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Brookerage Industry Self-Regulation: The Case of Analysts' Background Disclosures*

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1. Introduction

In the aftermath of recent Wall Street scandals, the efficacy of a self-regulatory model in the brokerage industry has been called into question (Boni and Womack 2002). This questioning is not surprising, considering the conflicts of interest faced by the National Association of Securities Dealers (NASD) in its dual roles as primary industry regulator and promoter of its constituents' interests.[1] Our study contributes to recent literature investigating the relevance of brokerage industry regulation by focusing on a disclosure initiative which informs investors of investment professionals' backgrounds.[2]

We focus on the following question: Can ex ante uninformed investors seeking earnings research gain knowledge pertaining to two important analyst forecasting characteristics — forecast accuracy and market credibility — by using analysts' background disclosures? We focus on earnings forecasts because of their importance to capital markets in forming earnings expectations (Fried and Givoly 1982; O'Brien 1988) and their value as

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1. Although the Securities and Exchange Commission (SEC) oversees the financial industry, it delegates the regulatory duties to the NASD and New York Stock Exchange (NYSE). When we began this study, the NASD was the industry's largest regulatory body; however, the NASD and NYSE have since consolidated most of their operations into the Financial Industry Regulatory Authority (FINRA; Davies 2007).
2. For example, Barber et al. (2006) examine industry regulation requiring analyst research reports to disclose the percentage of their recommendations that are buys, holds, and sells.

inputs to other research outputs such as stock recommendations (Loh and Mian 2006), target price forecasts (Bandyopadhyay, Brown, and Richardson 1995), valuation models (Frankel and Lee 1998), and growth and return on equity investment models (Easton et al. 2002). We focus on the accuracy of earnings forecasts because of its importance to investors (O'Brien 1991; Stickel 1992; Mikhail, Walther, and Willis 1997; Clement 1999; Loh and Mian 2006), and on the credibility of analysts' earnings forecast revisions because prior research reveals that investors consider a number of forecast factors in addition to past accuracy when evaluating the expected accuracy of an analyst's forecast (Stickel 1992; Clement and Tse 2003; Bonner, Walther, and Young 2003).

We hand-collect a sample of financial analysts with background disclosure events (hereafter "disclosed analysts"). There are eight types of background disclosures: criminal actions, customer complaints, bankruptcies, regulatory actions, terminations, civil judicial actions, investigations, and judgments/liens.[3] Because the primary focus of our study is to examine the potential benefits of using background disclosures for ex ante uninformed investors seeking earnings research, we require disclosed analysts' research outputs to be available in the period after the date of the original incident — that is, when an uninformed investor could potentially benefit from conducting a search of public disclosures on the analyst of interest. Hereafter, we refer to this date as the "incident date".[4]

Because we are the first to study disclosed analysts, we provide descriptive statistics, beginning with the frequency of analysts' earnings forecasts and the types of firms that they follow. We find that about 16 percent of firm-quarters at the intersection of the I/B/E/S and our hand-collected NASD databases have at least one disclosed analyst making an earnings forecast. Relative to nondisclosed analysts (i.e., analysts in the NASD database without disclosure events), disclosed analysts tend to follow firms with larger sales, earnings, and market capitalizations; smaller profitability; lower book-to-market ratios; and less debt in their capital structures. Because firm characteristics differ between firms followed by disclosed versus nondisclosed analysts, and because these characteristics may affect forecasting difficulty and stock returns, we match disclosed analysts to a control group of nondisclosed analysts by firm-quarter for both our accuracy and market tests.

Relying on firm-quarter relative measures, descriptive analyses of forecasting characteristics indicate that, compared to nondisclosed analysts, disclosed analysts forecast less accurately in both the current and prior four

3. The Appendix provides examples of specific disclosure events. Table 2 shows the frequency of analysts' background disclosures by type of event.

4. When there is more than one disclosure event, we use the date of the first disclosure as the incident date, reasoning that this is the first date when a disclosure event concerning the analyst is publicly known.

quarters, later in the quarter, and more frequently. They make their forecasts with greater delay relative to other analysts, follow more firms, work for brokerage firms of similar size, have more experience, have more job turnover, and are more consistent in the specific firms they follow. Past research has shown that many of these forecast characteristics are related to both forecast accuracy and market reaction, consequently we include them in our accuracy and market models.

Our forecasting performance model evaluates the quarterly earnings forecast accuracy of disclosed analysts by regressing earnings forecast accuracy on the disclosed analyst indicator variable and controls. Consistent with the notion that disclosed analysts' earnings forecasts are less reliable, we find that disclosed analysts' earnings forecasts are less accurate than those of nondisclosed analysts following the same firm-quarters. In supplemental analyses, we investigate whether this accuracy result is due to our disclosure variable capturing some persistent, unmeasured analyst characteristic versus being a product of the disclosure event per se. Based on several analyses, including examining disclosed analysts' forecasts in the predisclosure period, the impact of multiple disclosure events, and the effect of elapsed time since the disclosure event, the totality of the evidence suggests the disclosure event signals a persistent analyst characteristic.

To determine the market credibility of disclosed analysts' earnings research, we examine the short-window market reaction to analysts' earnings forecast revisions. Controlling for firm-level variables and analyst forecasting characteristics, we find a weaker market reaction to forecast revisions by disclosed analysts relative to those by nondisclosed analysts. We interpret this finding as indicating investors consider forecast revisions by analysts with disclosures to be less credible. We do not find a weaker market reaction to disclosed analysts' forecasts in the pre-incident period, suggesting the market is unaware of the analyst characteristic signaled by the disclosure prior to its release.

To gain perspective on whether market segments varying in sophistication respond differently to disclosed versus nondisclosed analysts' forecast revisions, we evaluate variation in trade size surrounding forecast revisions. Relative to small traders, we find stronger reactions by larger traders to analyst forecasts in general, but we do not find large investors discount disclosed analysts' forecasts to a greater extent than small investors.

A natural question arising from our results is why analysts with disclosure events remain employed at brokerage houses. We suggest several reasons for their continuing employment. First, their relatively poor performance with respect to forecasting and market credibility does not preclude the possibility that they are superior to analyst candidates who are not employed at brokerage firms. Second, we focus on earnings research, taking into account that analysts perform other important tasks, such as writing reports, making stock recommendations, and forecasting target prices. It is conceivable that disclosed analysts outperform other analysts in these research tasks. Third, disclosed analysts may add less value than non-disclosed analysts, but satisfy a minimum threshold and are are paid less in equilibrium.

Our study is important for several reasons. First, we add to the evidence that investors act as if past accuracy is not all that matters when reacting to analyst earnings forecast revisions. Stickel (1992) finds that investors react more to forecast revisions of analysts who are members of the *Institutional Investor* All-American team, and Clement and Tse (2003) show that investors react to a variety of analyst characteristics beyond past accuracy. We identify analyst background as another important characteristic that is associated with both forecasting accuracy and investor response to analyst revisions.

Second, in the aftermath of recent Wall Street analyst scandals, there have been calls for reforming the research analyst profession. During the congressional "Analyzing the Analysts" hearings in June–July 2001, the efficacy of the current model of brokerage industry self-regulation was called into question (Boni and Womack 2002). Although a summary examination of the efficacy of brokerage industry self-regulation is beyond the scope of our study, in the spirit of Barber et al. (2006) we add to the knowledge of the current state of industry regulation by examining the association between this self-regulatory disclosure mechanism and analysts' accuracy and credibility.

Third, investors rate integrity and professionalism as one of the most important attributes of an equity research firm (*Institutional Investor* 2006).[5] Indeed, this attribute increased the most in importance among analyst attributes between 1998 and 2005 (Bradshaw 2006). While some of the eight types of NASD disclosures overlap well with integrity and professionalism (e.g., criminal actions), admittedly others are more loosely connected (e.g., bankruptcies). To the extent that, collectively, these disclosures provide a reasonable — albeit noisy — proxy for integrity and professionalism, we provide evidence associating these characteristics with research performance and credibility.

We organize our paper as follows. The next section describes the institutional background and related literature. Section 3 describes the specification of our empirical tests. Section 4 describes our sample selection and provides results. Section 5 contains the results of supplementary analyses. Section 6 relates several sensitivity analyses and section 7 concludes.

5. It is the third most important attribute of the 12 examined. The most important attribute is industry knowledge.

2. Institutional background and related literature

Brokerage industry background disclosures

Created by the NASD in 1988, the public disclosure program provides investors with a mechanism for gaining knowledge of the background and conduct of registered investment professionals (NASD 2006).[6] According to the NASD, their public disclosure database was accessed more than 4 million times during 2006 by users who requested more than 200,000 reports (these reports are generated only in cases of actual instances of disclosure events, e.g., a past criminal conviction).[7] NASD disclosure events are based on information in the Central Registration Depository and include self-reported information from investment professionals (Form U-4 of the Uniform Application for Securities Industry Registration or Transfer), brokerage firms (Form BD of the Uniform Application for Broker-Dealer Registration and Form U-5), and governmental agencies such as the SEC (NASD 2006). As one example, Form U-5 is filed by a brokerage firm each time an investment professional leaves a firm; its purpose is to explain the reason for the separation (Slater 2007).

Critics of the NASD disclosure program argue that, because the NASD is supported financially by the firms it seeks to regulate, this support leads to conflicts of interest with respect to providing investors with a full and objective reporting of relevant disclosures (Loomis 2002; Jamieson 2006; Siedle 2006). Critics also maintain that NASD disclosures provide limited benefits to investors because they exclude important events such as customer settlements of under $10,000. In addition to criticizing the guidelines per se, some have accused the NASD of understating reportable events and of allowing firms to expunge their records too liberally (Siedle 2006; Davies 2007).

Earnings forecasting performance

Prior literature has shown variation in earnings forecasting accuracy conditional on a number of factors, including forecast horizon (Crichfield, Dyckman, and Lakonishok 1978; O'Brien 1988), forecast frequency (Jacob, Lys, and Neale 1999), days since the last forecast (e.g., Clement and Tse 2003), number of industries and firms followed (Clement 1999), broker size (Stickel 1995; Clement 1999), and experience (Mikhail et al. 1997; Clement 1999; Jacob et al. 1999). We consider analyst type — those with and

6. Although no evidence on the effectiveness of the NASD disclosure program exists to date, a similar disclosure program is being considered by the Internal Revenue Service. In particular, the agency would make public the names of tax attorneys and accountants under investigation for rules violations (Matthews 2006).
7. Investors can learn about an analyst's or other investment professional's background via the FINRA which maintains the BrokerCheck system, an online tool available for no charge at http://www.finra.org. When we began our study, the NASD maintained this database.

without background disclosures — as an additional factor impacting forecast accuracy.[8] Because an analyst's prior accuracy is related to her future forecast accuracy (Sinha, Brown, and Das 1997; Brown 2001), investors can benefit ex ante from knowledge of an analyst's type.

There are two reasons suggesting a link between background disclosures and analyst performance. First, as an industry regulator, the NASD has both the specialized knowledge and incentives to safeguard the brokerage industry's reputation and to avoid external intervention by providing informative disclosures to market participants. We maintain that the primary way for these disclosures to be viewed as informative is that they have performance implications. As one example of a disclosure position, the NASD reveals past brokerage industry terminations, and, empirically, analyst job turnover has been linked to poor earnings forecasting accuracy (Mikhail, Walther, and Willis 1999).

Second, the expertise literature considers domain knowledge to be an important component of superior task performance (e.g., Chase and Simon 1973; Chi, Glaser, and Rees 1982). Analysts with disclosures may have violated regulatory and industry guidelines due, in part, to their lack of training and the requisite knowledge of the boundaries of appropriate conduct in the financial service sector. Thus, finance training or knowledge deficiencies may contribute to their inferior performance relative to their peers.

On the other hand, there are two reasons suggesting the absence of a link between background disclosures and analyst performance. First, there is skepticism surrounding the NASD's conflict of interest in serving a dual role as promoter of its constituents' interests and as a regulator, resulting in challenges to the efficacy of the public disclosure program (Loomis 2002; Jamieson 2006; Siedle 2006; Davies 2007). Second, the disclosed analysts in the NASD database are still employed, so their earnings research outputs may *not* be less accurate.

While the accuracy of disclosed analysts' forecasts is an empirical issue, we expect industry incentives to safeguard reputation and avoid external intervention outweigh conflicts of interest.[9] Using earnings forecast accuracy to proxy for analyst earnings research performance, our first hypothesis is:

8. We use the term "analyst type" when referring to analysts to differentiate an *analyst* characteristic from other factors affecting forecast accuracy. For example, forecast characteristics (e.g., early versus late in the period), brokerage firm characteristics (e.g., small versus large), and followed-company characteristics (e.g., domestic versus multinational) may affect earnings forecast accuracy. Similarly, analyst characteristics — for example, team versus individual (Brown and Hugon 2009), male versus female (Green, Jegadeesh, and Tang 2008) — may affect earnings forecast accuracy.
9. Importantly, the benefits of industry reputation brought on by the perception of a well-functioning regulatory body accrue to all firms, while the short-horizon benefit of a particular firm wishing not to disclose an analyst transgression accrues primarily idiosyncratically. We reason that organizations have incentives to act in the interests of their larger base of constituents.

Hypothesis 1. *Analysts with background disclosures forecast earnings relatively less accurately than do analysts without background disclosures.*

Market credibility of earnings forecast revisions

Prior research on analyst earnings forecast revisions reveals that forecast revisions are price informative in the sense that they are associated with short-window market reactions centered on forecast revisions (e.g., Givoly and Lakonishok 1979; Imhoff and Lobo 1984; Stickel 1992; Park and Stice 2000; Clement and Tse 2003; Gleason and Lee 2003; Bonner et al. 2003; Bonner et al. 2007). Consistent with intuition, research has also established that past forecast performance is valued by the market and is an important predictor of future performance (Brown 2001). However, because earnings forecast accuracy is an ex post realization, contemporaneous factors add incrementally to determining the accuracy of an analyst's current earnings forecast (see, e.g., Clement 1999; Jacob et al. 1999; Clement and Tse 2003; Bonner et al. 2003).[10]

The market credibility of an earnings forecast, proxied via a short-window market reaction, can be viewed as a summary of the assortment of factors associated with ex post accuracy. Consistent with the market pricing analyst traits associated with accuracy, Stickel (1992) shows that the market pays more attention to earnings forecast revisions by members of the *Institutional Investor* All-American Research Team.[11]

We posit that market participants will recognize either the credibility attributes of analysts or reasonable proxies of these attributes and weight their earnings expectations accordingly. Because disclosed analysts have instances in their backgrounds revealing less credible patterns of behavior, our second hypothesis relates to the market perception of their research.

Hypothesis 2. *The short-window market reaction to disclosed analysts' earnings forecast revisions is weaker than that to nondisclosed analysts' revisions.*

Differential reliance on analyst disclosures conditional on investor sophistication

In the market reaction analysis, we examine the potential benefits of using background disclosures for investors who are uninformed as to an analyst's type. More specifically, we ask the question: Can uninformed investors gain the knowledge of informed investors on a dimension of analyst credibility

10. For example, analysts with a high level of past performance may exhibit deteriorating performance if their workloads increase.
11. Stickel (1992) also shows that earnings forecasts by members of the *Institutional Investor* All-American Research Team are more accurate than those of nonmembers.

through utilizing these public disclosures? We expect more sophisticated investors to pay less attention to disclosed analysts' earnings research than do less sophisticated investors.

One of the important differences between more and less sophisticated investors is the fiduciary responsibility and related legal accountability that sophisticated investors such as institutions assume (Del Guercio 1996). Prior research concludes that sophisticated investors consider the legal implications of their responsibilities when making investment choices (Del Guercio 1996; Badrinath, Gay, and Kale 1989; Gompers and Metrick 2001). Therefore, we expect that sophisticated investors will be relatively less likely to rely on earnings forecasts by analysts with public disclosures of questionable conduct. Enhancing their ability to recognize these credibility attributes, sophisticated investors are more proficient at evaluating relevant dimensions of brokerage research credibility (Bonner et al. 2003; Malmendier and Shanthikumar 2004; Cowen, Groysberg, and Healy 2006). This reasoning leads to our third hypothesis:

Hypothesis 3. *More sophisticated investors rely relatively less on disclosed analysts' earnings forecast revisions than do less sophisticated investors.*

3. Specification of empirical tests

Earnings forecast accuracy performance

We designate analysts as disclosed if they have one or more of the eight types of disclosure events in the NASD database as described in the Appendix. Due to variation in earnings forecasting difficulty arising from interfirm and temporal characteristics, we examine the performance of disclosed analysts by matching them with nondisclosed analysts by firm-quarter.[12] Because we focus on the incremental benefits of disclosure information, we control for other known determinants of forecast accuracy. Based on prior literature (Crichfield et al. 1978; O'Brien 1988; Stickel 1995; Mikhail et al. 1997; Mikhail et al. 1999; Clement 1999; Jacob et al. 1999; Clement and Tse 2003), we control for forecast horizon, forecast frequency, days since the last forecast, number of firms followed, broker size, experience, job turnover, and portfolio of companies followed.[13] In addition, because an analyst's past forecast accuracy is related to his current accuracy (Sinha et al. 1997; Brown and Mohammad 2010), we include past accuracy

12. All forecasts are made conditional upon knowledge of the same number of interim quarterly earnings announcements. We rely on quarterly rather than annual observations because accuracy models using annual observations and controlling for forecast horizon linearly do not adequately address differential information available to analysts who forecast immediately prior to versus immediately after interim quarterly earnings announcements.
13. Adding analyst-specific effects (Jacob et al. 1999) yields inferentially similar results.

in an extended model to see if past accuracy subsumes the information content of the disclosure events. Following Clement and Tse (2003), we use a relative measure for accuracy and the control variables and evaluate an analyst's earnings forecast accuracy by estimating the following model:

$$ACCUR_{i,j,t} = \alpha_0 + \alpha_1 DISC_{i,j,t} + \Sigma_k \lambda_k \, Forecast\ Controls_{i,j,t} + \varepsilon_{i,j,t} \qquad (1).$$

The model variables are defined as follows:

$ACCUR_{i,j,t}$ = Forecast accuracy, calculated as the maximum absolute forecast error for analysts who follow firm *j* in quarter *t* minus the absolute forecast error of analyst *i* following firm *j* in quarter *t*, with this difference scaled by the range of absolute forecast errors for all analysts following firm *j* in quarter *t*. The forecast error is the analyst's earnings estimate minus the actual earnings. Higher (lower) values of *ACCUR* correspond to better (worse) forecasting accuracy.

$DISC_{i,j,t}$ = Disclosed analyst, an indicator variable equal to 1 for analysts with background disclosures in the NASD database and 0 for analysts without background disclosures in the NASD database.

Forecast Controls

$FCAGE_{i,j,t}$ = Forecast age, calculated as the days from the forecast date to the earnings announcement date for analyst *i* following firm *j* in quarter *t* minus the minimum forecast horizon for all analysts who follow firm *j* in quarter *t*, with this difference scaled by the range of forecast horizons for analysts following firm *j* in quarter *t*.

$FCFREQ_{i,j,t}$ = Forecast frequency, calculated as the number of firm *j* forecasts made by analyst *i* following firm *j* in quarter *t* minus the minimum number of firm *j* forecasts for analysts following firm *j* in quarter *t*, with this difference scaled by the range of the number of firm *j* forecasts issued by analysts following firm *j* in quarter *t*.

$DAYS_{i,j,t}$ = Number of days between analyst *i*'s forecast of firm *j*'s earnings in quarter *t* and the most recent preceding forecast of firm *j*'s earnings by any analyst, minus the minimum number of days between two adjacent forecasts of firm *j*'s earnings by any two analysts in quarter *t*, with this difference scaled by the range of days between two adjacent forecasts of firm *j*'s earnings in quarter *t*.

$NOFIRM_{i,j,t}$ = Number of firms followed, calculated as the number of companies followed by analyst *i* following firm *j* in quarter *t* minus the minimum number of companies followed by analysts who follow firm *j* in quarter *t*, with this difference scaled by the range in the number of companies followed by analysts following firm *j* in quarter *t*.

$BSIZE_{i,j,t}$ = Brokerage size, calculated as the number of analysts employed by the brokerage house employing analyst i following firm j in quarter t minus the minimum number of analysts employed by a brokerage house for analysts following firm j in quarter t, with this difference scaled by the range of brokerage size for analysts following firm j in quarter t.

$EXP_{i,j,t}$ = Forecasting experience, calculated as the number of prior forecasting quarters for analyst i following firm j minus the minimum number of prior forecasting quarters for analysts following firm j in quarter t, with this difference scaled by the range of prior forecasting quarters for analysts following firm j in quarter t.

$TURN_{i,j,t}$ = Analyst turnover, calculated as an indicator variable equal to 1 if analyst i changes brokerage houses during the year or leaves the I/B/E/S database and does not reappear by the end of the sample period and equal to 0 otherwise, minus the minimum turnover for analysts following firm j in quarter t, with this difference scaled by the range of turnover for analysts following firm j in quarter t.

$PORT_{i,j,t}$ = Analyst portfolio turnover, calculated as the annual percentage change in firms followed by analyst i in quarter t minus the minimum change for analysts following firm j in quarter t, with this difference scaled by the range of change for analysts following firm j in quarter t.

$ACCPAST_{i,j,t}$ = Past forecast accuracy, calculated as *ACCUR* above except that an analyst's prior period forecasting accuracy is measured as the median of the age-adjusted forecast errors across all firms forecasted during the prior four quarters, where the age-adjusted errors are the residuals from firm-quarter-specific regressions of the absolute forecast error on the forecast age.

$\varepsilon_{i,j,t}$ = Error term.

We use (1) to evaluate Hypothesis 1 that disclosed analysts forecast relatively less accurately than do nondisclosed analysts.[14] A positive (negative) coefficient on the *DISC* dummy variable indicates higher (lower) quarterly earnings forecast accuracy for disclosed analysts relative to that of the matched sample of nondisclosed analysts, and Hypothesis 1 predicts the coefficient estimate on *DISC* to be negative.

Market credibility: Short-window reaction to earnings revisions

We examine the market credibility of disclosed analysts' earnings forecasts by comparing the market reaction to disclosed analysts' earnings forecast revisions to those of nondisclosed analysts for a given firm-quarter. When

14. We supplement this model with several sensitivity analyses, including evaluating nonlinear proxies for brokerage firm status, alternative experience measures such as firm-specific and industry experience, and alternative measure of lagged accuracy.

computing earnings forecast revisions, we use the analyst's most recent prior forecast for firm *j* in quarter *t* as the benchmark because it is a better proxy of investors' earnings expectations for the particular analyst-quarter than the prior consensus forecast (Imhoff and Lobo 1984; Stickel 1991; Gleason and Lee 2003). We use the following model to examine the market reaction:

$$\begin{aligned} CAR_{i,j,t} = \beta_0 + \beta_1 REVP_{i,j,t} + \beta_2 DISC_{i,j,t} + \beta_3 REVP_{i,j,t}{}^* DISC_{i,j,t} \\ + \Sigma_m \phi_m Controls_{i,j,t} + \Sigma_n \gamma_n REVP_{i,j,t}{}^* Controls_{i,j,t} + \omega_{i,j,t} \end{aligned} \quad (2).$$

The variables not previously defined are:

$CAR_{i,j,t}$	=	The three-day cumulative market-adjusted stock returns of company *j* surrounding analyst *i*'s forecast revision in quarter *t*.[15]
$REVP_{i,j,t}$	=	Analyst *i*'s price-deflated earnings forecast revision for firm *j* in quarter *t*.

Firm Controls

$BM_{j,t}$	=	Beginning of the quarter book-to-market ratio defined as COMPUSTAT items Data59/ (Data14 x Data61).
$SIZE_{j,t}$	=	Beginning of the quarter firm size defined as ln (Data14 x Data61).
$BETA_{j,t}$	=	Beta obtained from a firm-specific regression of the firm's daily return on the value-weighted market index daily return using the 100 trading days ending 10 days before the forecast revision date.
$FOLLOW_{j,t}$	=	The number of independent analysts following firm *j* in quarter *t*.
$\omega_{i,j,t}$	=	Error term.

We use (2) to evaluate Hypothesis 2. A significant positive (negative) coefficient for the interaction term *REVP*DISC* indicates investors react relatively more (less) to forecast revisions by disclosed analysts than to those of the matched sample of nondisclosed analysts. Hypothesis 2 predicts that the coefficient for the interaction term *REVP*DISC* will be negative.

Differential reliance on disclosed analysts' revisions conditional on investor sophistication

More sophisticated investors, such as institutions, are better informed and trade in larger order sizes than do less sophisticated investors (e.g., Lee 1992; Lee and Radhakrishna 2000; Bhattacharya 2001; De Franco, Lu, and Vasvari 2007). Consistent with Lee (1992) and more current applications

15. The market adjustment is value weighted; however, substituting an equal-weighted adjustment or a five-day accumulation window for both value-weighted and equal-weighted adjustments yields inferentially similar results.

(e.g., De Franco et al. 2007) our investor classification is based on the dollar value of each intraday transaction, where trades greater (less) than \$30,000 (\$7,000) are considered large (small).[16] The transaction data are obtained from the NYSE TAQ intraday database. We measure abnormal trading volume measure, *AVOLUME*, following Mikhail et al. (2007), and define the year as the calendar year.[17] To evaluate how different investor segments vary their reliance on disclosures, we condition the abnormal volume for each trade segment on our variable of interest, *DISC*, and forecast and firm control variables consistent with the preceding market model. The model is specified as follows:

$$\begin{aligned} AVOLUME^{k}_{\mathrm{i,j,t}} = {} & \delta^k_0 + \delta^k_1 REVP_{i,j,t} + \delta^k_2 DISC_{i,j,t} + \delta^k_3 REVP_{i,j,t}{}^{*} DISC_{i,j,t} \\ & + \Sigma_m \phi^k_m Controls_{i,j,t} + \Sigma_n \gamma^k_n REVP_{i,j,t}{}^{*} Controls_{j,t} + \xi^k_{\mathrm{i,j,t}} \end{aligned} \quad (3).$$

The variables not previously defined are as follows:

$AVOLUME^{k}_{\mathrm{i,j,t}}$ = Abnormal volume, calculated as the dollar trading volume for firm *j* in investor group *k* during the three-day window centered on analyst *i*'s earnings forecast revision minus the average dollar trading volume for firm *j* in investor group *k* during three-day, nonoverlapping windows during the year, scaled by the average dollar trading volume for firm *j* in investor group *k* during three-day, nonoverlapping windows during the year.

$\xi^k_{\mathrm{i,j,t}}$ = Error term.

We estimate (3) separately for small and large traders, represented by the superscript *k*, using a seemingly unrelated regression (SUR) approach to test for differences between coefficients across the two separate models. In both the large and small trade size models, the estimated coefficient on the term *REVP* indicates the response to nondisclosed analysts' revisions after controlling for the remaining factors. *REVP*DISC* indicates whether the unexpected trade size reaction to forecast revisions varies when conditioned on the disclosed analyst indicator, and the sum of *REVP* and *REVP*DISC* indicates the response to disclosed analysts' revisions. Evidence consistent with Hypothesis 3 would include a greater negative market reaction to disclosed analysts' revisions for more versus less sophisticated investors, as well as a negative incremental reaction to disclosed analysts. The latter result is necessary to ensure differentiation between reactions to nondisclosed versus disclosed analysts.

16. Following this line of literature, we also consider alternative dollar-based and volume-based classifications to identify large and small traders.
17. In untabulated results, we obtain similar evidence when we define the year as 125 trading days before and after an analyst forecast revision date.

4. Sample selection and results

Sample selection

Table 1 reports the sample selection procedures. Our sample selection begins with quarterly forecast observations in the I/B/E/S database during the period 1990–2005. We use 1990 as a starting point because in earlier years this database suffered a time lag in recording forecasts (Keane and Runkle 1998). We apply screens for missing data and omit both stale forecasts (those greater than 90 days in age) and anonymous analysts (those with 00000 codes). Consistent with prior research (O'Brien 1991; Clement 1999; Jacob et al. 1999; Mikhail et al. 1999), we use the last earnings forecast (the most recent) issued by an analyst for each firm-quarter combination, retaining 971,282 quarterly earnings forecasts.

We intersect the quarterly forecast sample with our hand-collected database of analyst disclosure events. Several steps are required in order to construct this database. Analysts are uniquely identified only by an analyst code in the I/B/E/S earnings forecast database; therefore, to retrieve their names, we utilize a broker translation file which contains analyst codes along with their first initials and last names. Because the broker translation file does not contain a complete first name, we supplement it with Nelson's Directory of Investment Services. For remaining ambiguous or missing analyst first names we use Google and search the string: *last_name* and *brokerage_house_name* and *analyst*. We next use our completed file of analyst names (those that can be linked to analyst codes) to query the NASD database. Querying the database requires manually inputting each analyst's first name, last name, and brokerage house name into relevant search fields and, in the event of a background disclosure event, retrieving a research report.

To control for known disclosures, we include only analysts who can be identified unambiguously in the NASD database. There are three possible results of a search in the NASD database based on our constructed name file: (1) matching the analyst name in our file to an observation in the NASD database and finding no disclosure event, (2) matching the analyst name in our file to an observation in the NASD database and finding a disclosure event, and (3) an inability to retrieve the analyst's name in our file from the NASD database. To mitigate noise in our sample arising from analysts with past disclosure events that are not in the NASD database, we retain only the first two search results. This yields 3,055 analysts without and 91 analysts with disclosure events.

After intersecting the I/B/E/S sample with the hand-collected database of NASD analyst disclosure events, our sample consists of 642,780 firm-quarters. Because descriptive analyses reveal a number of differences between the financial characteristics of firms that disclosed analysts cover relative to those not covered, we limit our sample to firms covered by at least one disclosed and one nondisclosed analyst, enabling us to control for the firm itself. This requirement reduces our sample to 118,339 firm-quarters.

TABLE 1
Sample selection

	Forecast observations (Unique analysts)		
	Total	Disclosed analysts	Nondisclosed analysts
I/B/E/S quarterly earnings forecasts, 1990–2005	1,610,177 (13,556)	—	—
Sample after omitting missing data, stale forecasts, etc.	1,308,948 (12,917)	—	—
Sample limited to one forecast (most recent) per analyst in a firm-quarter	971,282 (12,917)	—	—
Intersection of earnings forecasts and NASD coding (the NASD coding contains 3,146 total, 91 disclosed analysts, and 3,055 non-disclosed analysts)	642,780 (3,084)	22,649 (89)	620,131 (2,995)
To control for firm and temporal characteristics, sample limited to firm-quarters followed by at least one disclosed and nondisclosed analyst	118,339 (2,346)	19,356 (88)	99,043 (2,258)
Addition of forecasting control variables	68,807 (1,936)	11,429 (79)	57,378 (1,857)
Earnings forecasting sample:			
To allow tests from an ex ante uninformed investor perspective, only firm-quarters associated with a disclosed analyst postincident date or a nondisclosed analyst forecasting in the same firm-quarter are included	52,299 (1,812)	7,536 (73)	44,763 (1,739)
Market reaction to forecast revisions sample:			
Earnings forecast revisions requiring a prior forecast by the same analyst in the same firm-quarter CRSP and COMPUSTAT data requirements	16,737 (1,549)	1,902 (65)	14,835 (1,450)
Trade size response to forecast revisions sample:			
Earnings forecast revisions. CRSP, COMPUSTAT, and TAQ/ISSM data requirements	16,433 (1,506)	1,835 (64)	14,598 (1,442)

Because we focus on benefits of disclosures to those investors who are uninformed as to an analyst's background, we restrict our sample to analysts' forecasts after the initial date of the disclosed incident, resulting in 52,299 firm-quarters. We use this sample for our primary earnings forecast accuracy tests, but to compute the market reaction and trade size tests for the earnings forecast revisions, we also require prior earnings forecast (to compute the revision) and necessary CRSP, COMPUSTAT, and TAQ/ISSM (for the trade size analysis) data. Our samples for the market reaction and trade size tests consist of 16,737 and 16,433 observations, respectively.

Descriptive statistics

Table 2 provides descriptive statistics on disclosed analysts and disclosure frequency by event type. The number of disclosure events ($n = 116$) exceeds the number of analysts with disclosures ($n = 91$) because several analysts have multiple disclosure events. The disclosed analysts have the following breakdown of disclosed incidents: 27 criminal actions (23.2 percent), 26 customer complaints (22.4 percent), 18 bankruptcies (15.5 percent), 17 regulatory actions (14.7 percent), 17 terminations (14.7 percent), 5 civil judicial actions (4.3 percent), 3 investigations (2.6 percent), and 3 judgments/liens (2.6 percent). The Appendix provides an example of each of the eight disclosure categories.

Table 3 compares firm characteristics between firms followed and those not followed by disclosed analysts. Relative to nondisclosed analysts, disclosed analysts follow firms with greater sales revenue and earnings but smaller profitability as measured by return on assets.[18] Disclosed analysts follow larger firms (as measured by market value), firms with lower book-to-market ratios, and firms with less financial leverage as measured by the debt-to-equity ratio. All samples in the remaining tables are based on firm-quarters with at least one disclosed and one nondisclosed analyst.

Table 4 compares disclosed to nondisclosed analysts with respect to their forecast sample characteristics. Relying on firm-quarter relative measures, descriptive analyses of forecasting characteristics indicate that, compared to nondisclosed analysts, disclosed analysts forecast less accurately in the current and prior year's same quarter, later in the quarter, and more frequently.[19] Disclosed analysts make their forecasts with greater delay relative to other analysts' prior forecasts, follow more firms, have

18. These statements are based on Wilcoxon z-tests and two-tailed p-values; differences based on t-tests and two-tailed p-values are consistent except for the return on assets result which is insignificant based on a t-test.

19. These statements are based on Wilcoxon z-tests and two-tailed p-values; differences based on t-tests and two-tailed p-values are consistent except for the insignificance of portfolio turnover and past accuracy.

TABLE 2
Frequency of analyst background disclosures by disclosure event type

Disclosure event	Frequency	% of Total
Criminal actions	27	23.2
Customer complaints	26	22.4
Bankruptcies	18	15.5
Regulatory actions	17	14.7
Terminations	17	14.7
Civil judicial actions	5	4.3
Investigations	3	2.6
Judgments/liens	3	2.6
Total	116	100%

Notes:

This table provides descriptive statistics on analysts in the hand-collected NASD sample with at least one disclosure event in the NASD database. The number of total disclosure events ($n = 116$) exceeds the number of unique analysts ($n = 91$) because some analysts have multiple disclosure events. A disclosure event is represented by any of the following categories: *Criminal actions*, which include all felony charges and convictions and specified investment-related misdemeanor charges and convictions; *Customer complaints*, which include written consumer-initiated complaints reported within the past 24 months alleging sales practice violations and damages of $5,000 or more, or written consumer-initiated complaints reported within the past 24 months alleging forgery, theft, misappropriation or conversion of funds or securities, or consumer-initiated complaints alleging sales practice violations that settled for $10,000 or more; *Bankruptcies*, which include personal bankruptcies; *Regulatory actions*, which include actions taken by the SEC, Commodity Futures Trading Commission, other federal regulatory agencies, states, self-regulatory organizations, or foreign financial regulatory authorities resulting in a violation and/or incident; *Terminations*, which include employment terminations after allegations of a violation of investment-related statutes, regulations, rules, or industry standards of misconduct, fraud or wrongful taking of property, or failure to supervise in connection with investment-related activity; *Civil judicial actions* such as injunctions entered in connection with investment-related activities; *Investigations*, pending investigations and regulatory proceedings that could result in a regulatory action; and *Judgments/liens*, which include unsatisfied judgments and liens. Specific examples of disclosure events are provided in the Appendix.

more experience, have greater job turnover, and display more consistency in the specific firms they follow. Past research on analyst forecast accuracy has shown that these forecast characteristics are related to forecast

TABLE 3
Analysts with disclosure events: Financial characteristics of their followed firms

Firm-quarter financial characteristics

Variables:	Firms followed by analysts with disclosures $n = 12{,}936$ Mean	Median	Firms not followed by analysts with disclosures $n = 122{,}634$ Mean	Median	Test of differences *T*-test	Wilcoxon *Z*
Sales	1259	312	581	124	34.09***	50.54***
Earnings	81.03	17.44	33.97	5.71	28.17***	35.52***
ROE	0.012	0.028	0.012	0.029	0.02	−1.01
ROA	0.004	0.008	0.005	0.009	−1.47	−2.17**
Size	7603	2045	2915	611	37.13***	70.79***
BM	0.494	0.429	0.539	0.458	−14.15***	−11.85***
Debt to Equity	0.683	0.352	0.793	0.354	−10.00***	−2.48*

Notes:

This table reports a comparison of the financial characteristics of *Firms Followed by Analysts with Disclosures* and *Firms Not Followed by Analysts with Disclosures* during our sample period 1990–2005. The table reports means (medians) for all firm-quarter financial variables in the sample representing the intersection of analysts identified in the NASD database, I/B/E/S quarterly earnings forecasts, and COMPUSTAT required data on the reported variables. The variables are defined by the reported COMPUSTAT data items: Sales = Data2, Earnings = Data8, Return on equity (ROE) = Data8/Data59, Return on assets (ROA) = Data8/Data44, Size = Data14 x Data61, Book-to-market ratio (BM) = Data59/(Data14 x Data61), and Debt to Equity = Data51/Data59. The variables are winsorized at the 1st and 99th percentiles.

* Two-tailed $p < 0.10$; ** two-tailed $p < 0.05$; *** two-tailed $p < 0.01$.

accuracy and market reaction so we include them in our accuracy and market tests.

Table 5 reports Pearson and Spearman correlations for standardized quarterly accuracy and forecasting control variables. Consistent with our first hypothesis, firm-quarter relative forecasting accuracy is negatively associated with the disclosed analyst indicator variable.[20] Consistent with past research, current forecasting accuracy is positively related to past

20. These statements are based on Spearman correlations and two-tailed p-values; results based on Pearson correlations and two-tailed p-values are consistent.

TABLE 4
Analysts with and without disclosure events: Comparison of forecast sample characteristics

Variables	Analysts with disclosures $n = 7{,}536$		Analysts without disclosures $n = 44{,}763$		Test of differences	
	Mean	Median	Mean	Median	*T*-test	Wilcoxon *Z*
ACCUR	0.621	0.700	0.645	0.716	−5.30***	−2.03**
ACCPAST	0.600	0.601	0.603	0.626	−1.15	−4.51***
FCAGE	0.430	0.326	0.473	0.386	−8.80***	−12.56***
FCFREQ	0.396	0.500	0.343	0.333	11.02***	11.78***
DAYS	0.303	0.088	0.208	0.028	20.56***	23.01***
NOFIRM	0.558	0.545	0.422	0.381	29.58***	28.96***
BSIZE	0.450	0.396	0.450	0.412	−0.07	−1.22
EXP	0.501	0.461	0.379	0.286	26.68***	24.71***
TURN	0.321	0.000	0.270	0.000	11.16***	14.53***
PORT	0.374	0.250	0.374	0.288	−0.03	−3.15***

Notes:

This table reports the means and medians for the standardized descriptive statistics for the forecast sample. The variables are defined as follows:

ACCUR = Forecast accuracy, calculated as the maximum absolute forecast error for analysts who follow firm *j* in quarter *t* minus the absolute forecast error of analyst *i* following firm *j* in quarter *t*, with this difference scaled by the range of absolute forecast errors for all analysts following firm *j* in quarter *t*. The forecast error is the analyst's earnings estimate minus the actual earnings. Higher (lower) values of *ACCUR* correspond to better (worse) forecasting accuracy.

ACCPAST = Past forecast accuracy, calculated as *ACCUR* above except that an analyst's prior period forecasting accuracy is measured as the median of the age-adjusted forecast errors across all firms forecasted during the prior four quarters, where the age-adjusted errors are the residuals from firm-quarter specific regressions of the absolute forecast error on the forecast age.

FCAGE = Forecast age, calculated as the days from the forecast date to the earnings announcement date for analyst *i* following firm *j* in quarter *t* minus the minimum forecast horizon for all analysts who follow firm *j* in quarter *t*, with this difference scaled by the range of forecast horizons for analysts following firm *j* in quarter *t*.

(The table is continued on the next page.)

TABLE 4 (Continued)

FCFREQ	= Forecast frequency, calculated as the number of firm *j* forecasts made by analyst *i* following firm *j* in quarter *t* minus the minimum number of firm *j* forecasts for analysts following firm *j* in quarter *t*, with this difference scaled by the range of the number of firm *j* forecasts issued by analysts following firm *j* in quarter *t*.
DAYS	= The number of days between analyst *i*'s forecast of firm *j*'s earnings in quarter *t* and the most recent preceding forecast of firm *j*'s earnings by any analyst, minus the minimum number of days between two adjacent forecasts of firm *j*'s earnings by any two analysts in quarter *t*, with this difference scaled by the range of days between two adjacent forecasts of firm *j*'s earnings in quarter *t*.
NOFIRM	= The number of firms followed, calculated as the number of companies followed by analyst *i* following firm *j* in quarter *t* minus the minimum number of companies followed by analysts who follow firm *j* in quarter *t*, with this difference scaled by the range in the number of companies followed by analysts following firm *j* in quarter *t*.
BSIZE	= Brokerage size, calculated as the number of analysts employed by the brokerage house employing analyst *i* following firm *j* in quarter *t* minus the minimum number of analysts employed by a brokerage house for analysts following firm *j* in quarter *t*, with this difference scaled by the range of brokerage size for analysts following firm *j* in quarter *t*.
EXP	= Forecasting experience, calculated as the number of prior forecasting quarters for analyst *i* following firm *j* minus the minimum number of prior forecasting quarters for analysts following firm *j* in quarter *t*, with this difference scaled by the range of prior forecasting quarters for analysts following firm *j* in quarter *t*.
TURN	= Analyst turnover, calculated as an indicator variable equal to 1 if analyst *i* changes brokerage houses during the year or leaves the I/B/E/S database and does not reappear by the end of the sample period and equal to 0 otherwise, minus the minimum turnover for analysts following firm *j* in quarter *t*, with this difference scaled by the range of turnover for analysts following firm *j* in quarter *t*.
PORT	= An analyst's portfolio turnover, calculated as the annual percentage change in firms followed by analyst *i* in quarter *t* minus the minimum change for analysts following firm *j* in quarter *t*, with this difference scaled by the range of change for analysts following firm *j* in quarter *t*.

forecasting accuracy, forecast frequency, experience, broker size, job turnover, and turnover in the portfolio of firms covered by an analyst. It is negatively related to forecast age and the days since the prior forecast of

TABLE 5

Quarterly forecast accuracy variables: Pearson and Spearman correlations ($n = 52{,}299$)

	ACCUR	*DISC*	*ACCPAST*	*FCAGE*	*FCFREQ*	*DAYS*	*NOFIRM*	*BSIZE*	*EXP*	*TURN*	*PORT*
ACCUR		−0.025	0.052	−0.181	0.089	−0.028	0.000	0.023	0.007	−0.035	−0.031
		(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(0.97)	(< 0.01)	(0.09)	(< 0.01)	(< 0.01)
DISC	−0.009		−0.005	−0.038	0.048	0.099	0.142	0.000	0.128	0.049	−0.000
	(0.04)		(0.28)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(0.94)	(< 0.01)	(< 0.01)	(0.98)
ACCPAST	0.050	−0.020		0.038	0.001	−0.043	0.021	0.088	0.049	−0.027	−0.072
	(< 0.01)	(< 0.01)		(< 0.01)	(0.75)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)
FCAGE	−0.167	−0.055	0.046		−0.402	−0.359	−0.050	−0.008	−0.055	−0.035	−0.001
	(< 0.01)	(< 0.01)	(< 0.01)		(< 0.01)	(< 0.01)	(< 0.01)	(0.06)	(< 0.01)	(< 0.01)	(0.07)
FCFREQ	0.094	0.052	0.000	−0.387		0.192	0.069	0.055	0.035	0.006	0.007
	(< 0.01)	(< 0.01)	(0.93)	(< 0.01)		(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(0.15)	(0.09)
DAYS	−0.009	0.101	−0.044	−0.436	0.209		0.064	−0.002	0.123	0.083	0.046
	(0.04)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)		(< 0.01)	(0.73)	(< 0.01)	(< 0.01)	(< 0.01)
NOFIRM	−0.007	0.127	0.013	−0.042	0.063	0.068		0.104	0.200	−0.090	−0.057
	(0.14)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)		(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)
BSIZE	0.022	−0.005	0.088	0.012	0.046	−0.024	0.113		0.121	−0.074	−0.066
	(< 0.01)	(0.22)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)		(< 0.01)	(< 0.01)	(< 0.01)
EXP	0.010	0.108	0.042	−0.040	0.020	0.156	0.208	0.124		−0.025	−0.282
	(0.02)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)		(< 0.01)	(< 0.01)
TURN	−0.023	0.064	−0.03	−0.066	0.024	0.095	−0.092	−0.076	−0.032		0.042
	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)		(< 0.01)
PORT	−0.014	−0.014	−0.048	−0.004	0.003	−0.008	−0.038	−0.069	−0.297	0.023	
	(< 0.01)	(< 0.01)	(< 0.01)	(0.42)	(0.52)	(0.06)	(< 0.01)	(< 0.01)	(< 0.01)	(< 0.01)	

Notes:

The Pearson (Spearman rank) correlations of the standardized variables are above (below) the diagonal. The sample consists of firm-quarters with at least one disclosed analyst and one nondisclosed analyst forecast. The two-tailed p-value is in parentheses below the correlation. *DISC* is an indicator variable equal to 1 for an analyst with an NASD disclosure event and equal to 0 for an analyst without a disclosure event in the NASD database. All other variables are defined in Table 4.

TABLE 6
Analysts with and without disclosure events: Earnings forecast accuracy

$ACCUR_{i,j,t} = \alpha_0 + \alpha_1 DISC_{i,j,t} + \Sigma_k \lambda_k Forecast\ Controls_{i,j,t} + \varepsilon_{i,j,t}$

		Forecast accuracy models	
Variable	Exp. sign	(1) n = 56,576	(2) n = 52,299
Intercept		0.754 (117.72) ***	0.717 (89.63) ***
DISC	−	−0.020 (−2.58) ***	−0.023 (−2.62)***
FCAGE	−	−0.177 (−32.85)***	−0.177 (−31.13)***
FCFREQ	+	0.022 (4.74)***	0.025 (5.01)***
DAYS	−	−0.106 (−19.85)***	−0.102 (−18.28)***
NOFIRM	−	−0.007 (−1.36)	−0.007 (−1.31)
BSIZE	+	0.016 (2.65)***	0.015 (2.42)**
EXP	+	0.003 (0.47)	0.002 (0.29)
TURN	−	−0.029 (−6.53)***	−0.029 (−6.24)***
PORT	−	−0.023 (−4.43)***	−0.026 (−4.64)***
ACCPAST	+		0.069 (12.80)***
Adj. R^2		4.54%***	5.14%***
F-statistic		198.67	198.71

Notes:

This table reports results from estimating (1) to evaluate the earnings forecast accuracy of disclosed (*DISC*) versus nondisclosed analysts during the sample period 1990–2005. The sample consists of only firm-quarters with at least one disclosed analyst and at least one nondisclosed analyst forecast. All variables are defined in the notes to Table 4. For each variable included in (1), the coefficient estimate is presented; the *t*-statistic is provided in parentheses below the estimated coefficient. The standard errors are adjusted for heteroskedasticity and intra-analyst error correlation (Rogers 1994).

* Two-tailed $p < 0.10$; ** two-tailed $p < 0.05$; *** two-tailed $p < 0.01$.

any analyst. Current forecasting accuracy is not significantly related to the number of firms covered.

Multivariate results

Earnings forecasting performance

Table 6 reports results from estimating (1) to evaluate the earnings forecast accuracy of disclosed analysts. The model 1 estimation reveals a negative and significant coefficient on the disclosed analyst variable, *DISC*, ($\alpha_1 = -0.020$, $t = -2.58$, $p < 0.01$), supporting our first hypothesis that disclosed analysts forecast earnings less accurately than do analysts without disclosures.[21] Because we posit that uninformed investors can benefit incrementally from the NASD analyst disclosures, we also examine a second model, model 2, which includes analysts' past forecast accuracy, *ACCPAST*. This extension allows us to test if the disclosures provide information beyond that already incorporated in past accuracy. Consistent with past research, the control variable *ACCPAST* is positive and significant; and germane to our first hypothesis, *DISC* remains negative and significant, (α_1 = −0.023, t = −2.62, p < 0.01). Interpreting the parameter estimate on *DISC*, the unique contribution of the disclosed variable is to lower the predicted relative accuracy by 2.3 percent.[22]

We make several efforts to enhance the robustness of our results. First, we examine if the model 2 results are sensitive to alternative constructions of the *ACCPAST* variable. Specifically, we substitute a firm-specific measure of accuracy for general accuracy and we obtain inferentially similar results. Because different measures of experience exist in the forecasting literature, we examine the robustness of using general forecasting experience as our experience control variable in models 1 and 2. In particular, we substitute both firm-specific forecasting experience and industry forecasting experience for general experience and obtain similar results. Because the literature has used number of industries followed as well as number of firms followed, we substitute the former for the latter in models 1 and 2, and obtain inferentially similar results. Jacob et al. (1999) show the relevance of analyst-specific effects when examining the determinants of relative forecast accuracy. To allow for the coexistence of analyst effects with a disclosure indicator variable, we eliminate one random pairing at a time, without replacement, of a disclosed and a nondisclosed analyst indicator and estimate n separate regressions, where n equals the number of disclosed analysts. The mean parameter estimate on *DISC* after introducing analyst specific effects remains negative and significant (α_1 = −0.060, t = −2.51, p < 0.01).

Relevant to interpreting this accuracy result, in supplemental analyses in section 5, we investigate whether the result is due to our disclosure variable capturing some persistent, unmeasured analyst characteristic versus the disclosure event per se. Based on several analyses, including examining disclosed analysts' forecasts in the predisclosure period, the impact of

21. In regression models, reported t-statistics are based on standard errors adjusted for heteroskedasticity and intra-analyst error correlation (Rogers 1994); however, our results are insensitive to alternative strategies for addressing heteroskedasticity and dependence of residuals, including White (1980) heteroskedasticity consistent t-statistics, a quantile regression (Koenker and Bassett 1978), and t-statistics based on Newey and West (1987) heteroskedasticity and autocorrelation consistent standard errors.

22. Because a relative accuracy measure lacks a direct economic interpretation, we also examine the mean absolute forecast errors of disclosed (0.074) versus other analysts (0.065), revealing an average forecast error 13.8 percent larger for disclosed analysts.

multiple disclosure events, and the effect of elapsed time since the disclosure event, we conclude our evidence is consistent with the disclosure event signaling a persistent analyst characteristic.

Earnings forecast revision credibility

Table 7 reports results from estimating (2) to evaluate short-window market reactions to disclosed versus nondisclosed analysts' forecast revisions.[23] We evaluate several variations of the market model including a base model excluding control variables (model 1), a model with only forecast control variables (model 2), a model with only firm control variables (model 3), and a model with both forecast and firm control variables (model 4). For brevity, we only report results based on model 4 in the text. Consistent with prior literature, the estimated coefficient on the scaled forecast revision, *REVP*, is positive and statistically significant (β_1 = 2.098, t = 2.63, $p < 0.01$), indicating that the short-window market reaction centered on the release of the revision is associated with the signed magnitude of the revision.[24] Consistent with our second hypothesis, the interaction term, *REVP*DISC*, is negative and significant (β_3 = −0.198, t = −2.92, $p < 0.01$), indicating that market participants pay less attention to the forecast revisions of analysts with disclosure events. Calculating the partial derivative of *REVP* with respect to *CAR* and using the unique estimates in the full model, β_1 and β_3, 2.098 and −0.198 respectively, we evaluate the differential effect of *DISC* by comparing results of *DISC* = 1 versus 0. Evaluating *DISC* = 1 yields a marginal effect of 1.900 (=2.098−0.198), while *DISC* = 0 yields a marginal effect of 2.098. Thus, the marginal effect of disclosure on the forecast revision coefficient is approximately 9.4 percent, which can be safely interpreted as being economically meaningful.

We make several efforts to enhance the robustness of this result. The tabulated results present a three-day return [−1, +1] where the market adjustment is value weighted; however, in sensitivity checks, we use an equal-weighted adjustment and utilize a five-day window [−2, +2] to accumulate returns (for combinations of both value-weighted and equal-weighted adjustments) with similar results. Prior literature has raised concerns that other concurrent disclosures or economic events occurring may affect price and cause analysts to revise their earnings forecasts (Chen, Francis, and Schipper 2005). To address this concern, we evaluate the single-day excess return [0, 0] with both equal-weighted and value-weighted

23. Across tables we winsorize at the 1 percent and 99 percent levels the continuous variables that are not otherwise standardized to avoid inferences based on extreme values/errors. As a sensitivity check, we omit both the winsorization procedure and the minimum price deflator constraint and obtain similar results.

24. Due to space limitations, we exclude *t*-values in this table.

TABLE 7
Analysts with and without disclosure events: Market reaction to the forecast revisions

$$CAR_{i,j,t} = \beta_0 + \beta_1 REVP_{i,j,t} + \beta_2 DISC_{i,,j,t} + \beta_3 REVP_{i,j,t}{}^{*} DISC_{i,j,t} + \Sigma_m \phi_m Controls_{i,j,t} + \Sigma_n \gamma_n REVP_{i,j,t}{}^{*} Controls_{j,t} + \omega_{i,j,t}$$

Variable	Expected sign	Market reaction models (1) Base $n = 17{,}894$	(2) Forecast control $n = 16{,}737$	(3) Firm control $n = 17{,}894$	(4) Full $n = 16{,}737$
Intercept		−0.007***	−0.013***	−0.018***	−0.026***
REVP	+	2.271***	2.212***	1.906**	2.098***
DISC		−0.001	−0.002	0.000	−0.001
*REVP*DISC*	−	−0.099***	−0.174***	−0.093***	−0.198***
Forecast Controls					
FAGE			0.010***		0.008***
*REVP*FAGE*	+		−0.181**		−0.162*
FREQ			0.004**		0.006***
*REVP*FREQ*	+		0.104		0.125
DAYS			0.011***		0.011***
*REVP*DAYS*	−		−0.116		−0.111
NFIRM			0.002		0.002
*REVP*NFIRM*	−		0.151**		0.183***
BSIZE			0.000		−0.001
*REVP*BSIZE*	+		−0.001		0.028
EXP			−0.002		−0.002
*REVP*EXP*	+		−0.066		−0.057
TURN			−0.001		−0.001
REVP TURN*			0.037		0.051
PORT			−0.003		−0.001
REVP PORT*			−0.055		−0.054
ACCPAST			0.001		0.001
*REVP*ACCPAST*	+		−0.109		−0.140
Firm Controls					
BM				−0.006**	−0.005**
*REVP*BM*	+			−0.731**	−0.934***
SIZE				0.002***	0.002***
*REVP*SIZE*	−			−0.027	−0.033
BETA				−0.005***	−0.005***
*REVP*BETA*	+			0.527***	0.528***
FOLLOW				−0.000	−0.000

(The table is continued on the next page.)

TABLE 7 (Continued)

Variable	Expected sign	Market reaction models (1) Base $n = 17{,}894$	(2) Forecast control $n = 16{,}737$	(3) Firm control $n = 17{,}894$	(4) Full $n = 16{,}737$
*REVP*FOLLOW*	+			0.000	−0.002
Adj. R^2		7.28%***	7.72%***	8.26%***	8.88%***
F-value		108.84	34.19	50.39	32.44

Notes:

This table reports results from estimating (2) to evaluate the market reaction to earnings forecast revisions by disclosed (*DISC*) versus nondisclosed analysts during the sample period 1990–2005. The sample consists of firm-quarters with at least one disclosed analyst and at least one nondisclosed analyst forecast. The dependent variable, *CAR*, is the three-day market-adjusted cumulative stock return of company *j* surrounding analyst *i*'s forecast revision at time *t*. *REVP* is analyst *i*'s price-deflated forecast revision for firm *j* at time *t*. The Forecast Control variables and *DISC* are defined in Tables 4 and 5, respectively. The Firm Control variables are as follows: *BM* is the beginning of the quarter book-to-market ratio defined as COMPUSTAT Data59/(Data14 x Data61); *SIZE* is the beginning of the quarter firm size defined as ln (Data14 x Data61); *BETA* is obtained from a firm-specific regression of the firm's daily return on the value-weighted market index daily return using the 100 trading days ending 10 days before the forecast revision date; *FOLLOW* is the number of independent analysts following firm *j* in quarter *t*. Across models, we calculate *t*-statistics based on standard errors adjusted for both heteroskedasticity and intra-analyst error correlation (Rogers 1994). *CAR* and *REVP* are winsorized at the 1st and 99th percentiles; the minimum price deflator for *REVP* is $1.

* Two-tailed $p < 0.10$; ** two-tailed $p < 0.05$; *** two-tailed $p < 0.01$.

adjustments and obtain results which are consistent with those of our original analysis.

Differential reliance on analyst disclosures

Following prior literature, for example, Bhattacharya (2001), De Franco et al. (2007), and Mikhail et al. (2007), we evaluate how investor segments varying in sophistication respond to forecast revisions by disclosed analysts. Consistent with the short-window market model, we relate abnormal volume for each trade segment to forecast revisions, our analyst disclosure variable, and controls. The abnormal volume, *AVOLUME*, is defined following Mikhail et al. 2007 as the dollar trading volume for firm *j* in investor group *k* during the three-day window centered on the analyst *i* earnings forecast revision minus the average dollar trading volume for firm j in investor group k during three-day, nonoverlapping windows during the year, scaled by the average dollar trading volume for firm j in investor group k during three-day, nonoverlapping windows during the year. We estimate (3) separately for small (≤ $7,000) and large (≥ $30,000) traders using a SUR approach, which allows testing for differences between the coefficients of interest across the two separate models.

In both the large and small trade size models, the estimated coefficient on the term *REVP* indicates the response to nondisclosed analysts' revisions (after controlling for the remaining factors). *REVP*DISC* indicates whether the unexpected trade size reaction to forecast revisions varies when conditioned on the disclosed analyst indicator, and the sum of *REVP* and *REVP*DISC* indicates the response to disclosed analysts' revisions.

Table 8 reports a stronger large trader response to forecast revisions by nondisclosed analysts after controlling for other factors, *REVP*, ($\delta_1^{LG} = 31.575 > \delta_1^{SM} = 12.935$, $F = 289.07$, $p < 0.01$). Evaluating the sums of *REVP* and *REVP*DISC*, there is also a stronger large trader response to revisions by disclosed analysts, *REVP* + *REVP*DISC* ($\delta_1^{LG} + \delta_3^{LG} > + \delta_3^{SM}$, $31.575 - 0.851 = 30.724 > 12.935 - 0.536 = 12.399$, $F = 306.06$, $p < 0.01$). However, because neither trader reacts incrementally differently to disclosed analysts, *REVP*DISC*, ($\delta_3^{LG} = -0.851$, $t = -1.02$, $p > 0.10$; $\delta_3^{SM} = -0.536$, $t = -1.49$, $p > 0.10$), the totality of the results merely reveals that large traders react more strongly to analysts in general. This evidence does not support our third hypothesis that more sophisticated investors rely relatively less on disclosed analysts' earnings forecast revisions than do less sophisticated investors.

5. Supplemental analyses

Evaluating explanations for the accuracy and market reaction results: Some evidence from preincident analyses

Because our study seeks to determine the benefits of using disclosures for ex ante uninformed investors, we require disclosed analysts to be in our sample after the date of the related incident — that is, when an uninformed investor could benefit from seeking background knowledge of the analyst of interest. However, an examination of disclosed analysts' forecasting patterns in the preincident period can shed light on the underlying explanation of the postincident results. For example, if disclosed analysts exhibit inferior forecasting accuracy only in the postincident period, the disclosure incident may have driven the accuracy effect. That is, disclosed analysts' inferior accuracy may be due to fewer intra-firm resources available or deterioration of communication channels with managers of the companies they follow. In contrast, if analysts are similarly inaccurate in the preincident period, this is consistent with the disclosure signal capturing a persistent unmeasured analyst characteristic that is associated with analyst performance or ability.

TABLE 8
Analysts with and without disclosure events: Abnormal trading volume in response to forecast revisions

$$AVOLUME^k_{\mathrm{i,j,t}} = \delta^k_0 + \delta^k_1 REVP_{i,j,t} + \delta^k_2 DISC_{i,j,t} + \delta^k_3 REVP_{i,j,t}{}^* DISC_{i,j,t} + \Sigma_m \phi^k_m Controls_{i,j,t} + \Sigma_n \gamma^k_n REVP_{i,j,t}{}^* Controls_{j,t} + \xi^k_{\mathrm{i,j,t}}$$

Variable	Exp. sign	Trader type: Large	Trader type: Small
Intercept		0.502 (34.30)***	0.265 (42.16)***
REVP	+	31.575 (22.01)***	12.935 (21.00)***
DISC		−0.0585 (−1.41)	0.028 (1.55)
*REVP*DISC*	−	−0.851 (−1.02)	−0.536 (−1.49)
		Coefficient estimates of forecast and firm control variables are suppressed	
Adj. R^2		2.70%***	2.46%***
F (1): *Intercept*		449.83***	
F (1): *REVP*		289.07***	
F (1): *DISC*		7.36***	
F (1): *REVP*DISC*		0.24	
F (1): *REVP* + *REVP*DISC*		306.06***	

Notes:

This table reports results from estimating (3) to evaluate the abnormal trading volume response to earnings forecast revisions by disclosed (*DISC*) versus non-disclosed analysts during the sample period 1990–2005. The sample consists of firm-quarters with at least one disclosed analyst and at least one non-disclosed analyst forecast. The Forecast Control variables and *DISC* are defined in Tables 4 and 5, respectively. The Firm Control variables are as follows: *BM* is the beginning of the quarter book-to-market ratio defined as COMPUSTAT Data59/(Data14 x Data61); *SIZE* is the beginning of the quarter firm size defined as ln (Data14 x Data61); *BETA* is obtained from a firm-specific regression of the firm's daily return on the value-weighted market index daily return using the 100 trading days ending 10 days before the forecast revision date; *FOLLOW* is the number of independent analysts following firm *j* in quarter *t*. Across models, we calculate *t*-statistics based on standard errors adjusted for both heteroskedasticity and intra-analyst error correlation (Rogers 1994). *AVOLUME* is abnormal volume, calculated as the dollar trading volume for firm *j* in investor group *k* during the three-day window centered on analyst *i*'s earnings forecast revision minus the average dollar trading volume for firm *j* in investor group *k* during three-day, nonoverlapping windows during the year, scaled by the average dollar trading volume for firm *j* in investor group *k* during three-day, nonoverlapping windows during the year. *REVP* is analyst *i*'s price-deflated earnings forecast revision for firm *j* in quarter *t*. *AVOLUME* and *REVP* are winsorized at the 1st and 99th percentiles; the minimum price deflator for *REVP* is $1.

* Two-tailed $p < 0.10$; ** two-tailed $p < 0.05$; *** two-tailed $p < 0.01$.

Regarding the market response, if the weaker market reaction appears only in the postincident period, this suggests that the market is semi-strong efficient by pricing the public disclosure of the disclosed incident. However, if the market discount exists both in the pre and post periods, this suggests strong-form efficiency where the market discounted its responses to disclosed analysts' earnings forecast revisions even before the disclosure incident.

To evaluate which explanation is more descriptively valid, we examine the preincident period sample. Untabulated results from estimating (1) on the preincident sample reveal a negative and significant coefficient estimate on *DISC*, (α_1 = −0.020, t = −1.70, $p < 0.10$). Untabulated results from estimating (2) to evaluate the short-window market reaction based on the pre-event period reveals an insignificant parameter estimate *REVP*DISC*, (β_3 = 0.156, t = 0.48, $p > 0.10$). We interpret the accuracy result as indicating that the disclosure variable picks up a persistent analyst characteristic. The lack of a weaker market reaction in the pre-event period suggests that the disclosure signal is informative to capital markets.[25]

Analysts with multiple disclosure events

To address the possibility that analysts with multiple disclosure events exhibit either differential forecasting performance or market responses to their research, we create an indicator variable *MDISC*, equal to 1 for analysts with multiple disclosure events and 0 otherwise. Untabulated results from estimating (1) to evaluate forecast accuracy with the addition of *MDISC* reveals a negative and significant coefficient on our variable of interest, *DISC* (α_1 = −0.025, t = −2.39, $p < 0.05$), with no incremental effect related to analysts with multiple disclosure events, *MDISC* (α_2 = 0.012, t = 0.84, $p > 0.10$). Untabulated results from estimating (2) to evaluate the short-window market reaction with the addition *MDISC* reveals a negative and significant coefficient on our variable of interest, *REVP*DISC* (β_3 = −0.192, t = −2.90, $p < 0.01$), with no incremental effect related to analysts with multiple disclosure events, *REVP*MDISC* (β_5 = 0.091, t = 0.25, $p > 0.10$). In sum, there does not appear to be any differential effect for analysts having one versus multiple disclosure events in the accuracy and short-window market reaction analyses.

Age of disclosure incident

The NASD database identifies analysts with background disclosure events for analysts currently employed by a member brokerage firm and those employed by a member brokerage firm within the last two years; there is no

25. Untabulated results for accuracy and market response models based on an integrated model of both pre- and postincident samples and relying on a period indicator variable to compare relevant parameter estimates yield similar inferences. In addition, results from the integrated accuracy model do not reveal significant accuracy differences for the disclosed analysts between the preincident and postincident periods.

statute of limitations such that if an event occurs beyond a particular time frame it is removed. We now investigate if there is any variation in the association of the disclosed variable with relative accuracy or market credibility conditional on the length of time that has elapsed since the incident.

We examine the influence of elapsed time in years (all inferences are unchanged if we use time in days as an alternative) by splitting at the median elapsed time since the disclosed incident using an indicator variable, *LDISC*, equal to 1 if the age is greater than or equal to the median and 0 otherwise.[26] Untabulated results from estimating (1) to evaluate the earnings forecast accuracy with the addition of the *LDISC* variable reveal a negative and significant coefficient on our variable of interest, *DISC* ($\alpha_1 = -0.028$, $t = -2.19$, $p < 0.05$), but no mitigation of the accuracy effect for analysts with older incidents, *LDISC* ($\alpha_2 = 0.009$, $t = 0.57$, $p >$ 0.10). Untabulated results from estimating (2) to evaluate the short-window market reaction reveal a negative and significant coefficient on our variable of interest, *REVP*DISC* ($\beta_3 = -0.457$, $t = -2.26$, $p < 0.05$), but not a mitigation of the incremental market reaction effect for analysts with older disclosure incidents, *REVP*LDISC* ($\beta_5 = 0.288$, $t = 1.36$, $p > 0.10$).[27] In sum, the accuracy and market response effects do not decay with time, adding support to the notion that the disclosure variable captures an inherent analyst trait.

Categorizing disclosure incidents: Professional versus personal

Because the various types of disclosures may have different implications for forecasting performance and market credibility, we categorize disclosure event types into those of a professional nature and those of a personal nature.[28] We classify customer complaints, regulatory actions, terminations, and investigations as professional in nature, *PROF*, and bankruptcies and judgments/liens as personal in nature, *PERS*. Because criminal actions and civil judicial actions represent a mix of professional and personal incidents, we omit them from direct classification. Some analysts have more than one disclosure event; for these multiple-event cases, we categorize analysts as *PROF* (*PERS*) only if all their disclosure events are of a professional

26. For analysts with multiple disclosure incidents, we measure the time elapsed since their first disclosure incident, although we also verify that our inferences are unchanged if we rely on time elapsed since their last disclosure event.
27. In alternative analyses, we examine additional cutoffs, greater (less) than 75th (25th) elapsed time percentiles as alternative long (short) categorical variables, and greater (less) than 90th (10th) elapsed time percentiles as extreme long (short) categorical variables. We also examine the cross-sectional variation as to accuracy and market reaction with continuous and time-ranked variables. All of these alternatives yield inferences similar to those based on the median split.
28. An alternative approach is to evaluate the eight types of disclosures individually; however, given the relatively small samples of each type, we believe that the aggregate categories lend themselves to more general and stable inferences.

(personal) nature. If a professional (personal) disclosure event is accompanied by a "mixed" disclosure event, for example, a criminal action or a civil judicial action, we allow coexistence of a professional or personal disclosure with a mixed event in the primary analysis and then impose a stricter selection criterion disallowing such coexistence in a sensitivity check.

Untabulated results from estimating (1) to evaluate earnings forecast accuracy based on the *PROF* and *PERS* indicators reveal a negative and significant coefficient estimate on *PROF* ($= -0.025$, $t = -2.71$, $p < 0.01$) and an insignificant estimate on *PERS*. A second estimation using the stricter definition of *PROF* and *PERS* also reveals a negative and significant coefficient on *PROF* ($= -0.030$, $t = -2.94$, $p < 0.01$) and an insignificant estimate on *PERS*. Untabulated results from estimating (2) to evaluate the short-window market reaction based on the *PROF* and *PERS* indicators reveals negative parameter estimates on the interaction terms (as expected); however, the estimates are not significant at conventional levels in either estimation.[29] In sum, our results suggest that earnings forecast accuracy is driven more by professional than by personal disclosure events; however, the market does not appear to price these individual types of disclosure events.

6. Sensitivity analyses

Brokerage house status

In the preceding earnings accuracy and market models, it is possible that brokerage house status or resource availability is not well proxied by a linear brokerage size measure. To investigate, we replace *BSIZE* with *B_STATUS_SIZE* = 1 for the 10 largest brokerage houses each year and 0 otherwise; *B_STATUS_IIAA* = 1 for the 10 firms with the most *Institutional Investor* All-American analysts each year and 0 otherwise; and *B_STATUS_CM* = 1 for the 10 highest ranked firms each year based on a modified Carter-Manaster measure of brokerage house status relying on underwriters' relative placements in stock offering "tombstone" announcements (Carter and Manaster 1990; Carter, Dark, and Singh 1998; Loughran and Ritter 2004). Untabulated results from estimating (1) to evaluate earnings forecast accuracy based on these alternative brokerage house status proxies continue to reveal a negative and significant coefficient on our variable of interest, *DISC* (α_1: $SIZE = -0.022$, $t = -2.58$, $p < 0.01$; α_1: $CM = -0.022$, $t = -2.62$, $p < 0.01$; α_1: $IIAA = -0.022$, $t = -2.55$, $p < 0.01$). Untabulated results from estimating (2) to evaluate the

29. We believe the lack of differential market pricing is due to the market response effect hinging on the aggregate *DISC* measure. That is, the additional requirement of the returns data, the omission of two categories (the criminal actions and the civil judicial actions that mix professional and personal activities), and the separation of the remaining observations into categories of incidents do not allow for sufficient power to discriminate between these categories of incidents.

short-window market reaction based on the alternative proxies also reveal a negative and significant coefficient on our variable of interest, *REVP*DISC* (β_3: *SIZE* = −0.175, $t = -2.84$, $p < 0.01$; β_3: *CM* = −0.172, $t = -2.94$, $p < 0.01$; β_3: *IIAA* = −0.173, $t = -2.91$, $p < 0.01$). In sum, the alternative brokerage status proxies yield results that are inferentially similar to our main results.

Thinly followed firms

Our primary analyses require at least two analysts following a firm-quarter, one disclosed analyst and one nondisclosed analyst. To address the concern over inferences based on thinly followed firms, we conduct sensitivity analyses imposing a constraint of at least five analysts following the firm in both the forecast accuracy and market reaction tests. Untabulated results from evaluating forecast accuracy based on a constraint of at least five analysts continues to reveal a negative and significant coefficient estimate on our variable of interest, *DISC* ($\alpha_1 = -0.016$, $t = -1.76$, $p < 0.10$), and evaluating the short-window market reaction based on the same constraint reveals a negative and significant coefficient estimate on our variable of interest, *REVP*DISC* ($\beta_3 = -0.191$, $t = -2.82$, $p < 0.01$). Thus, our results are not significantly affected by the presence of thinly followed companies.

Broker expungement

An analyst or broker can appeal a disclosed incident; however, the NASD (currently the FINRA) expunges an incident only if an arbitration panel orders an expungement and a court confirms the finding. According to the NASD, instances that are most likely to lead to an expungement include the following: (1) the claim, allegation, or information that the disclosed incident is factually impossible or wrong; (2) the named person is incorrect; or (3) the claim, allegation, or information is false. While expungements are rarely granted, the majority of those successfully pursued are based on customer complaints, which are the most subjective of the eight types of disclosure events. As a sensitivity test, we reanalyze our results after omitting customer complaint disclosure incidents. Untabulated results from evaluating earnings forecast accuracy after eliminating customer complaints continues to reveal a negative and significant coefficient on our variable of interest, *DISC* ($\alpha_1 = -0.020$, $t = -2.13$, $p < 0.05$), and evaluating short-window market reactions after eliminating customer complaints also continues to reveal a negative and significant coefficient on *REVP*DISC* ($\beta_3 = -0.194$, $t = -2.85$, $p < 0.01$).

Media coverage and disclosed analysts

Bonner et al. (2007) show a larger market reaction to celebrity analysts, where celebrity is measured by the quantity of media coverage. If the disclosed analysts are not covered in the media due to the financial press wishing to avoid less reputable analysts, then it may be the reduced media coverage affecting the market reaction rather than the disclosed incident itself. Because undertaking a hand-collection of media coverage of individual analysts is beyond the scope of this study, as an alternative, we model the variables found to be related to media coverage in Bonner et al. 2007 as control variables in our market model as a sensitivity analysis. These media coverage determinants include number of firms followed, forecast frequency, general experience, forecast bias, award status, and forecast accuracy. Of these variables, the ones not already included as control variables are forecast bias and award status. Adding these additional variables and related interactions, untabulated results from estimating (2) continue to reveal a negative and significant coefficient on our variable of interest, *REVP*DISC* ($\beta_3 = -0.189$, $t = -2.58$, $p < 0.01$). Thus, it does not appear that including media coverage determinants alters the finding of a reduced market reaction to disclosed analysts' forecast revisions.

7. Conclusions

Skepticism concerning brokerage industry self-regulation is not surprising due to conflicts of interest faced by the NASD, the industry's largest regulatory body, in its role as both industry regulator and promoter of its constituents' interests. Barber et al. (2006) find beneficial outcomes to investors of an industry initiative requiring brokerage firm-level stock recommendation distribution disclosures in their analyst reports. We extend this line of research on brokerage self-regulation by examining an industry initiative designed to inform investors regarding investment professionals' backgrounds. Our aim is to evaluate if investors who are uninformed as to an analyst's background can benefit from accessing such disclosures.

Consistent with the notion that disclosed analysts' forecasts are less reliable, we find that disclosed analysts forecast earnings less accurately vis-à-vis a firm-quarter matched sample of nondisclosed analysts and that this effect is present both prior to and after their disclosure incidents. Based on this result, and supplemental analyses examining the impact of multiple disclosure events and the effect of the age of the disclosure event, we conclude that their inferior accuracy is consistent with the disclosure signal capturing a persistent analyst characteristic (rather than as a result of the disclosure signal per se). We find a relatively weaker market reaction to revisions by disclosed analysts only in the postincident period, consistent with the disclosure signal providing new information to equity investors.

In addition to these findings, we acknowledge two results deserving additional discussion. The first such result is that we did not find the market to discount forecast revisions from disclosed analysts in the predisclosure period. This is surprising because we find that disclosed analysts were less accurate than other analysts during this time period, and past research has shown that the market prices past accuracy (e.g., Park and Stice 2000; Bonner et al. 2003; Clement and Tse 2003; Chen et al. 2005).

We believe there are at least two explanations for why we failed to detect such an effect. First, prior work finds that the market reaction to forecast revisions is consistent with investors refining their knowledge of analyst ability over time (Chen et al. 2005). By construction, the disclosed analysts in the predisclosure period have fewer forecast observations available to the market than do the same analysts later in time (i.e., after the disclosure event). Thus, a less complete forecasting record may prevent the market from forming sufficiently precise performance assessments. Indeed, scenarios such as this may underpin the NASD's motivation to undertake the disclosure initiative in the first place. That is, if the NASD viewed available forecasting patterns as sufficient to evaluate analysts, there would be little need for such a program. Second, our research design entails a number of sample restrictions; among the most restrictive is inclusion of only analysts forecasting in both the pre and post periods. While we view this design feature as necessary to prevent comparison between two different samples of disclosed analysts, it reduces the sample available for our market tests well below samples in comparable studies.

With respect to another finding that deserves explanation, we did not find that large investors differentially discount forecasts from disclosed analysts. We believe there are at least two explanations for why we failed to detect such an effect. First, this analysis separates the large from the small trades, reducing the power of our test. Second, in contrast to our reasoning in the third hypothesis, large traders — the ones most likely to know of the NASD database — may not benefit much from disclosure initiatives because they are more likely to be ex ante informed concerning an analyst's background.

We draw the following conclusions from the totality of our results. First, ex ante uninformed investors seeking relatively more accurate or credible earnings research (as perceived by the market in general) can benefit from public disclosures of analyst backgrounds. Second, the market acts as if it prices this disclosure signaling a persistent analyst credibility characteristic associated with analysts' backgrounds.

We make several contributions to the literature. First, in an environment of skepticism regarding the self-regulation model of the financial industry, we add to the literature on regulatory initiatives by showing the relevance to market participants of a particular disclosure initiative involving investment professionals' backgrounds. Second, our finding that disclosed analysts forecast less accurately than other analysts is relevant to research on forecasting accuracy per se because the market relies on forecasts to form its earnings expectations and as inputs to other important research outputs. Third, investors rate integrity and professionalism as one of the most important attributes of an equity research firm (*Institutional Investor* 2006). To the extent that the NASD disclosures provide reasonable — albeit noisy — proxies for analyst integrity and professionalism, we show that such analysts are less accurate earnings forecasters and their forecast revisions are less credible.

Appendix

Examples of specific disclosure events

Criminal action

Reporting source: Individual broker (Form U-4). Court details: State Attorney Sixteenth Judicial Circuit of Florida. Charge detail: resisting arrest with violence charge — felony.

Customer complaint

Reporting source: Individual broker (Form U-4). Allegations: That an investment in Circle K stock in a retirement account resulted in a loss and that the investment was unsuitable. Alleged damages: $29,758.00. Case was settled for $10,000.

Bankruptcy

Reporting source: Individual broker (Form U-4). Mr. X filed chapter 7 bankruptcy due to real estate investment activities in 1980s. Attempted to reorganize via chapter 11, three times. Finally completed chapter 7, full liquidation, in February of 1998.

Regulatory action

Reporting source: Regulator (Form U-6). Violation of the rules of fair practice in that employee performed consulting services for a client company for which he sent, or caused to be sent, a personal bill, the proceeds of which he received and deposited to his personal bank account; and performed consulting services without informing his employer, and sent, or caused to be sent, a personal bill when the customer believed they had employed him as an employee of the brokerage. Decision rendered: employee censured and fined $5,000.

Termination

Reporting source: Individual broker (Form U-4). Allegations: Firm alleged that employee signed Firm into a lease and represented himself as an officer of the firm without authorization and approval from the firm and entered into a personal lease as the corporation.

Civil judicial action

Reporting source: Individual broker (Form U-4). Plaintiff brings this action pursuant to federal rule of civil procedure 23(a) and (b)(3) on behalf of all persons who purchased shares of Interspeed common stock during the period of 1/3/2000 through 7/20/00. Summary: Mr. X along with his previous employer is the subject of a securities class action involving the interaction between the investment banking and sales department regarding Interspeed and issuance of research.

Investigation

Reporting source: Individual broker (Form U-4). The division of enforcement of the NYSE is investigating Mr. X in connection with certain research reports he had a role in drafting.

Judgments/liens

Reporting Source: Individual broker (Form U-4). Holder of lien is IRS. Amount of lien is $9,500.28.

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