Supply Shortages in Sell-Side Analyst Coverage

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ABSTRACT

In this paper we argue that the lack of an explicit price for sell-side analysis leads to supply shortages in response to shocks to the cost of covering a firm. Using changes in firm-specific uncertainty due to CEO turnovers we document a 9% reduction in coverage relative to comparable companies. We confirm our findings using alternative sources of uncertainty (filings of securities class actions and industry shocks). Looking at individual analysts, we show that the probability of dropping coverage is higher for younger analysts, analysts with lower reputation and higher workloads, factors that indicate higher career concerns and marginal production cost.

JEL classification: G11, G14, G23.

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I. Introduction

The introduction of Mifid II in 2018 has forced asset managers to unbundle payment for sell-side research from trading business. Prior to this regulatory change, under the so-called "soft-dollar" model the cost of producing sell-side reports was covered by a portion of the trading fee generated by said research.

While US banks are currently still exempt from these rules, the effects of this change have already been felt globally, with a 10.6% reduction in the number of analysts employed by the 12 world largest investment banks [1] and estimates of reduction in fund managers research budgets of up to 30% [2].

This sharp reduction in the demand of sell-side research shows how the lack of an explicit price has led to a gap between perceived value and production cost of sell-side research. In this paper we document a second adverse effect of the soft-dollar model for the remuneration of sell-side research: supply shortages following a sudden increase in the cost of covering a firm.

When the firm-specific uncertainty surrounding a company increases, analysts face higher costs in terms of time, effort and reputational risk. In an competitive market with explicit pricing, analysts would cover these costs by charging more for this coverage. Under a soft-dollar model this is not possible and the analyst will have to internalize the additional cost. Analysts with the least efficient production function will be "driven out the market" and will simply stop providing coverage for that specific firm.

Pan, Wang, and Weisbach (2015) show that firm idiosyncratic volatility increases around the appointment of a new CEO and then slowly declines as uncertainty around her/his ability is resolved. Using this result to set up our experiment we show that, in the year following the CEO turnover event, coverage declines by 0.6 units (with respect to the year before) compared to a sample of comparable firms. This represents a drop of around 9% of the median coverage.

Since analysts themselves are largely responsible for the quality of the information environment surrounding the firm, assessing the causal relationship between analysts' actions and firm-specific

[1] Analyst coverage shrinks after fee shake-up, by Robin Wigglesworth and Philip Stafford, 12/03/2019, *The Financial Times.*

[2] Large fund houses slash broker research spend after mifid II, by Siobhan Riding, 13/04/2019, *The Financial Times.*

uncertainty can be problematic. While reverse causality is not a major concern for us, we cannot directly observe demand for analyst coverage. It has been argued (McNichols and O'Brien, 1997) that analysts prefer to cover firms with good prospects (higher revenue generation trough associated trading fees) so negative performance could drive both a decrease in coverage and CEO turnover[3].

We address this potential concern in a number of ways: first of all we show that a change in CEO is not followed by any decrease in trading activity (turnover), indicating that the demand of coverage (and the revenues from associated trading fees) is not affected. We then confirm our main finding in a multivariate diff-in-diff setting where we model coverage as a function of suitable proxies for demand (turnover, institutional ownership, etc.), an approach statistically equivalent to the "residual coverage" model of Yu (2008). We also confirm our main result on a subset of turnover events unlikely to be motivated by firm performance: events where the pre- or post-event firm performance is positive, where the resigning CEO is at least 65 years old and events where CEO turnover is not accompanied by a managerial shakeup.

As a second empirical strategy, we verify that our result is not weaker after the Global Analysts Settlement and the transparency regulation changes of 2002. In the new regulatory regime analysts have to report the distribution of their recommendations and thus they have to cover firms with negative prospects in order to maintain their reputation [4]. The fact that our main result grows stronger after 2002 supports our causal interpretation.

To confirm that our results are not specific to CEO turnover events we consider possible alternative sources of firm uncertainty. First we look at shareholders class actions where the firm is listed as defendant, and we show that subsequently to the filing of lawsuit there is a drop in analyst coverage of between 0.9 and 1.1 analysts compared to a sample of control firms independently of the outcome of the lawsuit (dismissal or settlement). We then look at three diverse industry-related events that are likely to increase uncertainty for firms in the affected industries: Hurricane Katrina for insurance companies, the default of Lehman brothers for financial firms and the collapse of Enron for firms with a similar business model. In the year following the different shocks, the affected firms experience a drop in coverage (compared to the average unaffected firm) of between 0.2 and

[3]For example, Scherbina (2008) empirically shows that a drop in coverage can be used to forecast negative equity returns.

[4]Both Barber, Lehavy, McNichols, and Trueman (2006) and Kadan, Madureira, Wang, and Zach (2009) show a significant increase in negative analysts' recommendations after the adoption of the new regulation.

1.3 analysts. While we cannot claim that these experiments enjoy the same level of exogeneity as the CEO turnovers, nonetheless they show that the drop in analyst coverage following an increase in uncertainty of firm prospects is general, and is not limited to a specific type of event.

All together these results show that an increase to firm-specific uncertainty is followed by a significant decrease in the supply of coverage. We argue that this is due to an increase in the "production cost" of coverage that cannot be matched by an increase in "price" since the service is remunerated by a portion of the associated trading fees.

The increase in production cost can take the form of additional time and effort necessary to acquire information on the skill of the new CEO. Moreover, Brown, Call, Clement, and Sharp (2015) show, using survey data, that "private communication with management" is one of the most important input in the production of sell-side coverage. A CEO turnover may force an analyst to invest time and effort to re-build personal connection with the top management.

From the point of view of the individual analyst, an increase in firm-specific uncertainty generates an additional cost in terms of higher reputational risk associated with producing an erroneous forecast or a bad recommendation (Hong, Kubik, and Solomon, 2000; Hong and Kubik, 2003).

If the drop in coverage that we document in this paper is indeed due to an increase in production cost, we should observe that the analysts dropping coverage are the one with the least efficient production function. We study the individual characteristics of analysts who discontinued coverage after a CEO turnover event, and we show that, compared with the analysts who were covering the same firms but did not discontinue their coverage, they are less experienced, are less specialized (by industry), have a higher workload (in terms of number of firms they cover), have weaker reputation and have stronger risk-aversion. In prior research these characteristics have been associated with stronger career and reputational concerns and with herding behavior (Hong et al., 2000; Clement and Tse, 2005). Overall they can be seen as proxies for higher cost of dealing with the increased uncertainty.

A reasonable objection could be raised to the economic relevance of the main result of this paper: since the median coverage in our sample is around 7 analysts is an average decline of 0.6 analysts important? While the percentage effect is sizable, 9%, one could argue that the firm is still covered by 6 analysts! To answer this question we can look at the evidence of a number of articles that have analyzed the effect of exogenous decreases in analyst coverage of one unit. He and Tian (2013) look at the effect of analysts on firm innovation and their analysis suggests that *"an exogenous average loss of one analyst following a firm causes it to generate 18.2% more patents over a three year window than a similar firm without any decrease in analyst coverage"*. Similarly Derrien and Kecskés (2013) look at the causal effects of analyst coverage on corporate investment and financing policies and conclude that *"firms respond to the loss of an analyst by decreasing total investment and total financing (the year after compared to the year before) by 1.9% and 2.0% of total assets, respectively, compared to our control firms"*. Finally Irani and Oesch (2016) analyze the effect of analyst coverage on earnings manipulation and conclude that *"a drop in coverage among treated firms [of one unit] causes an increase in the use of abnormal discretionary accruals that is about 9% of one standard deviation"*. All this evidence indicates that even the reduction in coverage of a single analyst has material effects on managerial behavior, so the effects that we document in this paper are economically significant.

The rest of the paper is organized as follows: Section II analyzes the relevant literature on financial analysts'; Section III presents our data; Section IV investigates the causal link between analyst coverage and idiosyncratic risk; Section V addresses the correlation between idiosyncratic volatility and negative return prospects; Section VI analyzes alternative sources of firm uncertainty; Section VII looks at individual analysts' characteristics and reputational concerns; Section VIII concludes.

II. Relevant Literature and Hypotheses

A. Career concerns for financial analysts

In this paper we argue that an increase in firm-specific uncertainty affects the "cost" of covering the firm. One specific form this cost can take is an increase in reputational risk for the analyst from issuing an erroneous forecast or recommendation.

Career and reputational concerns for financial analysts have been extensively studied in the literature. Hong et al. (2000) show that inexperienced analysts are more likely to be terminated for inaccurate earnings forecasts. On the positive side Hong and Kubik (2003) show that brokerage houses remunerate accuracy with positive career outcomes.

This empirical evidence seems to be at odds with recent survey data where financial analysts rank "accuracy" last in a list of nine determinants of their own compensation (Brown et al., 2015). Aside from the intrinsic problems of all self-reporting (ability to "generate underwriting business and trading commissions" also rank at the bottom of the list...) it is important to notice that a strict reading of this result could underestimate the importance of accuracy in determining other factors, such as "standing in analyst rankings" that are the top of the survey.

In response to career concern analysts they tend to produce forecasts more in line with the prevailing consensus (herding behavior), issue forecasts with a greater delay and revise them more frequently. Clement and Tse (2005) show that more accurate analysts and analysts working for larger brokerage houses are more "bold" in their forecasts, indicating that they have lower career concerns. Stickel (1992) and Jackson (2005) both show that higher accuracy leads to stronger reputation (measured by analysts rankings), and thus possibly lower career concerns. Kim and Zapatero (2011) find that herding is stronger for stocks with high volatility where reputational concerns are stronger.

While prior literature has focused on herding as a response to career concerns, here we argue that rather than issuing a forecast close to the prevailing consensus, analysts may respond to extreme uncertainty by dropping the coverage of a company altogether. We will show that the same variables that increase the likelihood of herding also lead to coverage termination.

B. Endogenous coverage decisions

We also contribute to the literature on the endogeneity of analyst coverage. Bhushan (1989) shows that analysts prefer to cover large firms, firms with strong institutional ownership and higher total volatility. These are all variables that can affect the profitability of the stock for the brokerage company who employs the analyst. On a related note Bushman, Piotroski, and Smith (2005) show that weak protection against insider trading lowers the incentive to cover a firm because there is no value in producing private information. McNichols and O'Brien (1997) argue that analysts do not like to issue negative recommendations and thus coverage decision is based also on their perception of the future prospects of the firm.

To the best of our knowledge only two papers deal with coverage termination. Hong and Kubik (2003) show that a brokerage house may punish an analyst for his/her lack of performance (accuracy) by assigning the coverage of a prestigious company to somebody else. Scherbina (2008) reiterates the idea that analysts do not like to issue negative forecasts and thus coverage terminations can be used to forecast stock returns.

In this paper we explore a new dimension of endogeneity of coverage decisions, namely the intensity of the uncertainty surrounding a firm's prospects.

C. Analyst coverage and idiosyncratic volatility

This paper also provides a new explanation for the negative correlation between analyst coverage and idiosyncratic volatility. From Roll (1988) we know that the higher is the amount of firm-specific information available the, higher should be the idiosyncratic volatility of its stock returns (compared to the total volatility). According to this argument a higher number of analysts following a firm should result in higher, rather than lower, idiosyncratic volatility. We are not the first to address this issue. Piotroski and Roulstone (2004) analyze the relationship between idiosyncratic risk and the activity of three categories of "information providers": company insiders, large institutional investors and financial analysts. They show that while higher trading volumes from insiders and stronger presence of large institutional investors lead to a higher level of idiosyncratic volatility, the opposite is true for financial analysts: a higher number of analysts covering a firm lead to lower levels of idiosyncratic risk. The way they rationalize this finding is by assuming that while insiders and large institutional investors have access to company-specific information, and thus can impound such information in stock prices with their trading activity, financial analysts lack such access and thus devote their effort to analyzing the impact of market-wide factors for the company. Chan and Hameed (2006) document the same negative relationship on a sample of 25 emerging countries and conclude that *"poor information disclosure and lack of corporate transparency increase the cost of collecting firm-specific information, so that security analysts generate their earnings forecasts based mostly on macroeconomic information"*.

While they offer slightly different explanations for the phenomenon at hand, both of these papers share a common characteristic: they assume that analyst coverage is the cause of the low idiosyncratic volatility. Either because they are unable to develop company specific information, or because it would be too expensive to do so, analysts prefer to simply map market-wide factors into the price of the specific stock. In doing so they generate an information environment where the price of the stock reacts mainly to general factors.

In this paper we show that the direction of the causal relationship is the opposite: financial analysts do not cause the level of idiosyncratic volatility but rather they react to it. Specifically we show that analysts react to an exogenous increase in idiosyncratic risk by discontinuing coverage of the firm.

III. Database

The sample examined in this paper includes US listed firms during the period of 1980—2018. We consider only companies with market capitalization greater than 100 m$ and stock price greater than 1$ (from the CRSP monthly file). We restrict the analysis to firms listed on the NYSE/AME/NASDAQ and having CRSP share code 10 or 11. We also require the firm to be present in the Institutional Brokers' Estimate System (I/B/E/S) database (covered by at least one analyst), Compustat (where we collect quarterly financial statements) and Thomson's CDA/Spectrum database form 13F (where we collect institutional holdings data). The final sample has 288,868 firm-quarter observations from 9,028 different companies (individual GVKEYs).

The choice of the quarterly rhythm of our sample aligns with the normal rhythm of analysts' activity based on quarterly release of new financial statements. From Execucomp we derive CEO turnover events from 1981 to the end of 2017 that, when matched with our sample yield 4,583 usable events.

Our key measure of analyst coverage is (the natural log of) the number of analysts covering a firm at the end of the quarter. From prior literature we know that coverage decisions are at least partially motivated by the profit opportunities generated for the brokerage house that employs the analyst. We model these factors with a number of firm-level control variables designed to capture the potential demand for brokerage services. Specifically, we consider firm size, quarterly turnover and institutional ownership. We take into account that analysts may prefer to cover "glamorous" stocks and thus consider Tobin's Q, ROA, leverage and quarterly excess return. The definition of all the control variables is detailed Table I. Summary statistics for our sample are reported in Table II.

[Place Table I about here]

[Place Table II about here]

IV. Firm uncertainty and analyst coverage

Assessing the causal relationship between firm uncertainty and analyst coverage is not trivial. While here we argue that coverage decisions are partially based on firm uncertainty (via the mediating effect the of "cost" of covering the firm) we also acknowledge that financial analysts, as information providers, contribute to the quality of the information environment surrounding a firm. Derrien and Kecskés (2013), for example, show that an exogenous decrease in the number of analysts covering a firm increases the uncertainty of said firm prospects and, via an increase in cost of capital, has a material effect on investment decisions.

To prove our main hypothesis we need to measure the reaction of analysts to a shock in firm uncertainty which is exogenous with respect to the actions of the financial analysts themselves. We base our main experiment on the evidence of Pan et al. (2015). In this paper the authors argue that when a CEO is appointed to lead a firm, his or her ability is largely unknown to the market (both in terms of underlying talent as well as the quality of the match between the job and personality). They present a model where, with the passage of time, the uncertainty is progressively resolved as the market learns about the unobservable quality of the new CEO from his/her visible actions. The authors provide strong empirical support for this model by showing that stock volatility, both total and idiosyncratic, realized and implied, spikes around CEO turnovers and then progressively declines over the following five years. They also estimate that uncertainty about managerial ability contributes to up to 26% of the total stock volatility in the post-turnover period. While the amount of uncertainty about the ability of the new CEO will vary from case to case (for example will be affected by the presence of a succession plan) this evidence nonetheless shows that, on average, the market perceives an increase in the uncertainty surrounding the economic prospects of a firm around CEO turnovers.

The change in the number of analyst following a firm before and after the appointment of the new CEO will allow us to assess how coverage responds to an exogenous shock in firm uncertainty. The reader should note that here we are not implying that the turnover events are exogenous in themselves, but rather that the increase in firm-specific uncertainty following the change in CEO is exogenous with respect to the actions of the financial analysts covering the firm.

A remaining concern is that if the CEO turnover is motivated by the bad performance of the company, the drop in coverage may signal the unwillingness of financial analysts to issue bad reviews rather than by their inability to tackle the increased uncertainty. We would be dealing, in other words, with a case of omitted variable: negative firm prospect could affect both the change of leadership and the offer of sell-side research (via, for example, a decrease in demand)h. We will deal with this issue in section V by repeating our analysis on a subsample of firms with positive pre- and post-turnover performance.

We define the change in coverage as the difference between the number of analysts following the firm one year after the appointment of the new CEO and the number of analysts following the firm one year before the turnover. This definition accommodates the fact that, while Execucomp reports the date the official appointment of the new CEO, usually the succession is announced some time before[5].

Our main test is based on a diff-in-diff estimation where the average change in coverage for treated firms is compared to a control sample. We consider four possible alternatives. In each of them every treated observation is matched with a single observation from the same calendar quarter and the same industry (Fama and French 12). The specific control observation is chosen with one the following methodologies:

1. Closest observation by market capitalization.
2. Closest observation by pre-event coverage.
3. Closest observation by propensity score estimated with a probit model on a group of control

[5]As a robustness we also measure the change in coverage as the difference between the average number of analysts covering the firm in the 12 months after and before the event. Results are comparable both in size and significance.

variables (Log-Size, Turnover, Institutional Ownership, Tobin's Q, ROA, Leverage, Excess Return and Log-Coverage) defined in Table I.

4. Closest observation by Mahalanobis distance on the same control variables.

The control sample is built with replacement as this tends to produce matches of higher quality than matching without replacement by increasing the set of possible matches (Abadie and Imbens, 2006). When multiple observations are tied in terms of distance we follow Abadie, Drukker, Herr, Imbens, et al. (2004) and include all of them[6].

Abadie and Imbens (2006, 2012) show that nearest-neighbor matching estimators are not consistent when matching on two or more continuous covariates even for infinitely large samples. When we match on the Mahalanobis distance on multiple variables we perform the bias adjustment suggested by Abadie and Imbens (2012).

Table III shows the results for this estimation. Panel A reports the results for the entire sample and shows that while the treated observations experience a drop in analysts equal to 0.3 units, the control firms experience, during the same period, an increase of around 0.3 analysts, leading to a diff-in-diff estimation of the causal effect of the CEO turnover over analyst coverage of -0.4 to -0.7, depending on the different control samples.

[Place Table III about here]

While this number is both statistically and economically significant, a possible concern is that the result may be driven by a subset of smaller firms. One could reasonably assume that for this firm the increase in uncertainty due to the CEO turnover is higher and, at the same time, it may be easier for analysts to drop coverage due to a relatively low demand for brokerage services for these stocks. To assess whether our result is driven by small firms we repeat the analysis dividing the sample by quartile of market capitalization (while adding the requirement that the matched control observations must belong to the same quartile of the corresponding treated observations). Results in Panel B show two interesting findings. First of all when we look at the change in coverage for treated observations we see that smaller firms experience a stronger drop in analysts following the change in CEO (close to one unit) while firms with above median size experience hardly any drop.

[6]This is relevant only for the match on pre-event coverage, given the discrete nature of this variable.

Secondly, when we match each treated firm with a control observation from the same size group we observe that the diff-in-diff estimator is significantly different from zero for every size group, and also that the size of the causal effect is similar for firms of different size.

The apparent contradiction between the two results is due to different time trends for large and small firms. As we see in 1 while the average coverage for small firms is roughly stable in time, larger firms experience wide variations, with the average coverage growing by 10 units between 1980 and 1990 and then dropping by a similar amount over the following twenty years before growing again in the last part of our sample period. The existence of significant time trends in the main variable of interest will bias any estimate based on the simple difference between coverage in two points in time. Our diff-in-diff estimators overcome this problem by matching every treated observation with a control observation from the same quarter. The analysis in Panel B of Table II confirms that the drop in coverage following a CEO turnover event is not restricted to small firms, and that our result has general validity.

[Place Figure 1 about here]

A key assumption of any matching estimator is that the change in the variable of interest experienced by the control observations represents the counterfactual change in the treatment group if there were no treatment. While it is impossible to formally test this assumption we can plot the time series of coverage for the two groups of observations before and after the treatment and see whether the two groups show similar behavior (parallel trends) before the event. Figure 2 shows time series for the two groups of firms in the 48 months around the CEO turnover event. For the sake of comparison we normalize the coverage at the beginning of the period, setting it equal to zero for both groups. While results vary differently across the four control samples we verify that they all behave similarly to the treatment sample until few months prior to the appointment of the new CEO, when there is an unmatched decline in the number of analysts following the treated firms (in all likelihood when the market was made aware for the first time of the intention of the company to replace the existing CEO).

[Place Figure 2 about here]

We interpret this drop in the supply of sell-side coverage as a ”shortage”. Alternatively, this could be seen as a response to a drop in demand of analyst coverage. While we cannot directly observe demand, in Figure 3 we plot the evolution of average monthly stock turnover for our treatment and control samples. We do not observe any drop in trading activity after the appointment of the new CEO. Treatment and control samples behave remarkably similarly, with the exception of a short-lived spike in trading activity around the time of the event.

[**Place Figure 3 about here**]

As a robustness test we also perform a more traditional multivariate diff-in-diff estimation where we regress (the natural log of) the number of analysts covering a firm on a dummy variable set equal to one if the company has appointed a new CEO in the last three years[7]. We control for a series of time-varying firm characteristics that could affect the demand of brokerage services (Size, Turnover, Institutional Ownership, Tobin's Q, ROA, Leverage and Excess Return, as defined in Table I) and include firm and quarter fixed effects (with standard errors clustered at the firm level). Our control variables can be seen as reasonable proxies for the demand of coverage. This approach is somewhat similar to the ”residual coverage” model of Yu (2008): we analyze the effect of the appointment of a new CEO after controlling for well known determinants of coverage demand (firm size, performance, turnover, institutional ownership, etc.).

$$Ln(Coverage_{it}) = \alpha + \beta NewCEO_{it} + Quarter_t + Firm_i + \epsilon \tag{1}$$

Models (1) and (2) in Table IV show the results of the estimation with and without firm-level control variables. In both cases results confirm a significant drop in coverage in the four quarters following the appointment of a new CEO.

In models (3) and (4) we interact all the explanatory variables with size quartile indicators, allowing for different intercepts and slopes for firms in different size groups. The results of the

[7]The choice of three years as the relevant period is motivated by the evidence provided by Pan et al. (2015) in Figure 1 where the authors show that after three years the uncertainty (as proxied by stock volatility) has decreased back to the pre-event level. Using a 5 years period produces similar, albeit less significant, results.

univariate analysis are confirmed, and we see that the reduction in coverage is stronger for, but not limited to, small firms.

Bertrand, Duflo, and Mullainathan (2004) show that multi-period diff-in-diff estimations with serially correlated outcome variables may lead to inconsistent standard errors. They propose a correction based on a two-stage methodology that ignores the time series information of the panel. In the first stage the outcome variable is regressed on all the explanatory covariates (including fixed effects) except the treatment dummy variable. The residuals from this regression are then averaged at the firm level, for the treated observations only, according to the treatment status. This leads to a panel of length two where for every treated firm we have the average residual outcome (analyst coverage) in the periods with or without treatment. The estimate of the treatment effect and its standard error can then be obtained from an OLS regression of the residual outcome over the treatment indicator variable in this two-period panel. Models (5) and (6) in Table IV report the results from the second stage estimation and show results similar to the regular OLS, both in terms of size and significance).

[**Place Table IV about here**]

V. Idiosyncratic volatility and firm prospects

Since quasi-natural experiments are not randomized, the quality of the causal inference has always to be carefully assessed. The two most common problems that affect empirical setups are reverse-causality and spurious correlation created by some unobserved quantity. While the first issue is unlikely to be relevant in our setting (it's hard to argue that a reduction in the number of analysts following a firm is the direct cause of a CEO turnover) the second could be problematic. Specifically one could argue that both our outcome and treatment variable are caused by negative prospects for the company.

On the one side McNichols and O'Brien (1997) argue that analysts are more likely to cover firms with positive prospects in order to maximize brokerage revenues and Scherbina (2008) shows that a drop in analyst coverage successfully predicts underperformance. On the other side we know that CEO turnover is inversely related to firm past performance (Coughlan and Schmidt, 1985; Warner, Watts, and Wruck, 1988; Barro and Barro, 1990; Kaplan, 1994b,a) as investors learn about managerial ability from stock returns (Bushman, Dai, and Wang, 2010; Pan et al., 2015). Finally Pan et al. (2015) show that idiosyncratic volatility increases around CEO turnovers while Ang, Hodrick, Xing, and Zhang (2006) and Ang, Hodrick, Xing, and Zhang (2009) document a negative correlation between idiosyncratic volatility and future stock returns[8].

In order to test whether our results are driven by performance-motivated forced turnovers we repeat our diff-in-diff analysis for a subset of turnover events that are less likely to be motivated by firm bad performance. According to Pan et al. (2015) forced turnovers are preceded by high volatility and bad performance, so following their example we define a subsample of events characterized by:

1. Idiosyncratic volatility in the year before the turnover lower than the industry median.
2. Stock return in the year before the turnover higher than the industry median.
3. ROA in the year before the turnover above the industry median.

We also consider subsamples of events where:

4. The CEO turnover is not followed, in the next three years, by a managerial shakeup in the four most highly paid positions according to Execucomp.
5. The age of the outgoing CEO is equal to or higher than 65 years.

Panel A of Table V shows that our results are confirmed in this subsample. All the diff-in-diff estimators have a negative sign indicating a drop in analyst coverage after an exogenous CEO turnover. We also see that, while the size of the effect varies in the different subsamples, it is generally economically relevant.

A possible concern with this results is that analysts my react to expected, rather than realized, performance. In other words, conditioning our estimation on positive performance in the pre-event period may not be sufficient to weed out the cases where the drop in coverage may be motivated by performance rather than uncertainty. To address this issue we repeat the experiment

[8]In most of the subsequent literature this phenomenon has been labeled a "puzzle" and has been attributed to market frictions (Fu, 2009), lottery preferences of investors (Chabi-Yo and Yang, 2010) and strategic risk shifting (Chen, Strebulaev, Xing, and Zhang, 2014).

by conditioning on post-event positive performance of the firm (using the same three measures outlined above but measured on the four quarters following the installation of the new CEO)[9]

Results of the second experiment in Panel B of Table V confirm our previous findings. We still document a significant drop in coverage following CEO turnover for the subsample with firms with positive post-turnover performance. To the extent that realized performance can be seen as a viable proxy for analysts expectations at the moment of the CEO change, this results confirm that the documented drop in coverage is not motivated by the unwillingness of analysts to cover firms with negative performance prospects.

[Place Table V about here]

A. Global settlement and contemporary regulation changes

In the previous analysis we have looked at a subsample of CEO changes not characterized by bad performance. We can also follow a slightly different route and look at a subsample of events where analysts have fewer incentives to drop coverage of bad-performing companies.

In 2002 a series of investigations on possible conflicts of interests of sell-side financial analysts employed by investment banking firms led to a series of regulation changes to reduce incentives to produce overly optimistic reports to boost brokerage and IPO activities.

In May 2002 the Securities and Exchange Commission (SEC) approved two proposals advanced by the National Association of Securities Dealers (Rule 2711) and the New York Stock Exchange (modified Rule 472). Among their provisions, these rules require all analyst research reports to display the percentage of the issuing firm's recommendations that are buys, holds, and sells.

In December 2002 the SEC announced the Global Analyst Research Settlement involving ten U.S. investment firms that engaged in actions and practices that led to the inappropriate influence of their research analysts by their investment bankers. The firms involved agreed to introduce a series of organizational changes in order to separate research and investment banking. Although this settlement only involved directly ten large institutions, its provisions became industry standards.

[9] One could argue that we could follow a more direct route and condition directly on analysts expectations by looking at the change of earnings forecasts around the CEO turnover event. This approach is problematic because the change in forecasts can only be measured conditionally on the analyst not dropping coverage of the firm.

Under the new regulatory regime the incentives to cover stocks with positive prospects hypothesized by McNichols and O'Brien (1997) is somewhat reduced[10]. Moreover, with this augmented transparency enforced by Rule 2711 and Rule 472, analysts have to cover stocks with negative prospects in order to maintain their credibility. Barber et al. (2006) document a dramatic shift in the distribution of recommendations, with "Buy" recommendations dropping from 60% to 45% and "Sell" recommendations changing from 5% to 15%. Kadan et al. (2009) report similar numbers and also show that the effect is stronger for "affiliated" analysts (whose employer has business relations with the covered firm).

If the relationship between analyst coverage and firm uncertainty that we document in this paper is an artifact of the correlation between negative return prospects and idiosyncratic risk we should observe a weakening of our result under the new regulatory regime. Table VI reports the result of our diff-in-diff experiment for two subsamples of turnover event before and after December 2002. While the drop in coverage is somewhat larger before the regulatory changes described above, the difference is small and generally not significant. The fact that the drop in coverage following an exogenous increase in firm uncertainty is not weaker in an environment where analysts have fewer incentives to discontinue coverage of bad performing firms shows that this result is not an artifact of the spurious correlation between uncertainty and firm prospects: financial analysts are not dropping coverage of firms with negative prospects, but rather of companies surrounded by increased uncertainty.

[Place Table VI about here]

VI. Alternative shocks to uncertainty

Here we test the robustness of our results by examining alternative sources of shocks to uncertainty. While we do not claim that these experiments enjoy the same level of exogeneity as CEO turnovers, nonetheless they show that the drop in analyst coverage following an increase in uncertainty of firm prospects is indeed general. As additional sources of uncertainty we consider the filing

[10]Under the provisions of the Global Settlement analysts' compensation cannot be based directly or indirectly upon investment banking revenues or input from investment banking personnel.

of a securities class action where the firm is listed as defendant and three industry-specific shocks that increased the uncertainty on the prospects of firms operating in the corresponding industries.

A. *Securities Class Action*

A securities class action is a lawsuit filed by investors who bought or sold a company's securities and claim they suffered economic injury as a result of violations of securities laws. While the filing of a class action has a predictable negative impact on the stock price of the firm (Griffin, Grundfest, and Perino, 2000) the long term effects on the company are quite uncertain. Cheng, Huang, Li, and Lobo (2010) show that on a sample of 1,213 lawsuit filed between 1996 and 2005, the "dismissal rate", i.e., the percentage of cases that were dismissed by the court without any negative consequence for the defendant, was 37%. Comolli and Starykh (2015) report that, on a more recent sample of cases filed and resolved between 2000 and 2014, a motion to dismiss was filed in 95% of the cases. Out of these, the motion was granted in 48% of the cases (in addition to 8% of cases where the plaintiff voluntary dismissed the claim). The authors also show a high degree of variation in the size of the settlement for cases that are not dismissed.

In such a situation, it is fair to assume that investors, especially those that are not involved in the class action, would express a strong demand for firm-specific information. Is the litigation going to affect the long term growth prospects of the firm? Does the drop in market price at the filing date make the stock attractive?

Using a sample of securities class actions filed from 1995 to 2018 we analyze the variation of analyst coverage around the filing date for the defendants compared to a control sample of matched firms. Since the event has a first-order effect on the market price of the stock we limit our matching strategies to those firms for which we can build a control sample with comparable characteristics, including return and turnover in the announcement quarter.

We take class actions data from the Stanford Law School Securities Class Action Clearinghouse, and after matching with our sample we obtain 1,522 events for which we have a known conclusion (dismissal or settlement).

Table VII reports the results of a diff-in-diff analysis where we compare the change in analyst coverage (quarter $t + 4$ minus quarter $t - 4$) for the firms named as defendants in securities class actions to the change in coverage for a control sample of similar observations. Each firm/quarter in the treatment sample is matched with the observation from the same quarter and industry closer for Mahalanobis distance or propensity score based on the complete set of control variables used in our main experiment (among which, turnover and excess return in the announcement quarter). We estimate the model separately for settled and dismissed cases. Results show a drop in analysts between 0.96 and 1.1 units, stronger than what measured in our main experiment. This larger magnitude may be due to the fact that this event has a more directional impact on expectations than the CEO turnover[11]. Figure 4 shows that both our control samples satisfy the parallel trends assumption.

[Place Table VII about here]

[Place Figure 4 about here]

While we cannot claim that this experiment is as strong and exogenous as our main one, we think it's fair to say that it offers a valuable corroboration. We show that coverage tends to drop after a variety of firm-specific events that increase the uncertainty surrounding the company.

B. Industry Shocks

Lastly, we consider the fact that uncertainty surrounding a firm (or a group of firms) may emerge from non-firm-specific events. Here we consider three different industry-related shocks that for a variety of reason make the prospects of firms in the affected industries more uncertain.

The first event that we consider is Hurricane Katrina in August 2005 and its effect on insurance companies. This hurricane is considered by far the most destructive extreme weather event in the US history, with an estimated property damage of 108 b$ (Blake, Landsea, and Gibney, 2011). As a comparison, the second most destructive storm (Sandy, October 2012) caused estimated damages for 50 b$.

[11]It is important to note that we match treatment and control observations by excess return in the announcement quarter so we control for the negative effect on firm prospects anticipated by investors at the time of the filing. Moreover Comolli and Starykh (2015) show that only 12% of cases are resolved (settled or dismissed) within the first 12 months after the filing so it is unlikely that our result is affected by the resolution of the case.

Assessing the impact on individual insurance companies of such an unprecedented amount of potential claims is intrinsically complicated. Analysts would need to know not only the exact exposure of the individual company to rather specific geographical areas but also the specificities of the insured events. In the two years following the disaster, the media reported a number of controversies on contentious damage assessments that spurred a number of lawsuits and class actions.

We argue that in the aftermath of this event, the uncertainty surrounding insurance companies in the US would increase. Analysts covering insurance companies will be pressed to update their estimates while, at the same time, facing an increased uncertainty.

The second event that we consider is the default of Lehman Brothers in September 2008. While we acknowledge that this event had a clearly systemic dimension, it is reasonable to assume that the uncertainty was particularly strong for financial firms and banks. Uncertainty not only about the financial strength of individual institutions but also about future government interventions and possible regulatory changes.

Lastly, we consider the bankruptcy of Enron in December 2001. At the time this was by far the largest bankruptcy in history with 63.4 b$ of pre-filing assets, and came only months after the demise of another energy company (Pacific Gas and Electric Co.) operating in the same market. Subsequent investigation by the Federal Energy Regulatory Commission would link these cases in an industry-wide investigation of what is today commonly known as the "Western Energy Crisis" (Commission et al., 2003). According to later accounts of the fact, it seems that this collapse was largely unanticipated both by analysts and by the public at large (Dyck and Zingales, 2003), leading to the demise of one of the, until then, most reputable auditing firms in the world. We argue that this collapse increased the uncertainty surrounding other companies operating in the same industry and with similar business models.

Here we argue that these events increased the uncertainty surrounding the prospects of affected firms. Since we are dealing with industry-related shocks, this assumption needs empirical verification. We can provide a partial proof by looking at the change in idiosyncratic volatility in the year after the event, compared with the year before. To test whether these events induced shocks in idiosyncratic volatility of the affected firms, we run the following diff-in-diff regression

$$IdiosyncraticVar_{it} = \alpha + \beta Affected_{it} + Quarter_t + Firm_i + \epsilon \tag{2}$$

Where *Affected* is the interaction between the indicator variables for the affected firms and the time fixed effects for the four quarters after the event. We also estimate an alternative version of this model where we replace firm fixed effects with industry fixed effects.

For the Katrina and the Lehman events we define affected companies (insurance companies and banks respectively) based on the Fama and French 49 classification (number 45 and 46 respectively), while for the Enron event we define affected companies those with SIC4 codes in the 4900—4932 range.

Results in Table VIII show that all three crisis events are associated with highly significant increases in idiosyncratic volatility (as defined both by the single index or the 4-Factor model).

[**Place Table VIII about here**]

Now that we have established our exogenous shocks we can observe the evolution of analyst coverage around the three events running a modified version of the diff-in-diff model where we add firm-level controls.

$$Ln(Coverage_{it}) = \alpha + \beta Affected_{it} + \gamma Z_{it} + Quarter_t + Firm_i + \epsilon \tag{3}$$

Results in Table IX show that when we control for other factors affecting coverage decisions we see a clear reduction in coverage associated with our crisis events. The size of the reduction varies across events, but is always relevant: estimated coefficients indicate a drop of 1.1–1.5 analysts after the Enron bankruptcy, 0.3–0.5 analysts after hurricane Katrina and 0.2–0.4 analysts after the Lehman collapse.

[**Place Table IX about here**]

While quite different in spirit from our main experiment, this last analysis offers additional corroboration of our main hypothesis. Financial analysts react to an increase in uncertainty surrounding the prospects of a firm by discontinuing coverage.

VII. Career Concerns and Analysts Characteristics

In the previous paragraph we have provided evidence that the number of analysts following a company drops after an exogenous increase in the uncertainty surrounding the prospects of the company.

Here we analyze possible reasons for this behavior by looking at the individual characteristics of analysts who dropped coverage after the appointment of a new CEO. The analysis will focus on all the analysts who were covering a stock prior to the turnover event (i.e. analysts who issued an EPS forecast in the six months prior to the event) and whose forecast is included in the "IBES Stopped Estimate File" within twelve months of the event.

According to IBES, the Stopped Estimate file contain forecasts that have been withdrawn by the estimator or that have not been updated or confirmed for 180 days. Estimates contained in this file may have been stopped for a variety of undisclosed reasons. For example, a broker may routinely "stop" estimates when the issuing analyst leaves the firm, or an estimate could be dropped and immediately replaced by a new estimate, perhaps issued on behalf of the same broker by a different analyst. We apply a series of filters to reduce our sample to a collection of "meaningful" decisions to interrupt coverage of a firm. Specifically we drop observations where:

- The analyst does not issue any other forecast for the same broker in the year following the coverage interruption. We want to avoid cases where the estimate has been stopped because the analyst has retired or has moved to another broker.
- The broker issues a new forecast for the same company within three months from the interruption. We want to avoid cases where the broker has simply replaced an old estimate with a new one. While brokers can routinely update existing estimates, one could imagine a situation where a broker decides to assign the company to a different analyst, and thus the old estimate is "retired" and a new one is immediately issued. The three months gap ensures

that we are analysing a situation where there is a “significant” interruption of coverage that spans at least one full fiscal quarter.

- The broker does not issue any new forecast in the year following the coverage interruption. We want to avoid situations where a broker drops all its existing forecasts before merging with a rival or leaving the industry.

In all these cases, it is likely that the forecast has been included in the stopped file for reasons different from the conscious decision that we are trying to model here. A natural control sample for this analysis is provided by all the analysts who were covering firms affected by the CEO turnover events and whose forecast is not included in the Stopped Estimate File. These are the analysts who have not discontinued coverage in spite of the increase in uncertainty. Our final sample contains 4,373 stopped estimates (from 2,844 individual analysts) and 42,942 retained estimates (from 9,094 analysts). In our selection criteria we impose that the stopped estimate cannot be followed by a new forecast (from the same broker) within three months. From the length of coverage interruption we see that for 59.5% of the stopped estimates there is no record of a new forecast in the sample period, indicating that the broker has dropped coverage in a permanent way. For the remaining events we observe an interruption shorter than one year in 26.9% of cases, and longer than one year in the remaining 13.6%. When coverage is resumed, the new forecast is issued by the same analyst in 58.3% of the cases and by a different analyst in the remaining 41.7%.

A. Analysts' Characteristics

The literature on herding behavior provides a useful guide to measure reputational and career concerns. Hong et al. (2000) show that analysts who produce erroneous forecasts may face negative career outcomes. Analysts may react to this termination risk by “playing safe”, that is by issuing forecast close to the prevailing consensus. Here we argue that dropping coverage of a firm is an alternative response to the same career and reputational concerns. From the existing literature on herding we extract a number of characteristics related to four areas: a) experience of the analyst, b) workload, c) reputation and d) risk aversion.

All these factors affect the cost of covering the firm: less experienced analyst may have to invest more time to overcome the new uncertainty. Analysts with a higher workload may value their time more and be less inclined to invest copious amount of hours in valuing a single firm. Analysts with a weaker reputation or more risk averse may be less willing to take the higher reputational risk.

Experience

The first three variables are related to the working experience of the analyst and capture the idea, common in the financial literature, that professionals in early stages of their career face stronger reputational risk. Hong et al. (2000) show that inexperienced analysts are more likely to suffer negative career outcomes as a result of inaccurate forecasts. The same phenomenon has been documented in other segments of the financial industry. For example, Chevalier, Ellison, et al. (1999) show that termination risk for mutual fund managers is more performance-sensitive for younger portfolio managers. Clement and Tse (2005), in a slightly different context, show that analysts with less experience (both general and firm-specific) exhibit stronger herding behavior. The authors interpret this result as a signal of stronger reputational concerns. Based on this research we record the following three variables:

- *General Experience* is the time since the first forecast ever issued by the analyst.
- *Firm Experience* is the time since the first forecast the analyst has produced for the specific company.
- *Tenure* is the time since the first forecast of the analyst with the present brokerage house.

Here we expect these three dimensions of professional experience to be associated with lower probability of dropping coverage of a company after a turnover event.

These measures pose a possible empirical challenge as they are, by construction, non-stationary in time. To avoid spurious results we follow Clement and Tse (2005) and Hilary and Hsu (2013) and normalize the variables so that each analyst is measured against his/her other peers covering the same firm. Specifically, we rank all the analysts following a given firm before the CEO turnover according to each raw measure (for example General Experience) and define the normalized measure as:

$$Normalized\ Measure = \frac{(rank - 1)}{(N - 1)} \tag{4}$$

Where N is the number of analysts covering the firm. With this measure the least experienced analyst following a firm will have a score of zero while the most experienced will have a score of one.

Workload The second two variables capture the idea that analysts who have to cover a large number of firms (or firms operating in a large number of different industries) have less time to dedicate to each individual forecast and develop more superficial knowledge of each individual firm. Clement and Tse (2005) show that analysts who cover a large number of firms (or firms in very diverse industries) are more likely to exhibit herding behavior as they know less of any specific firm or industrial context, and thus face higher risk of making costly mistakes. Based on this research we build the following two variables:

- *Companies* is a measure of workload and is defined as the (normalized) number of firms covered by the analyst in the current year.
- *Industries* is a measure of analyst specialization and, to a certain extent, workload, and is defined as the (normalized) number of different industries covered by the analyst in the current year (following the Fama and French 12 categorization).

Our expectation is that analysts covering multiple firms or industries will be more likely to drop coverage of a specific firm following an increase in uncertainty.

Reputation We also build two variables to proxy for analyst reputation. Analysts with a strong reputation are less likely to experience negative career outcomes after a wrong estimate. Jackson (2005) shows that reputation is strongly increasing in the size of the broker and argues that this result *"is consistent with larger brokers employing better analysts and having more resources at their disposal"*. Similarly Clement and Tse (2005) find that brokerage size and past accuracy are associated with lower herding behavior, a sign of lower reputational concerns. Based on this research we build the following two measures:

- *Broker Size* is the (normalized) size of the brokerage firm in terms of analysts employed in the year.

- *Accuracy* is the average relative accuracy of the analyst over his/her entire career.

In order to build this last measure, we follow Hilary and Hsu (2013) and consider, for every firm and fiscal period, the first forecast produced by each analyst covering the firm, rank them from the least to the most accurate (based on the absolute distance from the realized EPS) and calculate the normalized ranking. *Accuracy* is defined as the average ranking over the whole career of the analyst, thus considering it as a time-invariant attribute. We expect analysts working for larger brokerage houses and analysts with stronger track records in terms of accuracy to have a lower probability of dropping coverage of firms with uncertain prospects.

Risk Aversion Finally, we model analysts risk aversion looking at the average relative "boldness" of their forecasts. Several theories link reputational concerns to herding behavior (Scharfstein and Stein, 1990; Trueman, 1994), and Hong et al. (2000) show that bold forecasts increase the likelihood of termination for financial analysts. Here we argue that analysts who on average produce bolder forecasts are less afraid of taking risks (either because of a lower risk aversion or a higher assessment of their own ability) and thus show lower reputational and career concerns.

Following prior research, we define *Boldness* as the absolute distance from the prevailing consensus estimate and we measure the relative attribute with a ranking process similar to the one used to build our accuracy measure.

We expect analysts with lower risk aversion (higher *Boldness*) to be less likely to drop coverage after an exogenous increase of firm uncertainty.

Table X reports the correlation matrix between these analysts' characteristics. The relationships between the variables are consistent with existing literature: analysts working for large brokers and more experienced analysts are more accurate (Lim, 2001; Clement and Tse, 2005) and analysts covering a large number of firms / industries tend to be less accurate and less bold (Clement and Tse, 2005).

[Place Table X about here]

B. Individual characteristics and stopped forecasts

In order to assess whether the decision to drop coverage of "difficult firms" is due to reputational concerns we can look at differences in our proxy variables between analysts whose forecasts have been stopped or not.

Results in Table XI show that, in fact, dropped forecasts come from analysts with lower experience (both general, firm-specific and broker-specific), higher workload, weaker reputation (both in terms of accuracy and size of their employer) and higher risk aversion (measured by the average boldness of their forecasts). All these results are consistent with our hypothesis that the decision to drop coverage of firms whose prospects have become more uncertain due to a crisis event is due to reputational and career concerns: the same variables that, according to existing literature, drive analysts to produce forecasts close to the prevailing consensus (i.e. to herd) also increase the probability that the issued forecasts will be stopped in the aftermath of an exogenous increase of the uncertainty surrounding the firm.

[Place Table XI about here]

From previous research (and from Table X) we know that our proxy variables are correlated, and thus a multivariate analysis of the factors affecting the decision to drop coverage may be more appropriate. We estimate the following logit model (with robust standard errors) where the dependent variable is a dummy equal to one for the dropped forecasts.

$$Pr(Stopped) = logit^{-1}(b_0 + b_1 Experience + b_2 Workload + b_3 Reputation + b_4 Boldness + e) \quad (5)$$

From the results of this estimation, we can see in Table XII that all the findings of our univariate analysis are confirmed: the probability of dropping coverage of a firm after the appointment of a new CEO is higher for inexperienced analysts with a weaker reputation, higher workload and higher risk-aversion. This indicates that reputational concerns significantly affect the probability of dropping coverage when firm prospects become more unpredictable.

[Place Table XII about here]

Since most of our covariates are correlated, quantifying the individual marginal effects can be problematic. Table XIII reports realized frequency of dropped forecasts and average predicted probability of dropping coverage (from model (6) in Table XII) for subsamples of observations with "low" and "high" values of the different covariates, where low and high are defined as belonging to the first and fifth quintile respectively. Among the more relevant results we can see, for example, that the probability of discontinuing coverage is 2% lower for analysts with a long experience covering the firm compared to analysts with limited experience. Given that the unconditional probability of dropping coverage in our sample is 9.2%, this difference is substantial. Similarly, we can see that an analyst with a strong reputation is 2.6% less likely to interrupt coverage than a colleague with weaker reputation. Finally, we can observe that generalists (analysts covering a large number of industries) are 2% more likely to interrupt coverage than specialists.

[Place Table XIII about here]

Overall, the results of our investigation around the behavior of individual financial analysts around CEO turnover events introduced in the previous section shows that the decision to drop the coverage of a firm whose prospects have become more uncertain is affected by a number of factors that, according to previous research, are associated with reputational risk and career concerns.

VIII. Conclusion

In this paper we argue that the lack of an explicit price for sell-side analysis leads to supply shortages in response to shocks to the cost of covering a firm.

With explicit pricing, when the uncertainty surrounding a firm's prospects increases, the price of sell-side reports would also increase as analysts incur higher cost in order to overcome the new uncertainty. When research is paid for by "soft dollars" embedded in trading fees this is not possible, and analysts with less efficient production function stop covering the firm.

Empirically we observe analysts' reaction to an exogenous increase in firm-specific uncertainty due to CEO turnover, and show that in the twelve months after the appointment of the new CEO affected firms experience a drop in coverage of 0.6 units compared to a control sample of similar companies (or 9% of the median coverage). This result is surprising if we consider that a change in CEO should not systematically lead to a decrease in the demand of brokerage services.

In order to address the possible concern that our finding may simply capture analysts' reaction to a decrease in demand for coverage stemming from negative firm prospects we confirm the result on a subsample of turnover events less likely to be associated with bad return prospects for the firm. We also show that our findings become stronger in a regime where analysts have stronger incentives to cover firms with negative prospects (after the Global Analysts Settlement of 2002). Finally we show that stock turnover, as a proxy for demand of sell-side analysis, does not drop after the change in firm leadership.

An additional concern is that our results may capture something specific to CEO turnovers. To assess the general relevance of our findings we consider alternative sources of firm uncertainty. First of all we look at securities class actions and show that, relative to a control sample of similar firms, firms listed as defendants experience a drop in coverage of 0.9 to 1.1 analysts in the four quarters after the filing (irrespective of the outcome of the lawsuit). We then study industry-related shocks that are likely to increase uncertainty for all the firms in the affected industries. We consider Hurricane Katrina for insurance companies, the default of Lehman brothers for financial firms and the collapse of Enron for firms with a similar business model, and show that affected firms experience a drop in coverage (compared to the average unaffected firm) from 0.2 to 1.3 analysts. While we acknowledge that these experiments do not enjoy the same level of exogeneity as the CEO turnovers, they nonetheless show that the drop in analyst coverage following an increase in uncertainty of firm prospects is a general, and is not limited to a specific type of event.

Finally, using data on individual forecasts we show that the probability of dropping coverage after an exogenous increase in uncertainty is higher for less experienced and less reputable analysts, for analysts who cover more firms and more industries and for analysts with higher risk aversion. All these factors have been linked, in previous literature, to higher reputational and career concerns and they can be seen as proxies for the marginal cost of covering the firm.

Taken altogether these results show support for the action taken by European regulators with the introduction of Mifid II: the unbundling of research from trading business will allow a higher degree of price discrimination and a better match between demand and supply in the presence of exogenous variations of firm uncertainty and coverage cost.

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Table I
Variables Definition
The table reports the definition of all the variables used throughout the paper.

Panel A: Firm Characteristics	
Coverage	Natural log of the number of analysts covering a company at the end of each quarter, from the IBES summary file. When employed as a dependent variable in a regression model we consider the natural log of coverage.
ROA	Operating Income Before Depreciation (OIBDPQ) divided by Total Assets (ATQ).
Book Leverage	Ratio of book debt to total asset (ATQ). Book debt is defined as total asset minus book equity. Book equity is defined as total asset (ATQ) less total liabilities (LTQ) and preferred stocks (PSTKQ) plus deferred taxes and convertible debt (TXDITCQ). When preferred stocks is missing it is replaced with the redemption value of preferred stocks (PSTKRQ).
Tobin's Q	Market value of assets divided by book value of assets (ATQ). Market value of assets is calculated as Total Assets (ATQ) minus book value of common equity (CEQQ) and deferred taxes (TXDBQ) plus market value of equity. Market value of equity is defined as the product of market price at the end of the fiscal period (PRCCQ) and the number of common shares outstanding (CSHOQ).
Log (Size)	Natural log of stock market capitalization. Stock market capitalization is calculated as the product of the closing price at the end of the previous quarter and the number of outstanding stocks. From CRSP monthly file.
Excess Return	Average monthly difference between stock returns and returns of the CRSP value-weighted index within each quarter. From CRSP monthly file.
Turnover	Average monthly turnover within each quarter. From CRSP monthly file.
Institutional Ownership	Sum of Institutional holdings from 34f filings in the most recent quarter.

Panel B: Analysts Characteristics	
General Experience	Number of months since the first appearance of the analyst in IBES. This measure is normalized by ranking all the analysts covering a firm in certain period in according to the raw measure and calculating the normalized measure as $Normalized_Measure = \frac{(rank-1)}{(N-1)}$ Where N is the number of analysts covering the firm.
Firm Experience	(Normalized) Number of months since the first time the analyst covered the current firm.
Tenure	(Normalized) Number of months since the first appearance of the analyst in IBES associated with the current employer.
Companies	(Normalized) Number of different companies covered by the analyst during the current year.
Industries	(Normalized) Number of different industries (Fama and French 12) covered by the analyst during the current year.
Broker Size	(Normalized) Number of analysts employed by the broker in the current year.
Accuracy	For every firm and fiscal period we consider the first forecast produced by each analyst, rank them from the less to the most accurate (based on the absolute distance from the realized EPS) and calculate the normalized ranking as $AccuracyRank = \frac{(rank-1)}{(N-1)}$ Where N is the number of analysts covering the firm. Accuracy is calculated as the average *AccuracyRank* over the whole career of the analyst.
Boldness	For every firm and fiscal period we consider the first forecasts produced by each analyst, rank them from the less to the most bold (based on the absolute distance from the average estimate) and calculate the normalized ranking as $BoldnessRank = \frac{(rank-1)}{(N-1)}$ Where N is the number of analysts covering the firm. Boldness is calculated as the average *BoldnessRank* over the whole career of the analyst.

Table II
Descriptive statistics.
The table reports descriptive statistics for our main and control variables (described in Table I). The sample includes quarterly observations of US listed firms, from 1980–2018, with market capitalization greater than 100 m$ and stock price greater than 1$. Analysis is limited to firms listed on the NISE/AME/NASDAQ with CRSP share code 10 or 11, quarterly financial statements available in Compustat, covered by at least one analyst in IBES and with institutional ownership data available in Thompson Reuters CDA Spectrum (file 13f). Panel A contain mean and standard deviation for the different variable of interest calculated over the entire sample and over the subsample of observations (firm/quarter) when Execucomp reports the appointment of a new CEO. The last column reports the value of the t-statistic for a test on the equality of the mean value between the subsample of CEO Turnovers versus the complementary subsample. Panel B reports distribution of CEO Turnover event across industries (Fama and French 12) and time. ***, ** and * indicate significance at the 1%, 5% and 10% respectively.

Panel A: Sample Descriptive Statistics

	Entire Sample		**CEO Turnover**		
	N. of obs: 288,868		**N. of obs: 4,583**		
	Mean	**S. D.**	**Mean**	**S. D.**	**T- Stat**
Size	4,440	18,548	7,435	25,545	(8.033)***
Turnover	14.0%	15.2%	18.4%	17.3%	(17.424)***
Inst. Holdings	56.5%	27.8%	66.9%	24.1%	(29.412)***
Tobin's Q	1.707	1.200	1.758	1.194	(2.917)***
ROA	2.9%	3.1%	3.1%	3.0%	(6.46)***
Book Leverage	54.2%	23.7%	52.6%	21.6%	(-5.053)***
Excess Return	0.211%	6.454%	-0.121%	6.695%	(-3.386)***
Coverage	9.0	7.6	11.3	8.2	(18.943)***

Panel B: CEO Turnover Distribution

Industry	**N. of Events**	**Period**	**N. of Events**
Consumer non Durables	257	1980 - 1982	27
Consumer Durables	125	1983 - 1985	49
Manufacturing	610	1986 - 1988	129
Energy	196	1989 - 1991	207
Chemicals	157	1992 - 1994	323
Hi-Tech	814	1995 - 1997	420
Telecommunications	94	1998 - 2000	531
Utilities	315	2001 - 2003	526
Distribution	605	2004 - 2006	584
Healthcare	288	2007 - 2009	516
Finance	577	2010 - 2012	434
Other	545	2013 - 2015	512
		2016 - 2017	325

Table III
Change in Coverage around CEO Turnovers
The table reports results for a diff-in-diff estimation of the change in analyst coverage around CEO turnover events. The variable of interest is defined as the change between the number of analysts following a firm four quarters after and before the appointment of the new CEO. Each treated observation is matched with a control observation from the same quarter and the same industry (Fama and French 12). Among the possible candidates we choose the observation that is a) closest by size, b) closest by pre-event coverage, c) closest by propensity score estimated with a probit model on a series of firm level control variables described in Table I (Log-Size, Turnover, Institutional Ownership, Tobin's Q, ROA, Leverage, Excess Return and Log-Coverage), d) closest by Mahalanobis distance on the same firm-level control variables. In Panel B we add the condition that the matched observation has to belong to the same market capitalization quartile and report the results for the four subsamples of firms. The last column reports the t-statistic for the significance of the diff-in-diff estimator. . ***, ** and * indicate significance at the 1%, 5% and 10% respectively.

Panel A: Entire Sample					
	N. of Obs.	**Treatment**	**Control**	**Diff-in-Diff**	**t-stat**
Closest by Size					
Δ Coverage	4,567	-0.299	0.361	-0.660	(-8.592)***
S. E.		(0.060)	(0.057)	(0.077)	
Closest by Coverage					
Δ Coverage	4,567	-0.299	0.311	-0.610	(-9.519)***
S. E.		(0.060)	(0.014)	(0.064)	
Propensity Score					
Δ Coverage	4,567	-0.299	0.314	-0.613	(-7.711)***
S. E.		(0.060)	(0.059)	(0.079)	
Mahalanobis Distance					
Δ Coverage	4,567	-0.299	0.144	-0.442	(-6.201)***
S. E.		(0.060)	(0.056)	(0.071)	

[Panel B follows in next page]

[Table description and Panel A in previous page]

Panel B: Sample Divided by Market Capitalization Quartiles					
	N. of Obs.	**Treatment**	**Control**	**Diff-in-Diff**	**t-stat**
Closest by Size					
Small	472	-1.085	-0.407	-0.678	(-4.198)***
Q2	1,025	-0.606	0.204	-0.810	(-5.996)***
Q3	1,339	-0.038	0.689	-0.727	(-5.643)***
Large	1,731	-0.104	0.422	-0.526	(-3.525)***
Closest by Coverage					
Small	472	-1.085	-0.493	-0.592	(-4.307)***
Q2	1,025	-0.606	0.068	-0.673	(-5.757)***
Q3	1,339	-0.038	0.671	-0.710	(-5.877)***
Large	1,731	-0.104	0.604	-0.708	(-5.467)***
Propensity Score					
Small	472	-1.085	-0.682	-0.403	(-2.307)**
Q2	1,025	-0.606	0.153	-0.759	(-5.619)***
Q3	1,339	-0.038	0.591	-0.630	(-4.377)***
Large	1,731	-0.104	0.630	-0.734	(-4.945)***
Mahalanobis Distance					
Small	472	-1.085	-0.761	-0.323	(-1.938)*
Q2	1,025	-0.606	0.020	-0.626	(-4.856)***
Q3	1,339	-0.038	0.411	-0.449	(-3.482)***
Large	1,731	-0.104	0.266	-0.370	(-2.755)***

Table IV
Multivariate diff-in-diff estimation

The table reports results for diff-in-diff estimations of the causal effect of CEO change on analyst coverage. The dependent variable is (the natural logarithm of) the number of analysts following a firm at the end of each quarter. The main variable of interest ($NewCEO$) is a dummy variable equal to one if the firm has appointed a new CEO in the last three years. All models include firm and time fixed effects, models (2), (4) and (6) also include firm level control variables described in Table I. Models (3) and (4) also include interactions between all the dependent variables and four indicator variables for size quartiles. Models (5) and (6) report the result of the second stage OLS from the two stage model of Bertrand et al. (2004). Standard Errors are clustered at the firm level. T-Stats in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% respectively.

	OLS		**OLS with Interaction**		**Bertrand et al. (2004)**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Constant	1.305***	-0.505***	1.002***	-0.504***	0.011**	0.004
	(43.394)	(-11.245)	(35.907)	(-7.350)	(2.191)	(0.919)
NewCEO	-0.045***	-0.017***			-0.062***	-0.023***
	(-6.059)	(-2.887)			(-8.594)	(-4.104)
NewCEO x Size 1			-0.074***	-0.045**		
			(-3.313)	(-2.223)		
NewCEO x Size 2			-0.040***	-0.035***		
			(-2.741)	(-2.698)		
NewCEO x Size 3			-0.024**	-0.018*		
			(-2.159)	(-1.765)		
NewCEO x Size 4			-0.009	-0.001		
			(-1.311)	(-0.130)		
Firm Controls	N	Y	N	Y	N	Y
Time FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
N	288,868	288,868	288,868	288,868	5,274	5,274
adj. R-sq	0.753	0.818	0.791	0.821	0.014	0.003

Table V
Exogenous CEO Turnovers
The table reports results for a diff-in-diff estimation of the change in analyst coverage around CEO turnovers for a subsample of events characterized by a higher likelihood of exogeneity. In Panel A we consider events with: a) Pre-event idiosyncratic volatility below industry median; b) Pre-event equity return above industry median; c) Pre-event ROA above industry median; d) All three of the previous conditions; e) Absence of managerial shakeup in the three years after the event; f) Age of the outgoing CEO above 65 years. In Panel B we consider events with: a) Post-event idiosyncratic volatility below industry median; b) Post-event equity return above industry median; c) Post-event ROA above industry median; d) All three of the previous conditions. The variable of interest is defined as the change between the number of analysts following a firm four quarters after and before the appointment of the new CEO. Each treated observation is matched with a control observation from the same quarter and the same industry (Fama and French 12). Among the possible candidates we choose the observation that is a) closest by size, b) closest by pre-event coverage, c) closest by propensity score estimated with a probit model on a series of firm level control variables described in Table 1 (Log-Size, Turnover, Institutional Ownership, Tobin's Q, ROA, Leverage, Excess Return and Log-Coverage), d) closest by Mahalanobis distance on the same firm-level control variables. The last column reports the t-statistic for the significance of the diff-in-diff estimator. ***, ** and * indicate significance at the 1%, 5% and 10% respectively.

[Table in next page]

[Table description in previous page]

	N. of Obs.	Treatment	Control	Diff-in-Diff	t-stat
Panel A					
Closest by Size					
Ind. Adj. IVOL < 0	3,245	-0.240	0.231	-0.471	(-5.2)***
Ind. Adj. Return > 0	2,477	0.224	0.791	-0.568	(-5.441)***
Ind. Adj. ROA > 0	2,970	0.003	0.501	-0.498	(-5.163)***
All Three	1,456	0.151	0.600	-0.449	(-3.294)***
No Shakeup	901	0.262	0.466	-0.204	(-1.243)
Age >= 65	709	-0.340	0.292	-0.632	(-3.699)***
Closest by Coverage					
Ind. Adj. IVOL < 0	3,245	-0.240	0.260	-0.500	(-6.577)***
Ind. Adj. Return > 0	2,477	0.224	0.717	-0.493	(-5.575)***
Ind. Adj. ROA > 0	2,970	0.003	0.478	-0.476	(-5.857)***
All Three	1,456	0.151	0.669	-0.517	(-4.262)***
No Shakeup	901	0.262	0.578	-0.316	(-2.359)**
Age >= 65	709	-0.340	0.190	-0.530	(-3.59)***
Propensity Score					
Ind. Adj. IVOL < 0	3,245	-0.240	0.219	-0.459	(-5.048)***
Ind. Adj. Return > 0	2,477	0.224	0.618	-0.394	(-3.684)***
Ind. Adj. ROA > 0	2,970	0.003	0.544	-0.542	(-5.415)***
All Three	1,456	0.151	0.627	-0.476	(-3.513)***
No Shakeup	901	0.262	0.374	-0.112	(-0.668)
Age >= 65	709	-0.340	0.189	-0.529	(-2.81)***
Mahalanobis Distance					
Ind. Adj. IVOL < 0	3,245	-0.240	0.078	-0.317	(-3.794)***
Ind. Adj. Return > 0	2,477	0.224	0.554	-0.330	(-3.501)***
Ind. Adj. ROA > 0	2,970	0.003	0.413	-0.410	(-4.635)***
All Three	1,456	0.151	0.522	-0.371	(-3.075)***
No Shakeup	901	0.262	0.604	-0.342	(-2.222)**
Age >= 65	709	-0.340	0.018	-0.358	(-2.116)**

[Panel B in next page]

[Table description and Panel A in previous pages]

Panel B					
	N. of Obs.	**Treatment**	**Control**	**Diff-in-Diff**	**t-stat**
Closest by Size					
Ind. Adj. IVOL < 0	3,305	-0.183	0.320	-0.503	(-5.662)***
Ind. Adj. Return > 0	2,724	-0.181	0.392	-0.573	(-5.923)***
Ind. Adj. ROA > 0	2,831	0.025	0.473	-0.448	(-4.651)***
All Three	1,491	0.038	0.459	-0.421	(-3.251)***
Closest by Coverage					
Ind. Adj. IVOL < 0	3,305	-0.183	0.343	-0.526	(-6.973)***
Ind. Adj. Return > 0	2,724	-0.181	0.442	-0.623	(-7.412)***
Ind. Adj. ROA > 0	2,831	0.025	0.508	-0.483	(-5.797)***
All Three	1,491	0.038	0.485	-0.448	(-3.839)***
Propensity Score					
Ind. Adj. IVOL < 0	3,305	-0.183	0.369	-0.552	(-6.01)***
Ind. Adj. Return > 0	2,724	-0.181	0.306	-0.487	(-4.804)***
Ind. Adj. ROA > 0	2,831	0.025	0.544	-0.519	(-5.201)***
All Three	1,491	0.038	0.455	-0.418	(-3.08)***
Mahalanobis Distance					
Ind. Adj. IVOL < 0	3,305	-0.183	0.138	-0.321	(-3.803)***
Ind. Adj. Return > 0	2,724	-0.181	0.265	-0.446	(-4.945)***
Ind. Adj. ROA > 0	2,831	0.025	0.448	-0.423	(-4.684)***
All Three	1,491	0.038	0.371	-0.333	(-2.766)***

Table VI

Change in Coverage and CEO Turnovers before and after the Global Settlement

The table reports results for a diff-in-diff estimation of the change in analyst coverage around CEO turnover events. The analysis is performed separately for events occurring before and after the Global Analyst Research Settlement of December 2002 and the last two columns test for the difference between the two subsamples. The variable of interest is defined as the change between the number of analysts following a firm four quarters after and before the appointment of the new CEO. Each treated observation is matched with a control observation from the same quarter and the same industry (Fama and French 12). Among the possible candidates we choose the observation that is a) closest by size, b) closest by pre-event coverage, c) closest by propensity score estimated with a probit model on a series of firm level control variables described in Table 1 (Log-Size, Turnover, Institutional Ownership, Tobin's Q, ROA, Leverage, Excess Return and Log-Coverage), d) closest by Mahalanobis distance on the same firm-level control variables. The last column reports the t-statistic for the significance of the diff-in-diff estimator. ***, ** and * indicate significance at the 1%, 5% and 10% respectively.

	Before Global Settlement			**After Global Settlement**			**Before - After**	
	N. of Obs.	**Diff-in-Diff**	**t-stat**	**N. of Obs.**	**Diff-in-Diff**	**t-stat**	**Diff-in-Diff-in-Diff**	**t-stat**
Closest by Size								
Δ Coverage	2,544	-0.485	(-5.048)***	2,023	-0.878	(-7.084)***	0.393	(2.504)
S. E.		(0.096)			(0.124)		(0.157)	
Closest by Coverage								
Δ Coverage	2,544	-0.523	(-6.279)***	2,023	-0.719	(-7.215)***	0.196	(1.506)
S. E.		(0.083)			(0.100)		(0.130)	
Propensity Score								
Δ Coverage	2,544	-0.578	(-5.503)***	2,023	-0.563	(-4.632)***	-0.015	(-0.092)
S. E.		(0.105)			(0.122)		(0.161)	
Mahalanobis Distance								
Δ Coverage	2,544	-0.384	(-4.124)***	2,023	-0.484	(-4.383)***	0.100	(0.695)
S. E.		(0.093)			(0.110)		(0.144)	

Table VII
Change in Coverage around Securities Class Actions
The table reports results for a diff-in-diff estimation of the change in analyst coverage around Securities class action filings. Th estimation is conducted separately for lawsuits that have been subsequently dismissed (Panel A) and lawsuits for which the parties have reached a settlement (Panel B). The variable of interest is defined as the change between the number of analysts following a firm four quarters after and before the first filing of a class action. Each treated observation is matched with a control observation from the same quarter and the same industry (Fama and French 12). Among the possible candidates we choose the observation that is a) closest by propensity score estimated with a probit model on a series of firm level control variables described in Table 1 (Log-Size, Turnover, Institutional Ownership, Tobin's Q, ROA, Leverage, Excess Return and Log-Coverage), b) closest by Mahalanobis distance on the same firm-level control variables. The last column reports the t-statistic for the significance of the diff-in-diff estimator. ***, ** and * indicate significance at the 1%, 5% and 10% respectively.

Panel A: Dismissed Lawsuits					
	N. of Obs.	**Treatment**	**Control**	**Diff-in-Diff**	**t-stat**
Propensity Score					
Δ Coverage	754	-0.318	0.639	-0.958	(-4.02)***
S. E.		(0.171)	(0.171)	(0.238)	
Mahalanobis Distance					
Δ Coverage	754	-0.318	0.759	-1.078	(-5.216)***
S. E.		(0.171)	(0.158)	(0.207)	

Panel B: Settled Lawsuits					
	N. of Obs.	**Treatment**	**Control**	**Diff-in-Diff**	**t-stat**
Propensity Score					
Δ Coverage	768	-0.853	0.229	-1.082	(-4.422)***
S. E.		(0.180)	(0.166)	(0.245)	
Mahalanobis Distance					
Δ Coverage	768	-0.853	0.130	-0.983	(-4.397)***
S. E.		(0.180)	(0.171)	(0.223)	

Table VIII
Idiosyncratic risk around industry shocks
The table reports results for diff-in-diff estimations of the causal effect of three industry shocks on firm idiosyncratic volatility. The dependent variable is firm idiosyncratic volatility estimated every quarter on daily returns using the Single Index Model or the 4-Factors Model. The main variable of interest (*Affected*) is a dummy variable equal to one if the firm is in an industry affected by the shock and the period is in the four quarters after the event. In every regression new consider the four quarters before and after the event. All models include industry and time fixed effects in Panel A and time and firm fixed effects in Panel B. Standard Errors are clustered at the firm level. T-Stats in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% respectively.

Panel A: Industry Fixed Effects

	Enron		**Katrina**		**Lehman**	
	Single Index Model	**4-Factor Model**	**Single Index Model**	**4-Factor Model**	**Single Index Model**	**4-Factor Model**
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.084***	0.076***	0.043***	0.038***	0.210***	0.183***
	(40.253)	(39.969)	(43.244)	(41.247)	(42.684)	(41.149)
Affected	0.075***	0.070***	0.007**	0.007**	0.050***	0.044***
	(6.214)	(6.583)	(2.112)	(2.225)	(4.975)	(4.863)
Time FE	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y
N	18,852	18,852	19,540	19,540	19,838	19,838
adj. R-sq	0.238	0.225	0.11	0.105	0.184	0.159

Panel B: Firm Fixed Effects

	Enron		**Katrina**		**Lehman**	
	Single Index Model	**4-Factor Model**	**Single Index Model**	**4-Factor Model**	**Single Index Model**	**4-Factor Model**
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.090***	0.082***	0.042***	0.038***	0.213***	0.186***
	(48.176)	(47.923)	(53.076)	(50.387)	(56.479)	(54.347)
Affected	0.051***	0.049***	0.006*	0.006*	0.058***	0.050***
	(5.313)	(5.665)	(1.734)	(1.831)	(5.210)	(4.955)
Time FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
N	18,852	18,852	19,540	19,540	19,838	19,838
adj. R-sq	0.633	0.623	0.479	0.471	0.471	0.458

Table IX
Coverage around industry shocks
The table reports results for diff-in-diff estimations of the causal effect of three industry shocks on analyst coverage. The dependent variable is (the natural logarithm of) the number of analysts following a firm at the end of each quarter. The main variable of interest (*Affected*) is a dummy variable equal to one if the firm is in an industry affected by the shock and the period is in the four quarters after the event. In every regression we consider the four quarters before and after the event. Models are estimated with and without the firm-level controls detailed in Table I. All models include industry and time fixed effects in Panel A and time and firm fixed effects in Panel B. Standard Errors are clustered at the firm level. T-Stats in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% respectively.

Panel A: Industry Fixed Effects						
	Enron		**Katrina**		**Lehman**	
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	1.633***	-1.171***	1.751***	-1.413***	1.767***	-0.662***
	(89.651)	(-26.327)	(102.141)	(-26.437)	(105.122)	(-12.924)
Affected	-0.339***	-0.267***	-0.084	-0.056*	-0.068**	-0.084***
	(-6.605)	(-5.871)	(-1.635)	(-1.648)	(-2.249)	(-3.737)
Time FE	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y
Firm Controls	N	Y	N	Y	N	Y
N	18,852	18,852	19,540	19,540	19,838	19,838
adj. R-sq	0.117	0.661	0.089	0.6	0.102	0.57
Marginal Effect	-1.47	-1.20	-0.46	-0.31	-0.31	-0.40
Panel B: Firm Fixed Effects						
	Enron		**Katrina**		**Lehman**	
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	1.595***	0.399***	1.746***	-0.120	1.738***	0.238
	(242.474)	(3.033)	(318.850)	(-0.537)	(295.105)	(1.510)
Affected	-0.284***	-0.251***	-0.088***	-0.099***	-0.047**	-0.044**
	(-6.222)	(-5.591)	(-3.725)	(-4.005)	(-2.120)	(-2.110)
Time FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Firm Controls	N	Y	N	Y	N	Y
N	18,852	18,852	19,540	19,540	19,838	19,838
adj. R-sq	0.9	0.907	0.917	0.921	0.889	0.897
Marginal Effect	-1.26	-1.13	-0.48	-0.54	-0.20	-0.18

Table X
Correlation among analysts' characteristics
The table report the linear correlation coefficients among the analysts characteristics described in Table I.

	General Experience	Firm Experience	Tenure	Companies	Industries	Broker Size	Accuracy	Boldness
General Experience	1	0.458	0.542	0.226	0.124	-0.073	0.016	-0.008
Firm Experience	0.458	1	0.437	0.170	0.074	-0.013	0.039	0.009
Tenure	0.542	0.437	1	0.281	0.144	0.095	0.012	0.011
Companies	0.226	0.170	0.281	1	0.432	0.035	-0.059	-0.010
Industries	0.124	0.074	0.144	0.432	1	-0.098	-0.076	-0.045
Broker Size	-0.073	-0.013	0.095	0.035	-0.098	1	0.075	0.135
Accuracy	0.016	0.039	0.012	-0.059	-0.076	0.075	1	-0.308
Boldness	-0.008	0.009	0.011	-0.010	-0.045	0.135	-0.308	1

Table XI
Analysts' characteristics and stopped estimates. Univariate analysis
The table reports the average value for a series of analysts' characteristics (defined in Table I) for all the analysts covering the firms affected by the CEO turnover events. The sample is split in two according to whether the analyst has stopped coverage of the firm in the twelve months following the appointment of the new CEO. The table also provides a t-test for the difference between the two samples. ***, ** and * indicate significance at the 1%, 5% and 10% respectively.

	Stopped	**Non Stopped**	**Difference**	**t-stat**	**Expected Sign**
N. of Obs.	4373	42942			
General Experience	0.490	0.501	-0.011	(-2.126)**	-
	(0.005)	(0.002)	(0.005)		
Firm Experience	0.475	0.503	-0.028	(-5.447)***	-
	(0.005)	(0.002)	(0.005)		
Tenure	0.489	0.501	-0.012	(-2.412)**	-
	(0.005)	(0.002)	(0.005)		
Companies	0.504	0.500	0.005	(0.903)	+
	(0.005)	(0.002)	(0.005)		
Industries	0.514	0.499	0.016	(3.098)***	+
	(0.005)	(0.002)	(0.005)		
Broker Size	0.477	0.502	-0.025	(-4.939)***	-
	(0.005)	(0.002)	(0.005)		
Accuracy	0.497	0.501	-0.003	(-6.227)***	-
	(0.000)	(0.000)	(0.001)		
Boldness	0.499	0.499	-0.001	(-0.812)	-
	(0.001)	(0.000)	(0.001)		

Table XII

Analysts' characteristics and stopped estimates. Multivariate analysis

The table reports the result of a logit model where the dependent variable is a dummy equal to one if an EPS forecast has been stopped in the twelve months following the appointment of the new CEO. The sample contains all the EPS forecasts issued in the six months prior to the turnover event. All the independent variables are described in Table I. T-Stats in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% respectively.

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Constant	1.560***	1.527***	1.559***	1.618***	1.540***	1.528***
	(3.864)	(3.804)	(3.854)	(4.047)	(3.848)	(3.809)
General Experience	-0.131***			-0.024	-0.022	-0.025
	(-2.807)			(-0.421)	(-0.389)	(-0.447)
Firm Experience		-0.271***		-0.266***	-0.262***	-0.264***
		(-5.795)		(-4.972)	(-4.915)	(-4.937)
Tenure			-0.115**	0.013	0.014	0.008
			(-2.408)	(0.222)	(0.250)	(0.141)
Companies	0.019	0.036	0.020	0.086*		0.039
	(0.371)	(0.701)	(0.376)	(1.773)		(0.725)
Industries	0.116**	0.114**	0.117**		0.129***	0.114**
	(2.271)	(2.228)	(2.294)		(2.780)	(2.237)
Broker Size	-0.205***	-0.202***	-0.184***	-0.217***	-0.202***	-0.205***
	(-4.274)	(-4.231)	(-3.875)	(-4.508)	(-4.202)	(-4.249)
Accuracy	-2.880***	-2.745***	-2.907***	-2.803***	-2.750***	-2.739***
	(-5.917)	(-5.681)	(-5.966)	(-5.809)	(-5.705)	(-5.668)
Boldness	-0.797*	-0.747*	-0.807*	-0.786*	-0.748*	-0.744*
	(-1.909)	(-1.798)	(-1.926)	(-1.897)	(-1.805)	(-1.794)
Firm Controls	Y	Y	Y	Y	Y	Y
N	47,315	47,315	47,315	47,315	47,315	47,315
pseudo R-sq	0.028	0.029	0.028	0.028	0.028	0.029

Table XIII
Marginal Effects

The table reports the realized frequency of dropped estimates and the average predicted probability (from logit model (6) in Table 12) for analysts with low and high levels of each relevant characteristic (defined as belonging to the bottom and top quintile respectively). T-Stat and Z-Stat in parenthesis. ***, ** and * indicate significance at the 1%, 5% and 10% respectively.

	Realized Frequency				**Forecasted Probability**			
	Low	**High**	**Difference**	**z-stat**	**Low**	**High**	**Difference**	**t-stat**
General Experience	9.72%	9.12%	-0.60%	(-1.41)*	10.03%	9.11%	-0.91%	(-15.427)***
Firm Experience	10.53%	8.54%	-1.99%	(-4.645)***	10.51%	8.44%	-2.07%	(-35.588)***
Tenure	9.84%	8.68%	-1.16%	(-2.745)***	10.00%	9.00%	-1.00%	(-16.781)***
Companies	9.70%	10.01%	0.32%	(0.728)	9.48%	9.76%	0.28%	(4.698)***
Industries	9.31%	10.62%	1.32%	(3.023)***	9.01%	10.15%	1.14%	(19.473)***
Broker Size	10.89%	8.93%	-1.95%	(-4.488)***	10.51%	8.52%	-1.99%	(-34.186)***
Accuracy	10.87%	8.29%	-2.58%	(-6.03)***	10.62%	8.27%	-2.35%	(-40.584)***
Boldness	9.74%	9.42%	-0.33%	(-0.763)	9.60%	9.03%	-0.57%	(-9.863)***

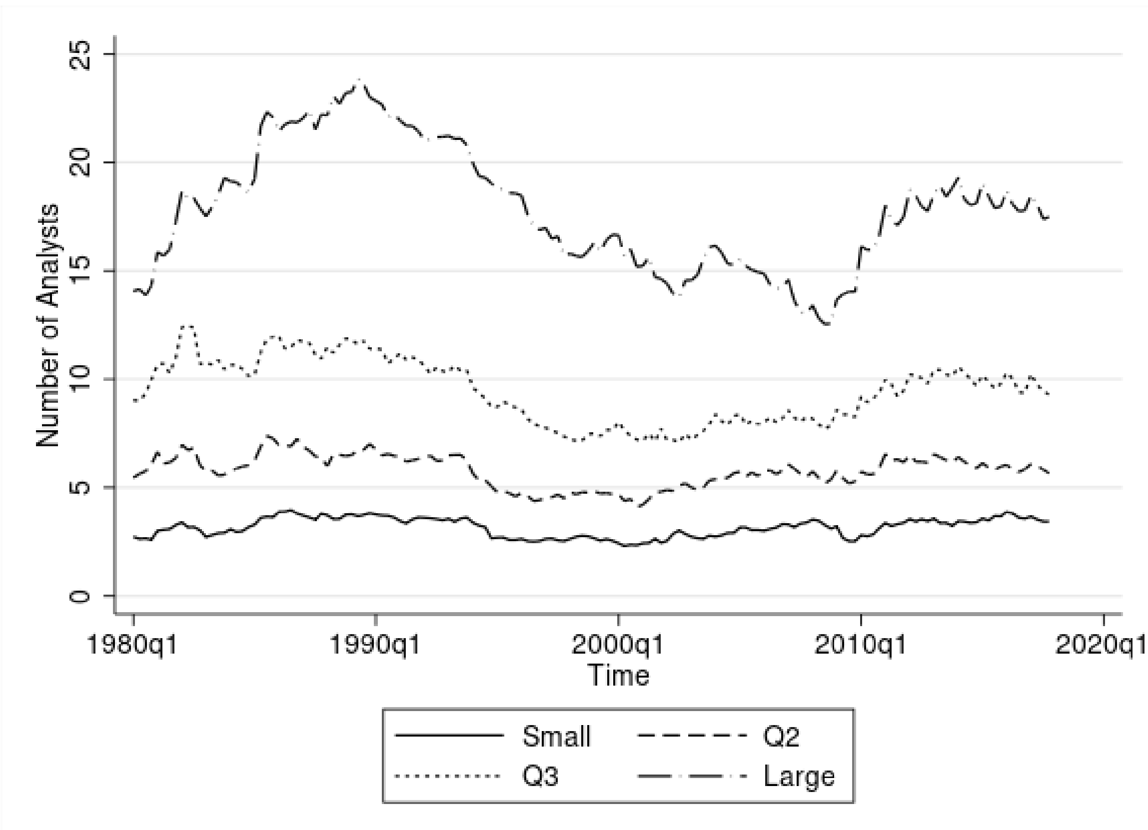

Figure 1. Average Coverage in time. The figure shows the average number of analysts following the firms in our sample grouped by market capitalization quartile.

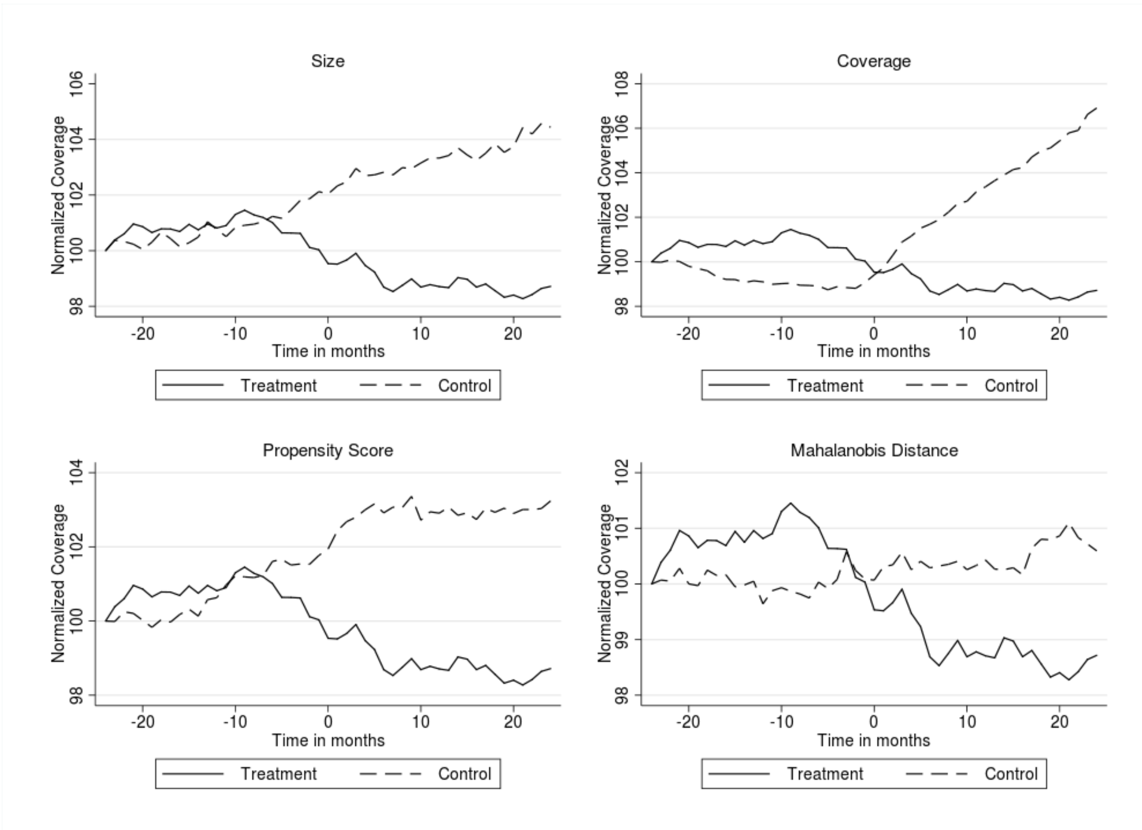

Figure 2. Average Coverage around CEO Turnover Events. The figure shows the average number of analysts following the firms in our treatment and four different control samples in the 48 months around the appointment of the new CEO. For both groups the average coverage is normalized to 100 at the beginning of the period.

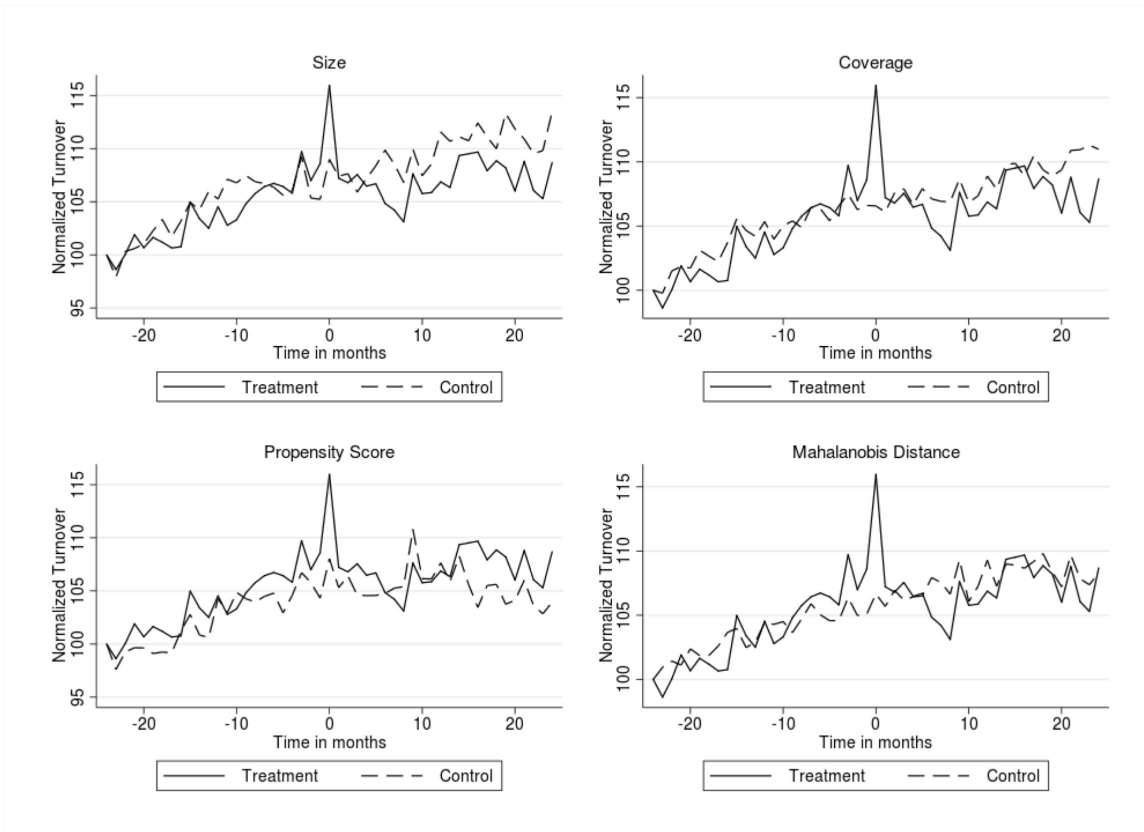

Figure 3. Average Stock Turnover around CEO Turnover Events. The figure shows the average stock turnover in our treatment and four different control samples in the 48 months around the appointment of the new CEO. For both groups the average coverage is normalized to 100 at the beginning of the period.

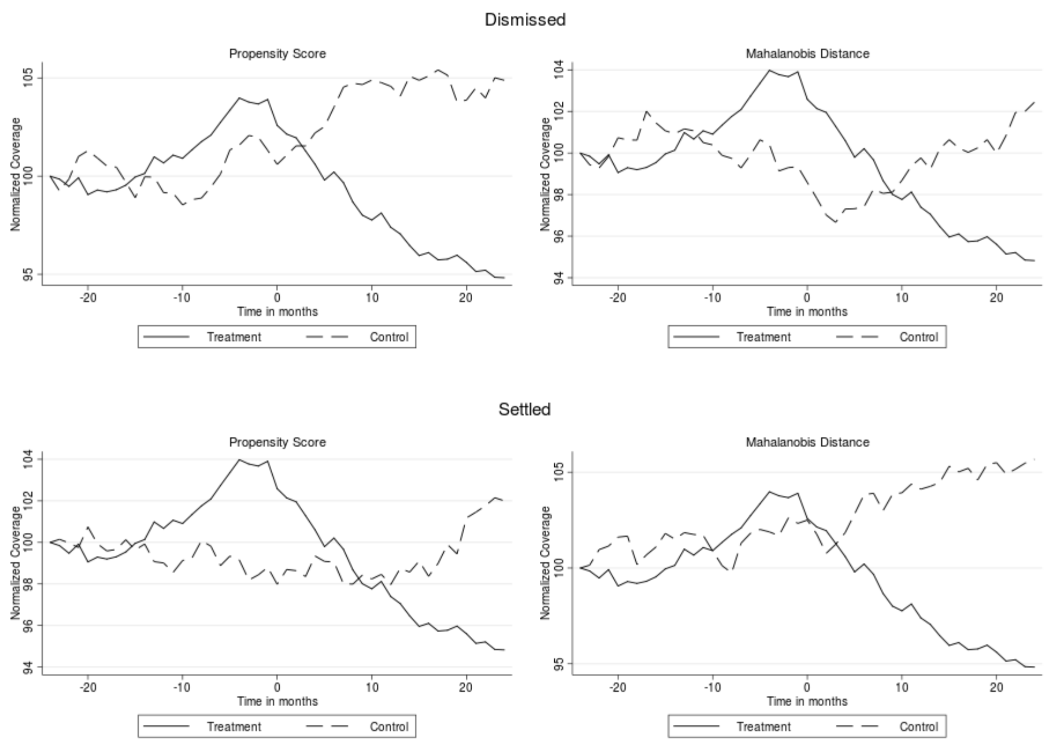

Figure 4. Average Coverage around filings of securities class actions. The graph shows the average number of analysts following the firms in our treatment and two different control samples in the 48 months around the filing of a class action. For both groups the average coverage is normalized to 100 at the beginning of the period.