Does the size of sell-side analyst industry matter? An examination of bias, accuracy and information content of analyst reports

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Abstract:
This paper examines determinants of changes in the size of the sell-side analyst industry and whether such changes impact the aggregate bias and accuracy of analyst reports as well as how analyst information is impounded into prices. We first document changes in the aggregate number of analysts and the factors related to these changes. We find that aggregate analyst following increases following periods of high returns, high IPO activity, and increased trading activity. We also find that regulations that impacted analyst-related profits in the early 2000s reduced the number of analysts at brokers whose profits were highly sensitive to market activity. Using several controls for endogeneity (including an exogenous shock based on brokerage house closures and mergers), we next show that increases in the aggregate number of analysts result in more accurate and less biased aggregate forecasts at the industry level. Indeed, we also find that increases in the number of analysts result in greater average market response to subsequent releases of analyst reports and lower market response to the actual earnings announcement. Overall, our results suggest that the aggregate presence of the sell-side analyst industry has a significant impact on the quality of analysts' reports and on the information content of prices.

JEL Classifications: G10, G24

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1. Introduction

Sell-side financial analysts as a whole play an important role in the U.S. capital markets. Analysts facilitate the distribution of financial information and their reports provide information that is valuable to market participants.[1] Further, analysts help shape capital markets through their interactions with underwriters, brokers, institutional investors, and management. Analyst activities are of particular interest to investors, regulators, and the financial press and significant regulatory changes have been enacted over the past decade to preserve the integrity of analysts' research as well as their interactions with other key financial market participants (e.g., Reg FD, Global Settlement, NASD Rule 2711, NYSE Rule 472).

Despite the broad interest in the impact of financial analysts on capital markets, our understanding of this group as an industry in its own right is rather limited. Most prior studies examine the behavior of analysts at the firm level (e.g., IBM), rather than taking a broader industry perspective. One stream of this research finds that analysts tend to follow firms that are larger, provide better public disclosure, have more intangible assets, have underpriced IPOs, generate higher trading volume and manage earnings less (Bhushan 1989; Lang and Lundholm 1996; Rajan and Servaes 1997; Barth, Kasznik and McNichols 2001; Yu 2008; Loh Stulz 2011; Jegadeesh, Kim, Krische and Lee 2004). These studies implicitly assume that changes in the number of analysts covering a firm occur because existing analysts make changes to their coverage portfolio, rather than considering the entry and exit of individual analysts.

Along a different dimension, other studies find that the number of analysts following a firm affects information asymmetry and earnings forecast bias at the firm level (Hong and Kacperczyk 2010; Derrien and Kecskes 2012; Kelly and Ljungqvist 2012; Fong, Hong, Kacperczyk and Kubik 2012). These studies argue that changes in the number of analysts can

[1] See Ramnath, Rock and Shane (2008) and Bradshaw (2011) for recent reviews of this literature.

have stock market consequences because these changes affect the flow of information to external stakeholders and influence the competition between analysts for access to management and investment banking activities (Lang and Lundholm 1996; Mehran and Stulz 2007). While firm-level incentives clearly matter, analysts are more heavily evaluated and compensated at aggregate levels (e.g., industry) and often compete for Institutional Investor rankings, prestige, and higher compensation based on their specific sector or industry (Groysberg, Healy, and Maber 2011). Further, some studies argue that analysts largely produce market and industry information, rather than firm specific content (e.g., Piotroski & Roulstone 2004; Kadan, Madureira, Wang and Zach 2011; Hui and Yeung 2013). Thus, by construction, firm-level analyst studies are not designed to shed direct light on how changes in the total number of analysts affect properties of the aggregate information environment. As such, we consider whether changes in the aggregate number of analysts affect the aggregate accuracy, bias, or informativeness of analyst reports.

We first document changes in the aggregate number of analysts and the factors that relate to these changes. We begin by examining aggregate changes at the market and industry level. The market level is a natural starting point because it allows us to examine aggregate changes in analysts more broadly without assuming any specific grouping scheme. However, this level of analysis has limitations. Recent studies emphasize that brokers tend to organize their efforts by specific industries, suggesting that brokers likely make resource allocation decisions at this level (Boni and Womack 2006; Kadan, Madureira, Wang and Zach 2011). In addition, the industry level analysis captures greater variation in changes in analysts, because it allows industries to exhibit different trends based on differences in analyst activity. For example, analysis at the market level would not identify much change in analyst activity if the number of analysts is expanding in some industries while contracting in others. In addition, market level analysis requires a time-series methodology that suffers from well-known econometric problems (e.g., serially correlated residuals, non-stationarity, etc.). Thus, we further conduct our analysis at the industry level.

Standard economic analysis suggests that the number of analysts employed depends on the marginal value they bring to the investment banks that employ them. As most analysts work for brokerage houses in which the research department generates no significant direct revenue, it is difficult to directly capture the costs and benefits of analyst research to their employers (e.g., Jegadeesh, Kim, Krische and Lee 2004). As such, our proxies for the net benefits of expansion or contraction of the analyst-industry are based on general economic conditions which are likely to affect analyst-related profits. Specifically, we use aggregate returns, IPO activity, and trading volume as measures of overall economic conditions. We expect the aggregate number of analysts to be greater when stock returns are higher because expected profits related to analyst activities should be higher. Similarly, we expect the aggregate number of analyst to be higher when there is more IPO activity and trading volume because of higher trading commissions and underwriting fees (Hayes 1998; Chen and Ritter 2000). We also consider changes in market volatility. However, the direction of this relation is unclear because volatility can lead to both lower market activity and potentially greater demand for analyst activities.[2]

Our results are consistent with these expectations. At the market level, we find that aggregate returns and IPO activity are positively and significantly correlated with changes in the number of analysts providing forecasts. We find similar results at the industry level, but also

[2] Volatility may proxy for uncertainty or information asymmetry. To the extent that it proxies for information asymmetry, prior studies at the firm-level provide contradictory results on how this affects analyst coverage. Lang and Lundholm (1996) argue that analysts prefer to follow firms with better disclosure (i.e., less information asymmetry). However, Barth, Kasznik and McNichols (2001) find that analysts follow firms with high levels of intangible assets (i.e., more information asymmetry).

find that aggregate analyst changes are positively related to trading volume and negatively related to volatility. These additional findings are consistent with the industry level analysis providing more powerful tests. Overall, our findings suggest that increases in the aggregate number of analysts are correlated with economic conditions that are expected to be related to higher analyst-related profits.

One limitation of the above analysis is that OLS regressions are subject to concerns regarding endogeneity. It can be particularly problematic to separately identify changes in IPO and analyst activity (i.e., reverse causality or correlated omitted variables). To circumvent some of these concerns, we further examine changes in the aggregate number of analysts by considering recent regulations that likely affected the expected profits of many analyst-related activities, including IPOs, as a natural experiment (e.g., Reg FD and Global Settlement). These regulations restricted the activities of analysts by limiting access to private information as well as brokers' ability to involve analysts in investment banking activities. In fact, following the Global Settlement Act of 2003, 12 of the largest brokers were fined nearly $1.4 billion and all brokers were banned from using their research divisions to support future IPO activity. Thus, we perform further analysis by aggregating at the broker level to take advantage of this change.

Using a difference-in-difference analysis, we examine the incremental differences in monthly changes in the aggregate number of analysts following these regulations for brokers whose profits were highly affected versus those less likely to be affected. We find that brokers directly sanctioned by the GS and brokers with high IPO activity reduced their workforce by one more analyst per month, on average, than other brokers in the three years following regulation. These results provide further evidence in support of our findings that changes in the number of aggregate analysts depend on factors related to the expected profits of analyst-related activities.

Having provided evidence on changes in aggregate analyst activity, we next turn to the more important task of investigating the impact that these changes have on the aggregate information environment. Analysts are one of the most influential sources of information in capital markets and, naturally, one would expect that changes in the aggregate number of analysts could have an important impact on analysts behavior and stock market information environment. On the one hand, increasing the number of analysts presumably increases the amount of information impounded in prices and increases competition among analysts thereby reducing the extent of bias (Hong and Kacperczyk 2010) and increasing accuracy. On the other hand, new analysts entering the fray may be less experienced and provide less accurate forecasts and recommendations.[3] Accordingly, we examine the effect of changes in aggregate analyst following on the aggregate characteristics of their earnings forecasts (i.e., average forecast accuracy and bias) as well as the overall informativeness of public disclosure and analyst reports. We perform this analysis using a standard regression framework as well as a measure of exogenous changes in aggregate analyst following based on brokerage house mergers and closures.

We find that increases (decreases) in the number of analysts providing forecasts are associated with lower (higher) aggregate forecast errors and less (more) aggregate optimistic bias. Specifically, we find that a drop in one analyst covering an industry, based on changes in analysts from brokerage house mergers and closers, results in a 2.1% increase in aggregate forecast error and a 2.0% increase in optimistic bias. These findings are consistent with the notion that having a larger sell-side analyst presence improves the quality of analyst reports. In addition, we find that increases (decreases) in the number of analysts are also associated with

[3] Hong, Kubik and Solomon (2000) provide empirical evidence that inexperienced analysts are less likely to provide innovative forecasts, issue less timely forecasts, and revise their forecasts more frequently.

increases (decreases) in aggregate analyst forecast informativeness and decreases (increases) in aggregate earnings announcement informativeness. These findings suggest that the market perceives the informativeness of analyst reports to increase when the number of analysts covering an industry increases and are consistent with firm-level evidence that analysts serve an "information discovery" role in the period leading up to the earnings announcement (Chen, Cheng and Lo 2010). Thus, our results suggest that changes in aggregate analyst presence have important consequences for the aggregate forecasting and information environments.

Overall, the aggregate level analyses we perform throughout this study allow us to add a new perspective to the sell-side equity analyst literature. Recent studies explore the effects of aggregate analyst outputs such as earnings forecasts and recommendations at both the market and industry levels. Howe, Unlu and Yan (2009) provide evidence that aggregate analyst recommendations can predict future aggregate returns and earnings. In a similar vein, Hann, Ogneva, and Sapriza (2013) find that, despite the persistent bias in aggregate analyst forecasts, markets continue to fixate on aggregate earnings forecasts and overweight their value in forming expectations about the economy. We complement these findings by providing evidence about the changes in the originators of these reports (i.e., the analysts themselves) and show that increases in aggregate analyst presence can improve the quality of the aggregate analyst forecast and potentially enhance the flow of information.

In addition, aggregate analyses allow us to examine the effects of broader economic forces, such as industry-wide competition, that, by design, cannot be fully explored in a firm-level analysis. We complement recent findings that suggest that analyst competition occurs at the firm-level (e.g., Hong and Kacperczyk 2010) and provide evidence to suggest that the effects are more pervasive and may extend across an industry. Since analysts are industry experts that often create value through their ability to rank firms within their industry as well as across other industries (Boni and Womack 2006; Kadan et al. 2011), firm-level evidence may be incomplete to the extent that it is unable to consider the implications that changes in one firm's coverage have for other firms that an analyst follows.

The remainder of the paper proceeds as follows. Section 2 introduces the sample and data. Section 3 provides results the market- and industry- level tests as well as the broker-level regulation tests. In Section 4, we explore the consequences associated with changes in aggregate analyst following at the industry level. Section 5 concludes.

2. Data and Sample

2.1 Sample Selection

Our sample tracks the aggregate number of analysts providing reports each month from 1990 to 2010 based on data available from I/B/E/S. We begin our sample with about 12.3 million US quarterly and annual EPS forecasts issued between 1989 and 2011 that have sufficient data for industry classification. In order to facilitate tracking analysts and brokers over time, we require each forecast to be associated with a unique analyst and broker code (i.e., we remove anonymous analysts).

We aggregate the number of analysts at three levels: market, industry, and broker. At the industry level, we aggregate analysts using the Global Industry Classification Standard (GICS) using the GICS historical database from S&P Compustat. We choose the GICS classification because it most closely resembles how analysts organize themselves in practice and is common among many brokerage houses (Kadan, Madureira, Wang and Zach 2011). Additionally, prior research suggests that the GICS classification outperforms other classifications (e.g., SIC, NAICS, and Fama-French 48) in terms of its ability to explain stock returns (Bhojraj, Lee and

Oler 2003) and is a reliable proxy for how sell-side analysts specialize by industry (Boni and Womack 2006).

The GICS taxonomy consists of 10 sectors, 24 industry groups, 68 industries, and 154 sub-industries.[4] We aggregate our variables across 24 GICS industry groups rather than at the broader sector level or the more detailed industry or sub-industry level because industry groups provide the most consistent level of classification across all analyst firms. Broker size varies significantly across our sample, with some brokers employing as few as one analyst and others employing over 100 analysts. While larger brokers have sufficient resources to employ analysts at a finer level of coverage detail, smaller brokers are more constrained in their coverage decisions and likely staff at a coarser level of coverage detail. Aggregation at the *GICS industry group level* seems most appropriate for several reasons. First, prior research documents a mean brokerage size of 34 analysts, implying that many brokers are unable to provide coverage beyond the 24 GICS Industry Group level of aggregation (Clement and Tse 2003). Additionally, Kadan et al. 2011 are unable to find any broker that provides coverage across all 68 GICS industries. Finally, using a coarser level of aggregation poses challenges in terms of the power of our statistical tests and our ability to capture enough variation across industries.[5] As such, we believe GICS industry groups provide the best choice in terms of understanding both the system by which analysts organize themselves (i.e., GICS) as well as the precision (i.e., level of aggregation) at which analysts aggregate.

2.2 Aggregate Analyst Presence

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[4] Prior to 2003, there were only 23 GICS Industry Groups. In April 2003, GICS introduced an additional Industry Group for Semiconductors & Semiconductor Equipment (i.e., 4530) and reclassified some of the firms previously included in GICS Industry Group 4520. We drop observations for these two industry groups between 2002Q4 and 2003Q2 to allow for reclassification of analysts and firms in our sample.

[5] In untabulated analyses, we examine the results using other levels of aggregation and obtain similar inferences across other GICS aggregation methods (i.e., sector and industry).

Our primary variable of interest in this study is monthly changes in the aggregate number of analysts. We define a new analyst as any analyst issuing her first forecast in the sample, or who has not issued a forecast in the past 12 months. Similarly, we define an exiting analyst as any analyst issuing her last forecast in the sample, or who does not issue a forecast in the next 12 months. Beginning in 1990, we count the number of new analysts and the number of exiting analysts each month. We measures changes in the aggregate number of analysts as the difference between the number of new analysts and the number of exiting analysts each month. This measure is constructed separately at the market, industry and broker level. Formally:

$$\Delta Analyst_t = NewAnalyst_t - ExitAnalyst_t$$

$$\Delta Analyst_{it} = NewAnalyst_{it} - ExitAnalyst_{it}$$

$$\Delta Analyst_{bt} = NewAnalyst_{bt} - ExitAnalyst_{bt}$$

where t indexes time, i indexes industry, and b indexes broker.[6]

2.3 Measuring Proxies for Analyst-Related Profits

Standard economic analysis suggests that the aggregate number of analysts depends on the marginal value they bring to the investment banks that employ them. As it is difficult to capture this value directly, we consider proxies that relate to analyst activities. Specifically, we examine stock returns, changes in trading volume, the number of IPOs, and changes in stock return volatility. We expect the aggregate number of analysts to be positively related to stock returns, trading volume, and IPOs because these measure relate to higher analyst-related profits in terms of trading commissions and underwriting fees (Hayes 1998; Chen and Ritter 2000). However, we have less clear expectations about the role of stock return volatility as it can relate to both lower market activity and greater demand for analyst reports.

[6] Our results are qualitatively similar if we scale ΔAnalyst by the number of analysts in the prior month (i.e., % change in analysts), however, similar to studies at the firm level we expect our independent variables to be more linearly related to the number of analysts.

To construct these variables, we obtain market data from CRSP and IPO data from Thomson ONE SDC Platinum. Each of these variables is then measured monthly and aggregated at both the market and industry level. While it is reasonable that brokers may examine horizons longer than one month when determining their hiring decisions, aggregating these variables over longer horizons may alter the underlying concept in many instances. For example, longer horizon returns may capture momentum in addition to being a proxy for an analyst-related profit source (e.g., Jegadeesh and Titman 1993). Similarly, longer horizon trading volume may represent contrarian signals that stocks are overvalued and may not proxy for analyst-related profits (Lee and Swaminathan 2000). Thus, we restrict our variables to a one month horizon in order to examine cleaner measures of analyst-related profits. However, in untabulated analysis, our results remain significant and in the predicted direction when we examine longer horizons, including both quarterly and annual measures.

We measure stock returns as the value-weighted returns from holding the market (industry) portfolio over the past month. Each month, we form value-weighted market (industry) portfolios using firm returns and the prior month's market cap obtained from the CRSP Monthly Return File.[7] Specifically, $Ret_t = \sum_{i=1}^{N}(Ret_{i,t} * \frac{MCAP_{i,t-1}}{\sum_{i=1}^{N} MCAP_{i,t-1}})$, where i denotes firms in the market (industry) group and t denotes month.[8]

We measure stock return volatility using value-weighted daily returns over the past 1 month from the CRSP Daily Return File. Each day, we form a value-weighted market (industry) portfolio based on the prior month's end market cap i.e., $Ret_d = \sum_{i=1}^{N}(Ret_{i,d} * \frac{MCAP_{i,t-1}}{\sum_{i=1}^{N} MCAP_{i,t-1}})$. At the end of each month, we compute volatility as the standard deviation of the daily market

[7] We incorporate delistings into monthly data following procedures outlined in Beaver, McNichols, and Price (2007).
[8] Our results are similar if we use equal-weighted returns.

(industry) portfolio returns over the prior month. Changes in volatility are measured as monthly changes in the level of return volatility. This measure is consistent with the firm-level measure used by Lang and Lundholm (1996), who examine the relationship between the level of analysts and the standard deviation of firm ROE.

Consistent with studies that examine changes in analysts and changes in trading volume at the firm level (Barth et al. 2001), we compute trading volume by summing the trading volume (in number of shares) of all firms in the market (industry) over the past month. Changes in trading volume are measured as monthly changes in the level of trading volume.

Finally, we measure the number of IPOs as the number of completed offerings in the market (industry) group over the past month. We exclude IPOs with an offer price below $5 per share, ADRs, and IPOs not listed on CRSP within 30 days of the issuance date.[9]

2.4 Sample Characteristics

[Figure 1]

We begin our investigation by examining how the analyst industry has evolved over the past two decades.[10] In Figure 1, we plot the number of analysts employed in the market each month, beginning in 1990, for our full sample. It is important to note that this figure only includes the lead analyst on the research team and does not include associates or junior analysts (Jegadeesh, Kim, Krische and Lee 2004). We also include a constant sample of 36 brokers, who exist throughout the entire sample period, to consider whether the trends we observe relate simply to changes in the brokerage houses reporting to I/B/E/S over the years. Throughout the

[9] These filters are common in the IPO literature. For example, most companies prefer an offer price between $10 and $20 per share, and offer prices below $5 per share are subject to provisions in the Securities Enforcement Remedies and Penny Stock Reform Act of 1990 (Ritter 1998). More importantly, these IPOs represent offerings that are most likely to generate significant profits for the brokers and attract high levels of interest from analysts.

[10] The total number of analysts providing forecasts in a month is the number of analysts from the prior period plus the change in analysts (i.e., $\Delta\,Analyst$).We use the number of unique analysts providing forecasts in 1989 as a base.

1990s, aggregate analyst presence rises steadily from a starting population of around 2,000 to a peak of 3,226 analysts by the end of 1999. In particular, this trend coincides with the rise of tech IPOs in the late 1990s and the deregulation of brokers following the Gramm-Leach-Bliley Act of 1999. Thus, one potential explanation for this trend is that increasing profitability from IPO activity allowed the analyst industry to expand during this time period. In the 2000s, the number of analysts remains relatively steadier, ranging between 2,700 and 3,200 analysts.

The figure also depicts declines in the total number of analysts around market downturns, particularly around the recession of the early 1990s, the dot-com bubble in 2001, and the recent Global Financial Crisis in 2007. The comovement of aggregate analyst presence with market recessions is consistent with the profitability of analyst services declining when the market is performing poorly. More recently, aggregate analyst presence has begun to rise again. This trend is perhaps consistent with recent projected growth in this industry. The United States Department of Labor expects employment of financial analysts to grow 23 percent in the next 10 years due to an increasing demand for understanding complex financial products.[11]

[Table 1]

Our sample descriptive statistics also enhance our understanding of the analyst industry. Panels A, B and C of Table 1 provide descriptive statistics for the market sample, industry sample and broker sample, respectively. The mean number of analysts employed in the market (industry) across our sample period is 2,848 (300), similar to numbers cited in Jegadeesh et al. (2004). On average, the number of analysts in the market and across industries is increasing. The market adds about 4-5 new analysts per month, whereas industries add about 1 new analyst

[11] http://www.bls.gov/ooh/business-and-financial/financial-analysts.htm#tab-6

every 5 months (.22 analysts per month * 5 months).[12] Differences in the magnitude of other variables between the market sample and industry sample are driven by differences in the aggregation methods we use. For example, value-weighted returns for the industry sample represent the average of the value-weighted returns from each of the 24 industry groups.

At the broker level, the number of analysts employed is also increasing, but the changes are less pronounced, at a rate of about 1 new analyst every two years (.04 analysts per month * 24 months). At this level, it is important to note that we are examining an unrestricted sample of 913 different brokerage houses providing forecasts to I/B/E/S over the course of the sample period. The small level and change in analysts we document at the broker level is due, in part, to a significant number of small independent brokerage houses with only 1 publishing analyst. While it is difficult to directly quantify the dollar value associated with the costs of adding new analysts, we can provide conservative estimate based on recent data from the Bureau of Labor Statistics. Based on a median salary of $74,350 per year and at least 3 analysts per team,[13] the market spends at least $13 million per year on hiring new analysts ($74,350 per analyst * 3 analysts per team * 5 analysts per month * 12 months per year). The actual costs are likely much larger than this, however, since lead analysts earn substantially more than other team members, and BLS data does not take into account the large bonuses that equity analysts enjoy.

3. Factors Related to Aggregate Analyst Presence

3.1 Market-wide and Industry Analyst Following

What determines the number of analysts employed in the market? Our first analysis examines how our market-wide proxies for analyst-related profit sources influence changes in

[12] While uncommon in general, our measure of aggregate industry analysts allows for some analysts to overlap across industries. For example, some analysts provide forecasts for firms in different industries within a month and will be counted multiple times in separate industries.
[13] Anecdotal evidence and the authors' conversations with equity analysts suggest that many analyst teams consist of 3 to 4 individuals.

aggregate analyst following. Our tests at the market level make no assumptions about the way in which analysts organize themselves (i.e., industries or brokers) and simply examine relationships between market trends and changes in the number of analysts. We examine how sources of analyst-related profit in period t affect changes in the number of analysts in period $t+1$ by employing the following regression model:[14]

$$\Delta \mathrm{Analyst}_{t+1} = \alpha_0 + \alpha_1 \mathrm{Returns}_t + \alpha_2 \mathrm{IPOs}_t + \alpha_3 \Delta \mathrm{Trading\ Volume}_t + \alpha_4 \Delta \mathrm{Ret\ Vol}_t + \varepsilon_t \qquad (1).$$

[Table 2]

Table 2, Panel A presents the results from tests of Equation (1). t-statistics are based on Newey-West adjusted standard errors.[15] In Columns 1-4, we first examine each proxy separately. Returns and IPOs are positively and significantly associated with changes in analysts ($\Delta Analyst$) at the market-wide level (p<.01), consistent with analysts entering the market following periods of high returns and high levels of IPOs. Changes in trading volume ($\Delta Trading\ Volume$) also relate positively to aggregate changes in analysts and changes in return volatility ($\Delta Ret\ Vol$) are negatively related to changes in analysts, but neither result is significant at traditional levels. When we examine all four variables together in Column 5, both Returns and IPOs remain significant (p<.01 and p<.05, respectively). Economically, market performance appears to have the greatest impact on changes in analysts. The results suggest that a one standard deviation increase in market returns in the current month is associated with about 5 more analysts in the market in the following month (i.e., 108.768*.0458). The multivariate

[14] By lagging the analyst-related profit proxies by one month, we reduce the potential for simultaneity and omitted variable bias.

[15] Following procedures specified in Greene (2003), standard errors include a Newey-West correction for four lags. Further, we test each of the explanatory variables for non-stationarity. Both augmented Dickey-Fuller and Phillips-Perron unit-root tests strongly reject the null that any of the variables in the above model contains a unit root (p<.01), thus providing strong evidence that the variables are stationary.

specification has modest explanatory power, especially given that it uses a changes-based specification (R-squared = .09).[16]

While the market-level results provide some insight into what economic forces influence aggregate analyst following, it is likely that industry-level analyses could produce more powerful tests. Prior literature indicates that analysts are generally organized along industry lines (Boni and Womack 2006; Kadan et al. 2011) and market-level tests cannot detect offsetting movements between industries and do not account for heterogeneity across different industry groups or brokers. If aggregate analyst following changes in response to changes within particular industries, our market level tests may fail to adequately explain changes in analysts. For example, consider a scenario in which the Software & Services industry has a high number of IPOs in a particular period and the Real Estate industry has a low number of IPOs. These industries may respond by increasing and decreasing the number of analysts, respectively. However, the aggregate effect could be small in the overall market , even though the period is associated with a high number of IPOs.

For our industry tests, we re-examine Equation (1), but use variables that are aggregated across 24 GICS Industry Groups. Specifically:

$$\Delta \mathrm{Analyst}_{\mathrm{it+1}} = \alpha_{\mathrm{i}} + \alpha_1 \mathrm{Returns}_{\mathrm{it}} + \alpha_2 \mathrm{IPOs}_{\mathrm{it}} + \alpha_3 \Delta \mathrm{Trading\ Volume}_{\mathrm{it}} + \alpha_4 \Delta \mathrm{Ret\ Vol}_{\mathrm{it}} + \varepsilon_{\mathrm{it}} \quad (2)$$

where i indexes industry and t indexes time. We include industry fixed effects in order to control for unobserved constant industry factors that relate to changes in analysts ($\Delta Analyst$) at the industry level, but are not explicitly controlled for in the model. In all industry tests, standard errors are clustered by both industry and month.

Table 2, Panel B provides the results from these tests. In Columns 1-4, we present the results from the univariate tests of each variable and, in Column 5, we present the results from

[16] Our results are similar when we examine quarterly and annual horizons for our independent variables.

the full multivariate model. Consistent with the market-level tests, returns and IPOs are both positive and significantly correlated with changes in analysts ($p<.05$ and $p<.01$, respectively).[17] However, changes in trading volume ($\Delta Trading\ Volume$), while not a significant factor in the market-level tests, are positively and significantly related to changes in analysts at the industry-level ($p<.01$). Similarly, changes in return volatility ($\Delta RetVol$) are negatively and significantly related to changes in analysts at this level. In addition, all four of these variables remain significant when tested jointly in the multivariate model, consistent with the industry-level analysis having more statistical power.[18] Recall that in the market regressions, only returns and IPOs remained significant in the multivariate setting. Thus, it appears as if aggregation has an important effect on explaining changes in aggregate analyst presence.

In terms of statistical and economic significance, $IPOs$ appears to be the most important component in determining changes in the aggregate number of analysts at the industry level. A one standard deviation increase in the number of IPOs in an industry results in almost one new analyst per industry-month (i.e., 2 * .4103 = .8206). In contrast, IPOs had a much smaller effect at the market level. A one standard deviation increase in the number of IPOs in the market resulted in less than 4 new analysts per industry-month (i.e., 22 * .1719 = 3.7818). Across 24 industry groups, the implied economic effect at the industry level is more than 5 times greater than the market effect. More generally, both analyses suggest that our proxies of analyst-related

[17] We also scale IPOs by the number of listed companies in an industry and find similar results.

[18] One explanation for finding stronger results in the multivariate specification at the industry level as compared to the market level is that we provide a model that allows for heterogeneity of analyst following across different industry groups. Another explanation for finding stronger results is that our tests are simply more powerful at the industry level compared to the market level, as we increase the number of observations from 252 to 5,877.

profit are correlated with changes in aggregate analyst following, but also, that assumptions in aggregation methods drive important differences in the results.[19]

3.2 Broker-Level Regulation Tests

The market and industry analyses are subject to concerns regarding endogeneity and correlated omitted variables. This problem is most severe for IPOs since analyst-activity (e.g., constructing roadshow materials) can potentially influence the number of IPOs in the market. To directly address some of these endogeneity issues, we further examine changes in the aggregate number of analysts by considering recent regulations that affected analyst-related profits as a natural experiment.

Specifically, we are interested in a series of analyst-related regulations that emerged in the early 2000s. These regulations restricted the activities of analysts by limiting access to private information as well as brokers' ability to involve analysts in investment banking activities. In particular, Global Settlement (GS) and related sell-side research regulations were initiated to curb the biased research produced by brokerage houses (Kadan et al. 2009). These sanctions were primarily directed toward 10 (and later, 12) of the largest, most prestigious investment banks for producing research that was inappropriately influenced by the investment banking divisions of the banks.[20] The sanctioned banks were required to pay nearly $1.4 billion in settlement fines and relief funding for independent research. The regulation also prohibited the cross-subsidization of research activities from underwriting activities across all affiliated

[19] In untabulated analysis, we find that many of the results are qualitatively similar when we examine other measures of analyst presence across industries including changes in the number of firms receiving coverage and the changes in the concentration of analysts covering existing firms.

[20] The ten original investment banks include Bear Stearns; Credit Suisse First Boston; Goldman Sachs; Lehman Brothers; J.P. Morgan; Merrill Lynch, Pierce, Fenner & Smith; Morgan Stanley; Citigroup Global Markets; UBS Warburg; and U.S. Bancorp Piper Jaffray. In August 2004, Deutsche Bank and Thomas Weisel were added to the settlement.

brokerage houses which strongly affected the ties between IPO activities and analyst compensation.

Prior to the regulation, brokerage houses frequently used research analysts to assist with the book-building, roadshow and promotion of IPOs. Among other provisions, GS created a "Chinese Wall" between the research divisions and the investment banking divisions of brokerage houses, effectively prohibiting analysts from aiding or influencing underwriting in any form. Thus, these regulations likely had a significant impact on analyst contributions to the profits of certain brokerage houses which in turn likely affected the number of analysts they employed.

To test this prediction, we use a difference-in-difference (DD) design to examine differences in changes in the number of analysts employed at the broker level ($\Delta Analyst_{bt}$) before and after the regulation using three year windows around September 2001. While GS was officially enacted in April 2003, the effects of the regulation were likely anticipated much earlier. For example, during the summer of 2001, Congress held the "Analyzing the Analysts" hearings, which led self-regulatory organizations, NASD and NYSE to enact new rules (NYSE Rule 472 and NASD Rule 2711) that affected almost every brokerage house in the U.S. (Kadan, Madureira, Wang and Zach. 2009). We also searched news wires over the surrounding period and found several reports indicating that many major brokerage houses were reducing budgets and staffing prior to GS. For example, in 2002, Brad Hintz, an analyst at Sanford C. Bernstein & Co., warned that reductions in research budgets and staffing levels could exceed 20%.[21] Further, in 2003 the WSJ notes that, as a result of strict reforms that reduced salaries to top analysts, many high-profile analysts, including senior research managers at Merrill Lynch, Morgan

[21] "Wall Street Braces for Deepest Job Cuts Yet --- Credit Suisse First Boston Is Set To Reduce Work Force About 7%; J.P. Morgan Also Weighs a Move," *The Wall Street Journal*, October 8, 2002.

Stanley, and Citigroup, had left the industry.[22] Accordingly, we consider the regulation period to begin in Summer 2001 in order allow for brokers to respond to anticipation of the regulation at different points in time.[23]

We estimate the following model:

$$\Delta Analyst_{b,t} = \alpha_0 + \alpha_1(Post_t * Treated_b) + \alpha_2 Post_t + \alpha_3 Treated_b + \alpha_4 Merge_{bt} + \beta' MktVars_t + \gamma'(Post_t * MktVar_t) + \epsilon_{bt} \quad (3).$$

Where b indexes broker and t indexes time. $Post$ is an indicator variable that takes the value of 0 for the 36 months prior to September 2001 and a value of 1 for the 36 months after September 2001. We require that all brokers in the analysis employ at least 10 analysts over the period and exist on I/B/E/S for at least 1 full year prior to and after the regulation date; however, our results are robust to this decision. $Treated$ is an indicator variable that takes the value of 1 for if the broker is in the treatment group or 0, otherwise. We explain more details about the alternative treatment groups below. $Merge$ is an indicator variable that takes the value of 1 if the broker merges with another broker in the month, 0 otherwise. We control for months in which a broker acquires another broker, because these mergers are likely to directly increase the number of analysts. $MktVars$ is a vector of control variables that include the determinants variables from Section 2.2 above (market returns, IPOs, trading volume, and stock return volatility). We include interactions of the control variables and the $Post$ dummy to allow for changes in broker sensitivity to our proxies following the regulation. The coefficient on $Post * Treated$ (i.e., α_1) indicates the incremental change in monthly analyst changes between the pre and post periods for treated versus untreated brokers.

[22] "Latest Call on Wall Street: Get a Real Job --- Some Analysts Leave Industry In Search of `New Adventures' As Down Market Takes Its Toll," *The Wall Street Journal*, February 28, 2003.
[23] However, as we discuss below, our results are robust to various window lengths and event dates.

We expect that sanctioned brokers and brokers who underwrite high numbers of IPOs will be most affected by the regulation. Following regulation, these brokers can no longer fund the research division of the bank using underwriting fees. Furthermore, following the regulation, sanctioned brokers will be forced to pay relief funding to independent research firms for 5 years, further reducing the available funds to support the research divisions of their institutions. Based on this reasoning, we use three alternative sets of treatment and control groups based on brokerage house characteristics. The first potential treatment group consists of the 12 sanctioned brokers, where all other firms are considered part of the control group.[24] The second treatment group is comprised of firms who have above the median level of IPO issues between 1999 and 2004. And the third treatment group is comprised of firms who have any IPO activity between 1999 and 2004.[25] In each case, we expect the treatment brokers whose profits are highly sensitive to GS-related sanctions to reduce analysts more than their peers (i.e., non-treatment brokers) following the regulation (i.e., $\alpha_1 < 0$).

Table 3 provides the results from the DD analysis. In Column 1, we examine the results when the treatment group is Sanctioned Brokers. In Column 2, we provide the results when the treatment group is High IPO Issuers (i.e., above the median). Column 3 provides the results when the treatment group is any IPO Issuer. The coefficients on each of the alternative treated types (Sanctioned, High IPO Issuer, and IPO Issuer) indicate that each of the treated groups increases the number of analysts, on average, more than the non-treated groups. More importantly, the coefficient on the interaction terms for each treatment type is negative and significant. Following regulation, sanctioned brokers lose, on average, 1 more analyst per month

[24] Recently, several brokers stopped reporting to I/B/E/S (e.g., Merrill Lynch and Lehman Brothers). For this portion of the analysis, we use a 2006 vintage of the I/B/E/S data. This data should be free of the biases identified in Ljungqvist, Malloy and Marston (2009) since we are not evaluating the recommendation quality.

[25] IPO data is collected from Bloomberg and manually matched to brokers using the most recent BRAN (Broker Translation file) available. I/B/E/S no longer produces this table, so we use a version from 2007.

than the non-sanctioned brokers. We obtain similar results for the two IPO treatment groups. High IPO Issuers lose .789 analysts more per month than low IPO Issuers; and IPO issuers lose .723 analysts more per month than non IPO issuers.[26] These results are highly significant at the 1% level. The results for the IPO sample go beyond just explaining the differential effects of regulation on different types of brokers. They also help to buttress our determinants analysis from Section 3, by highlighting the importance of IPOs as a proxy for analyst-related profits.

The results are robust to various window lengths and to different event dates. For example, we obtain similar inferences when we adjust our event period to begin in the first quarter of 2002 as opposed to 2001 or when we reduce the window length from 36 months to 24 or 16 months. Further, including controls for $Post * MktVars$, allows for sanctioned brokers to have different sensitivities to market proxies following the regulation, and reduces the possibility that the results are driven by changes in market conditions following the 2001 crisis as opposed to regulation.

However, to further examine the notion that our results are driven by regulation changes that affect analyst-related profits and not simply adverse market conditions, we perform a placebo test around the Recession of 1991. This recessionary period differs from the one in 2001 as it was not associated with any regulatory changes in the analyst industry. We find no evidence to suggest that the treated brokers identified in our sample respond differently to non-treated brokers around this alternative recessionary period. Our results also continue to hold if we exclude all tech analysts, providing evidence that this phenomenon is not driven by the tech bubble. Taken together, our results suggest that brokers reduced analyst presence in the early

[26] In untabulated analyses, we also find that treated brokerage houses lower coverage by 5-8 more firms per month than non-treated brokers.

2000s in response to regulation aimed at reducing profits related to analyst services, and not due to poorer overall economic conditions.

4. Consequences of Changes in Industry Analyst Following

4.1 Background & Motivation

Thus far, our evidence suggests that past market conditions, including returns, IPOs, turnover, and volatility, influence aggregate analyst presence. However, the question remains as to whether increased analyst presence in the aggregate impacts the quality of the aggregate information environment (e.g., aggregate forecast accuracy and bias)? Prior studies suggest that changes in the number of analysts affect information asymmetry, stock returns, and analyst forecast bias at the firm level, because these changes affect a firm's flow of public information as well as the level of competition among its analysts (Hong and Kacperczyk 2010; Derrien and Kecskes 2012; Kelly and Ljungqvist 2012). However, analysts also have important incentives at more aggregate levels and produce a significant amount of market and industry information (Piotroski & Roulstone 2004; Kadan, Madureira, Wang and Zach 2011; Hui and Yeung 2013). In fact, many significant analyst career outcomes are evaluated at the aggregate level including Institutional Investor (II) rankings, industry or sector prestige, and overall compensation. As firm level studies do not consider these more aggregate incentives, they are unable to address whether analyst presence affects the aggregate information environment.

We address this question by examining how changes in the number of analysts in an industry ($\Delta Analyst_{it}$) affect two key features of the aggregate information environment: 1) aggregate earnings forecast properties (accuracy and bias) and 2) the informativeness of public disclosure (analyst reports and earnings announcements). We focus exclusively on the industry level for these analyses as it provides a reasonable setting by which analysts aggregate themselves and poses less econometric issues than the overall market setting.[27]

Aggregate earnings forecast properties are a natural starting point for examining the economic consequences of changes in the number of sell-side equity analysts in an industry (i.e., aggregate analyst presence). Analysts' earnings forecasts are one of the most visible features of their reports. High quality forecasts are important to institutions, can lead to prestigious awards (e.g., II Ranking/WSJ Survey) and have been shown to increase analysts' chances of promotion (Hong and Kubik 2003). Indeed, many brokerage houses now subscribe to services such as Starmine which allow them to track their teams' performance via quantitative analysis of analysts' forecasts. Hence, earnings forecasts are an important performance metric for use in evaluating the quality of analyst reports and in turn the aggregate information environment.

A priori, it is not clear how changes in aggregate analyst presence might affect the quality of earnings forecasts in terms of their aggregate accuracy and bias. On the one hand, analysts often compete with each other for prestigious rankings and increased analyst following may lead to less biased forecasts at the industry level (similar to the firm-level results of Hong and Kacperczyk 2010) and potentially more accurate forecasts. On the other hand, new analysts are often less experienced and provide less accurate forecasts and recommendations than their seasoned peers (Hong, Kubik and Solomon 2000). They also tend to issue overly optimistic earnings forecasts in hope of career promotion (Hong and Kubik 2003). Thus, increases in aggregate analyst following may result in an information environment with less accurate and more optimistically biased forecasts in the aggregate.

[27] As mentioned above, the market level analysis requires a time-series methodology that suffers from well-known econometric problems (e.g., serially correlated residuals, non-stationarity, etc.).

We can also evaluate the benefits of increased analyst presence by examining the aggregate informativeness of their reports to market participants (i.e., the amount of information reflected in stock prices). Analysts forecast earnings components that are common to other firms and often adopt a top-down industry perspective. As such, they frequently provide information about industry fundamentals in their reports and in their earnings forecasts (Hui and Yeung 2013). For example, Meredith Whitney reduced her forecasts for many of the banking stocks she followed in October 2007 after predicting that problems in banks' bond exposure would hurt their bottom lines. Similarly, the steel stock industry rally came to a halt in January 2013 after a Goldman Sachs analyst predicted shortages of copper in China. If the number of analysts in an industry increases the overall production of this type of information, then we would expect that higher analyst presence increases the chance that this news is impounded in market prices. However, if the new analysts do not generate new industry information (i.e., issue fewer reports or repeat the information provided by other analysts) then it is possible that having more analysts does not affect the aggregate informativeness of analyst reports.

Relatedly, the quality of analyst reports can also influence the informativeness of earnings announcements because information in analyst reports can preempt information in actual earnings. In particular, more informative analyst reports can lead to less informative earnings announcements if analysts fill an information discovery role in the period leading up to the earnings announcement (Chen, Cheng and Lo 2010). At the industry level, increased analyst presence can affect the informativeness of the earnings announcements of all firms in the industry, even if all firms do not receive added coverage, because of spillover effects or intra-industry information transfers (e.g., Hilary and Shen 2013). For example, consider a tech analyst covering Microsoft. If an additional analyst enters the industry and covers Apple, but not

Microsoft, the information in the analyst's reports for Apple may help Microsoft's investors to understand the industry better and preempt information in Microsoft's earnings announcement. Thus, we expect that higher analyst presence at the industry level increases the chances that earnings announcement news is already incorporated in the stock prices of the industry's firms, decreasing the aggregate informativeness of earnings announcements.

4.2 Data and Methodology

We employ two different methodologies in our empirical analysis. Each varies in terms of the type of econometric issues it helps to address as well as its accompanying limitations. First, we conduct our analysis using OLS regressions with industry fixed effects. This approach allows us to specifically control for unobserved heterogeneity that is constant across industry groups over time. With this approach, we can examine the full sample of changes in aggregate analysts ($\Delta Analyst$) and not restrict ourselves to any specific events. However, OLS coefficients will be biased if there are time-varying correlated omitted variables. For example, analysts might enter or exit industries based on variation in the industry's performance which can also relate to analysts' ability to make accurate and unbiased forecasts. While it is possible to include some of these factors as control variables, we cannot be certain that we have captured all potential confounding variables.

Our second approach employs a measure of changes in analyst following in an industry based on brokerage house closures and mergers, a setting examined in recent finance studies (e.g., Kelly and Ljungqvist 2007; Hong and Kacperczyk 2010; Derrien and Kecskes 2012). This approach allows for better identification of the effect of changes in the aggregate number of analysts because the drops in analyst coverage are presumably exogenous to the aggregate information environment outcomes and other explanatory variables. However, the brokerage house closure and merger setting necessarily examines only drops in analyst coverage, which limits our ability to generalize our results to increases in coverage. In addition, while this approach mitigates endogeneity concerns better then OLS, it is possible that there are additional correlated omitted variables that have not been dealt with. Thus, both approaches, taken together, serve as two distinct, but somewhat complementary, methods for addressing endogeneity concerns.

To construct our measures of aggregate forecast accuracy and aggregate bias, we first collect monthly consensus (mean) annual EPS forecasts for all firms in our sample. Each month, we compute signed forecast errors for each firm as the difference between actual EPS minus the monthly consensus EPS forecast, scaled by the absolute value of the consensus EPS forecast. As such, more negative forecast errors indicate higher optimistic bias. Similarly, we compute unsigned forecast errors for each firm by computing the absolute value of the difference between actual EPS less the monthly consensus EPS forecast, scaled by the absolute value of the consensus EPS forecast. We average the unsigned (signed) forecast errors across firms for each industry-month to create our measures of aggregate forecast errors. We do not scale by stock price as to avoid issues identified with market price scaling (Mian and Teo 2004). Following Hribar and McInnis (2012), we exclude firms with absolute consensus forecast errors less than .10 per share from our analysis. Formally:

$$|AFE|_{i,t} = \frac{1}{n}\sum_{j=1}^{n}\frac{|EPS_{j,t} - Cons.EPS_{j,t}|}{|Consensus\ EPS_{j,t}|}$$

$$AFE_{i,t} = \frac{1}{n}\sum_{j=1}^{n}\frac{EPS_{j,t} - Cons.EPS_{j,t}}{|Consensus\ EPS_{j,t}|}$$

where t indexes time, j indexes firm, and i indexes industry. Intuitively, $|AFE|$ is a proxy for accuracy and AFE is a proxy for forecast bias.

We also create two measures to proxy for the informativeness of public disclosure based on the amount of information incorporated into stock prices on disclosure release dates. The first measure of public disclosure informativeness is a proxy for the aggregate informativeness of analyst forecasts. Similar to Frankel, Kothari, and Weber (2007) and Lehavy, Li and Merkley (2011), we first measure the informativeness of the analyst reports at the firm level as the proportion of stock return information related to analyst forecast revision dates relative to the total information impounded in stock returns in a month. If analyst forecast revisions provide useful information to investors, there should be a greater proportion of information related to the days in which analyst reports are issued.[28]

In order to compute a monthly measure of aggregate Analyst Informativeness (AnalystINFO) across an industry, we use the following procedure. For each firm, we compute analyst informativeness (AI) by summing the absolute size-adjusted returns for all forecast revision dates in a given month and then divide this amount by the sum of all absolute size-adjusted returns for all trading days in a month, excluding days within a 3-day window around the earnings announcement.[29] Then, for each industry-month, we average all firm analyst informativeness ratios. Formally:

$$AnalystINFO_{i,t} = \frac{1}{N}\sum_{j=1}^{N} AI_{j,t}$$

[28] For example, if analyst revision dates occur for a firm on 5 of the 20 trading days in a month, analyst reports are informative if they explain more than 25% (5/20) of the sum of the absolute returns. On the other hand, if analyst reports simply repeat information that is already publicly available, their revision dates should explain no more than 25% of the returns.

[29] Frankel et al. (2006) scale the analyst informativeness measure by the number of forecast revision dates. Similar to Lehavy, Li and Merkley (2011), we choose to not scale this measure since our variable of interest is the overall firm information that comes from analysts, and not the average per forecast impact. Scaling by the number of analysts also induces a negative correlation between $AnalystINFO$ and $\Delta Analyst$ that biases against our finding a result. In order to examine specifications that are more similar to Frankel et al. (2006) at an aggregate level, we have reexamined our analyses by both directly controlling for the number of analysts in an industry as well as by scaling the dependent variable, $AnalystInfo$, by the number of analysts in an industry. Our inferences remain unchanged in terms of economic sign and significance when we implement either of these approaches.

$$AI_{j,t} = \frac{\sum_{d=1}^{NREVS} |Ret_{j,d} - DecRet_{j,d}|}{\sum_{d=1}^{20} |Ret_{j,d} - DecRet_{j,d}|}$$

where d denotes trading days in a month, $NREVS$ denotes the number of unique days for which there is at least one analyst forecast, j denotes firm, i denotes industry, and t denotes month. Ret and $DecRet$ are as defined previously (i.e., Returns & Size-Adjusted Returns). Intuitively, this ratio proxies for the amount of information content released on dates in which analysts issue forecasts.[30] If analysts provide new and useful information (rather than repeating information in existing forecasts), increasing the number of analysts in an industry increases the probability that this information is released to the public. One concern with this measure is that the news in the analyst reports may have been released through other information channels (e.g., merger announcements) that correspond to analyst report dates (i.e., measurement error in the dependent variable). However, our measures of changes in analyst presence in the industry are unlikely to be correlated with these firm specific events.

The second measure is an aggregate measure of firm information content around earnings announcement dates. We examine a measure that is similar in spirit to the one employed by Francis, Schipper and Vincent (2002) and Peress (2010), which proxies for the "usefulness of earnings announcements." Earnings announcements are significant public events for firms that increase the supply of public information. If this information is new, there should be heightened levels of absolute abnormal returns around the release of this information.

To construct a measure of Earnings Announcement Informativeness, we first collect all quarterly Earnings Announcement Dates from Compustat. In order to minimize coding errors, we retain earnings announcements from I/B/E/S that fall within 5 days of the firm's quarterly

[30] One might also consider calculating this measure based on industry returns rather than averaging firm observations. However, this approach is less useful because analyst reports for individual firms in an industry correspond to most of the days in a month for most industries, providing very little variation across time.

report date (obtained from Compustat). We then calculate size-adjusted absolute cumulative abnormal returns (ACAR) for each firm for the 3-day window around the earnings announcement. For each industry-month, we then average all of the ACARs for firms within the industry. Formally:

$$EAINFO_{i,t} = \frac{1}{N}\sum_{j=1}^{N} ACAR_{j,t}$$

$$ACAR_{j,t} = \sum_{d=-1}^{1} |Ret_{j,d} - DecRet_{j,d}|$$

where d denotes days around a firm's earnings announcement date, j denotes firm, i denotes industry, and t denotes month. Ret is the daily return obtained from the CRSP Daily Stock File and $DecRet$ is the size decile adjusted return obtained from the CRSP Portfolio file (rebalanced annually).

Table 4 presents samples statistics for the four information environment measures. The mean (median) industry error (i.e., |AFE|) over the entire sample period is .470 (.388), implying that aggregate analyst forecasts deviate substantially from announced earnings. Consistent with prior literature (e.g., Hribar and McInnis 2012), aggregate analyst forecasts also tend to be optimistically biased with a mean AFE of -.285. We also find that analyst reports and earnings announcements are associated significant levels of information content, consistent with prior studies at the firm level. The mean (median) informativeness of aggregate analyst forecasts is .148 (.143), suggesting that, on average, 14.8% of all information impounded in stock prices in a month is related to analyst reports. The mean (median) aggregate informativeness of earnings announcements is .102 (.096), which represents a relatively large absolute return to earnings announcements, on average.

4.3 Results from OLS-Fixed Effects Models

As the first step of our investigation, we examine the earnings forecast properties effects (i.e., bias and accuracy) using the following OLS model with industry and month fixed effects:

$$\mathrm{FChar}_{it} = \alpha_i + \alpha_1 \Delta\mathrm{Analyst}_{it-1} + \alpha_2 \mathrm{Returns}_{it} + \alpha_3 \mathrm{IPOs}_{it} + \alpha_4 \Delta\mathrm{Trading\ Volume}_{it} + \alpha_5 \Delta\mathrm{Ret\ Vol}_{it} + \gamma' \mathrm{Month}_t + \epsilon_{it} \quad (4),$$

where i indexes industry and t indexes time. The forecast characteristic ($Fchar$) is one of two measures: Accuracy ($|AFE|$) or bias (AFE). We examine the change in the number of analysts ($\Delta Analyst$) as both a binary and a continuous variable. In the binary form, $\Delta Analyst$ is an indicator variable that takes the value of 1 when $\Delta Analyst$ is greater than or equal to 0 in the period, or 0 otherwise. We include a vector of calendar month fixed effects (i.e., January, February, etc.) to account for seasonal differences in earnings. We lag $\Delta Analyst$ by one month to correct for potential simultaneity issues between aggregate forecast characteristics and changes in analyst following (i.e., reverse causality). All other variables (i.e., Returns, IPOs, Trading Volume and Return Volatility) are defined as in prior tests and standard errors are clustered by industry and month.

If there are benefits associated with increasing aggregate analyst presence, we expect to see higher levels of accuracy and lower optimistic bias associated with changes in aggregate analyst following. Conversely, if adding analysts is costly, we expect reduced accuracy and increased bias following changes in aggregate analyst following. When the forecast characteristic is Accuracy, $\alpha_1 < 0$ ($\alpha_1 > 0$) implies that increases in the aggregate number of analysts providing forecasts in an industry in the current month increase (reduce) the aggregate accuracy in the next month. When the forecast characteristic is Bias, $\alpha_1 > 0$ ($\alpha_1 < 0$)implies that increases in aggregate industry analysts reduce (increase) the aggregate optimistic bias in the next month.

[Table 5]

Table 5 provides the results from these tests. In columns 1 and 2, we examine the average effect of net increases in analysts on the aggregate analyst forecast accuracy in the following month. In the binary model, the coefficient on $\Delta Analyst$ suggests that when industries add analysts, scaled absolute forecast errors decline by 4.46% (p<.01), on average. The results for the continuous variable for $\Delta Analyst$ reinforce this finding. Increases in the number of analysts in an industry are associated with significant declines in aggregate absolute forecast errors in the following month (p<.01).

Columns 3 and 4 examine the effects of increases in the number of analysts in an industry on aggregate forecast bias. In the binary model, forecast errors increase by 5.07% in the following month, suggesting that the average forecast error is significantly less optimistic when industries add more analysts. Similar results are obtained when we examine the continuous variable. The coefficient on $\Delta Analyst$ is positive and statistically significant at the 1% level. Taken together, these results suggest that increases in the number of analysts in an industry result in aggregate analyst earnings forecasts that are both more accurate and less optimistically biased, potentially due to increased competition within industries. Additionally, these benefits appear to outweigh any negative effects that new entrants may have on average industry accuracy.

We next examine the effect that changes in the number of industry analysts have on the informativeness of analyst reports and earnings announcements using the following specification:

$$\begin{aligned}\text{InfoType}_{\text{it}} = {} & \alpha_{\text{i}} + \alpha_1 \Delta\text{Analyst}_{\text{it}-1} + \alpha_2 |\text{Returns}_{\text{it}}| + \alpha_3 \text{Returns}_{\text{it}} + \alpha_4 \text{IPOs}_{\text{it}} + \\ & \alpha_5 \Delta\text{Trading Volume}_{\text{it}} + \alpha_6 \Delta\text{Ret Vol}_{\text{it}} + \gamma' \text{Month}_{\text{t}} + \epsilon_{\text{it}} \qquad (5),\end{aligned}$$

where i indexes industries and t indexes time. $InfoType$ is one of two informativeness measures: Earnings Announcement Informativeness ($EAINFO$) or Analyst Report Informativeness ($AnalystINFO$). We control for the magnitude of information content in a given month (i.e., $|Returns|$) as well as industry conditions (i.e., Returns, IPOs, Trading Volume and Return Volatility) as defined in prior equations. Similar to Equation 4, we again examine changes in the number of analysts ($\Delta Analyst$) as both a binary and a continuous variable. If higher analyst presence increases the aggregate informativeness of the analyst reports, we expect to find $\alpha_1 > 0$ when we examine Analyst Report Informativeness ($InfoType = AnalystINFO$). Similarly, if higher analyst presence also results in less reliance on earnings announcements as a source of new information, we expect $\alpha_1 < 0$ when we examine Earnings Announcements ($InfoType = EAINFO$).

[Table 6]

Table 6 provides the results from these tests.[31] The results in columns 1 and 2 show that following an increase in the aggregate number of industry analysts, analyst report informativeness ($AnalystINFO$) increases by .36% (p<.01). Similarly, when we examine the continuous changes in the number of analysts in an industry, we find a positive and significant relationship between increases in the number of analysts and analyst report informativeness (p<.01). These results suggest that the market perceives the aggregate informativeness of analyst reports to increase when industries add analysts, consistent with analysts producing more industry-related information. Also consistent with these findings, we find that an increase in the number of analysts in an industry reduces the average informativeness of earnings announcements in the aggregate (Columns 3 and 4 of Table 6). $EAINFO$ decreases by .4% (p<.05), on average, for increases in the number of analysts and is also negatively and significantly related to continuous changes in the number of analysts (p<.01).[32] These findings suggest that increasing the number of analysts providing coverage within an industry can

[31] Sample sizes vary slightly since some industry-months have no earnings announcements (e.g., Banking (GICS 4010)).
[32] Results are similar if we include time fixed effects.

improve the overall flow of information, thereby disseminating important value-relevant information to investors earlier than the earnings announcement date. Further, to the extent that the information is industry-specific rather than firm-specific, our industry-level tests examine improvements in information flow not captured in firm-level studies.

4.4 Results from Brokerage Closures and Mergers (Exogenous Shock)

Even in regressions with industry fixed effects, our inferences could be affected by biases in the estimated coefficients due to time-varying correlated omitted variables. Accordingly, this section revisits the previous tests using a measure of changes in the number of analysts based on brokerage house closures and mergers. To the extent that these changes are exogenous to other potential explanatory variables, this measure allows us to better estimate the effect of changes in analyst coverage and circumvent concerns that our results may be biased due to the exclusion of important omitted variables.

We consider drops in the number of analysts relating to the mergers and closures of 52 brokers between 1994 and 2008 that were previously examined in Derrien and Kecskes (2012).[33] We employ similar sample procedures and assume that an analyst is dropped if there is no earnings estimate for him in I/B/E/S during the year after the broker disappearance date. For broker closures, an analyst in an industry is identified as dropped if the analyst no longer appears on I/B/E/S after the broker closure date, but issued a forecast for firms in that industry during the prior 12 months. For broker mergers, an analyst in an industry is identified as dropped if analysts at both the target and acquirer brokerage houses covered firms in the industry during the 12 months before the merger and only one analyst covers the industry following the merger. We

[33] We thank the authors for sharing this data. For more details on how brokers are identified, please refer to their study.

further decompose $\Delta Analyst$into an *exogenous* portion ($\Delta Analyst^{exogenous}$) and a remaining *other* portion ($\Delta Analyst^{other}$).

It is important to note that that our procedure differs from a standard difference-in-difference analysis. For example, Hong and Kacperczyk (2010) and Derrien and Kecskes (2012) estimate the firm effect of drops in analyst coverage using a control sample of firms who do not receive a drop in coverage and are similar to the treated firms. In our empirical setting, since our variables are aggregated at the industry level, we cannot create an adequate control sample. A drop in analyst coverage affects multiple industries (potentially, all industries for the larger closure/mergers) which limits the candidates for a control sample. Further, at the level of aggregation we employ, it is not feasible to consider a "peer" industry as a control group since industries differ systematically.

We first re-examine our results for aggregate forecasting properties based on a modification of equation (4):

$$\mathrm{FChar}_{\mathrm{it}} = \alpha_{\mathrm{i}} + \alpha_1 \Delta \mathrm{Analyst}_{\mathrm{it-1}}^{\mathrm{exogenous}} + \alpha_2 \Delta \mathrm{Analyst}_{\mathrm{it-1}}^{\mathrm{other}} + \alpha_3 \mathrm{Returns}_{\mathrm{it}} + \alpha_4 \mathrm{IPOs}_{\mathrm{it}} + \alpha_5 \Delta \mathrm{Trading\ Volume}_{\mathrm{it}} + \alpha_6 \Delta \mathrm{Ret\ Vol}_{\mathrm{it}} + +\gamma' \mathrm{Month}_{\mathrm{t}} + \epsilon_{\mathrm{it}} \quad (6),$$

where i indexes industry and t indexes time. We examine two forecast characteristics ($Fchar$) Accuracy ($|AFE|$) and Bias (AFE). $\Delta \mathrm{Analyst}^{\mathrm{exogenous}}$ is the exogenous drop in analyst coverage resulting from any broker house merger or closures in a month and $\Delta \mathrm{Analyst}^{\mathrm{other}}$ is the remaining change in the number of analysts. All other variables (Returns, IPOs, Trading Volume and Return Volatility) as well as model specifications are as described in Equation (4) above.

[Table 7]

Table 7, Panel A provides the results from this test. Consistent with the OLS-FE models in Section 4.3, changes in the number of analysts within an industry are associated with decreases in average forecast error and optimistic bias in the following month. The coefficient on the exogenous portion of $\Delta Analyst$ is negative and significant (p<.05) when the dependent variable is $|AFE|$ and positive and significant when the dependent variable is AFE. Interestingly, while both the results for $|AFE|$ and AFE have the same predicted sign for the exogenous and other components of $\Delta Analyst$, the magnitudes for the exogenous portion are much larger. The implied economic effect from the exogenous portion of $\Delta Analyst$ ($\text{i.e.}, \Delta\text{Analyst}^{\text{exogenous}}$) is nearly 4 to 5 times larger than the effect from the remaining portion of $\Delta Analyst$ ($\text{i.e.}, \Delta\text{Analyst}^{\text{other}}$). While these results cannot be directly compared to Hong and Kacperczyk (2010) since their study is performed at the firm-level, the implications concerning the OLS regressions are similar. OLS appears to underestimate the effects of changes in industry analysts on aggregate accuracy and bias.

We expect changes in the number of analysts to have less of an impact on industries with a relatively large number of analysts. To test this notion, in Panel B, we partition the sample into high and low industry analyst following based on the median number of analysts in an industry at the start of the month. Consistent with our expectation, we find that the effect of exogenous drops in analysts is much more severe in industries with low median analyst following. The effect on accuracy (|AFE|) is nearly 5 times larger in low industries and the effect on bias (AFE) is about 4 times larger in low industries. The differences are also statistically significant (p-value <.05 and p-value<.1, respectively). Thus, the results from both OLS and the exogenous shock provide consistent implications and suggest that increases in aggregate industry analysts improve the overall quality of analysts' reports, with greater effects in industries with low analyst following.

Our second extension using the exogenous shock re-examines our public disclosure results from Section 4.3. We modify Equation (5) as follows:

$$\mathrm{InfoType_{it}} = \alpha_\mathrm{i} + \alpha_1\Delta\mathrm{Analyst}_{\mathrm{it-1}}^{\mathrm{exogenous}} + \alpha_2\Delta\mathrm{Analyst}_{\mathrm{it-1}}^{\mathrm{other}} + \alpha_3|\mathrm{Returns_{it}}| + \alpha_4\mathrm{Returns_{it}} + \alpha_5\mathrm{IPOs_{it}} + \alpha_6\Delta\mathrm{Trading\ Volume_{it}} + \alpha_7\Delta\mathrm{Ret\ Vol_{it}} + +\gamma'\mathrm{Month_t} + \epsilon_{it} \quad (7),$$

where i indexes industry and t indexes time. We again examine the two informativeness measures ($InfoType$) from above: Earnings Announcement Informativness ($EAINFO$) and Analyst Report Informativeness ($AnalystINFO$). $\Delta\mathrm{Analyst}^{\mathrm{exogenous}}$ is the exogenous drop in analyst coverage resulting from any broker house merger or closures in a month and $\Delta\mathrm{Analyst}^{\mathrm{other}}$ is the remaining change in the number of analysts. All other variables (|Returns|, Returns, IPOs, Trading Volume and Return Volatility) as well the model specifications are as described in Equation (5) above.

Table 8, Panel A provides the results from this test. Consistent with the tests in Section 4.3, changes in the number of analysts within an industry are positively associated with analyst informativeness and negatively associated with earnings announcement informativeness. Importantly, the exogenous component of $\Delta Analyst$ is related to the informativeness measures in the predicted directions. Exogenous changes in analysts are positively and significantly related to $AnalystInfo$ (p<.01) and negatively and significantly related to $EAInfo$ (p<.05). The magnitudes of the coefficients for the exogenous portion of $\Delta Analyst$are also much larger in magnitude than the remaining portion. Depending on the public disclosure variable, the implied economic effect of exogenous drops in analysts (i.e., $\Delta\mathrm{Analyst}^{\mathrm{exogenous}}$) on the information environment is nearly 7-10 times the effect of the other portion of $\Delta Analyst$ (i.e., $\Delta\mathrm{Analyst}^{\mathrm{other}}$).

In Table 8, Panel B, we again examine industries with high and low analyst following, with the expectation that $\Delta Analyst$ will have more severe effects for the informativeness of public disclosure in industries in which there is low levels of analyst following. The results for $AnalystInfo$ confirm this prediction. The coefficient on the exogenous component of $\Delta Analyst$ is about 1.5 times larger in industries that have low analyst following compared to those with high analyst following. This difference is also statistically significant at the 1% level. However, we find no difference in the effect of $\Delta Analyst$ on $EAInfo$ across high and low industries.[34]

5. Conclusion

In this study, we provide the first examination of the economic factors related to changes in the aggregate number of analysts as well as the implications these changes have for the quality of analyst reports and the amount of information the market extracts from them. The unique feature of our study is that we analyze the aggregate number of analysts, rather than the number of individual analysts covering a firm. Importantly, we find that the analyst industry appears to expand when analyst-related profits are higher and that this expansion benefits market participants by improving the overall flow of information.

In general, our results show that the aggregate number of analysts increases following periods of high returns, high IPOs and high trading volume. At the broker level, we strengthen our inferences by employing a natural experiment around analyst-related regulations that better allows us to identify how brokerage profits relate to increases in analyst presence. Following regulatory changes that de facto increase the cost of employing analysts, we find that brokers sanctioned by the Global Settlement and brokers with high levels of IPO activity reduce their workforce by about 1 more analyst per month than other brokers. While these regulations affected many sources of brokerage house profits, the Global Settlement likely had a particularly

[34] Our inferences remain unchanged in terms of signs and significance for all tests using the exogenous shock models when we examine univariate models with just $\Delta \text{Analyst}^{\text{exogenous}}$ or multivariate models with $\Delta \text{Analyst}^{\text{exogenous}}$ and control variables.

strong impact on IPO related revenues, an important source of funding for analyst services. Accordingly, our results suggest that when this source of analysts' revenue was reduced, brokers responded by reducing their analyst workforce.

We also show that changes in the number of analysts have important market consequences. We find that when aggregate analyst following falls, the average quality of aggregate analyst reports suffers. Following periods of reductions in analyst following, aggregate forecast error and bias increase, and market prices incorporate less information from analyst reports in the aggregate. We test and confirm the robustness of these findings using an exogenous shock in analyst coverage resulting from brokerage house mergers and closures. Our results suggest that the aggregate presence of the sell-side analysts industry has a significant impact on the quality of analysts' reports and on the information content of prices in the aggregate.

Our findings are particularly relevant to regulators, academics, and capital market participants because they suggest that there are significant benefits to having a larger sell-side analyst industry. While we do not consider all of the potential costs, our findings highlight an important trade-off regarding regulations seeking to improve the quality of analyst reports. Our results suggest that such actions can negatively affect the quality of analyst reports, if they result in lower numbers of analysts in the aggregate. Overall, our results extend our understanding of the sell-side analyst industry by assessing the impact of total analyst presence and competition on the aggregate quality of analyst reports and their effect on market prices, issues that are important to a well-functioning capital market.

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Figure 1 – Time Series Plot of the Number of Analysts (1990-2010)

This figure plots the number of analysts reporting on I/B/E/S each month between 1990 and 2010. The blue line represents the full sample and the red line is for a constant sample of brokers with reports every month throughout the period. The total number of analysts providing forecasts in a month is the number of analysts from the prior period plus the monthly change in the number of analysts, where the change in the number of analysts is the difference between the number of new analysts and the number of exiting analysts in a month. We define a new analyst as an analyst issuing her first forecast in the sample, or who has not issued a forecast in the past 12 months. An exiting analyst is defined as an analyst issuing her last forecast in the sample, or who does not issue a forecast in the next 12 months. We use the number of unique analysts providing forecasts in 1989 as a base year for computing the running total.

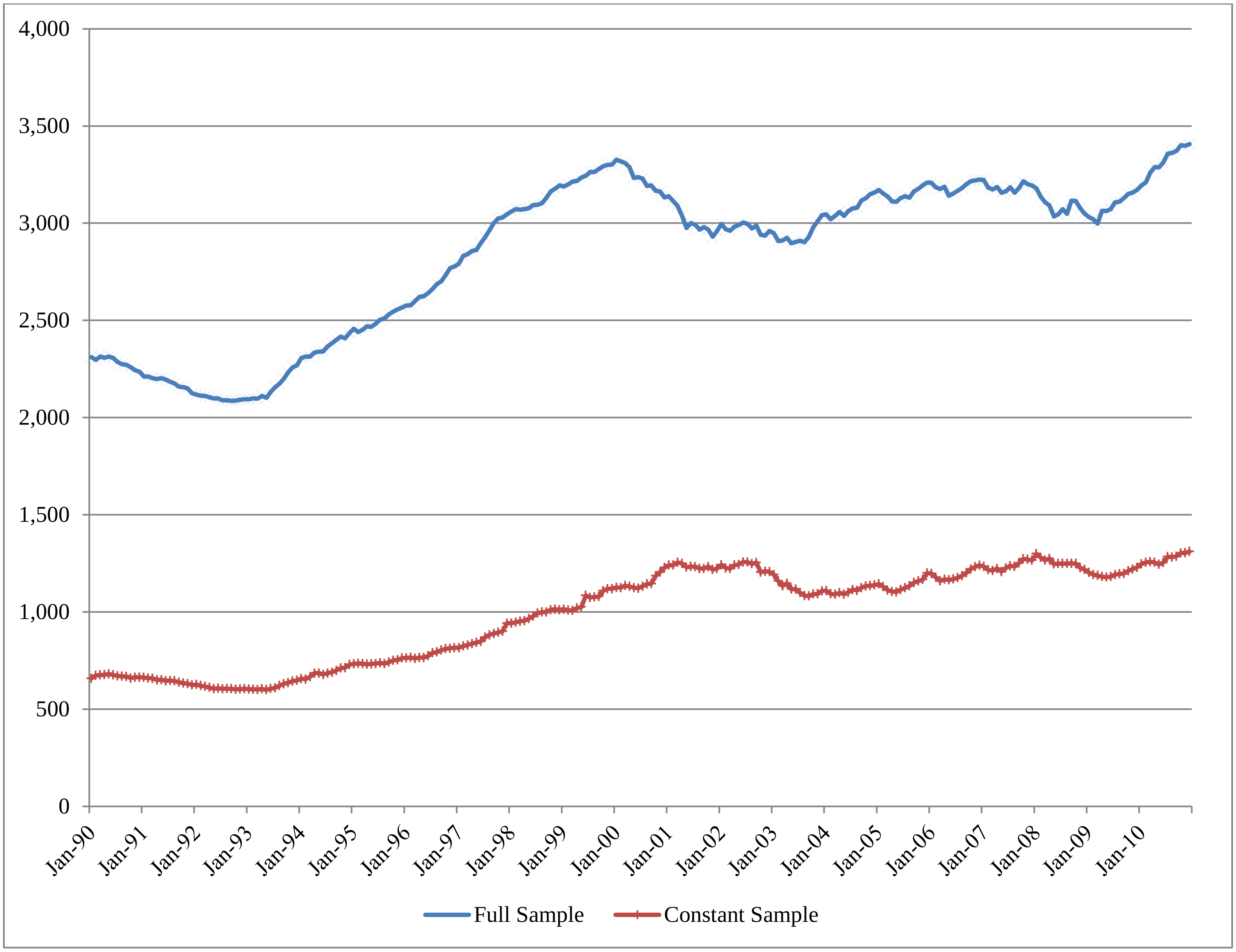

Table 1 – Sample Statistics

This table presents the summary statistics for the market, industry, and broker samples. Panel A contains the market sample and is comprised of 252 monthly observations from 1990-2010. Panel B contains the industry sample and is comprised of monthly observations for 24 GICS Industry Groups. Panel C contains the broker sample and is comprised of monthly observations for 855 brokers. ΔNumber of Analysts is the difference between the number of new analysts and the number of existing analysts in a month. We define a new analyst as an analyst issuing her first forecast in the sample, or who has not issued a forecast in the past 12 months. An exiting analyst is defined as an analyst issuing her last forecast in the sample, or who does not issue a forecast in the next 12 months. The Number of Analysts is the number of analysts from the prior period plus ΔNumber of Analysts. We use the number of unique analysts providing forecasts in 1989 as a base year for computing the running total. ΔNumber of Analysts and Number of analysts are aggregated at the market, industry, and broker level. Value Weighted Returns are calculated by computing the value-weighted market (industry) portfolio returns each month, using a firm's prior month market-cap weight in the market (industry). The number of IPOs is the number of completed offerings in the market (industry) group over the past month. We exclude IPOS with an offer price below $5 per share, ADRS, and IPOS not listed on CRSP within 30 days of the issuance date. Trading Volume is the sum of all firms' trading volume (in number of shares) in the market (industry) over the past month. Return Volatility is the standard deviation of the market (industry) value-weighted daily returns over the month.

Panel A: Market Sample (N=252)

Variable	Mean	STD	Q1	Median	Q3
Number of Analysts	2,848	402	2,462	3,020	3,164
ΔNumber of Analysts	4.52	21.19	-7.50	5.50	17.50
Value Weighted Returns	0.0083	0.0458	-0.0201	0.0145	0.0392
Number of IPOs	24	22	7	16	39
Trading Volume (Billions of shares)	159.45	105.81	82.97	150.40	195.62
ΔTrading Volume	0.25	89.05	-21.07	0.65	22.17
Return Volatility	0.0100	0.0064	0.0059	0.0086	0.0115
ΔReturn Volatility	0.0000	0.0043	-0.0020	-0.0003	0.0018

Panel B: Industry Sample (N=5,877)

	Mean	STD	Q1	Median	Q3
Number of Analysts	300	165	175	241	410
ΔNumber of Analysts	0.22	4.63	-2.00	0.00	3.00
Value Weighted Returns	0.0090	0.0592	-0.0221	0.0122	0.0434
Number of IPOs	1	2	0	0	1
Trading Volume (Billions of shares)	6.77	22.89	0.86	2.28	4.95
ΔTrading Volume	0.01	17.67	-0.29	0.01	0.33
Return Volatility	0.0120	0.0080	0.0072	0.0099	0.0142
ΔReturn Volatility	0.0000	0.0053	-0.0024	-0.0002	0.0021

Panel C: Broker Sample (N=59,575)

	Mean	STD	Q1	Median	Q3
Number of Analysts	11	18	1	4	13
ΔNumber of Analysts	0.04	1.14	0.00	0.00	0.00

Table 2 –Factors Related To Aggregate Changes in Analysts Following

This table provides the results from regressions of ΔAnalyst at the market and industry levels. Panel A provides OLS market regressions of $\Delta\text{Analyst}_{t+1}$ on market proxies for analyst-related profit sources. Panel B provides the results from panel regressions of industry $\Delta\text{Analyst}_{i,t+1}$ on industry proxies for analyst-related profit sources. Market (Industry) proxies for analyst-related profit sources include current month Returns, # of IPOs, ΔTrading Volume, and ΔReturn Volatility aggregated across all firms in the market (industry) and are defined in Table 1. Industry regressions contain a set of industry fixed effects. The market sample consists of monthly observations from 1990-2010. The industry sample consists of monthly observations from 1990-2010 for each of the 24 GICS Industry Groups. Panel A regression results are based on Newey-West adjusted standard errors. In Panel B, standard errors are clustered by industry and month. ***,**, and * denote 1%, 5% and 10% level of significance respectively.

Panel A: Market Sample

VARIABLES	(1)	(2)	(3)	(4)	(5)
Returns (Market)	**112.0833*** **				**108.7680*** **
	(3.56)				**(2.95)**
# of IPOs (Market)		**0.1930*** **			**0.1719** **
		(2.60)			**(2.36)**
ΔTrading Volume (Market)			0.0154		0.0098
			(1.12)		(0.71)
ΔReturn Volatility (Market)				-357.6785	136.4779
				(-1.06)	(0.35)
Constant	**3.4962* **	-0.2714	**4.4248** **	**4.4275** **	-0.6645
	(1.90)	(-0.10)	**(2.34)**	**(2.33)**	(-0.25)
Observations	252	252	252	252	252
R-squared	0.06	0.04	0.00	0.01	0.09

Panel B: Industry Sample

VARIABLES	(1)	(2)	(3)	(4)	(5)
Returns (Industry)	**6.3737** **				**4.4266* **
	(2.25)				**(1.82)**
# of IPOs (Industry)		**0.4187*** **			**0.4103*** **
		(6.68)			**(6.69)**
ΔTrading Volume (Industry)			**0.0088*** **		**0.0074*** **
			(14.15)		**(6.56)**
ΔReturn Volatility (Industry)				**-68.3685** **	**-52.4743* **
				(-2.24)	**(-1.87)**
Observations	5,877	5,877	5,877	5,877	5,877
R-squared	0.01	0.04	0.01	0.01	0.05

Table 3 – The Effects of Regulation

This table provides difference-in-differences regression results of $\Delta \text{Analyst}_{b,t}$ across brokers that were more and less likely to be affected by sell-Side analyst industry regulations:

$$\Delta Analyst_{b,t} = \alpha_0 + \alpha_1(Post_t * Treated_b) + \alpha_2 Post_t + \alpha_3 Treated_b + \alpha_4 Merge_{bt} + \beta' MktVars_t + \gamma'(Post_t * MktVar_t) + \epsilon_{bt}$$

Post is an indicator variable that takes the value of 0 for the 36 months prior to September 2001 and a value of 1 for the 36 months after September 2001. Treated is an indicator variable that takes the value of 1 for if the broker is in one of three treatment groups (defined below) or 0, otherwise. Merge is an indicator variable that takes the value of 1 if the broker merges with another broker in the period, or 0 otherwise. MktVars is a vector of control variables that include analyst-related profit proxies at the market level including: Returns, # of IPOs, ΔTrading Volume, and ΔReturn Volatility. MktVars and $\Delta \text{Analyst}_{b,t}$ are as defined in Table 1.

The three alternative treatment groups include (1) *Sanctioned Brokers*; (2) *High IPO Issuers*; and (3) *IPO Issuers*. *Sanctioned Brokers* include the 12 Sanctioned GS Brokers (i.e., Credit Suisse First Boston; Goldman Sachs; Lehman Brothers; J.P. Morgan; Merrill Lynch, Pierce, Fenner & Smith; Morgan Stanley; Citigroup Global Markets; UBS Warburg; U.S. Bankcorp Piper Jaffray; Deutsche Bank; and Thomas Weisel). *High IPO Issuers* include all firms with IPO issues above the median value between 1999 and 2004. *IPO Issuers* include any firm that issues an IPO between 1999 and 2004. IPO data is obtained from Bloomberg for 1999-2004. Standard errors are clustered by broker and month. ***,**, and * denote 1%, 5% and 10% level of significance respectively.

Variable	(1)	(2)	(3)
Sanctioned * Post Regulation	**-1.094*** **		
	(-3.94)		
High IPO Issuer * Post Regulation		**-0.789*** **	
		(-3.35)	
IPO Issuer * Post Regulation			**-0.723*** **
			(-3.37)
Sanctioned	**0.590*** **		
	(3.82)		
High IPO Issuer		**0.320** **	
		(2.21)	
IPO Issuer			**0.431** **
			(2.37)
Post Regulation	**-0.340* **	-0.300	-0.109
	(-1.70)	(-1.48)	(-0.48)
Constant	**-0.023**	0.005	-0.180
	(-0.15)	(0.03)	(-0.93)
Market Controls?	Yes	Yes	Yes
Post * Market Controls?	Yes	Yes	Yes
Number of Brokers	36	36	36
Observations	2,647	2,647	2,647
R-squared	0.06	0.06	0.06

Table 4 –Descriptive Statistics For Information Environment Consequences

This table presents monthly industry summary statistics for two sets of aggregate information environment variables: 1) aggregate earnings forecast properties and 2) informativeness of public disclosure. The information environment variables are accuracy (|AFE|) and bias (AFE). To construct these measures, we first compute signed forecast errors for each firm as the difference between actual EPS minus the monthly EPS forecast scaled by the absolute value of the consensus EPS forecast. Similarly, we compute unsigned forecast errors for each firm by computing the absolute value of the difference between actual EPS less the monthly consensus EPS forecast, scaled by the absolute value of the consensus EPS forecast. |AFE| is a proxy for accuracy and is the average of the unsigned forecast errors for each industry-month. AFE is proxy for forecast bias and is the average of the signed forecast errors for each industry-month. The informativeness of public disclosure variables are Earnings Announcement Informativeness (EAINFO) and Analyst Report Informativeness (AnalystINFO). EAINFO is calculated by averaging all firm absolute cumulative abnormal returns (ACAR) within a 3-day window around the earnings announcement within an industry-month. AnalystINFO is calculated by averaging firm-level Analyst Informativeness (AI) across all firms within an industry-month, where AI is the ratio of absolute-size adjusted forecast revision dates in a given month divided by the sum of all absolute size-adjusted returns for all trading days in a month.

	N	Mean	STD	Q1	Median	Q3
|AFE|	5,877	0.470	0.415	0.250	0.388	0.573
AFE	5,877	-0.285	0.404	-0.381	-0.213	-0.075
AnalystInfo	5,877	0.148	0.038	0.123	0.143	0.169
EAInfo	5,697	0.102	0.049	0.073	0.096	0.122

Table 5 – The Effects of Analyst Following on Aggregate Forecasting Properties

This table provides panel regressions of aggregate earnings forecast properties on changes in aggregate analyst following at the industry level:

$$\mathrm{FChar}_{it} = \alpha_i + \alpha_1 \Delta\mathrm{Analyst}_{it-1} + \alpha_2 \mathrm{Returns}_{it} + \alpha_3 \mathrm{IPOs}_{it} + \alpha_4 \Delta\mathrm{Trading\ Volume}_{it} + \alpha_5 \Delta\mathrm{Ret\ Vol}_{it} + \gamma' \mathrm{Month}_t + \epsilon_{it}$$

$\mathrm{Fchar}_{i,t}$ is one of two forecast characteristics: Accuracy (|AFE|) or Bias (AFE) and are defined in Table 4. $\Delta\mathrm{Analyst}_{i,t-1}$ and the control variables (Returns, # of IPOs, ΔTrading Volume, and ΔReturn Volatility) are as defined in Table 1. Under columns labeled "Binary", $\Delta\mathrm{Analyst}_{it-1}$ is replaced with an indicator variable that takes the value of 1 when $\Delta\mathrm{Analyst}_{it-1}$ is greater than or equal to 0 in the period, or 0 otherwise. All models include month fixed effects and industry fixed effects. Standard errors are clustered by industry and month. ***,**, and * denote 1%, 5% and 10% level of significance respectively.

	|AFE|		AFE	
VARIABLES	*Binary*	*Continuous*	*Binary*	*Continuous*
$\Delta\mathrm{Analyst}_{t-1}$	**-0.0446*** **	**-0.0047*** **	**0.0507*** **	**0.0053*** **
	(-3.34)	**(-3.41)**	**(3.56)**	**(3.54)**
Returns (Industry)	**-0.3387***	**-0.3408***	**0.6816*** **	**0.6840*** **
	(-1.84)	**(-1.85)**	**(3.68)**	**(3.69)**
# of IPOs (Industry)	-0.0001	0.0009	-0.0034	-0.0045
	(-0.02)	(0.19)	(-0.83)	(-1.08)
ΔTrading Volume (Industry)	0.0000	0.0000	0.0000	0.0000
	(0.21)	(0.24)	(0.29)	(0.26)
ΔReturn Volatility (Industry)	0.6948	0.8507	-1.1671	-1.3425
	(0.34)	(0.41)	(-0.56)	(-0.65)
Observations	5,854	5,854	5,854	5,854
R-squared	0.17	0.17	0.12	0.12

Table 6 – The Effects of Analyst Following on Public Disclosure Informativeness

This table provides panel regressions of disclosure informativeness on changes in aggregate analyst following at the industry level:

$$\mathrm{InfoType}_{it} = \alpha_i + \alpha_1 \Delta \mathrm{Analyst}_{it-1} + \alpha_2 |\mathrm{Returns}_{it}| + \alpha_3 \mathrm{Returns}_{it} + \alpha_4 \mathrm{IPOs}_{it} + \alpha_5 \Delta \mathrm{Trading\ Volume}_{it} + \alpha_6 \Delta \mathrm{Ret\ Vol}_{it} + \gamma' \mathrm{Month}_t + \epsilon_{it}$$

$\mathrm{InfoType}_{i,t}$ is one of two informativeness measures: Earnings Announcement Informativeness (EAINFO) or Analyst Report Informativeness (AnalystINFO) and are defined in Table 4. $|\mathrm{Returns}_{it}|$ is the absolute value of industry value-weighted returns ($\mathrm{Returns}_{it}$). $\Delta\mathrm{Analyst}_{i,t-1}$ and the other control variables (Returns, # of IPOs, ΔTrading Volume, and ΔReturn Volatility) are as defined in Table 1. Under columns labeled "Binary", $\Delta\mathrm{Analyst}_{t-1}$ is replaced with an indicator variable that takes the value of 1 when $\Delta\mathrm{Analyst}_{i,t-1}$ is greater than or equal to 0 in the period, or 0 otherwise. All models include month fixed effects and industry fixed effects. Standard errors are clustered by industry and month. ***,**, and * denote 1%, 5% and 10% level of significance respectively.

	AnalystINFO		EAINFO	
VARIABLES	*Binary*	*Continuous*	*Binary*	*Continuous*
ΔAnalyst$_{t-1}$	**0.0036*** **	**0.0004*** **	**-0.0040** **	**-0.0005*** **
	(3.33)	**(3.65)**	**(-2.38)**	**(-2.78)**
|Returns (Industry)|	0.0111	0.0114	**0.2391*** **	**0.2387*** **
	(0.49)	(0.50)	**(7.95)**	**(7.89)**
Returns (Industry)	-0.0179	-0.0178	-0.0370	-0.0369
	(-1.21)	(-1.21)	(-1.53)	(-1.52)
# of IPOs (Industry)	-0.0004	-0.0005	-0.0003	-0.0002
	(-1.28)	(-1.55)	(-0.48)	(-0.28)
ΔTrading Volume (Industry)	0.0000	0.0000	0.0000	0.0000
	(0.26)	(0.26)	(1.60)	(1.54)
ΔReturn Volatility (Industry)	0.1257	0.1089	-0.0199	0.0008
	(0.60)	(0.51)	(-0.06)	(0.00)
Observations	5,854	5,854	5,674	5,674
R-squared	0.45	0.45	0.27	0.27

Table 7 – Forecasting Consequences Based on Brokerage House Closures and Mergers

This table provides panel regressions of aggregate earnings forecast properties on changes in industry analyst following resulting from both exogenous drops and other changes in analyst coverage:

$$\mathrm{FChar}_{it} = \alpha_i + \alpha_1 \Delta\mathrm{Analyst}_{it-1}^{\mathrm{exogenous}} + \alpha_2 \Delta\mathrm{Analyst}_{it-1}^{\mathrm{other}} + \alpha_3 \mathrm{Returns}_{it} + \alpha_4 \mathrm{IPOs}_{it} + \alpha_5 \Delta\mathrm{Trading\ Volume}_{it} + \alpha_6 \Delta\mathrm{Ret\ Vol}_{it} + +\gamma' \mathrm{Month}_t + \epsilon_{it}$$

$\Delta\mathrm{Analyst}_{it-1}^{\mathrm{exogenous}}$ is the number of analyst drops resulting from mergers or closures of brokerage houses. An analyst is considered to have dropped if she provides reports for the closed/merged broker house in the 12 months prior to the event date and provides no reports for any brokerage house in the 12 months after the event date. $\Delta\mathrm{Analyst}_{it-1}^{\mathrm{other}}$ is the difference between $\Delta\mathrm{Analyst}_{it-1}$and $\Delta\mathrm{Analyst}_{it-1}^{\mathrm{exogenous}}$. $\mathrm{Fchar}_{i,t}$ is one of two forecast characteristics: (Accuracy) $|\mathrm{AFE}|$ or (Bias) AFE and are defined in Table 4. Other variables are as defined in Table 1. All models include month fixed effects and industry fixed effects. Panel A reports the main effects. Panel B reports cross-sectional differences where Low (High) indicates an industry where the number of analysts at the beginning of the month is less than or equal to (greater than) the median number of analysts across all industries in a given month. Standard errors are clustered by industry and month. ***,**, and * denote 1%, 5% and 10% level of significance respectively.

Panel A: Forecast Consequences

VARIABLES	|AFE|	AFE
$\Delta\mathrm{Analyst}^{\mathrm{exogenous}}$	**-0.0208****	**0.0199****
	(0.00928)	**(2.12)**
$\Delta\mathrm{Analyst}^{\mathrm{other}}$	**-0.00401*****	**0.0047*****
	(0.00123)	**(3.43)**
Returns (Industry)	**-0.301***	**0.6477*****
	(0.178)	**(3.62)**
# of IPOs (Industry)	0.00107	-0.0047
	(0.00454)	(-1.17)
ΔTrading Volume (Industry)	2.47e-05	0.0000
	(7.69e-05)	(0.18)
ΔReturn Volatility (Industry)	1.150	-1.6140
	(2.127)	(-0.76)
Observations	5,854	5,854
R-squared	0.18	0.13

Panel B: By Industry Analyst Following

	|AFE|			AFE		
VARIABLES	*Low*	*High*	*Difference*	*Low*	*High*	*Difference*
ΔAnalyst^{exogenous}	**-0.0504***	**-0.0116*****	**0.0084****	0.0426	**0.0129*****	**-0.0078***
	(-1.95)	**(-3.06)**	**(2.21)**	(1.55)	**(3.12)**	**(-1.79)**
ΔAnalyst^{other}	**-0.0088****	**-0.0024****		**0.0095****	**0.0032*****	
	(-2.57)	**(-2.55)**		**(2.45)**	**(2.92)**	
Returns (Industry)	-0.3652	-0.2299		**0.7517*****	**0.5477*****	
	(-1.61)	(-1.58)		**(3.73)**	**(2.97)**	
# of IPOs (Industry)	**-0.0297****	**0.0045***		**0.0305*****	**-0.0086*****	
	(-2.40)	**(1.70)**		**(3.38)**	**(-3.97)**	
ΔTrading Volume (Industry)	-0.0002	-0.0000		-0.0006	**0.0001****	
	(-0.29)	(-0.14)		(-0.62)	**(2.17)**	
ΔReturn Volatility (Industry)	1.7497	0.6995		-2.2378	-1.1252	
	(0.64)	(0.48)		(-0.91)	(-0.63)	
Observations	3,022	2,832		3,022	2,832	
R-squared	0.17	0.28		0.13	0.17	

Table 8 – Disclosure Consequences Based on Brokerage House Closures and Mergers

This table provides panel regressions of aggregate industry informativeness measures on industry changes in analyst following resulting from both exogenous drops and other changes in analyst coverage:

$$\mathrm{InfoType_{it}} = \alpha_i + \alpha_1 \Delta\mathrm{Analyst}_{it-1}^{exogenous} + \alpha_2 \Delta\mathrm{Analyst}_{it-1}^{other} + \alpha_3 |\mathrm{Returns_{it}}| + \alpha_4 \mathrm{Returns_{it}} + \alpha_5 \mathrm{IPOs_{it}} + \alpha_6 \Delta\mathrm{Trading\ Volume_{it}} + \alpha_7 \Delta\mathrm{Ret\ Vol_{it}} + +\gamma' \mathrm{Month_t} + \epsilon_{it}$$

$\Delta\mathrm{Analyst}_{it-1}^{exogenous}$ is the number of analyst drops resulting from mergers or closures of brokerage houses. An analyst is considered to have dropped if she provides reports for the closed/merged broker house in the 12 months prior to the event date and provides no reports for any brokerage house in the 12 months after the event date. $\Delta\mathrm{Analyst}_{it-1}^{other}$ is the difference between $\Delta\mathrm{Analyst}_{it-1}$and $\Delta\mathrm{Analyst}_{it-1}^{exogenous}$. $\mathrm{InfoType_{it}}$ is one of two informativeness measures: Earnings Announcement Informativeness (EAINFO) or Analyst Report Informativeness (AnalystINFO) and are defined in Table 4. Other variables are as defined in Table 1. All models include month fixed effects and industry fixed effects. Panel A reports the main effects. Panel B reports cross-sectional differences where Low (High) indicates an industry where the number of analysts at the beginning of the month is less than or equal to (greater than) the median number of analysts across all industries in a given month. Standard errors are clustered by industry and month. ***,**, and * denote 1%, 5% and 10% level of significance respectively.

Panel A: Disclosure Consequences

VARIABLES	AnalystINFO	EAINFO
$\Delta\mathrm{Analyst}^{exogenous}$	**0.0030*** **	**-0.0027** **
	(5.16)	**(-2.40)**
$\Delta\mathrm{Analyst}^{other}$	**0.0003*** **	**-0.0004** **
	(2.69)	**(-2.48)**
|Returns (Industry)|	0.0174	**0.2337*** **
	(0.79)	**(7.60)**
Returns (Industry)	**-0.0247***	-0.0311
	(-1.77)	(-1.24)
# of IPOs (Industry)	**-0.0005***	-0.0002
	(-1.73)	(-0.25)
ΔTrading Volume (Industry)	0.0000	0.0000*
	(0.09)	(1.73)
ΔReturn Volatility (Industry)	0.0533	0.0491
	(0.26)	(0.14)
Observations	5,854	5,674
R-squared	0.46	0.27

Panel B: By Industry Analyst Following

	AnalystInfo			EAINFO		
VARIABLES	*Low*	*High*	*Difference*	*Low*	*High*	*Difference*
$\Delta \text{Analyst}^{\text{exogenous}}$	**0.0042*** **	**0.0027*** **	**-0.0007*** **	-0.0026	**-0.0027*** **	-0.0001
	(4.34)	**(5.80)**	**(-3.95)**	(-1.07)	**(-3.03)**	(-0.21)
$\Delta \text{Analyst}^{\text{other}}$	**0.0008*** **	**0.0002***		-0.0003	**-0.0005*** **	
	(3.86)	**(1.77)**		(-0.94)	**(-3.24)**	
|Returns (Industry)|	0.0259	0.0080		**0.2192*** **	**0.2471*** **	
	(0.95)	(0.31)		**(6.43)**	**(7.39)**	
Returns (Industry)	-0.0230	**-0.0283** **		-0.0447	-0.0178	
	(-1.48)	**(-2.14)**		(-1.62)	(-0.77)	
# of IPOs (Industry)	-0.0009	**-0.0004***		-0.0014	0.0001	
	(-0.78)	**(-1.67)**		(-1.04)	(0.12)	
ΔTrading Volume (Industry)	-0.0001	0.0000		**0.0002** **	0.0000	
	(-0.85)	(0.98)		**(2.13)**	(0.92)	
ΔReturn Volatility (Industry)	0.1312	-0.0108		-0.1262	0.1935	
	(0.57)	(-0.06)		(-0.33)	(0.56)	
Observations	3,022	2,832		2,908	2,766	
R-squared	0.36	0.59		0.24	0.25	