflicted research is not only first and foremost present in recommendations, but also in price targets. To illustrate, the price targets on affiliated firms are 2% higher than those on rival firms before SOX (the difference is statistically significant at 1% level). We reject Hypothesis 3.1a over all four specifications. Similar to recommendations, after the introduction of SOX, the gap between the relative price targets for affiliated and rival firms continues to be significantly positive (p-value = 0.00). Therefore, we reject Hypothesis 3.1b.

Figure 3.1, Panel A (B) displays the affiliation bias in relative recommendations (price targets) for affiliated and rival firms over the entire period. For each panel, we run quarterly regressions of the baseline model and plot the evolution of the gap between affiliated and rival relative recommendations and price targets. First, we remark that the bias drops just after the enactment of SOX, showing that affiliated brokers became more pessimistic. Although less obvious for recommendations in the first years, the parallel evolution of the two biases confirms our results so far. Shortly after 2002, this parallel trend slowly diverges for as long as two years, showing that affiliated brokers downgraded the rival firms in the short-run. Likewise, the bias for rival firms turns mostly negative in both recommendations and price targets after SOX.

[Insert Figure 3.1 about here]

To provide a different perspective on our findings, we assess the effect of SOX on the over-optimism bias for affiliated firms as compared separately with rival, and then with neutral firms. The results are presented in Table 3.4. In columns (1) and (3), the coefficient on *AFFILIATED x SOX* is significantly

negative (t-stat -4.63 and -4.85 respectively), so that affiliated brokers decreased their over-optimism compared to neutral firms after SOX. When comparing this result with columns (2) and (4), we observe that SOX had no impact in curbing the existing gap in ratings between affiliated and rival firms. Indeed, the coefficient on *AFFILIATED x SOX* is not statistically different from zero in neither recommendations nor price targets. These findings highlight a striking aspect of the consequences of regulations aiming to curb conflicts of interest in investment banking. Although in all appearance necessary, this regulation left the door open for the affiliated brokers to continue favoring their clients through another channel.

[Insert Table 3.4 about here]

3.4.3. Evaluating alternative hypotheses

3.4.3.1. Selection hypothesis

The issuer may select for underwriting or advisory mandates those brokers that have the most positive views on their business. This process may therefore lead to more optimistic brokers for affiliated firms but only because they were already more optimistic *before* (i.e. *the selection hypothesis*); see Lin and McNichols (1998). We compare the coefficient on *AFFILIATED* in the baseline specification with the corresponding one when considering the affiliation period over six months. When the affiliation relationship is considered solely over the next six months from the deal date, the coefficient is 33% higher compared to the one obtained with a one year window around the date at which the broker had the deal with the covered firm. In addition, compared to the baseline specification, the *AFFILIATED* coefficient is 12% (29%) lower in recommendations (price targets) when we extend the affiliation period over five years. As such, brokers are the most aggressive in issuing over-positive ratings especially in the periods following the recent deals.

Our results also demonstrate that brokers were significantly more positive on rival than on neutral firms before SOX, and this was reversed subsequently. According to the selection channel, we should observe a significantly greater proportion of rival firms that became subsequently clients for the issuing broker before SOX, and conversely after SOX. We check for future investment banking ties for each rival and

neutral firm that became a client for the same broker in the next year (six to 18 months) or three years (six to 42 months). We exclude the six month period before the deal since during that period the investment bank is likely to have already access to private information related to the firm.

[Insert Table 3.5 about here]

Table 3.5, Panel A details the results. As the interpretation of recommendations and price targets samples is similar, we restrict the comments to the former group. On average, 2.32% (4.67%) of all firms that were rivals before SOX become clients for the same broker in the next year (three years), while the corresponding percentage for neutral firms is 1.11% (2.09%). The difference between the two groups is statistically significant at the 1% level. We observe a similar pattern after SOX, with 1.65% (3.31%) of rival, and 0.75% (1.29%) of neutral firms becoming clients in the next year (three years). Likewise, the difference between the two groups is statistically significant at the 1% level, irrespective of the period for future deals. Accordingly, the selection channel could at most explain the more positive views on rival than neutral firms before the enactment of the law. However, this hypothesis is unable to explain the changing behavior of affiliated brokers after SOX, with no more positive views on rival than neutral firms.

3.4.3.2. Information hypothesis

Another potential explanation for the over-optimism in brokers' ratings is that they have a privileged access to the firm-specific information. If this information is incorporated into price targets and recommendations, we could systematically observe over-optimism for affiliated firms even in the absence of conflicts of interest (i.e. *the information hypothesis*); see, e.g., Michaely and Womack (1999). More recently, Green, Jame, Markov and Subasi (2014) show that analysts' access to management is one important source of informational advantage. As such, their research is more informative and leads to abnormal returns. If the information channel was at play, in both periods we should observe a significant and a higher abnormal return when the broker issues a rating on an affiliated firm.

Following this conjecture, and the common characteristics of the business for affiliated and rival firms, the positive information on affiliated firms should at least partially spread into positive information on

rival firms - but to a lesser extent, since brokers do not have an equal access to these firms. This is in line with what we find before SOX. After SOX, however, affiliated firms continue to enjoy the same positive bias with respect to rival firms, while the gap between rival and neutral firms widens. As such, we extend the information hypothesis to the possibility that affiliated brokers have additional information on the long-term prospects of rival and neutral firms. If this was the case, we should observe that rival firms perform significantly better (equal to, or significantly worse) than neutral firms before (after) SOX.

To examine whether our results reflect the information channel, in Table 3.5, Panel B we analyze the abnormal returns of recommendations and price targets for affiliated, rival and neutral stocks before and after SOX. We follow Womack (1996) and construct added-to-buy (i.e. initiations and upgrades to Buy or Strong Buy) and added-to-sell (i.e. initiations and downgrades to Sell or Strong Sell) lists. The cumulated market-adjusted abnormal returns over the next six months in added-to-buy recommendations are 6.7% for rival, 2.7% for affiliated, and 2.8% for neutral stocks before SOX, and 0.7%, 1.4% and 2.1% respectively after SOX. Importantly, these differences are not statistically significant in either period. Before and after SOX, the abnormal performance of affiliated stocks is not significantly different from the one of rival stocks (p-value = 0.16 and 0.69 respectively). The weak performance of affiliated stocks before SOX relates to similar prior findings suggesting that brokers do not rely on private information concerning the firm's prospects when issuing positive ratings. However, in light of the small number of observations we obtain for these tests, the results should be interpreted with caution. For price targets, we find no significant difference between the six-month performance of rival and affiliated stocks before SOX (p-value = 0.50). After SOX, affiliated stocks exhibit the highest performance (statistically higher than both rival and neutral stocks at the 1% level). However, rival and neutral stocks do not perform differently, suggesting that brokers would have no motive to issue more negative price targets on rival, than on neutral stocks. Overall, we conclude that our results do not support the information hypothesis.

3.5. Short-term market reaction to affiliated and rival bias

In the previous section, we document that the conflicts of interest appear in two forms. First, brokers issue higher ratings on affiliated firms over all periods, although this bias decreases after the adoption of SOX; see Ertimur et al. (2007). Second, brokers issue significantly less favorable ratings on rival firms, and this behavior is more prevalent after SOX. When a broad swath of investors is unaware of such conflicts, they are likely to hinder the market by distorting the perceptions of future stocks performance. Consequently, we check whether the market recognizes the two forms of bias previously documented. Specifically, we measure to what extent investors recognize the positive (negative) bias for affiliated (rival) firms, and whether SOX had an impact in bringing more awareness of such conflicts.

To do so, we focus our analysis on added-to-buy and added-to-sell recommendations. The reason we investigate these recommendations is that investors are most likely to react when brokers issue extreme ratings; see Womack (1996). We start by reporting the average short-term market reaction in Table 3.6. The variable of interest is the two-day cumulated abnormal return *(CMAR)*, computed from the event trading date to the next trading day following the recommendation. Daily excess returns are computed as the difference between the stock's return and the CRSP value-weighted daily index.

[Insert Table 3.6 about here]

We identify a positive (negative) two-day market reaction to added-to-buy (added-to-sell) recommendations, significantly different from zero (at the 1% level). In line with previous studies, investors react more strongly to unfavorable recommendations prior to SOX; see, e.g., Kadan et al. (2009). To illustrate, the average two-day cumulated abnormal return for added-to-buy (added-to-sell) recommendations is 1.52% (-3.96%) for affiliated firms, and 2.8% (-4.85%) for rival firms. After SOX, there is no difference in the two-day cumulated abnormal return among affiliated, rival and neutral recommendation in neither added-to-buy nor added-to-sell sample.

To test hypotheses 3.2a and 3.2b, we estimate the following model.

$$CMAR_{b,i,t} = {}_0 + {}_1 AFFILIATED_{b,i,t} + {}_2 RIVAL_{b,i,t} + {}_3 SOX_t + {}_4 AFFILIATED_{b,i,t} \times SOX_t + {}_5 RIVAL_{b,i,t} \times SOX_t + {}_6 \Delta ER_{b,i,t} + ¥'X_{b,i,t} + £_{b,i,t} \quad (2)$$

As we document a bias in both recommendations and price targets, we control for cases when recommendations and price target changes are released simultaneously. We introduce Δ*ER* (i.e., change in expected return), an indicator variable that equals one when both recommendations and price targets are released during the window.[23] If investors react to new information, then the simultaneous price targets should absorb most of the short-term market reaction around recommendations.

In line with prior research, we iron out the remaining idiosyncrasies among recommendations by controlling for several broker and firm characteristics. Since investors' reaction may be associated with the size of the broker issuing the rating, we control for the broker size in terms of the number of analysts employed in the current quarter *(BSIZE)*; see Ertimur et al.(2007); Salva and Sonney (2011); Stickel (1995). We also control for the number of years for which the broker covers the stock *(EXPERIENCE)*; see Kadan et al, (2009); Ertimur et al. (2007); Loh and Stulz (2011). To account for momentum, we control for firm past performance *(PASTFIRMPERF)* and market past performance *(PASTMKTPERF)*; see Kadan et al. (2009). Finally, prior findings suggest that the market reaction is partly captured by concurrent earnings announcements; see, e.g., Salva and Sonney (2011); Asquith et al. (2005); Loh and Stulz (2011); Michaely and Womack (1999). We control for such cases by introducing an indicator variable that equals one if the recommendation is issued in the three-day window surrounding an earnings announcement *(EPSDATE)*.

According to Hypothesis 3.2a, if the market does not recognize the brokers' bias before SOX, then investors should not discount recommendations for affiliated stocks compared to rival ones.

[Insert Table 3.7 about here]

Table 3.7 reports the regression results of model (2). We observe that the short-term market reaction for added-to-buy recommendations is significantly weaker for affiliated stocks before SOX. Compared to rival stocks, the difference in the two-day market-adjusted abnormal return (i.e. $_1 - _2$) is significantly negative at the 1% level. For example, in specification (1), the two-day cumulated market-adjusted

[23] The results are qualitatively equivalent if we do not impose this constraint, nor if we define Δ*ER* as the difference between current price target scaled by price and the most recent price target scaled by price issued by the same broker on the same stock.

abnormal return for affiliated stocks is 1.29% smaller than that of rival stocks (statistically different from zero at the 1% level). This difference holds after adding the vector of control variables in specifications (2) and (3).[24] Therefore, we reject Hypothesis 3.2a.

The above results suggest that investors are skeptical to favorable recommendations for brokers' clients. At the same time, in all specifications, the estimated coefficient on *RIVAL* is not statistically different from zero, consistent with the conjecture that, before SOX, market participants did not recognize the brokers' negative bias for rival stocks.

Following Hypothesis 3.2b, if investors consider that SOX was effective in limiting the affiliation bias, then we should observe no difference in the short-term market reaction to recommendations between affiliated and rival stocks after SOX. We identify no difference in the two-day performance of rival, affiliated and neutral stocks in all specifications for both added-to-buy and added-to-sell recommendations. Specifically, the p-value associated with the difference in abnormal returns between affiliated and rival stocks after SOX (i.e. $(\ _1 + \ _4) - (\ _2 + \ _5)$) lies between 0.35 and 0.61. Therefore, we do not reject Hypothesis 3.2b. This finding shows that investors translated the current regulation as an *ex-post* decrease in brokers' positive bias on affiliated firms, despite the persistence of the conflicts.

Taken together, these results illustrate that the sophistication of investors in correctly depicting biased recommendations for the brokers' clients before SOX has not insulated them from recognizing the persistence of such conflicts after the passage of SOX. Consequently, investors expect the conflicts of interest to have disappeared and remain unaware of brokers' negative bias on rival firms in the aftermath of the regulations.

[24] We cannot reject that the difference in the market reaction before SOX between affiliated and rival firms is zero for the sample of added-to-sell recommendations. However, this result is driven by the small sample size in this panel (i.e., the power of the test is less than 0.10 compared to 0.86 in the panel of added-to-buy recommendations).

3.6. Conclusions

The main message of this paper is that the inherently subjective nature of affiliated research continues to exist in spite of the amount of regulatory initiatives aiming to curb conflicts of interest in the investment banking industry. More specifically, we find that affiliated brokers issue persistently higher recommendations and price targets on their client firms compared to their clients' rivals. To the best of our knowledge, this form of conflict was not investigated previously in the literature. While affiliated brokers become, on average, more pessimistic after the enactment of SOX, they nevertheless continue to favor their clients by keeping their rivals on significantly lower ratings. Our findings are robust to several alternative specifications and definitions of the variables of interest. Additionally, we find no consistent support for other hypotheses (selection bias and positive information) that could mislead the interpretation of our results. As such, we document that brokers' alternatives for hyping their clients' stocks were all but dismal after the passage of SOX. We further show that, while investors discount to some extent the positive recommendations bias for affiliated stocks before SOX, the downward bias for rival firms is not recognized in the short-run. Furthermore, the brokers' biases (i.e. positive for affiliated and negative for rivals) remain unperceived by investors in the post-regulation period.

This study contributes to the ongoing debate on the effects of the current regulations. It is now a befitting question to ask whether regulators are able to account in the future for the various innovations that can sidestep regulatory restrictions, and more importantly, at what costs. While we attempt to show that conflicts of interest persist in spite of regulatory efforts, identifying the channels through which financial analysts' incentives change after the regulations is out of the scope of this paper. We look forward to research clarifying whether shaping efficient regulations is indeed achievable, or the conflicts of interests in the investment banking industry are stronger and bound to prevail.

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Appendix

Table 3.A.1: Sample selection

This table presents the sample construction for recommendations (Panel A) and price targets (Panel B) respectively. Affiliated firms have at least one business deal (IPO, SEO, public debt offering, M&A, syndicated loan) with the broker one year or less prior to the recommendation or price target issue. Rival represent the competitor firm of an affiliated with the highest pairwise TNIC score, and above the yearly average, during the current year. Neutral firms are defined as neither Affiliated nor Rival.

Panel A: Recommendations	
IBES outstanding recommendations, 1996-2008	311,769
Less	
Recommendations issued on multiple times during the day per firm-broker (keep the most recent)	(633)
Recommendations issued in bulk around rating scale changes	(12,765)
Recommendations with no corresponding data in CRSP/COMPUSTAT	(10,987)
Simultaneous affiliated and rival recommendations	(4,262)
Recommendations issued on multiple times during the quarter per firm-broker (keep the most recent)	(27,719)
Recommendations with no corresponding data for control variables	(10,026)
Recommendations with less than two outstanding brokers to compute consensus	(129,721)
Final sample	115,656
Affiliated	6,711
Rival	6,603
Neutral	102,342
Panel B. Price targets	
IBES outstanding price targets, 1999-2008	633,732
Less	
Price targets with horizon different from 12 months	(13,102)
Price targets issued on multiple times during the day per firm-broker (keep the most recent)	(2,142)
Price targets with no corresponding data in CRSP/COMPUSTAT	(31,185)
Simultaneous affiliated and rival price targets	(8,201)
Price targets issued on multiple times during the quarter per firm-broker (keep the most recent)	(131,389)
Price targets with no corresponding data for control variables	(6,982)
Price targets with less than two outstanding brokers to compute consensus	(96,716)
Final sample	334,015
Affiliated	26,680
Rival	15,019
Neutral	292,316

Table 3.A.2: Variables definition and source

Variable	Definition	Source
$RELREC_{b,i,t}$	The recommendation for firm *i* issued by broker *b* at time t minus the mean recommendation of the rest of the brokers covering the same firm during the last three months.	I/B/E/S
$RELPT_{b,i,t}$	The price target for firm *i* issued by broker *b* scaled by the current stock price minus the consensus price target. Consensus price target is computed as the mean price target scaled by the current stock price of the rest of the brokers covering the same firm during the last three months.	I/B/E/S, CRSP
$AFFILIATED_{b,i,t}$	Dummy variable equal to one if broker *b* had at least one business deal (IPO, SEO, debt raising, M&A, syndicated loan) with firm *i* less than one year prior to the recommendation or price target date, zero otherwise.	I/B/E/S, SDC,LPC DealScan
$RIVAL_{b,i,t}$	Dummy variable equal to one if, during the year of the recommendation or price target issue, firm *i* is classified as a competitor for at least one of the broker *b*'s affiliates with the highest TNIC pairwise score (and above the annual average score of the affiliated), and zero otherwise.	CRSP, I/B/E/S, SDC,LPC DealScan
SOX_t	Dummy variable equal to one if the recommendation or price target is issued after July 31th 2002, zero otherwise.	
$BSIZE_{b,t}$	Number of analysts working for broker *b* during the current quarter.	I/B/E/S
$\Delta ER_{b,i,t}$	Dummy variable equal to one if the recommendation issued by broker *b* on firm *i* is accompanied by a price target revision in the two-day event window, and zero otherwise.	I/B/E/S
$EPSDATE_{b,i,t}$	Dummy variable equal to one if broker *b* issues a recommendation in the three-day window surrounding an earnings announcement of firm *i*, zero otherwise.	I/B/E/S
$EXPERIENCE_{b,i,t}$	Number of years for which the broker covers the stock.	I/B/E/S
$INITIATION_{b,i,t}$	Dummy variable equal to one if broker *b* initiates the coverage for firm *i* at time *t*, zero otherwise.	I/B/E/S
$NBROKER_{i,t}$	Logarithm of the number of brokers covering firm *i* during the current quarter.	I/B/E/S
$PASTFIRMPERF_{i,t}$	Cumulative market-adjusted return over the preceding six months (months -6 through -1) for firm *i*.	CRSP
$PASTMKTPERF_t$	Cumulative market return over the preceding six months (months -6 through -1).	CRSP

Table 3.1: Descriptive statistics

This table reports the proportion of stock recommendations by rating (Panel A), the average price target to current price ratio by tercile over the last three months (Panel B), and the number of recommendations (price target to current price ratio), brokers, and firms covered by year. Favorable (unfavorable) recommendations are defined as Buy and Strong Buy (Sell and Strong Sell) recommendations.

Panel A: Recommendations

Year	Favorable (%)	Hold (%)	Unfavorable (%)	# Rec	# Firms	# Brokers
1996	59.60	34.47	5.94	5,492	1,187	184
1997	62.25	33.20	4.55	6,156	1,375	219
1998	63.75	33.06	3.19	8,143	1,602	233
1999	67.44	29.70	2.85	8,696	1,656	223
2000	70.84	27.13	2.03	8,978	1,557	224
2001	61.12	35.88	3.00	11,453	1,710	203
2002	51.00	40.94	8.05	16,813	2,003	216
2003	40.88	46.55	12.57	11,678	1,744	261
2004	41.63	46.10	12.27	8,606	1,483	276
2005	42.05	46.26	11.69	6,965	1,487	270
2006	39.69	47.68	12.62	6,996	1,509	255
2007	39.05	49.24	11.71	7,217	1,601	243
2008	39.68	46.39	13.93	8,463	1,597	242
Total	52.17	39.84	7.99	115,656	5,445	667

Panel B: Price targets

Year	3^{rd} tercile	2^{nd} tercile	1^{st} tercile	# PT/P	# Firms	# Brokers
1999	1.563	1.246	1.082	12,898	1,779	141
2000	1.757	1.279	1.088	23,025	2,102	173
2001	1.745	1.252	1.059	27,701	2,179	160
2002	1.635	1.222	1.040	33,173	2,225	173
2003	1.367	1.144	0.968	32,500	2,254	245
2004	1.347	1.136	0.977	36,240	2,462	278
2005	1.327	1.133	0.984	37,663	2,652	279
2006	1.339	1.134	0.985	39,946	2,774	271
2007	1.322	1.129	0.987	42,728	2,840	265
2008	1.597	1.195	0.991	48,141	2,765	267
Total	1.496	1.168	0.999	334,015	5,018	546

Table 3.2: Percentage distribution of recommendations and price targets by affiliation

This table reports the percentage of favorable (Buy and Strong Buy) and unfavorable (Sell and Strong Sell) recommendations (Panel A) and 3rd and 1st tercile of price targets (Panel B) for rival, affiliated and neutral firms. Affiliated firms have at least one business deal (IPO, SEO, public debt offering, M&A, syndicated loan) with the broker one year or less prior to the recommendation or price target issue. Rival represents the competitor firm of an affiliated with the highest pairwise TNIC score, and above the yearly average, during the current year. Neutral firms are defined as neither Affiliated nor Rival.

Panel A: Recommendations

	Favorable (%)						Unfavorable (%)					
Year	Affiliated (i)	Rival (ii)	Neutral (iii)	p-val. (i) - (ii)	p-val. (ii) - (iii)	p-val. (i) - (iii)	Affiliated (i)	Rival (ii)	Neutral (iii)	p-val. (i) - (ii)	p-val. (ii) - (iii)	p-val. (i) - (iii)
Before (a)	70.48	63.89	62.82	0.00	0.22	0.00	2.14	2.71	3.78	0.13	0.00	0.00
After (b)	39.86	35.32	40.65	0.00	0.00	0.37	11.14	15.46	12.54	0.00	0.00	0.02
p-val. (b) - (a)	0.00	0.00	0.00	-	-	-	0.00	0.00	0.00	-	-	-
1996	68.13	58.72	59.18	0.02	0.87	0.00	3.30	4.07	6.22	0.61	0.11	0.05
1997	75.74	63.25	61.44	0.00	0.50	0.00	2.30	2.56	4.80	0.82	0.05	0.04
1998	69.20	65.67	63.27	0.25	0.28	0.01	2.01	1.80	3.36	0.81	0.06	0.12
1999	71.37	70.33	67.02	0.72	0.13	0.05	2.11	2.28	2.93	0.85	0.41	0.30
2000	77.41	69.59	70.45	0.00	0.68	0.00	1.23	2.14	2.08	0.24	0.92	0.16
2001	67.59	61.64	60.71	0.04	0.67	0.00	1.30	2.29	3.14	0.20	0.27	0.01
2002	58.70	48.01	50.59	0.00	0.13	0.00	5.71	9.29	8.16	0.00	0.23	0.00
2003	41.80	34.38	41.16	0.01	0.00	0.74	10.78	18.48	12.38	0.00	0.00	0.22
2004	43.07	35.85	41.92	0.02	0.01	0.62	9.17	14.26	12.33	0.01	0.21	0.04
2005	35.83	37.74	42.70	0.58	0.05	0.01	10.83	14.18	11.57	0.16	0.11	0.67
2006	37.99	30.46	40.49	0.02	0.00	0.32	11.76	15.89	12.44	0.08	0.03	0.69
2007	38.91	37.47	39.18	0.64	0.46	0.90	12.06	12.63	11.61	0.79	0.50	0.76
2008	37.28	37.50	40.00	0.94	0.23	0.25	12.50	15.17	13.92	0.22	0.40	0.39
Total	55.61	49.51	52.11	0.00	0.00	0.00	6.51	9.13	8.01	0.00	0.00	0.00

(continued)

Table 3.2
(continued)

Panel B: Price targets												
	3rd tercile (%)						1st tercile (%)					
Year	Affiliated (i)	Rival (ii)	Neutral (iii)	p-val. (i) - (ii)	p-val. (ii) - (iii)	p-val. (i) - (iii)	Affiliated (i)	Rival (ii)	Neutral (iii)	p-val. (i) - (ii)	p-val. (ii) - (iii)	p-val. (i) - (iii)
Before (a)	37.16	32.21	34.49	0.00	0.00	0.00	28.00	33.39	33.19	0.00	0.79	0.00
After (b)	32.24	28.78	35.04	0.00	0.00	0.00	32.47	37.80	32.89	0.00	0.00	0.23
p-val. (b) - (a)	0.00	0.00	0.00	-	-	-	0.00	0.00	0.00	-	-	-
1999	39.35	32.40	34.06	0.00	0.33	0.00	27.60	33.74	33.28	0.00	0.79	0.00
2000	37.86	33.71	35.91	0.02	0.13	0.09	27.98	31.89	33.02	0.02	0.43	0.00
2001	32.76	30.46	31.35	0.18	0.52	0.18	30.32	34.68	35.01	0.01	0.82	0.00
2002	37.38	29.95	34.28	0.00	0.00	0.00	27.63	35.98	34.00	0.00	0.11	0.00
2003	32.19	24.25	32.56	0.00	0.00	0.70	32.39	40.32	34.27	0.00	0.00	0.05
2004	31.66	28.29	34.31	0.02	0.00	0.00	31.96	38.17	32.77	0.00	0.00	0.35
2005	31.27	28.18	35.38	0.03	0.00	0.00	33.29	37.24	32.21	0.01	0.00	0.24
2006	30.47	28.24	34.30	0.09	0.00	0.00	34.20	37.13	33.32	0.03	0.00	0.29
2007	32.79	29.68	36.40	0.02	0.00	0.00	31.07	37.58	31.71	0.00	0.00	0.43
2008	35.15	32.50	37.91	0.04	0.00	0.00	32.49	36.52	31.65	0.00	0.00	0.34
Total	33.50	29.70	34.90	0.00	0.00	0.00	31.33	36.61	32.96	0.00	0.00	0.00

Table 3.3: Brokers' bias and SOX

This table presents the results of regressions examining the brokers' bias in recommendations and price targets by affiliation. In column (1), the dependent variable is the relative recommendation (price target/current price), defined as the difference between the current recommendation (price target/current price) and the consensus. In column (2), we require each broker-stock pair to appear at least once in the sample both before and after the enactment of SOX. In column (3), we limit the heterogeneity of firms and brokers by reducing the sample to firms that are simultaneously affiliated rival and neutral for different brokers and to brokers that cover simultaneously affiliated, rival and neutral firms. In column (4) we define Rival as the same three-digit SIC stock with which the affiliated firm had the highest weekly return correlation during the current year. Affiliated firms have at least one business deal (IPO, SEO, public debt offering, M&A, syndicated loan) with the broker one year or less prior to the recommendation or price target issue. Rival represents the competitor firm of an affiliated with the highest pairwise TNIC score, and above the yearly average, during the current year. All specifications include firm fixed effects. Robust standard errors (in parenthesis) are clustered at the quarter level. ***, ** and * denote statistical significance at 1%, 5% and 10% respectively. The control variables are defined in Table 3.A.2.

Panel A: Recommendations	Baseline (1)	Pre/Post SOX (2)	Reduced sample (3)	Rivals SIC3 (4)
AFFILIATED ($_1$)	0.1332***	0.1386***	0.1539***	0.1306***
	(0.0182)	(0.0238)	(0.0199)	(0.0207)
RIVAL ($_2$)	0.0671***	0.0776***	0.0701***	0.0767***
	(0.0162)	(0.0194)	(0.0193)	(0.0143)
SOX	0.0212	0.0062	0.0108	0.0173
	(0.0232)	(0.0257)	(0.0258)	(0.0233)
AFFILIATED x SOX ($_4$)	-0.1243***	-0.1314***	-0.1519***	-0.0972***
	(0.0274)	(0.0327)	(0.0300)	(0.0264)
RIVAL x SOX ($_5$)	-0.1359***	-0.1551***	-0.1499***	-0.1091***
	(0.0281)	(0.0306)	(0.0350)	(0.0261)
BSIZE	-0.0010***	-0.0012***	-0.0010***	-0.0010***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
NBROKER	-0.0746***	-0.0766***	-0.0676***	-0.0745***
	(0.0097)	(0.0119)	(0.0124)	(0.0097)
PASTFIRMPERF	-0.0255**	-0.0344**	-0.0105	-0.0252**
	(0.0119)	(0.0141)	(0.0131)	(0.0116)
PASTMKTPERF	0.2724***	0.2895**	0.2987***	0.2663***
	(0.0895)	(0.1156)	(0.0948)	(0.0889)
INITIATION	0.1223***	0.0903***	0.0581***	0.1240***
	(0.0143)	(0.0177)	(0.0194)	(0.0145)
Constant	0.0762***	0.1274***	0.1104***	0.0780***
	(0.0223)	(0.0261)	(0.0245)	(0.0220)
Obs.	115,656	74,744	49,265	114,000
Adj. R-sq.	0.01	0.01	0.01	0.01
Hypothesis (p-val.)				
$\beta_1 - \beta_2 = 0$	0.00	0.01	0.00	0.01
$(\beta_1 + \beta_4) - (\beta_2 + \beta_5) = 0$	0.03	0.02	0.04	0.02

(continued)

Table 3.3
(continued)

Panel B: Price targets	Baseline (1)	Pre/Post SOX (2)	Reduced sample (3)	Rivals SIC3 (4)
AFFILIATED ($_1$)	0.0460***	0.0415***	0.0440***	0.0469***
	(0.0056)	(0.0062)	(0.0055)	(0.0061)
RIVAL ($_2$)	0.0252***	0.0274***	0.0275***	0.0271***
	(0.0080)	(0.0077)	(0.0082)	(0.0068)
SOX	-0.0095	-0.0130	-0.0082	-0.0085
	(0.0118)	(0.0116)	(0.0109)	(0.0117)
AFFILIATED x SOX ($_4$)	-0.0351***	-0.0289***	-0.0322***	-0.0367***
	(0.0073)	(0.0073)	(0.0066)	(0.0076)
RIVAL x SOX ($_5$)	-0.0246***	-0.0252***	-0.0289***	-0.0290***
	(0.0091)	(0.0079)	(0.0091)	(0.0079)
BSIZE	-0.0005***	-0.0004***	-0.0004***	-0.0005***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
NBROKER	0.0132**	0.0122**	0.0129**	0.0133**
	(0.0050)	(0.0051)	(0.0054)	(0.0050)
PASTFIRMPERF	0.0176**	0.0169**	0.0184**	0.0179**
	(0.0069)	(0.0065)	(0.0072)	(0.0070)
PASTMKTPERF	-0.0335	-0.0095	-0.0241	-0.0338
	(0.0383)	(0.0349)	(0.0322)	(0.0381)
INITIATION	-0.0120*	-0.0130**	-0.0118*	-0.0114*
	(0.0064)	(0.0060)	(0.0061)	(0.0064)
Constant	0.0088	0.0086	0.0045	0.0077
	(0.0115)	(0.0123)	(0.0126)	(0.0112)
Obs.	334,015	251,111	198,393	331,962
Adj. R-sq.	0.01	0.01	0.01	0.01
Hypothesis (p-val.)				
$\beta_1 - \beta_2 = 0$	0.00	0.05	0.04	0.00
$(\beta_1 + \beta_4) - (\beta_2 + \beta_5) = 0$	0.00	0.00	0.00	0.00

Table 3.4: The affiliation bias relative to rival and neutral recommendations and price targets

This table presents the results of the baseline model examining the brokers' affiliation bias for their clients in two distinct samples. In column (1) and (3) the sample is restricted to affiliated and rival recommendations and price targets. In column (2) and (4) the sample is restricted to affiliated and neutral recommendations and price targets. The dependent variable is the relative recommendation (price target/current price), defined as the distance between the current recommendation (price target/current price) and the consensus. Affiliated firms have at least one business deal (IPO, SEO, public debt offering, M&A, syndicated loan) with the broker one year or less prior to the recommendation or price target issue. Rival represents the competitor firm of an affiliated with the highest pairwise TNIC score, and above the yearly average, during the current year. All specifications include firm fixed effects. Robust standard errors (in parenthesis) are clustered at the quarter level. ***, ** and * denote statistical significance at 1%, 5% and 10% respectively. The control variables are defined in Table 3.A.2.

	Recommendations		Price targets	
	Affiliated+Neutral	Affiliated+Rival	Affiliated+Neutral	Affiliated+Rival
	(1)	(2)	(3)	(4)
AFFILIATED ($_1$)	0.1336***	0.0653**	0.0460***	0.0143*
	(0.0181)	(0.0278)	(0.0055)	(0.0074)
SOX	0.0216	-0.1489***	-0.0096	-0.0348***
	(0.0234)	(0.0331)	(0.0117)	(0.0104)
AFFILIATED x SOX ($_4$)	-0.1241***	-0.0064	-0.0352***	-0.0003
	(0.0268)	(0.0421)	(0.0073)	(0.0080)
BSIZE	-0.0010***	-0.0005	-0.0005***	-0.0003***
	(0.0002)	(0.0004)	(0.0001)	(0.0001)
NBROKER	-0.0738***	-0.0774***	0.0130**	0.0123**
	(0.0098)	(0.0232)	(0.0051)	(0.0055)
PASTFIRMPERF	-0.0257**	-0.0035	0.0175**	0.0139*
	(0.0113)	(0.0244)	(0.0069)	(0.0075)
PASTMKTPERF	0.2732***	0.4130***	-0.0332	-0.0344
	(0.0903)	(0.0968)	(0.0403)	(0.0275)
INITIATION	0.1253***	0.0173	-0.0115*	-0.0166**
	(0.0145)	(0.0271)	(0.0065)	(0.0068)
Constant	0.0722***	0.1483***	0.0093	0.0241
	(0.0229)	(0.0459)	(0.0113)	(0.0144)
Obs.	109,053	13,314	318,996	41,699
Adj. R-sq.	0.01	0.01	0.01	0.01
Hypothesis (p-val.)				
$\beta_1 + \beta_4 = 0$	0.69	0.13	0.01	0.00

Table 3.5: Alternative explanations to conflicts of interests

This table reports the results of evaluating the alternative explanations for the affiliation bias. In Panel A, we test whether the current bias is associated with future potential deals. We report the percentage of rival and neutral firms that became clients for the issuing broker in the future. In Panel B, we test whether the current rating is associated with the brokers' private information on the stock's prospects. We report the monthly excess return over the next six months computed as the difference between stock's return and the CRSP value-weighted index. Affiliated firms have at least one business deal (IPO, SEO, public debt offering, M&A, syndicated loan) with the broker one year or less prior to the recommendation or price target issue. Rival represents the competitor firm of an affiliated with the highest pairwise TNIC score, and above the yearly average, during the current year. Neutral firms are defined as neither Affiliated nor Rival. Added-to-buy (added-to-sell) recommendations consist of upgrades (downgrades) from the previous recommendation (issued by the same broker on the same stock) and initiations to Buy or Strong Buy (Sell or Strong Sell).

Panel A: Selection hypothesis

	Affiliated in the next year			Affiliated in the next three years		
	Rival (i)	Neutral (ii)	p-val. (i) - (ii)	Rival (i)	Neutral (ii)	p-val. (i) - (ii)
Recommendations						
Before SOX	2.32%	1.11%	0.00	4.67%	2.09%	0.00
After SOX	1.65%	0.75%	0.00	3.31%	1.29%	0.00
Price targets						
Before SOX	8.07%	3.60%	0.00	16.61%	7.83%	0.00
After SOX	7.98%	3.31%	0.00	13.50%	5.47%	0.00

Panel B: Information hypothesis

	Affiliated (i)	Rival (ii)	Neutral (iii)	p-val. (i) - (ii)	p-val. (ii) - (iii)	p-val. (i) - (iii)	Affiliated (i)	Rival (ii)	Neutral (iii)	p-val. (i) - (ii)	p-val. (ii) - (iii)	p-val. (i) - (iii)
Recommendations	Added-to-buy						Added-to-sell					
Before SOX	2.69%	6.71%	2.82%	0.16	0.07	0.95	-3.36%	-4.04%	0.59%	0.93	0.40	0.45
N	(236)	(226)	(4,247)				(38)	(35)	(809)			
After SOX	1.42%	0.67%	2.13%	0.69	0.25	0.60	3.59%	4.88%	5.46%	0.59	0.71	0.32
N	(349)	(396)	(5,845)				(216)	(315)	(3,205)			

	Price targets					
	Affiliated (i)	Rival (ii)	Neutral (iii)	p-val. (i) - (ii)	p-val. (ii) - (iii)	p-val. (i) - (iii)
Before SOX	0.56%	0.91%	-0.15%	0.50	0.01	0.03
N	(9,334)	(5,559)	(104,302)			
After SOX	3.60%	2.14%	2.45%	0.00	0.14	0.00
N	(24,926)	(13,454)	(280,494)			

Table 3.6: Short-term abnormal returns by affiliation

This table reports the average two-day cumulative abnormal returns (in percentage) and the breakdown of recommendations by type of firm relative to added-to-buy (Panel A) and added-to-sell recommendations (Panel B). The short-term abnormal return is the two-day excess return, computed as the difference between stock's return and the CRSP value-weighted index. Added-to-buy (added-to-sell) recommendations consist of upgrades (downgrades) from the previous recommendation and initiations to Buy or Strong Buy (Sell or Strong Sell). Affiliated firms have at least one business deal (IPO, SEO, public debt offering, M&A, syndicated loan) with the broker one year or less prior to the recommendation or price target issue. Rival represents the competitor firm of an affiliated with the highest pairwise TNIC score, and above the yearly average, during the current year. Neutral firms are defined as neither Affiliated nor Rival.

	Affiliated (i)	Rival (ii)	Neutral (iii)	p-val. (i) - (ii)	p-val. (i) - (iii)	p-val. (ii) - (iii)
Panel A. Added-to-buy						
Before SOX	1.52	2.80	2.08	0.00	0.12	0.04
After SOX	2.95	3.36	3.12	0.15	0.46	0.28
Panel B. Added-to-sell						
Before SOX	-3.96	-4.85	-3.82	0.71	0.93	0.44
After SOX	-3.50	-4.03	-3.95	0.38	0.37	0.85

Table 3.7: Multivariate tests on short-term market reaction to biased recommendations

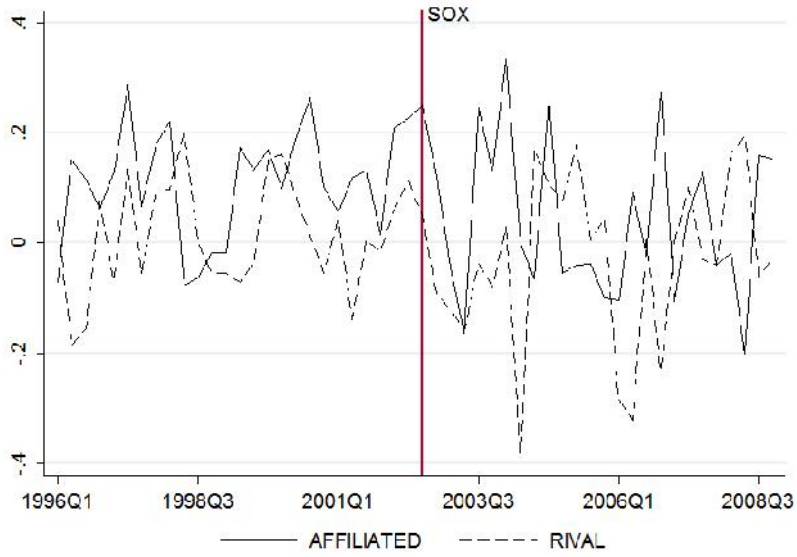
This table presents the two-day cumulative abnormal returns (in percentage) relative to added-to-buy (Panel A) and added-to-sell recommendations (Panel B). Daily excess return is computed as the difference between stock's return and the CRSP value-weighted index. Added-to-buy (added-to-sell) recommendations consist of upgrades (downgrades) from the previous recommendation (issued by the same broker on the same stock) and initiations to Buy or Strong Buy (Sell or Strong Sell). Affiliated firms have at least one business deal (IPO, SEO, public debt offering, M&A, syndicated loan) with the broker one year or less prior to the recommendation or price target issue. Rival represents the competitor firm of an affiliated with the highest pairwise TNIC score, and above the yearly average, during the current year. Robust standard errors (in parenthesis) are clustered at the quarter level. ***, ** and * denote statistical significance at 1%, 5% and 10% respectively. The control variables are defined in Table 3.A.2.

	Panel A. Added-to-buy			Panel B. Added-to-sell		
	(1)	(2)	(3)	(1)	(2)	(3)
AFFILIATED ($_1$)	-0.5968**	-0.6849**	-0.6820**	-0.0798	-0.1685	-0.2049
	(0.2580)	(0.2666)	(0.2651)	(2.6312)	(2.5065)	(2.5455)
RIVAL ($_2$)	0.6891	0.5232	0.5262	-0.9145	-0.9075	-0.8080
	(0.4349)	(0.4416)	(0.4362)	(1.9285)	(1.9273)	(1.9622)
SOX	0.8988***	1.0785***	1.0489***	0.3288	-0.1394	-0.0177
	(0.1994)	(0.1935)	(0.1906)	(0.5542)	(0.5328)	(0.5345)
AFFILIATED x SOX ($_4$)	0.3923	0.5475	0.5942	0.5804	0.4087	0.4312
	(0.3723)	(0.3743)	(0.3729)	(2.6706)	(2.5680)	(2.6098)
RIVAL x SOX ($_5$)	-0.4644	-0.2612	-0.2119	0.9144	0.8000	0.7055
	(0.5306)	(0.5291)	(0.5230)	(2.0848)	(2.0798)	(2.1123)
Δ*ER*	0.4443***	0.5104***	0.4285**	-1.0313***	-1.2083***	-1.1153***
	(0.1623)	(0.1807)	(0.1805)	(0.3010)	(0.2924)	(0.2916)
PASTMKTPERF		-1.2418	-1.3114		-1.3430	-1.0141
		(0.9430)	(0.9503)		(1.6868)	(1.6904)
PASTFIRMPERF		-0.7356***	-0.7452***		2.6476***	2.5284***
		(0.2488)	(0.2482)		(0.5744)	(0.5585)
EXPERIENCE		-0.1561***	-0.1487***		0.2320***	0.2190***
		(0.0161)	(0.0163)		(0.0476)	(0.0471)
BSIZE		0.0083***	0.0090***		-0.0092*	-0.0099**
		(0.0019)	(0.0019)		(0.0049)	(0.0048)
EPSDATE			-1.2107***			2.1829***
			(0.1840)			(0.3115)
Constant	1.8740***	2.2125***	3.1257***	-3.5238***	-3.6209***	-5.3984***
	(0.1957)	(0.2086)	(0.2402)	(0.4861)	(0.4799)	(0.5614)
Obs.	28,315	28,315	28,315	9,995	9,995	9,995
Adj. R-sq.	0.01	0.01	0.02	0.00	0.02	0.02
Hypothesis (p-val.)						
$\beta_1 - \beta_2 = 0$	0.01	0.02	0.01	0.81	0.83	0.86
$(\beta_1 + \beta_4) - (\beta_2 + \beta_5) = 0$	0.35	0.37	0.37	0.41	0.57	0.61

Figure 3.1: The affiliation bias over time

This figure displays the affiliation bias for recommendations (Panel A) and price target (Panel B). The affiliation bias is obtained from quarterly regressions on the effect of brokers' affiliation (Affiliated and Rival) on their relative recommendation or price target. Affiliated firms have at least one business deal (IPO, SEO, public debt offering, M&A, syndicated loan) with the broker one year or less prior to the recommendation or price target issue. Rival represents the competitor firm of an affiliated with the highest pairwise TNIC score, and above the yearly average, during the current year. All specifications include the set of variables defined in Table 3.A.2 as well as firm fixed effects.

Panel A. Recommendations



Panel B. Price targets