Causal Effect of Analyst Following on Corporate Social Responsibility

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Comments welcome.

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Abstract

I examine the influence of sell-side financial analysts on corporate social responsibility (CSR), and find that firms with greater analyst coverage tend to be less socially responsible. To establish causality, I employ a difference-in-differences (DiD) technique, using brokerage closures and mergers as exogenous shocks to analyst coverage, as well as an instrumental variables approach. Both identification strategies suggest that analyst coverage has a negative causal effect on CSR. My findings are consistent with the view that spending on CSR is a manifestation of agency problem, and that financial analysts exert pressure on managers to cut back such spending, which is wasteful for shareholders.

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1. Introduction

The term corporate social responsibility (CSR) has gained prominence in the business world in the past few decades. A growing number of firms, especially large public firms, spend significant time and resources in promoting their commitment to the well-being of the greater community, the environment, and other stakeholders, beyond their legal obligations. For instance, Apple's 2011 supplier responsibility report states, "Apple is committed to driving the highest standards of social responsibility throughout our supply base. We require that our suppliers provide safe working conditions, treat workers with dignity and respect, and use environmentally responsible manufacturing processes wherever Apple products are made." Similarly, Microsoft's employee giving campaign has donated over $1 billion from employee contributions with an equal amount contributed by the company, and Google's "Don't be evil" policy includes a promise to direct 1% of its profits to philanthropic purposes.[1] Furthermore, many companies produce voluntary CSR reports, and trillions of dollars of professionally managed money is invested in socially responsible funds.[2]

Why do firms engage in CSR? There are two views on this. One view is that CSR increases firm value because doing good is good for business. This view is supported by empirical evidence that CSR activities increase firm value by building customer loyalty and reputation among key stakeholders (see, e.g., Servaes and Tamayo (2014), Elfenbein, Fisman and McManus (2012) and List (2006)). Deng, Kang and Low (2013) find that acquirers with high CSR ratings experience higher announcement returns and better post-merger performance arguably because these firms' reputation helps them retain key stakeholders after the merger. Servaes and Tamayo (2014) find a positive relation between CSR and firm value among firms with higher advertising expenses

[1] Other examples include Intel's contribution of $100 million for global education programs and energy conservation, GE's $160 million contribution for community and employee philanthropic program and a commitment of billions of dollars for developing eco-friendly products, and CVS Pharmacy's decision to stop selling cigarettes at its retail stores which would result in an estimated loss of about $2 billion in sales per year (see, Hong, Kubik, and Scheinkman (2012), and Cheng, Hong and Shue (2014)).

[2] A 2012 report by Sustainable and Responsible Investing Trends in the United States says ,"$3.31 trillion in US-domiciled assets at year-end 2011 held by 443 institutional investors, 272 money managers and 1,043 community investment institutions that apply various environmental, social and governance (ESG) criteria in their investment analysis and portfolio selection" (http://www.ussif.org/files/publications/12_trends_exec_summary.pdf)

consistent with the idea that CSR activities, if communicated effectively, create value by increasing customer loyalty. Survey evidence also finds that people's willingness to buy from, recommend, work for and invest in a company is guided by a company's image, many aspects of which relate to CSR (see Reputation Institute (2013)). Kecskés, Mansi, and Nguyen (2013) find that CSR benefits shareholders of firms with more long-term institutional investors.

The second view considers CSR as a manifestation of an agency problem. In an op-ed article, Milton Friedman argued that the only responsibility of corporations is to increase profits, and 'socially responsible' managers, who are hired by shareholders to work for them, act as *public* employees when they spend shareholders' money on CSR.[3] Some recent studies support this view by showing that CSR does not contribute to shareholders' interests but may serve managers' personal interests. For example, Cheng, Hong and Shue (2014) find that firms with higher managerial ownership and better internal governance mechanisms are less likely to engage in CSR, suggesting that managers do good with other people's money. Masulis and Reza (2014) also arrive at a similar agency-based conclusion by finding that CEOs personally gain from corporate charitable giving because most of the giving goes to CEO-affiliated charities, especially when CEOs have little ownership stakes in their firms. Di Guili and Kostovetsky (2014) uncover a behavioral explanation of CSR by showing that political leaning of key employees and directors significantly affects CSR, which, in turn, hurts firm performance.

Despite a large literature, the debate on whether CSR is beneficial to shareholders or is an agency problem seems far from settled. Endogeneity issues compound the problem of establishing a causal link between CSR and firm performance, and lack of strong identification strategies limits the extent to which the results from some of the studies can be causally interpreted. Hong, Kubik and Scheinkman (2012) illustrate this issue by showing that financial constraints, which are often difficult to observe, can serve as an omitted variable in the relation between CSR and performance. They argue that a positive relation between profitability and CSR is more likely to be a result of higher profitability leading to more corporate goodness rather than the other way round. In this paper, I attempt to alleviate some of these concerns by examining firms' response to exogenous changes in the intensity of governance to test whether CSR is driven by agency issues. I ask a

[3] *The New York Times Magazine*, September 13, 1970.

simple question: how do firms adjust their involvement in CSR in response to a change in the level of monitoring?

To answer this question, I consider sell-side financial analysts as an external monitoring mechanism. Jensen and Meckling (1976, p. 353) argue that ‘‘security analysis activities reduce the agency costs associated with the separation of ownership and control.’’ Indeed, a rich subsequent literature speaks for the role of financial analysts as a monitoring/governance mechanism by showing that analyst coverage decreases information asymmetry between investors and managers, puts pressure on managers for performance and restricts their non-value-maximizing behaviors (see, e.g., Brennan and Subrahmanyam (1995), Hong, Lim and Stein (2000), Ellul and Panayides (2009), and Cheng, Subramanyam, and Zhang (2007)). Financial analysts monitor managers by probing into their business strategies, asking questions during conference calls, and analyzing and disseminating information about firm performance. Highlighting analysts’ role as an effective monitoring mechanism, Yu (2008) finds that firms with greater analyst coverage manage their earnings less, and Irani and Oesth (2013) show that an exogenous loss in analyst coverage leads to deterioration in financial reporting quality. Chen, Harford and Lin (2012) provide broader evidence of analysts’ role as a governance mechanism by examining a number of corporate policies and performance measures. They find that a decline in analyst coverage leads to a number of agency problems as manifested in a decrease in the value of cash, an increase in excess CEO compensation, more value-destroying acquisitions, and higher earnings management.

The fact that analysts act as an influential governance mechanism offers a straightforward way to examine whether firms’ CSR activities represent an agency problem or are valuable to shareholders. If CSR activities are an agency problem (i.e. they are negative NPV projects), then better monitoring due to greater analyst coverage should force managers to cut back on CSR activities. If the net effect of CSR on shareholder value is insignificant, then analyst coverage should have no effect on CSR. But, if CSR activities are beneficial to shareholders (i.e., they are positive NPV projects), then, depending on the availability of other competing positive NPV projects and financing, greater analyst coverage might lead to an increase or no change in CSR.

This study tests these alternative predictions by using an observable CSR output, the Kinder, Lydenberg and Domini’s (KLD) CSR scores. KLD rates U.S. companies in several dozen categories within seven broad dimensions of CSR, and provides the most comprehensive CSR scores used in the literature. I examine five dimensions of KLD scores that are more likely to be driven by an apparent motive of social welfare, and find support for the agency motive of CSR. In particular, I find that firms with greater analyst coverage tend to be less socially responsible as measured by KLD scores.

My baseline regression estimates find a negative relation between analyst coverage and CSR, consistent with CSR being an agency problem. However, analyst coverage is likely to be endogenous because analysts choose which firms to follow (e.g, McNichols and O'Brien (1997)). The implication is that the regression specification might omit some important variables that are correlated both with analysts' choice of covering a firm and its CSR activities. To help establish causality, I employ two identification strategies. First, following Hong and Kacperczyk (2010), Kelly and Ljungqvist (2012), and He and Tian (2013), I use brokerage closures and mergers as plausibly exogenous shocks to a firm's analyst coverage, and employ a difference-in-differences (DiD) technique. Results from DiD estimates show that firms that exogenously lose analysts due to brokerage closures and mergers (the treatment group) subsequently achieve higher KLD scores compared to another group of firms with similar characteristics which do not lose analysts (the matched control group). This result is robust to alternate matching procedures, alternate ways to calculate CSR, and is not driven by a few outlying events. Moreover, the effect of exogenous loss of analysts on CSR is stronger among treatment firms that are less financially constrained, are more profitable, and have smaller analyst following.

Second, I conduct a two-stage least squares (2SLS) technique that uses an instrumental variable for analyst coverage. In particular, following Yu (2008) and He and Tian (2013), I exploit the time-series variation in the size of brokerage houses and construct a variable, *expected following*, as an instrument for the realized analyst coverage. Results from 2SLS regressions also support the conclusion that greater analyst coverage causes firms to achieve lower CSR scores. While the DiD analysis depends on a smaller sample of treatment and matched control firms, the 2SLS analysis uses almost the entire sample. In addition, 2SLS analysis also helps reveal the direction of the bias in the regression coefficient if endogeneity in analyst coverage is not corrected for.

Overall my findings are consistent with the view that analysts decrease agency problems by putting pressure on managers to focus on value-increasing projects and cut back on unproductive discretionary spending on CSR.

I next explore some potential channels through which financial analysts might affect a firm's CSR spending. First, Yu (2008) finds that greater analyst coverage makes accruals-based earnings management more difficult. This may force managers to cut discretionary CSR spending to meet analysts' earnings expectations. Second, Kelly and Ljungqvist (2012) find that stock prices fall following the loss of analysts. This reduces the value of managers' ownership stake in the firm, which reduces the cost to them of investing in pet projects such as CSR. Supporting these conjectures, I find that firms which lose analysts for exogenous reasons increase their selling, general and administrative (SG&A) expenses, which include a part of CSR spending.

This paper proceeds as follows. Section 2 briefly discusses the relevant literature. Section 3 describes the data and presents summary statistics. Section 4 presents the baseline results and addresses endogeneity issues. Section 5 discusses some potential economic mechanisms. Section 6 points out some caveats and concludes.

2. Relation and contribution to the existing literature

This paper contributes to the literature in several ways. First it adds to the long-standing and unsettled debate on the causes and consequences of CSR activities. Most of the debate in the large interdisciplinary literature on CSR focuses on whether CSR activities are driven by shareholder value-maximization motive or embody an agency problem (see, e.g., Bénabou and Tirole (2010)'s review article). One popular way of testing these hypotheses has been to examine the relation between CSR and some measure of firm performance such as profitability or market valuation. However, the results are generally inconclusive (see, e.g., the review by Margolis, Elfenbein, and Walsh (2009)).

Recently researchers have recognized a serious issue of endogeneity in the relation between CSR and firm performance (see, e.g., Bénabou and Tirole (2010), Hong, Kubik, and Scheinkman (2012) and Cheng, Hong and Shue (2014)). An endogeneity problem arises because it is hard to disentangle whether firm performance leads to CSR or *vice versa*, and also whether both CSR and firm performance respond to a common factor omitted from the estimation models. My paper is closely related to newer studies that try to break this endogeneity by employing exogenous events. For instance, using the late 1990s' Internet bubble as an exogenous shock to financial constraints, Hong, Kubik, and Scheinkman (2012) find that corporate goodness increases when a firm's financial constraints are relaxed. Similarly, Cheng, Hong and Shue (2014) employ the 2003 dividend tax cut as a shock to managerial ownership in the firm, and conclude that firms with higher managerial ownership obtain lower CSR scores. Masulis and Reza (2014) also use the 2003 dividend tax cut as a positive shock to managerial ownership and find that firms reduce charitable giving after an increase in managerial ownership stake. Di Guili and Kostovetsky (2014) uncover a behavioral explanation of CSR. They find that firms with Republican cultures tend to be less socially responsible than those with Democratic cultures, and that being more socially responsible harms shareholders' interests. This paper contributes to this literature in a unique way. It uses a large sample of firms, exogenous shocks to analyst following that affects multiple firms in multiple time periods, and draws inferences based on prior empirical findings on financial analysts' influence on firms. Because of these features, my identification strategy is less subject to potential criticism that some time-series event coincident with events that some previous studies have used to establish causality might be biasing the findings. Also, Hong, Kubik, and Scheinkman (2012), Cheng, Hong and Shue (2014) and Masulis and Reza (2014) mainly test the influence of *internal* corporate governance mechanisms on CSR. So, an important contribution of this paper is to examine the role of an external monitoring mechanism, namely analyst coverage, on CSR.[4]

This paper also adds to the growing literature on the effect of financial analysts on corporate policies. Survey evidence of Graham, Harvey and Rajgopal (2005) shows that it is very important for managers to meet or exceed analysts' earnings expectations. Rather intriguingly, these authors find that about 80% of the managers admit their willingness to cut investments to meet analysts' earnings estimates. Consistent with this observation, recent studies uncover a significant impact of financial analysts on a variety of corporate policies. For example, using brokerage closures and mergers as exogenous shocks to analyst coverage, He and Tian (2013) find that financial analysts impede firm innovation. On the other hand, using a similar approach,

[4] In a concurrent paper, Jo and Harjoto (2014) test similar hypotheses and find a positive relation between analyst coverage and CSR. The authors estimate Granger causality models to deal with endogeneity issues. While Granger causality does demonstrate the likelihood of causation, it suffers from an omitted variable bias if CSR and analyst coverage both are driven by common variables with different lags omitted from the model (such as expected future profitability). My analyses, which are based on an exogenous shocks to analyst coverage and are unlikely to have such a problem, obtain results that are different from theirs.

Derrien and Kecskés (2013) argue that increase in the cost of capital resulting from an increase in information asymmetry caused by loss of financial analysts leads firms to cut back on investment and financing activities. Yu (2008) finds a negative relation between the magnitude of analyst coverage and a firm's engagement in accruals-based earnings management. This paper makes a contribution to the literature by examining financial analysts' influence on firms' CSR activities, which has gained substantial interest from academics and policy-makers due to its implications for the welfare of a wider group of stakeholders.

3. Data and Descriptive Statistics

My sample starts from 2001 and ends in 2011. I obtain the required data from several sources. The data on firms' CSR scores come from STATS database of MSCI ESG Research, which is the successor of Kinder, Lydenberg and Domini (KLD), Innovest and IRRC, which were acquired through MSCI's acquisition of RiskMetrics. For simplicity, I will refer to this dataset as KLD data. While KLD data begins in 1991, I use KLD scores from 2001 for the following reasons. KLD increased its coverage of companies from 650 to 1,100 largest companies in 2001, and to 3,000 largest companies in 2003. My difference-in-differences analysis requires a large dataset because it hinges on finding an appropriate control firm for each treatment firm identified from a pool of many candidate control firms. Second, some of KLD data are more complete (e.g., strengths and concerns for labor rights and endogenous peoples) in the 2000s. Third, this sample period mostly falls in the post-Reg FD era in which analysts arguably assess firms more objectively because they have lower incentives to curry favor with managers of companies they cover to try to obtain private information (see e.g., Herrmann, Hope and Thomas (2008)).

I obtain company financials and stock price data from Compustat and CRSP, respectively. Analyst coverage data come from I/B/E/S, which I supplement with the information on brokerage closures and mergers from Hong and Kacperczyk (2010) and Kelly and Ljungqvist (2012). I use the I/B/E/S Broker Translation File (BRAN) to match brokerage firm names with I/B/E/S identifiers.

KLD evaluates a firm's social performance along seven major dimensions: community, diversity, employee relations, environment, human rights, product quality/safety, and corporate governance. For each dimension, KLD rates firms on several sub-topics and counts the number of strengths and concerns. Following the prior literature (e.g., Hong, Kubik, and Scheinkman (2012), and Servaes and Tamayo (2013)), I do not consider issues related to corporate governance as CSR activities. Moreover, as in Servaes and Tamayo (2013), I also exclude product-related issues, which focus on product quality, safety and innovation, which have clear strategic implications for firms. Besides, prior studies have separately studied the relation between analyst coverage and some elements of product-related activities, such as innovativeness (e.g., He and Tian (2013)).

With the remaining five categories[5], I calculate a company's CSR score in a given year by subtracting its total concerns from total strengths as follows:

$$CSR_{it} = \Sigma CSR\ Strengths_{it} - \Sigma CSR\ Concerns_{it}$$

where *i* indexes firm and *t* indexes time.

My main explanatory variable of interest is a company's analyst coverage. To measure analyst coverage, I follow the previous literature (e.g., He and Tian (2013)) and, for each fiscal year and firm, calculate the average of the 12 monthly number of earnings forecasts obtained from I/B/E/S summary file (*Coverage*). My control variables include measures of firm size (book assets), valuation (market to book ratio), and performance (profitability) because large, profitable and highly valued companies are more likely to engage in CSR (see, e.g., Hong, Kubik and Scheinkman (2012)). On the other hand, constraints on discretionary spending created by debt, dividends and business risks might reduce a firm's discretionary spending on CSR. So I also control for firms' total risk proxied by stock return volatility, book leverage and a dummy variable indicating whether the firm pays dividends or not. These control variables are similar to those found to be important by previous studies in predicting a firm's involvement in CSR (e.g., Di Giuli and Kostovetsky (2014)).

Panel A of Table 1 defines the main variables of interest and panel B presents their summary statistics. The sample for my baseline analyses consists of up to 16,529 firm years. The mean (median) number of CSR strengths and concerns are 1.35 (1.0) and 1.45 (1.0), resulting in a mean (median) CSR score of -0.11 (0.00). Each CSR component, namely community, diversity, employee relations, environment and human rights has a median of 0 but these components assume values ranging from -5 to +7. The sample firm receives a mean (median) of 6.05 (5.09) average of

[5] My main conclusions do not change if I include the product category in calculating CSR scores.

the 12 monthly number of earning forecasts (*Coverage*) in a year. The average (median) firm has book assets of about \$6.9 (\$1.2) billion, market to book ratio of 1.74 (1.30), profitability of 3% (5%), an annual stock return of 18% (10%), and book leverage of 19% (15%). About 14% of firm-years pay dividends during my sample period.

4. Results and Discussion

I begin this section by estimating some baseline regressions of CSR on analyst coverage in section 4.1. In section 4.2, I deal with identification issues by employing the DiD technique by using exogenous shocks to analyst coverage caused by brokerage mergers and closures, and conduct some robustness checks. In section 4.3, I estimate a two-stage least squares regression using an instrumental variable for analyst coverage.

4.1 Baseline Regressions

I estimate the following regression to examine how analyst coverage affects CSR activities:

$$CSR_{i,\,t+1} \text{ or } CSR_{i,\,t+2} = \alpha + \beta Coverage_{i,t} + \gamma Controls_{i,t} + Year_t + Firm_i + \varepsilon_{it}$$

where *i* and *t* represent firm and year. CSR is defined as the difference between total strengths and total concerns obtained from KLD data. Analyst coverage *(Coverage)* is measured as the average of the 12 monthly number of earnings forecasts a firm receives over the fiscal year *t*. I estimate the effect of analyst coverage in year *t* on CSR activities both in year *t+1* and *t+2* because I expect the effect of analyst following on CSR to show up with some lag as investments in CSR are likely to take some time to come to fruition. For example, it might take a few years to change the production technology to make them more environmentally friendly, or to build amenities for the surrounding community. Consistent with this idea, Di Guili and Kostovetsky (2014) notice persistence in KLD scores. *Controls* is a vector of control variables, as discussed in section 2, that are also likely to affect CSR activities. *Year* stands for year fixed effects, which controls for any common trend in CSR over time. *Firm* captures firm fixed effects.

The results from different regression specifications are shown in Table 2. First, I estimate a parsimonious regression of CSR_{t+1} only on my main variable of interest, *Coverage*, and year fixed effects. As shown in column 1 of Table 2, this model obtains a positive and significant coefficient on the *Coverage* variable suggesting a positive correlation between analyst following and CSR. In column 2, when I add firm fixed effects to the model, the coefficient on *Coverage* changes sign to negative and becomes statistically significant at the 5% level, suggesting a negative relation between analyst coverage and CSR. These results indicate that time-invariant firm effects that are omitted from the regression are important in the relation between analyst coverage and CSR, and emphasize the role of endogeneity in the relation between these two variables. Perhaps a firm's time-invariant culture of philanthropy, location, business model etc. are important determinants of both its CSR activities and analyst following. As shown in column 3, the coefficient on *Coverage* remains negative and statistically significant when other time-varying control variables are introduced to the model.

Columns 4, 5 and 6 estimate a similar set of regressions of the effect of analyst coverage on CSR scores two years in the future (CSR_{t+2}). Once again, the regression model without firm fixed effect obtains a positive sign on *Coverage* but the sign changes once firm fixed effects are introduced in the model as shown in column 5. The result continues to hold in column 6 when other time varying control variables are introduced. Confirming the sticky nature of KLD scores, the negative influence of analyst coverage manifest more strongly on CSR activities two years in the future both in terms of economic magnitude and statistical significance.

Among the control variables, profitability seems to have a positive effect on CSR. On the other hand, stock volatility, perhaps reflecting higher business risks, tends to have a negative effect on CSR. Similarly, dividend paying firms tend to have lower CSR scores plausibly because a commitment to dividend payments reduces the funds available for more discretionary spending. Some control variables such as firm size and book to market do not obtain statistically significant signs, which is mainly due to the inclusion of firm fixed effect in the model.

Panel B presents the results of the effect of analyst coverage on different components of the *CSR* scores, namely community, diversity, employment, environmental and human rights. Each of these components are calculated by subtracting total concerns from total strengths in their respective categories. As the effect of analyst coverage seems to manifest more strongly in two years, I present the regressions of CSR scores two years in the future using the full set of controls. The results reveal that analyst coverage has a negative and significant influence on four out of five components of my CSR measure. I do not delve into why analyst coverage does not have the expected sign on one of the components because individual components tend to be noisier than the sum. However, it is reassuring that the results are not entirely driven by any one component.

4.2 Identification

The results from the baseline models with firm fixed effects are suggestive of a causal effect of analyst coverage on CSR. However, there is still a concern if time-varying factors that are correlated with both analyst coverage and CSR activities, but are omitted from the regressions, might be biasing the results.

In this section, I deal with this endogeneity issue by employing brokerage closures and mergers as quasi-natural experiments that can lead to a plausibly exogenous decrease in firms' analyst coverage. These events have several desirable qualities that make them suitable instruments for a clean identification of the effect of analyst coverage on CSR. First, prior studies have done extensive analyses to establish that loss of analysts due to brokerage closures and mergers are exogenous to the policies of firms they follow. Second, these events are spread out over time and across industries and affect a large number of firms (see, e.g., Kelly and Ljungqvist (2010, 2012)), a feature that mitigates the concern that that some other time-series events which coincide with brokerage disappearance could be driving the results.

I estimate the effect of the loss of analyst coverage on CSR by employing a difference-in-differences (DiD) methodology, which is designed and implemented as follows.

4.2.1 Design of the Experiment

In the 2000s, many brokerage houses were forced to close their research departments due to adverse changes in revenue from trading, market-making and investment banking, which traditionally subsidized their equity research function. Since these closures were because of reasons unrelated to the firms they followed, these events serve as a source of exogenous shock to these firms' analyst coverage. For more details, see Kelly and Ljungqvist (2010, 2012).

The second source of plausibly exogenous variation in analyst coverage is due to brokerage mergers (see Hong and Kacperczyk (2010) and Kelly and Ljungqvist (2010, 2012)). When two brokerage houses merge, the new entity often ends up with duplicate analysts covering the same firm, so one analyst is often let go. This results in a plausibly exogenous decrease in the number of analysts following a firm.

In their papers, Hong and Kacperczyk (2010) and Kelly and Ljungqvist (2012) publish tables with information about the brokerage houses that either closed or merged with another brokerage houses. I use these tables along with the I/B/E/S detail history file and the 2009 version of the I/B/E/S Broker Translation File (BRAN) to identify the closed/merged brokerage houses and firms they used to cover[6]. In case of brokerage closures, the treatment firms are the ones which, in a given year, experience the closure of at least one brokerage house that used to follow them. In case of brokerage mergers, the treatment firms are the ones which are followed by both the acquirer and target brokerage houses before the merger so that there is duplication in coverage after the merger. In particular, for each merger, I use the I/B/E/S identifiers of the merging brokerage houses and identify all the firms that were issued at least one earnings forecasts by the target and acquirer brokerages one year prior to the merger date. For each merger, I create two sets of companies: one which were followed by the bidding brokerage house, and the other by the target brokerage house. The intersection of the two sets is the set of the companies that are covered by both houses before the merger. After the merger, due to overlapping coverage, one of the analysts is often let go. So firms followed by both analysts lose one of them.

These two sets of firms that lose analysts due to brokerage closures and mergers constitute my treatment group. On the other hand, the control group is comprised of firms which do not experience the disappearance of a covering brokerage firm in the given year, but are similar to the treatment firms in several important dimensions. For each treatment firm, I find one control firm in the same fiscal year, in the same size (book assets) and market to book ratio quintiles in the year before the brokerage disappearance (year *t-1)* that has the same number of unique analysts in the year of brokerage disappearance (*t*).[7] For a cleaner test, I also require that the control firm is not a treatment firm in the immediately preceding year (*t-1).* If there are more than one candidate control firms for a treatment firm, I calculate the difference in book assets between the treatment firm and the candidate control firms and, for each treatment firm, retain one control firm with the smallest difference in book assets.[8]

[6] On occasion, I had to look up the SDC platinum database, Factiva news archives and other Internet sources to gather more precise information on these mergers and closures.
[7] For example, a firm loses a brokerage house sometime during its fiscal year 2003 (*t*=2003). I find a control firm matched on firm characteristics in the year 2002 (*t-1*). Usually analysts that disappear in 2003 are still counted in 2003 because they exist during part of the year. So I match the number of analysts in year *t*.
[8] Any matching scheme involves a tradeoff between minimizing the distance between treatment and control firms in important characteristics, and obtaining a large enough sample size to allow for powerful statistical tests. These rather

Since investments in CSR activities are likely to be sticky and require some time to adjust, I examine the effect of a loss of analyst coverage on CSR up to two years after the brokerage disappearance, which is consistent with the baseline regressions in sub-section 3.1. Therefore, I require both my treatment and control firms to have CSR data for two years before and after the brokerage closure/merger (*t-2* to *t+2*), and to have non-missing matching variables in year *t-1*. Thus, I focus on brokerage closures and mergers from fiscal years 2001 to 2008, and I end up with 394 treatment-control pairs for the main DiD estimation.

Figure 1 shows the difference in analyst following between the treatment group and the matched control group five years around the shock. The average difference in the number of analysts one year before the event is 0.185, and after the event it is -0.800, which verifies that the treatment group lost an average of about one analyst due to the disappearance of the brokerage house compared to the control group (-0.808-0.185=-0.993).

4.2.2 The DiD Estimation

A valid DiD estimation requires at least two conditions. First, the treatment and control samples should be similar in all important dimensions before the event; the only difference between the two groups should be that the former experiences an exogenous decrease in analysts but the latter does not. This restriction ensures that the estimated partial effect is not an artifact of systematic differences in treatment and control firms. Second, there should not be a difference in the trend in CSR before the event (parallel trend assumption).

Panel A of Table 3 shows the comparison of key firm characteristics between the treatment and control samples after matching. Before the shock, all the differences in important firm characteristics between the treatment and the matched control groups are insignificant. In particular, the treatment and control samples have similar sizes both in terms of book assets and market capitalization, they have similar book to market ratio, book leverage, dividend paying status, and past returns. The treatment firms seem to have marginally higher levels of stock volatilities but the economic magnitude of the difference is very small. These matched variables are important predictors of both CSR activities and analysts' decisions to follow a firm (Di Guili

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stringent matching criteria for my main difference-in-differences (DiD) tests accomplish the objective of obtaining a control group that is similar to the treatment group in important dimensions (in particular the control variables in the baseline regressions, and initial analyst following), with a reasonable sample size. My main conclusions remain the same when I match on other plausible criteria, as discussed in my robustness checks.

and Kostovetsky (2014), and He and Tian (2013)). Equally important, as shown in the last row of Panel A, there is no statistically significant difference in the growth rates of CSR from year *t-2* to year *t-1* between the treatment group and the control group. This shows that the treatment and the control groups did not have different pre-trends in CSR before the loss of brokerage house. So the parallel trend assumption is satisfied. Figure 2 shows graphically that the lines representing the average CSR scores of the treatment and the control group from *t-2* to *t-1* are parallel to each other. It is important to note that parallel trend assumption does not require the treatment and control groups to have the same *level* of CSR before the shock, but only that the *trend* in the changes in CSR leading up to the shock should not be different.

Overall, the matching process seems to have removed most of the important observable differences between treatment and control groups before the event. Since the treatment and control samples are very similar in important dimensions, and there is no pre-trend in CSR leading up to the shock, the difference in differences in CSR before and after the shock between the two samples can plausibly be attributed only to the exogenous loss of analysts.

Panel B of Table 3 reports the main results of DiD estimation using the matched sample. The first row shows that the average difference in raw CSR in the treatment group between year *t-1* and *t+1* is 0.24, whereas this difference for the control group is zero. Consequently, the DiD estimate for the CSR score is 0.24, which is statistically significant at the 5% level. In economic terms, an exogenous decrease in analyst coverage causes a firm to increase its CSR scores by about 11% of its standard deviation between year *t-1* and *t+1*. The second row shows the DiD estimates of the CSR scores between years *t-1* and *t+2*. The difference in average CSR in the treatment group is 0.35 while the difference in the control group is a much smaller -0.05. This yields a DiD estimate of 0.40 which is statistically significant at the 1% level. In terms of economic significance, an exogenous drop in analyst coverage leads to an increase in CSR score equal to 18% of its standard deviation.

The positive causal effect of coverage loss on CSR implies that firms followed by more (fewer) analysts tend to have lower (higher) CSR scores. This result supports the agency-based explanation that pressure from financial analysts to perform leads managers to cut back on discretionary spending, such as CSR. To further explore this explanation, I next examine if the effect of analyst coverage on CSR is more pronounced among firms that are typically more likely to spend on CSR activities. Hong, Kubik and Scheinkman (2012) find that less financially constrained firms are more likely to engage in socially responsible activities, casting doubt on the profit motive and supporting the agency view of CSR spending. An implication of their finding for this study is that that effect of loss of analysts on CSR should be more pronounced among firms that are less financially constrained. To test this conjecture, I employ two measures of financial constraint: Hadlock and Pierce (2010)'s size–age (SA) Index and their improved version of Kaplan and Zingales index (New KZ index).[9] For both indices, a higher number implies a higher degree of financial constraint. As shown in panel B of Table 3, the positive effect of exogenous loss in analysts on CSR is stronger among firms which are less (below median) financially constrained before the event. Similarly, this effect is stronger among profitable firms than among loss-making firms.

Next, I examine if the negative effect of analyst following on CSR activities depends on the number of analysts following the firm. Intuitively speaking, loss of an analyst should have a larger effect on CSR in firms which are followed by very few analysts to begin with. I test this conjecture in the next two rows of Table 3, panel B, where I find that treatment firms that are followed by few analysts (<10) CSR gain higher scores than those that are covered by many analysts before the shock.

The next test serves to further verify whether the increase in CSR scores after the brokerage mergers/closures is indeed due to loss of analysts. For the main DiD test, I focus on the loss of analyst due to only two reasons, brokerage closures and mergers because these events are plausibly exogenous to firm policies. However, firms gain and lose analysts for many other reasons. It is possible that loss of analysts due to brokerage closures and mergers is offset by gain in analysts for other reasons. In other words, even if the expected (average) decrease in analyst due to brokerage mergers/closures is 1 (as shown by Figure 1), the realized loss is not always 1. In some cases, such offsetting effects might dampen the impact of the loss of an analyst due to brokerage mergers/closures on CSR. The last two rows find that the effect of loss of an analyst on CSR due

[9] Following Hadlock and Pierce (2010), I calculate the SA Index as follows: -0.737*min(log(45,000), log(AT)) + 0.043*min(log(45,000)^2, log(AT)^2)- 0.040*min(37, Firm Age), and the improved KZ index as follows: -0.009*(IB + DP))/lag(PPENT)+ 0.031*(AT + PRCC_F*CSHO - CEQ - TXDB)/AT+ 2.463*(DLTT+DLC)/(DLTT+DLC+ SEQ) +0.224*(DVC+DVP)/lag(PPENT) + 0.017*CHE/lag(PPENT). All the variables are from Compustat. Firm age is estimated by using the first year a firm appears in Compustat.

to brokerage mergers/closures is more pronounced among firms for which the *realized* difference in analyst following is less than zero from *t-1* to *t-2*. In other words, firms are more likely to increase spending on CSR because of exogenous loss of analyst coverage if the loss is not offset by a gain in analysts for other reasons.

4.2.3 Robustness

The exercise of constructing samples for my main DiD analysis aimed at obtaining control firms that comparable to the treatment firms in important dimensions before the exogenous loss in analyst coverage. This allowed me to establish that the change in CSR scores after the brokerage mergers/closures is caused by the loss in analyst coverage, rather than by differences in firm characteristics. In this section, I perform a number of robustness checks of my main DiD results to address the concern that our observed DiD results might be an artifact of a specific matching scheme.

Panel A of Table 4 reports the results of DiD estimation with several alternate matching strategies, and an alternate calculation of CSR. I start by a crude matching of the treatment and control firms, where I match each treatment firm with control firms only on the basis of the fiscal year in which the treatment firm experienced a brokerage closure/merger. As shown in row 1 of Table 5, even with this crude matching scheme, my main DiD results continue to hold. Interestingly, while this approach obtains much larger t-statistics due to larger sample sizes, the point estimates are similar to those from the main DiD estimates. This result suggests that the conclusions drawn from the main matched-sample DiD analysis is not an artifact of the specific matching scheme.

My main DiD estimation allows at most one control firm per treatment firm because I only retain the control sample with the smallest difference in size. However, as shown in row 2, the DiD estimates essentially remain unchanged when I do not apply the “minimum size difference” filter, and allow for all the control firms that the rest of the matching criteria generate.

Third, in addition to being in the fiscal same year, and in the same size and book-to-market ratio quintiles, I now require the treatment and control firms to be in the same Fama-French 12 industry, and the same quintile of analyst following. In row 3, the DiD estimates are positive and significant at the 1% level, in agreement with the main results.

Fourth, in addition to the usual matching criteria, I also require the treatment and control firms to be in the same tercile of CSR scores before the shock. This criterion alleviates the concern that that economic magnitudes of the effects of the shock is not comparable because the level of CSR between treatment and control samples is not the same before the shock. The DiD estimates obtain positive coefficients as before. However, the statistical significance becomes weaker mainly because this much more stringent matching scheme obtains a much smaller sample size.

Fifth, as another way to address the concern that CSR scores of treatment and control samples are not the same before the shock, I follow He and Tian (2013)'s method and run the DiD analysis on CSR scores normalized by the pre-event averages of CSR over the treatment and control groups. CSR scores can be negative but total strengths and concerns cannot be. So, to obtain a pre-scaled CSR score, I scale the total strength (total concern) of each firm by the pre-event average total strength (total concern) of the treatment and the corresponding control firm. I then subtract the pre-scaled total concern from the pre-scaled total strength to calculate the pre-scaled CSR. This method mitigates the issue arising from the difference in pre-event CSR scores between the treatment and control samples. As shown in row 5 of panel A, the DiD estimates on the normalized (pre-scaled) CSR supports the conclusion from my main analysis

Sixth, I employ an alternative way of calculating CSR scores (*Scaled CSR*) which adjusts for the possibility that firms are not evaluated along the same dimensions of CSR each year. Following Servaes and Tamayo (2013), I calculate *Scaled CSR* by scaling the number of each firm's CSR strengths and concerns by the maximum number of strengths and concerns *any* firm receives in a given year, and subtract the scaled concerns from the scaled strengths as follows.

$$Scaled\ CSR_{it} = \Sigma(CSR\ Strength_{it}/Max\ CSR\ Strengths_{t}) - \Sigma(CSR\ Concerns_{it}/Max\ CSR\ Concerns_{t})$$

The results of DiD estimates of scaled CSR, reported in row 6, supports the conclusions from the analysis of raw CSR scores. Specifically, an exogenous loss of analysts leads to an increase in scaled CSR scores, and this effect is larger in year $t+2$. This result suggests that the observed effects of broker disappearance on CSR are unlikely to driven by the changes in how CSR is measured.

Next, I address the concern that the observed effect of brokerage disappearance on CSR might be driven by time-series events coincident with the events of brokerage disappearance. My main DiD analysis pools together all the brokerage disappearance events, which may raise a concern that my results are driven by one particular year of large number of brokerage mergers and closures. To mitigate this concern, in the spirit of Irani and Oesch (2013), I conduct DiD analyses for each year of analyst disappearance separately. The results are presented in panel B of Table 4. In most years, DiD estimates obtain a positive coefficient, although they are statistically stronger in 2001, 2004 and 2008. However, my full sample results are not driven only by these years. As shown in the last four rows of panel B, the main results hold if I exclude observations for 2001, 2004 and 2008 either separately or together from the sample. Not surprisingly, the statistical significance weakens in the last case.

Overall, my main results are robust to different matching schemes, alternative definitions of CSR, and are not driven by an outlier event in a single year.

4.3 More on Identification: Instrumental Variable Approach

My main identification strategy of using the DiD technique exploits complete disappearance of brokerage houses either through closures or mergers. However, quite often brokerage houses respond to changes in revenue and profitability simply by expanding or downsizing their research departments rather than completely shutting them down or selling them off. Reducing the size of the research department involves laying off some of the existing analysts, which consequently leads some firms to lose analyst coverage. Importantly, the expansion or downsizing of brokerage houses are mostly driven by these houses' internal reasons and are unlikely to be related to CSR activities of the firms they follow. Yu (2008), and He and Tian (2013) provide several real-world examples that supports this view.[10] Therefore, the variation in the size of the brokerage houses provides an opportunity to exploit plausibly exogenous variation in a firm's analyst coverage.

To provide broader evidence of causality from my entire sample, I next use a two-stage least squares regression (2SLS). Specifically, I follow Yu (2008), and He and Tian (2013) and create an instrumental variable called *Expected Following* to capture the variation in analyst coverage due to a change in brokerage size as follows:

[10] Yu (2008) discusses Lehman Brothers' decision to downsize its research department as a result of a large operating loss in 1990. He and Tian (2013) discuss the examples of Prudential Financial Inc.'s 2007 announcement to substantially wind down its equity research group because of its underperformance compared to its parent company, Barclays's 2012 decision to expand its Taiwan-based equity research team due to its continued earnings growth in brokerage business, and William Capital Group's expansion of its equity research group in 2011 to remain competitive by catering to its clients' demand for research.

$$ExpFollow_{i,t,j} = (Broker\ Size_{j,t}/Broker\ Size_{j,0}) \times Follow_{i,0}$$

and

$$ExpFollow_{i,t} = \sum_{j=1}^{N} ExpFollow_{i,t,j}$$

Where $ExpFollow_{i,t,j}$ is the expected number of analysts following firm *i* from broker *j* in year *t*. $Broker\ Size_{j,t}$ and $Broker\ Size_{j,0}$ are the numbers of analysts employed by broker *j* in year *t* and the benchmark year 0, respectively. N is the total number of brokers following the firm and $ExpFollow_{i,t}$ is the total *expected* number of analysts following firm *i* in year *t*, conditioned on changes in brokerage house sizes. Following Yu (2008), I constrain $ExpFollow_{i,t,j}$ to a maximum of one because brokerage houses seldom assign more than one analyst at a time for a firm. I set my benchmark year (t = 0) as 2001, the first year in my sample. Since some firms are not covered by any analysts in 2001, I lose some observations because I cannot calculate $ExpFollow_{i,t,j}$ for them. I also exclude observations from the benchmark year 2001.

I employ $ExpFollow_{it}$ as an instrumental variable for *Coverage* and estimate a two-stage least squares (2SLS) regression. One concern with this instrument is that brokerage houses choose which firms to stop following after a downsizing. However, as Yu (2008) argues, this potential issue of selection bias only affects *realized* (i.e. actual) coverage, not expected coverage, which measures the propensity of coverage continuation before the brokerage house decides which firms to follow.

Column 1 of Table 5 presents the results of the first stage regression of the 2SLS with *Coverage* as the dependent variable and *ExpFollow* as an exogenous explanatory variable. The control variables, not reported for brevity, are the same as in my baseline regressions presented in Table 2 and include firm and year fixed effects. The standard errors are robust to heteroscedasticity and are clustered at the firm-level.[11] The first stage regression obtains a positive and highly significant coefficient on *ExpFollow* in predicting *Coverage*. The level of statistical significance of *ExpFollow* in predicting *Coverage* suggests that the instrument is not weak.

Columns 2 and 3 of Table 5 present the results of the second stage regressions of CSR_{t+1} and CSR_{t+2}, respectively with the predicted *Coverage* as the main explanatory variable. Consistent with my baseline findings, coefficients on instrumented coverage are negative, and are highly

[11] I estimate this regression using Stata module "xtivreg2" written by Schaffer (2005)

statistically significant. This 2SLS estimation utilizes most of the available sample, and reinforces the conclusion of a negative causal effect of analyst coverage on CSR obtained by my DiD analysis.

The economic magnitudes of *Coverage* in predicting CSR are substantially larger than those obtained from regressions without the correction for endogeneity reported in Table 2. This difference is likely a product of two underlying forces. First, it indicates the direction of bias in the regression estimates if the endogeneity in coverage is not controlled for. In particular, it suggests that some omitted variables simultaneously affect CSR spending as well as analyst coverage in the same direction. For instance, an increase in a firm's business uncertainty might force it to cut back on CSR activities, and at the same time, deter sell-side analysts, whose job of predicting future earnings becomes more difficult (see, e.g., Graham, Harvey, and Rajgopal (2005)). Also, a favorable change in a firm's investment opportunity might attract more analyst coverage as well as more attention of local community and CSR lobbyists. This positive correlation between analyst coverage and CSR due to time-varying omitted variables biases the coefficient of *Coverage* upwards (i.e., makes it less negative) in predicting CSR. The second reason could be that both cross-sectional and time series variation in analyst coverage is likely to have significant randomness. So, analyst coverage is a noisy proxy for external monitoring that can put pressure on managers for performance. Consequently, regression attenuation due to this measurement error might bias the coefficient on (endogenous) *Coverage* towards zero (less negative).

Overall, our evidence from DiD estimates and the instrumental variable technique suggests that there is a negative causal relation between firms' analyst coverage and CSR. This result supports the agency-based hypothesis of analysts' influence on CSR.

5. Underlying channels

In this section, I discuss some underlying channels through which analysts cause a decrease in a firm's CSR activities. Several existing studies point out a number of potential underlying channels that help explain the negative causal effect of analysts on CSR found in this study. So, the focus of this section is primarily discussing existing findings and reconciling them to the findings of the present study. Section 5.3 provides empirical evidence that reinforces the conclusion from these discussions.

5.1 Less opportunity to manage earnings

Greater analyst following has two effects that can compound the problem of managers who are worried about their reputation. First, analysts put pressure on managers to meet their earnings benchmarks. Second, they make accruals-based earnings management, one popular tool managers use to meet those benchmarks, more difficult (Yu (2008)). Given the recent finding that CSR has no positive impact on future sales or profitability (Di Guili and Kostovetsky (2014)), at least in the short run, one plausible pathway to meet analyst expectation is to cut back on discretionary CSR spending. Consistent with this idea, Irani and Oesch (forthcoming) find that a decrease in analyst coverage leads to a decrease in real earnings management.

5.2 Decrease in managerial ownership

Managers derive utility from at least two sources at their work: 1) pecuniary benefits from cash compensation and equity holdings, and 2) non-pecuniary benefits from managerial perks and pet projects, which further managers' personal interests and are considered agency problems. Cheng, Hong and Shue (2014) show that CSR is due to managerial agency problems because an increase in managerial ownership leads to a decline in corporate goodness. Kelly and Ljungqvist (2012) find that firms that lose analysts due to exogenous reasons experience share price declines of 1.12% to 2.61%, with no short-term reversal. This decline in share price potentially decrease managers' equity interest in these firms. As Cheng, Hong and Shue (2014) suggest, a drop in managerial ownership leads managers to invest more in CSR because marginal utility from altruistic activities exceed those from shareholder value-maximizing projects. So, a decrease in managerial equity ownership in the firm may be one of the mechanisms through which loss in analyst coverage causes an increase in CSR activities.

5.3 Evidence from SG&A spending

Guili and Kostovetsky (2014) find that KLD CSR scores can be significantly predicted with a firm's Selling, General and Administrative (SG&A) expenses. This is because better KLD score comes from spending on charitable giving, prevention of pollution, employee benefits, and health and safety programs etc., most of which are a part of SG&A. However, they find that better KLD scores do not contribute to sales and, in fact, contribute negatively to stock returns at least in the short run. In other words, a higher KLD score is associated with a higher SG&A expense for a given level of sales without any measurable benefit to the shareholders. Naturally, to the extent that financial analysts put pressure on managers for short-term financial performance, a firm followed by more analysts should have a lower SG&A expenses on average.

To test this conjecture, I examine whether firms which exogenously lose analysts increase their SG&A to sales ratio subsequently. As before, I use a DiD framework. Since SG&A to sales ratios tend to vary significantly across industries (see e.g., Baumgarten, Bonenkamp, Homburg (2010)), for cleaner comparison, I employ the matching scheme that requires the treatment and control firms to also be in the same industry, as shown in row 3 of Table 4, panel A.

The DiD estimates reported in Table 6 suggest that the treatment group increase their SG&A to sales ratio by 0.019 more than the control group from the year *t-1* to *t+1*. This estimate is statistically significant at the 10% level. This difference becomes larger (0.039) and statistically significant at the 1% level from the year *t-1* to *t+2*.

Furthermore, Di Guili and Kostovetsky (2014) show that the SG&A expense is more likely to contribute to CSR in larger (higher market capitalization) firms. Accordingly, I find the immediate change in SG&A to sales ratio, i.e., from year *t-1* to *t+1,* is larger (0.026 vs. 0.013) and statistically more significant (1% vs. insignificant) among the sample of treatment firms with above-median market capitalization.

Overall, the evidence of increase in SG&A ratio following the loss of analyst coverage suggests that pressure from financial analysts lead firms to cut on SG&A expenses which are wasteful from shareholders' perspective but which contribute positively to their CSR scores.

6. Conclusion

Using a unique setting, this study contributes to the debate on whether corporate social responsibility (CSR) is an agency issue or something beneficial to shareholders. I build on a rich prior literature that uncovers financial analysts' role as an influential external monitoring mechanism that improves firm governance. I examine how firms adjust their involvement in CSR as a response to a change in their monitoring environment caused by a change in analyst coverage.

I find that firms with greater analyst coverage obtain lower CSR scores as measured by their KLD ratings. To establish causality, I employ two identification strategies. First, I implement a DiD technique by using brokerage closures and mergers as plausibly exogenous shocks to analyst coverage. I find that firms which lose analysts for exogenous reasons subsequently achieve higher CSR scores. Second, I estimate a 2SLS regression by using an instrumental variable for analyst coverage, and find similar results.

My finding of a negative causal effect of analyst coverage on CSR is consistent with the view that firms' CSR activities represent an agency problem (i.e., managers do good with other people's money), and that financial analysts act as effective external monitors and force managers to reduce discretionary spending on CSR. One caveat is that we still do not know a great deal about the benefits and costs of CSR to either equityholders or a broader group of stakeholders especially in the long-run, which makes it difficult to assess the net impact of CSR. Furthermore, even if my findings suggest that financial analysts generally have positive effect on a firm's existing shareholders by showing that they curtail spending that is wasteful for shareholders, these results are agnostic to the issue of whether CSR is good for society as a whole, and how financial analysts affect the welfare of stakeholders other than shareholders.

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Table 1: Variable definitions and summary statistics

Panel A of this table defines main variables of interest. Panel B shows summary statistics of the main variables based on the sample of firms from 2001 to 2011.

Panel A: Variable definitions

Variable	Definition
CSR Strengths	The sum of strength scores for community, diversity, employee relations, environment, and human rights components (com_str_num + div_str_num + emp_str_num + env_str_num + hum_str_num): From KLD
CSR Concerns	The sum of concern scores for community, diversity, employee relations, environment, and human rights components (com_con_num + div_con_num + emp_con_num + env_con_num + hum_con_num): From KLD
CSR	CSR Strengths - CSR Concerns
Community	Community: Number of Strengths - Number of Concerns (com_str_num - com_con_num)): From KLD
Diversity	Diversity: Number of Strengths - Number of Concerns (div_str_num - div_con_num)): From KLD
Employee Rel.	Employee Relations: Number of Strengths - Number of Concerns (emp_str_num - emp_con_num)): From KLD
Environment	Environment: Number of Strengths - Number of Concerns (env_str_num - env_con_num)): From KLD
Human Rights	Human Rights: Number of Strengths - Number of Concerns (hum_str_num - hum_con_num)): From KLD
Coverage	Arithmetic mean of 12 monthly number of earnings forecasts a firm receives over the fiscal year: From I/B/E/S
Book Assets ($ millions)	Total Assets (AT): From Compustat
Market to Book Ratio	(Market value of common stock + total debt + preferred stock – deferred taxes and investment tax credit) / Book Assets (PRCC_F*CSHPRI+DLC+DLTT+PSTKL-TXDITC/AT): From Compustat
Profitability	Net Income/Book Assets (NI/AT): From Compustat
Stock Return	Holding period stock return over the fiscal year: From CSRP
Total Risk	Standard deviation of daily stock return for the fiscal year: From CSRP
Dividend Payer	An indicator variable that equals one if a firm pays cash dividends on common equity (DVC), and zero otherwise: From Compustat
Leverage	Book leverage ((DLTT+DLC)/AT): From Compustat

Panel B: Summary statistics

	Obs.	Mean	S.D.	10th Percentile	25th Percentile	Median	75th Percentile	90th Percentile
CSR Strengths	16529	1.35	2.20	0.00	0.00	1.00	2.00	4.00
CSR Concerns	16529	1.45	1.47	0.00	0.00	1.00	2.00	3.00
CSR	16529	-0.11	2.28	-2.00	-1.00	0.00	1.00	2.00
Community	16529	0.06	0.51	0.00	0.00	0.00	0.00	0.00
Diversity	16529	0.11	1.41	-1.00	-1.00	0.00	1.00	2.00
Employee Rel.	16529	-0.20	0.89	-1.00	-1.00	0.00	0.00	1.00

Environment	16529	-0.02	0.83	-1.00	0.00	0.00	0.00	1.00
Human Rights	16529	-0.06	0.27	0.00	0.00	0.00	0.00	0.00
Coverage	16529	6.05	4.21	1.54	2.94	5.09	8.27	11.97
Book Assets ($ millions)	16529	6878	33587	183	412	1191	3836	13568
Market to Book Ratio	16529	1.74	1.39	0.64	0.88	1.30	2.09	3.41
Profitability	16529	0.03	0.13	-0.07	0.01	0.05	0.09	0.13
Stock Return	16529	0.18	0.59	-0.40	-0.15	0.10	0.38	0.78
Total Risk	16529	0.11	0.13	0.02	0.04	0.07	0.13	0.24
Dividend Payer	16529	0.14	0.35	0.00	0.00	0.00	0.00	1.00
Leverage	16529	0.19	0.20	0.00	0.00	0.15	0.31	0.45

Table 2: Baseline regressions of CSR on analyst coverage

Panel A presents the regression of future CSR scores on analyst coverage and other control variables. All variables are defined in Panel A of Table 1. Panel B presents regressions of different components of CSR. Standard errors are robust to heteroscedasticity and clustered at the firm level; t-statistics are reported in parentheses. ***, **, and * indicate statistical significance levels of 1%, 5% and 10%, respectively.

Panel A: Analyst Coverage and CSR

	(1) CSR_{t+1}	(2) CSR_{t+1}	(3) CSR_{t+1}	(4) CSR_{t+2}	(5) CSR_{t+2}	(6) CSR_{t+2}
Coverage	0.152***	-0.023**	-0.024**	0.160***	-0.059***	-0.063***
	(11.50)	(-2.21)	(-2.16)	(10.84)	(-4.79)	(-4.90)
Log(Book Assets)			-0.092			-0.033
			(-1.35)			(-0.43)
Log(Market to Book)			-0.028			0.089
			(-0.48)			(1.32)
Profitability			0.448***			0.683***
			(3.11)			(3.89)
Stock Return			-0.070**			-0.076**
			(-2.55)			(-2.36)
Total Risk			-1.004***			-1.471***
			(-6.99)			(-6.76)
Dividend Payer			-0.195***			-0.198***
			(-3.00)			(-2.96)
Leverage			0.256*			0.222
			(1.76)			(1.35)
Constant	-0.516***	-0.290***	1.239**	-0.536***	-0.060	1.423**
	(-3.62)	(-3.27)	(2.47)	(-3.43)	(-0.48)	(2.45)
Firm Fixed Effects	No	Yes	Yes	No	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16529	16529	16529	13908	13908	13908
R^2	0.087	0.023	0.032	0.092	0.040	0.054

Panel B: Analyst coverage and CSR components

	(1) $Community_{t+2}$	(2) $Diversity_{t+2}$	(3) Employee $Rel._{t+2}$	(4) $Environment_{t+2}$	(5) Human $Rights_{t+2}$
Coverage	-0.006*	-0.018***	0.003	-0.034***	-0.007***
	(-1.78)	(-2.68)	(0.43)	(-6.29)	(-3.47)
Log(Book Assets)	0.014	0.095**	-0.038	-0.094***	-0.009
	(0.63)	(2.42)	(-1.07)	(-2.85)	(-0.71)
Log(Market to Book)	0.006	0.083**	-0.028	0.019	0.009
	(0.29)	(2.31)	(-0.90)	(0.68)	(0.78)

Profitability	-0.052	0.199**	0.275***	0.228***	0.033
	(-1.16)	(2.10)	(3.28)	(3.24)	(1.31)
Stock Return	-0.002	-0.044***	-0.003	-0.021	-0.006
	(-0.26)	(-2.68)	(-0.20)	(-1.62)	(-1.26)
Total Risk	-0.284***	-0.267**	-0.223**	-0.666***	-0.031
	(-5.93)	(-2.15)	(-2.27)	(-7.27)	(-1.23)
Dividend Payer	0.023	-0.086**	0.029	-0.152***	-0.013
	(1.25)	(-2.50)	(0.88)	(-5.01)	(-1.21)
Leverage	0.108**	0.078	-0.036	0.074	-0.003
	(2.34)	(0.84)	(-0.44)	(1.01)	(-0.12)
Constant	0.216	-0.822***	0.304	1.604***	0.121
	(1.34)	(-2.74)	(1.14)	(6.36)	(1.39)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	13908	13908	13908	13908	13908
R^2	0.049	0.175	0.051	0.198	0.034

Table 3: Difference-in-differences (DiD) tests

This table reports the results from difference-in-differences (DiD) analysis of how exogenous shock to analyst coverage affects CSR scores. The sample covers 394 treatment-control pairs from fiscal year 2001 to 2008 with non-missing matching variables and non-missing CSR variables during a five year window around brokerage closures/mergers (*t+2*, *t-2*). Construction of the treatment and control samples is described in section 4.2.1 of the text in detail. Treatment firms are the ones which lose brokerage houses due to mergers or closures in a given year. Control firms are obtained by matching the candidate control firms to the treatment firms on the number of analyst following a firm in year *t*, book assets quintile and market to book ratio quintile in year *t-1*. Panel A reports the difference in the treatment and the control sample in several important dimensions after the matching. Panel B reports the main results of the DiD estimation. Panel C reports the DiD estimates conditional on two measures of financial constraints: 1) Hadlock and Pierce (2010)'s size–age index (*SA Index*), and 2) their improved version of Kaplan Zingales' Index (*New KZ Index*); profitability; number of analysts before the brokerage closure and merger shocks (*Initial Coverage*); realized loss of analysts after the shocks (*Realized Difference Analysts*). ***, **, and * indicate statistical significance levels of 1%, 5% and 10%, respectively.

Panel A: Differences in treatment and control post-match before the event

	Treatment	Control	Difference	p-value
Number of Analysts$_{t-1}$	16.42	16.23	0.19	0.38
Market to Book Ratio$_{t-1}$	1.86	1.89	-0.03	0.48
Total Assets, in $Millions$_{t-1}$	11339.90	12014.03	-674.13	-0.22
Market Cap, in $Millions$_{t-1}$	11980.32	11316.22	664.10	0.73
Profitability$_{t-1}$	0.05	0.05	-0.01	-1.25
Stock Return$_{t-1}$	0.19	0.22	-0.03	0.23
Volatility$_{t-1}$	0.05	0.05	0.00*	0.07
Dividend Payer$_{t-1}$	0.21	0.21	0.00	1.00
Leverage$_{t-1}$	0.20	0.20	-0.00	0.80
CSR Growth (*t-2* to *t-1*)	-0.02	-0.01	-0.01	0.98

Pan el B: Difference-in-differences (DiD) estimates

Variable	Treatment	Control	DiD	t-stat
Diff. CSR (*t-1*, *t+1*)	0.24	0.00	0.24**	2.44
Diff. CSR (*t-1*, *t+2*)	0.35	-0.05	0.40***	3.37

Panel C: DiD conditional on financial constraints, initial coverage and realized coverage loss

Sub-samples	DiD (*t-1*, *t+1*)	t-stat	DiD (*t-1*, *t+2*)	t-stat
Less Financially Constrained (SA Index<Median)	0.48***	2.88	0.68***	3.6
More Financially Constrained (SA Index>=Median)	0.05	0.42	0.15	1.07
Less Financially Constrained (New KZ Index<Median)	0.26**	2.02	0.46***	3.12

More Financially Constrained (New KZ Index>=Median)	0.23	1.42	0.31	1.64
Profitability>0	0.26**	2.33	0.45***	3.52
Profitability<=0	0.18	0.74	0.08	0.29
Initial Coverage <10	0.54***	2.69	0.51**	2.32
Initial Coverage >=10	0.19*	1.66	0.37**	2.77
Realized Difference in Analysts<0	0.30**	1.99	0.48**	2.59
Realized Difference in Analysts>=0	0.20	1.46	0.32**	2.16

Table 4: Robustness checks for Difference-in-differences (DiD) analysis

This table reports several robustness tests for the main DiD analysis. Panel A presents DiD analysis with sample matched on different criteria, and alternative ways to define CSR. See section 4.2.3 of the text for more details. *Crude Match* matches each treatment firm with control firm only on the basis of the fiscal year of brokerage closure/merger. *One to Many Match* does not apply "minimum size difference" filter and allows for multiple matches per treatment firm. *Industry Match* requires treatment and control firms to be in the same Fama-French 12 industry. *CSR Match* requires the treatment and control firms to be in the same tercile of CSR scores before the shock. *Pre-scaled CSR* is CSR score normalized by the pre-event averages of CSR over the treatment and control groups. *Scaled CSR* is obtained by scaling the number of each firm's CSR strengths and concerns by the maximum number of strengths and concerns *any* firm receives in a given year, and subtracting the scaled concerns from the scaled strengths. Panel B presents separate DiD estimates for each fiscal year associated with brokerage closures/mergers. The sample covers fiscal year 2001 to 2008 with non-missing matching variables and non-missing CSR variables during a five year window (*t+2*, *t-2*) around the event year. ***, **, and * indicate statistical significance levels of 1%, 5% and 10%, respectively.

Panel A: Alternative matching schemes and alternative definitions of CSR

	DiD (*t-1*, *t+1*)	t-stat	DiD (*t-1*, *t+2*)	t-stat
1. Crude Match	0.26***	5.51	0.35***	6.00
2. One to Many Match	0.22***	2.67	0.36***	3.87
3. Industry Match	0.34***	4.07	0.44***	4.63
4. CSR Match	0.16	1.28	0.31**	2.29
5. Pre-scaled CSR	0.30***	3.08	0.49***	4.05
6. Scaled CSR	0.02**	2.43	0.03***	3.05

Panel B: Year-by-year DiD

Year by Year	DiD (*t-1*, *t+1*)	t-stat	DiD (*t-1*, *t+2*)	t-stat
2001	0.8*	2.14	1.4**	3.5
2002	0.08	0.21	0.0435	0.08
2003	0	0	-0.75	-0.4
2004	0.75*	1.81	0.97**	2.05
2005	0.2179	1.49	0.3652*	1.68
2006	0.3333	0.52	0.1333	0.25
2007	0.1325	0.83	0.1453	0.93
2008	0.25	1.14	1.88***	3.46
Omit 2001	0.2435**	2.35	0.3821***	3.23
Omit 2004	0.1994*	1.91	0.337***	2.82
Omit 2008	0.2527**	2.31	0.29**	2.5
Omit all three years	0.1863*	1.65	0.2036*	1.68

Table 5: Two-stage least squares (2SLS) estimates using expected analyst following as an instrument

This table presents the estimates of 2SLS regression of one- and two-year-ahead CSR outcomes on analyst coverage, with expected following (*ExpFollow)* as an instrumental variable. The R^2 on the first stage is overall and for second stage, they are within firm, obtained by separately running first and second stage regressions. ***, **, and * indicate statistical significance levels of 1%, 5% and 10%, respectively.

	(1) First Stage: Coverage	(2) Second Stage: CSR_{t+1}	(3) Second Stage: CSR_{t+2}
Coverage (Instrumented)		*-0.367****	*-0.300****
		(-9.18)	*(-7.78)*
ExpFollow	0.441***		
	(24.62)		
Firm Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	14899	14899	12524
R^2	0.520	0.063	0.074

Table 6: Potential underlying channel

This table reports the results from difference in difference (DiD) estimation on how exogenous shock to analyst coverage affects selling, general and administrative expenses to sales ratio (*SG&A/Sales*). The sample covers fiscal year 2001 to 2008 with non-missing matching variables and non-missing CSR variables during a five year window (*t+2, t-2*) around the event year. The control sample is found by matching the candidate control firms to the treatment firms on the quintile of analyst following a firm in year *t*, book assets quintile and market to book ratio quintile and Fama-French 12 industry in year *t-1*. ***, **, and * indicate statistical significance levels of 1%, 5% and 10%, respectively.

	DiD (*t-1, t+1*)	t-stat	DiD (*t-1, t+2*)	t-stat
SG&A/Sales	0.019*	1.84	0.039***	3.12
SG&A/Sales (Larger Firms)	0.026***	2.74	0.031***	3.05
SG&A/Sales (Smaller Firms)	0.013	0.69	0.046**	2.08

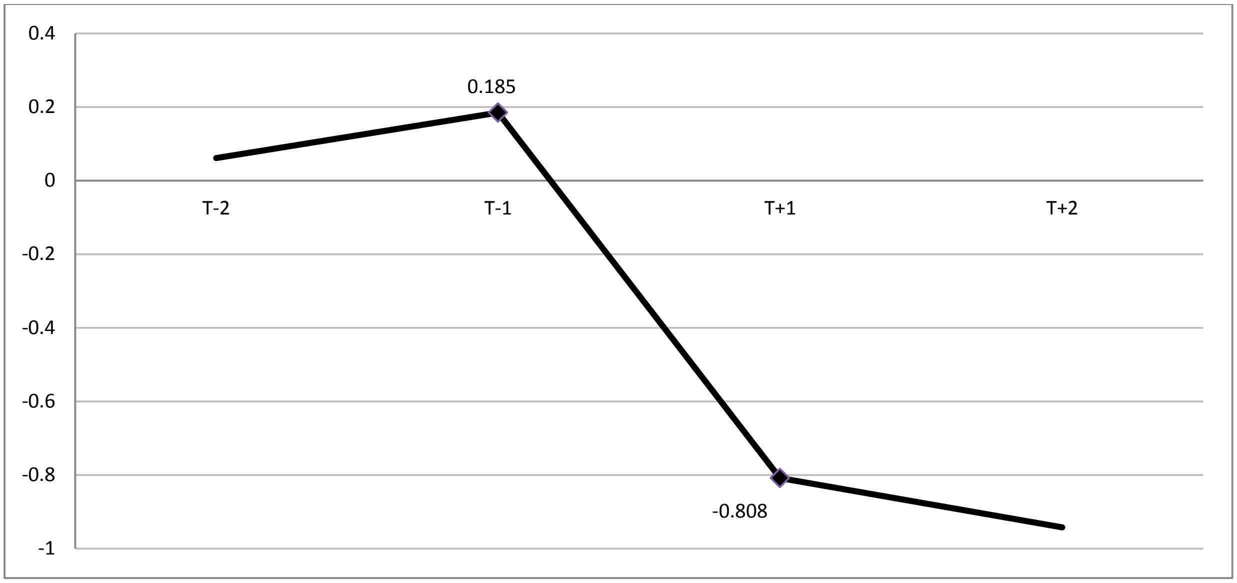

Figure 1: Difference in the average number of analysts between treatment and control group five years around brokerage closures/mergers

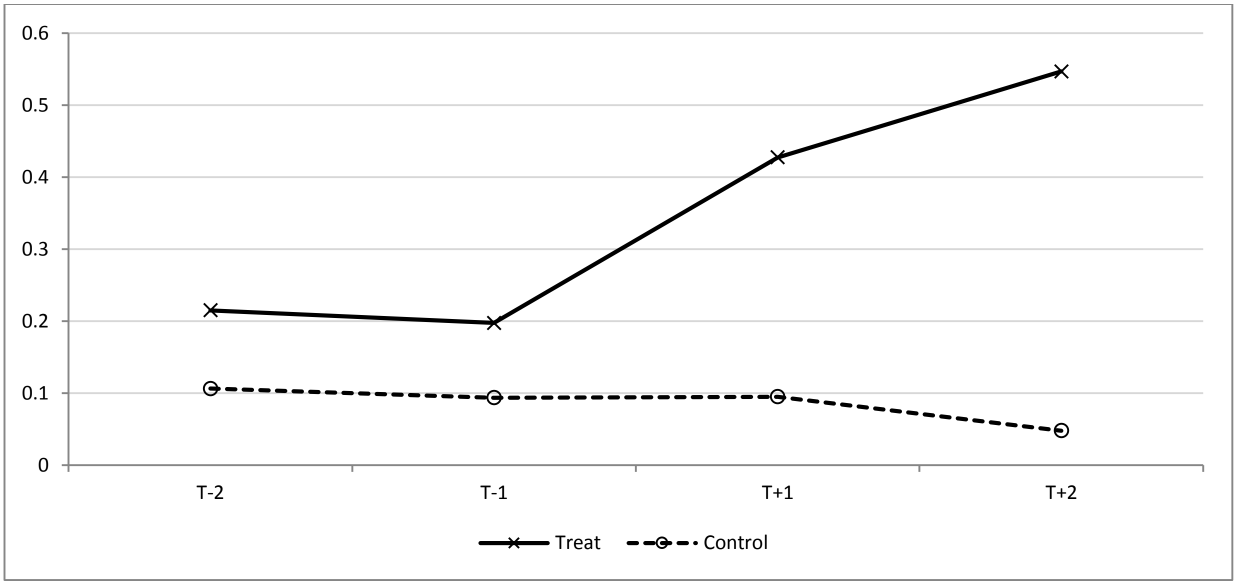

Figure 2: Average CSR scores of treatment and control groups five years around the brokerage closures/mergers