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Causal effect of analyst following on corporate social responsibility

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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. Analyst coverage seems to influence CSR activities via analysts' influence on the value of managerial ownership and discretionary spending. My findings are consistent with the view that spending on CSR is a manifestation of an agency problem and that financial analysts curb such discretionary spending by disciplining managers.

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, 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 (2013); Elfenbein et al. (2012) and List (2006)). Deng et al. (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 (2013) find a positive relation between CSR and firm value among firms with higher advertising expenses consistent with the

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[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 et al. (2012), and Cheng et al. (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 1043 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).

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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 et al. (2013) find that CSR is valuable to the shareholders of firms which have more long-term institutional investors.

The second view considers CSR 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 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 et al. (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. Similarly, Masulis and Reza (2014) find that CEOs personally gain from corporate giving because most of the money goes to CEO-affiliated charities. Di Giuli and Kostovetsky (2014) uncover a behavioral explanation of CSR. They show that political leaning of key employees and directors significantly affects CSR, which hurts firm performance.

Despite large literature, the debate on whether CSR is beneficial to shareholders or is an agency problem seems far from settled. Many studies find a positive correlation between CSR and measures of firm performance. However, endogeneity issues and lack of strong identification strategies limit the extent to which many of these results can be causally interpreted. Hong et al. (2012) illustrate an endogeneity problem 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 better firm performance likely leads to better CSR rather than the other way round. In this paper, I attempt to circumvent endogeneity issues by examining firms' responses to exogenous changes in the intensity of governance to test whether CSR is driven by agency issues.

I ask a simple question: how do firms adjust their CSR practices in response to a change in the level of monitoring that managers face? To answer this question, I consider sell-side financial analysts as an external monitoring mechanism. Financial analysts monitor managers by probing into their business strategies, asking questions during conference calls, and analyzing and disseminating information about firm performance. Jensen and Meckling (1976, p. 353) posit that "security analysis activities reduce the agency costs associated with the separation of ownership and control." Subsequent studies have exemplified many ways financial analysts serve as a monitoring/governance mechanism: they help decrease information asymmetry between investors and managers, put pressure on managers for performance and restrict their value-destroying behaviors (see, e.g., Brennan and Subrahmanyam (1995); Hong et al. (2000); Ellul and Panayides (2009), and Cheng et al. (2007)). They also champion more transparent financial reporting (e.g., Yu (2008) and Irani and Oesch (2013)). Recently, Chen et al. (2015) uncover broader evidence of analysts' role as a governance mechanism. They show that a decline in analyst coverage exacerbates agency problems and leads to a decrease in the value of cash, an increase in excess CEO compensation, more value-destroying acquisitions, and higher earnings management.

Do analysts care about CSR issues? Anecdotal evidence suggests that financial analysts do not regard CSR as a value-enhancing activity. A study by United Nations Environment Program (UNEP, 2004), which conducts in-depth interviews with analysts from many countries, concludes, "Young analysts appear unconvinced over the materiality of most environmental, social, and governance issues to business."[4] Another study by Ernst and Young (1997) finds that environmental and social policies are one of the least valued (ranked 37 out of 39) non-financial factors by analysts when making earnings forecasts.

The fact that analysts act as an influential governance mechanism offers a straightforward way to test whether CSR is an agency problem. If it is an agency problem (i.e., a negative NPV project), 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 is beneficial to shareholders (i.e., a positive NPV project), 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.

I test 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 four dimensions of KLD scores that are most likely to be driven by a motive of social goodness.

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 mainly because analysts choose which firms to follow (e.g., McNichols and O'Brien (1997)). 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

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

[4] This study features the following direct quotes from some sell analysts

- "Why not use government, rather than trying to cajole business to do something that runs counter to its own interests?" Former sell-side equity analyst, investment bank, US.
- "These issues don't crop up in quarterly conference calls." Equity analyst, research institution, US.

achieve higher CSR scores compared to a similar set of firms which do not lose analysts this way (the matched control group). This finding is robust to various matching procedures, an alternate way of calculating CSR, and is not driven by a few outlying events. Moreover, analysts' effect on CSR is concentrated among treatment firms with relatively abundant cash flows and cash reserves, which likely make them more vulnerable to agency conflicts. Also, this effect is stronger among treatment firms that experience a realized coverage loss after the shock compared to control firms, and is more persistent among treatment firms with smaller initial analyst coverage.

My second identification strategy involves two-stage least squares (2SLS) regressions using an instrumental variable for analyst coverage. 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 decreases CSR activities. Instrumental variables analysis also helps reveal the direction of bias if endogeneity in analyst coverage is not corrected for.

Overall, my findings are consistent with the view that analysts decrease agency problems by making managers cut back on discretionary spending on CSR.

Finally, I discuss two potential economic channels through which financial analysts might affect a firm's involvement in CSR. First is the managerial ownership channel. In a DiD framework, I find that firms that face brokerage closure/mergers experience significantly greater loss of market equity (consistent with Kelly and Ljungqvist (2012)). Consequently, CEOs, especially those who own sizeable fractions of these firms, lose significant firm-related wealth. Then Cheng et al.'s (2014) finding leads to the possibility that this decline in the CEOs' "skin in the game" exacerbates the agency problem and encourages CEOs to spend more on CSR. The second potential channel is discretionary real spending. I argue that analysts curtail CSR activities by forcing managers to reduce discretionary real spending (Irani and Oesch (2016)), part of which contributes to programs that firms institute for better CSR (e.g., Di Giuli and Kostovetsky (2014)). Finally, I show that these two channels account for about one fifth of the effect of analyst coverage.

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 explores some potential economic mechanisms. Section 6 discusses an alternative "earnings target" explanation of my finding. Finally, Section 7 points out some caveats and concludes.

2. Relation and contribution to the 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's (2010) 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 et al. (2009)), and most test strategies suffer from serious endogeneity issues (see, e.g., Bénabou and Tirole (2010); Hong et al. (2012), and Cheng et al. (2014)). An endogeneity problem arises because it is hard to disentangle whether better firm performance leads to better CSR or vice versa, and whether both CSR and firm performance respond to variables omitted from the estimation models. My paper is closely related to newer studies that try to tackle this endogeneity by employing exogenous events. For instance, using the late 1990s' Internet bubble as an exogenous shock to financial constraints, Hong et al. (2012) find that corporate goodness increases when a firm's financial constraints are relaxed. Similarly, Cheng et al. (2014) employ the 2003 dividend tax cut as a positive shock to managerial ownership and conclude that firms with higher managerial ownership obtain lower CSR scores. Masulis and Reza (2014) also use the same tax shock and find that firms reduce charitable giving after an increase in managerial ownership stake.

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.

Also, Hong et al. (2012), Cheng et al. (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.[5]

This paper also adds to the growing literature on the effect of financial analysts on corporate policies. Recent studies uncover significant influence of financial analysts on a variety of corporate policies. For example, He and Tian (2013) find that financial analysts impede firm innovation. Derrien and Kecskés (2013) argue that the 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 that greater analyst coverage leads to less accruals-based earnings management. This paper examines financial analysts' influence on firms' CSR activities, which, despite numerous studies about them, are still among less well-understood corporate policies.

[5] 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.

3. Data and descriptive statistics

My sample starts from 2001 and ends in 2011. 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 the 1100 largest companies in 2001 and to the 3000 largest companies in 2003. My difference-in-differences analysis requires a large dataset because it hinges on finding a large enough sample of very similar control firms for each treatment firm. Second, some of the 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 et al. (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 et al. (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)). Finally, I exclude employee relations-related variables because prior literature has shown direct benefit of employee satisfaction to shareholders (e.g., Edmans (2011)).

With the remaining four categories,[6] I calculate a company's CSR score in a given year by subtracting its total concerns from total strengths as follows:

$$CSR_{it} = \Sigma CSR\ Strengths_{it} - \Sigma CSR\ Concerns_{it} \tag{1}$$

where i indexes firm and t indexes time.

My main explanatory variable of interest is a company's analyst coverage. To measure analyst coverage (*Coverage*), I follow He and Tian (2013) and, for each fiscal year and firm, calculate the average of the 12 monthly number of earnings forecasts obtained from the I/B/E/S summary file. My control variables include measures of firm size (book assets), valuation (market to book ratio), and performance (profitability) because larger, more profitable and more highly valued companies are more likely to engage in CSR (see, e.g., Hong et al. (2012)). On the other hand, constraints on free cash flows created by debt, dividends and business risks might reduce a firm's discretionary spending on CSR. So I also control for firms' risk proxied by stock return volatility, book leverage and an indicator variable for its dividend paying status. 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 19,830 firm years. The mean (median) number of CSR strengths and concerns are 1.01 (0) and 0.9 (1.0), resulting in a mean (median) CSR score of 0.11 (0.00). Each CSR component, namely community, diversity, 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 5.8 (4.82) average number of 12 monthly earnings forecasts (*Coverage*) in a year. The average (median) firm has book assets of about \$11.4 (\$1.5) billion, market to book ratio of 1.57 (1.16), profitability of 3% (4%), an annual stock return of 17% (10%), and book leverage of 20% (16%). About 24% of my sample firm-years pay dividends.

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:

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

where i and t represent firm and year. *CSR* and analyst coverage (*Coverage*) are defined in Section 3. I estimate the effect of analyst coverage in year t on CSR activities both in year $t+1$ and $t+2$. This is because the effect of analyst following on CSR might

[6] My conclusions do not change if I include the product and employee relations in calculating CSR scores.

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, environment, and human rights components (com_str_num + div_str_num + env_str_num + hum_str_num): From KLD
CSR concerns	The sum of concern scores for community, diversity, environment, and human rights components (com_con_num + div_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
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 Cap.	Market value of common stock (PRCC_F * CSHPRI): From Compustat
Market to book	(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	Annualized standard deviation of daily stock returns 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
Stock turnover	A stock's average monthly volume divided by shares outstanding.
Real EM	The sum of negative abnormal discretionary expenses and abnormal production costs, as defined by Irani and Oesch (2016)
CEO firm wealth	Sum of the value of stock and option portfolio held by the CEO (in $ millions).
High CEO ownership	An indicator variable for whether CEO's % ownership in the firm exceeds the median among treatment group (0.86%)

Panel B: Summary statistics

	Obs.	Mean	S.D.	5th Percentile	25th Percentile	Median	75th Percentile	95th Percentile
CSR strengths	19,830	1.01	1.8	0	0	0	1	5.0
CSR concerns	19,830	0.9	1.1	0	0	1	1	3.0
CSR	19,830	0.11	1.96	−2	−1	0	1	4.0
Community	19,830	0.07	0.53	−1	0	0	0	1.0
Diversity	19,830	0.11	1.41	−2	−1	0	1	3.0
Environment	19,830	−0.01	0.77	−1	0	0	0	1.0
Human rights	19,830	−0.05	0.25	−1	0	0	0	0.0
Coverage	19,830	5.8	4.2	1	2.64	4.82	8.04	14.4
Book assets ($ millions)	19,830	11,410	66,992	133	495	1531	4737	33,647
Market to book	19,830	1.57	1.35	0.27	0.76	1.16	1.90	4.36
Profitability	19,830	0.03	0.13	−0.17	0.01	0.04	0.08	0.16
Stock return	19,830	0.17	0.56	−0.54	−0.14	0.10	0.36	1.09
Total risk	19,830	0.46	0.24	0.19	0.29	0.39	0.56	0.93
Dividend payer	19,830	0.24	0.43	0	0	0	0	1
Leverage	19,830	0.20	0.20	0.00	0.01	0.16	0.32	0.60

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 it more environmentally friendly, or to build amenities for the surrounding community. Consistent with this idea, Di Giuli and Kostovetsky (2014) notice persistence in KLD scores. *Controls* is a vector of control variables, as discussed in Section 3. *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, Panel A. First, I estimate a parsimonious regression of CSR_{t+1} only on the main variable of interest, *Coverage*, and year fixed effects. As shown in column 1, 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* turns negative and becomes statistically significant at the 1% 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. For example, a firm's culture of philanthropy, location, business model, etc., which are largely time-invariant, may affect its CSR activities and analyst following. In column 3, the coefficient on *Coverage* remains negative and statistically significant when other 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 to negative once firm fixed effects are introduced in column 5. The result continues to hold in column 6 when other control variables are introduced. Confirming the sticky nature of KLD scores, the negative influence of analyst coverage on CSR activities manifests more strongly two years in the future both in economic magnitude and statistical significance.

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 each component of CSR. Other control variables are included in the regression but not reported in panel B. 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)	(2)	(3)	(4)	(5)	(6)
	CSR_{t+1}	CSR_{t+1}	CSR_{t+1}	CSR_{t+2}	CSR_{t+2}	CSR_{t+2}
Coverage	0.141***	−0.025***	−0.026***	0.149***	−0.056***	−0.060***
	(14.74)	(−3.07)	(−3.04)	(14.24)	(−5.78)	(−5.99)
Log(book assets)			−0.066			0.001
			(−1.27)			(0.01)
Log(market to book)			−0.005			0.058
			(−0.10)			(1.04)
Profitability			0.104			0.397***
			(0.99)			(2.77)
Stock return			−0.065***			−0.066***
			(−2.83)			(−2.61)
Total risk			−0.550***			−0.578***
			(−6.76)			(−6.31)
Dividend payer			−0.197***			−0.216***
			(−4.28)			(−4.58)
Leverage			0.275**			0.169
			(2.31)			(1.23)
Constant	−0.675***	−0.360***	1.031***	−0.644***	−0.324***	1.083**
	(−6.21)	(−5.77)	(2.59)	(−5.62)	(−3.70)	(2.30)
Firm fixed effects	No	Yes	Yes	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19,830	19,830	19,830	16,724	16,724	16,724
R^2	0.099	0.046	0.054	0.106	0.063	0.076

Panel B: Analyst coverage and CSR components

	(1)	(2)	(3)	(4)
	$Community_{t+2}$	$Diversity_{t+2}$	$Environment_{t+2}$	Human $rights_{t+2}$
Coverage	−0.005	−0.018***	−0.037***	−0.006***
	(−1.52)	(−2.71)	(−7.28)	(−3.04)
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	16,724	16,724	16,724	16,724
R^2	0.035	0.181	0.190	0.027

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 coefficients, which is mainly because the model controls for firm fixed effects.

Panel B of Table 2 presents the results of the effect of analyst coverage on different components of the *CSR* scores. Other firm level variables are included but not reported. Each of these components are calculated by subtracting total concerns from total strengths in their respective categories. I present the regressions of CSR scores two years in the future using the full set of controls. *Coverage* obtains a negative coefficient in predicting all four components of my CSR measure, i.e., Community, Diversity, Environment, and Human Rights, and is statistically significant for three of the measures. This analysis reassures 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, 2012), a feature that mitigates the concern 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 exogenous shocks to these firms' analyst coverage. For more details, see Kelly and Ljungqvist, 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, 2012). When two brokerage houses merge, the new entity often ends up with duplicate analysts covering the same firm. So one of these analysts 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 house. 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.[7] 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 forecast by the target and acquirer brokerages one year prior to the merger date. For each merger, I create two sets of companies: one set 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. The candidate 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 the DiD tests, I largely follow He and Tian's (2013) matching procedure, which ensures that the treatment and the candidate control firms are similar in important observable characteristics before the shock. Specifically, I require the candidate control firms to have been traded at least three years before the brokerage disappearance year. For each treatment firm and year, I find control firms in the same tercile of market capitalization, market to book ratio, stock return, stock volatility and stock turnover, and number of analysts before the brokerage disappearance year.[8] Moreover, since there are large differences in CSR variables across industries, I also require the treatment and control firms to be in the same Fama-French 48 industry. I compute the difference in the number of analysts between treatment and candidate control firms, and retain control firms with *at most* five fewer analysts than the respective treatment firm.[9]

Akin to the baseline regressions in Section 3.1, I examine the effect of a loss of analyst coverage on CSR up to two years after the brokerage disappearance. 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 the matching year. Thus, I focus on brokerage closures and mergers from fiscal years 2001 to 2008, and I end up with 278 treatment-control pairs for the main DiD estimation. The average difference in the number of analysts one year before the event is -0.23, and after the event it is -1.26, which verifies that the treatment group lost an average of about one analyst due to brokerage closures/mergers compared to the control group ($-1.26 + 0.23 = -1.02$).

4.2.2. The DiD estimation

A valid DiD estimation should satisfy 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. First, as shown in the first row of Panel A, there is no statistically significant difference in the average growth rates of CSR between the treatment group and the control group from year $t-2$ to year $t-1$. Fig. 1 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

7 Occasionally, I had to look up the SDC platinum database, Factiva news archives, and other Internet sources to gather more information on these mergers and closures.

8 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 ($t-1$). 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 the year 2003.

9 These rather 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 and also fetch a large enough sample. My main conclusions remain the same when I match on other plausible criteria, some of which are discussed in my robustness checks.

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 278 treatment-control pairs from fiscal year 2001 to 2008. 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 firms which do not lose brokerage houses, to the treatment firms on several dimensions described in Section 4.2.1 of the text. Panel A reports the post-matching differences in firm characteristics of the treatment and the control sample. Panel B reports the main results of the DiD estimation. Panel C reports the DiD estimates conditional on treatment group's cash flows (*Cash Flow Diff.*) and level of cash holdings (*Cash/Assets Diff.*) relative to the matched control group. Panel C also reports the DiD estimates conditional on the treatment group's analyst coverage before the brokerage closure and merger (*Initial Coverage*) and the difference in realized change in analysts after the shocks (*Realized DiD in 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
CSR growth $_{(t-2 \text{ to } t-1)}$	−0.0159	−0.0161	0.0002	0.742
Market to book ratio$_{t-1}$	1.918	1.864	0.054	0.945
Market Cap., in $ millions$_{t-}$	8728.95	10,759.4	−2030.45	0.131
Total assets, in $ millions$_{t-1}$	20,220.98	25,967.53	−5746.55	0.353
Annual stock return$_{t-1}$	0.186	0.199	−0.013	0.930
Total risk (annualized)$_{t-1}$	0.441	0.454	−0.013	0.529
Stock turnover$_{t-1}$	3.066	3.004	0.062	0.496
Number of analysts$_{t-1}$	16.13	16.36	−0.23	0.480

Panel B: Difference-in-differences (DiD) estimates

Variable	Treatment	Control	DiD	t-Stat
Diff. CSR $_{(t-1, t+1)}$	0.330	0.098	0.232***	2.98
Diff. CSR $_{(t-1, t+2)}$	0.393	0.148	0.245**	2.48

Panel C: DiD conditional on initial cash holdings, cash flow, coverage and difference in coverage loss

Sub-samples	DiD $(t-1, t+1)$	t-Stat	DiD $(t-1, t+2)$	t-Stat
Cash flow diff. > 0	0.368***	3.63	0.375**	2.88
Cash flow diff. ≤ 0	0.079	0.66	0.94	0.5
Cash holding diff. > 0	0.297***	2.62	0.316**	2.08
Cash holding diff. ≤ 0	0.186*	1.75	0.196	1.56
Initial coverage ≤ median (7.8)	0.223**	2.19	0.346***	2.86
Initial coverage > median (7.8)	0.235**	1.97	0.164	1.07
Realized DiD in analysts < 0	0.245**	2.44	0.325***	2.67
Realized DiD in analysts ≤ 0	0.22	1.38	0.22	1.13

other. The parallel trend assumption is satisfied because the treatment and control groups do not have different pre-trends in CSR before the loss of analysts.

Moreover, Panel A shows that before the shock, all the differences in important firm characteristics between the treatment and the matched control groups are statistically insignificant. In particular, the treatment and control samples have similar sizes, both

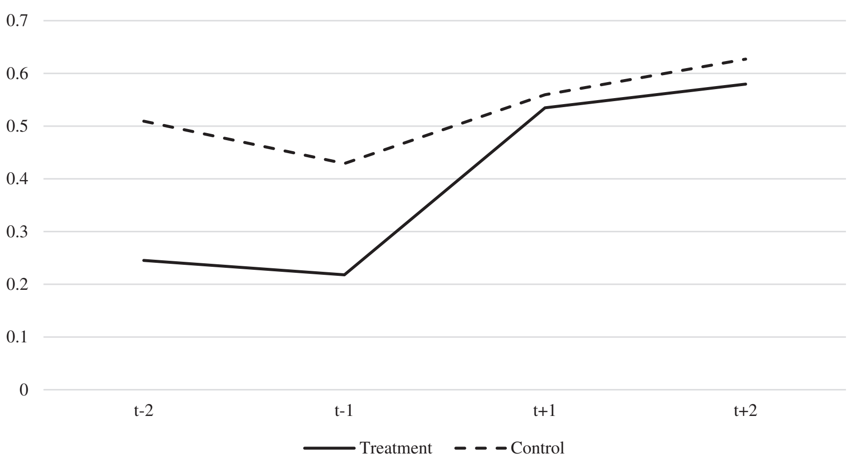

Fig. 1. Average CSR scores of treatment and control groups five years around the brokerage closures/mergers. This figure presents the average CSR scores of the treatment and the matched control samples five years around the brokerage closure/merger years. The sample covers 278 treatment and matched control pairs from fiscal year 2001 to 2008. Construction of the treatment and control samples is described in Section 4.2.1 of the text in detail.

in terms of market capitalization and book assets; they have similar book to market ratios, past returns, stock volatility, stock turnover, and the number of analysts following them.

Overall, the matching process seems to have removed most of the important observable differences between the treatment and control groups before the event. As a result, 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 0.330, whereas this difference for the control group is a much smaller 0.098. Consequently, the DiD estimate for the CSR score is 0.232, which is significant at the 1% level. In economic terms, an exogenous decrease in analyst coverage causes a firm to increase its CSR scores by about 11.8% 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 0.393 while the difference in the control group is a much smaller 0.148. This yields a DiD estimate of 0.245, which is significant at the 5% level. In terms of economic significance, an exogenous drop in analyst coverage leads to an increase in the CSR score by 12.5% 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 monitoring from financial analysts leads managers to cut back on discretionary spending, such as CSR.

Next, I conduct two tests of cross-sectional differences that help shed light on the agency issue of CSR and monitoring role of analysts. Jensen (1986) argues that large free cash flows lead to a conflict between managers and shareholders. Greater cash reserves make it easier for managers to transform firm assets and expropriate value from investors (Myers and Rajan, 1998), and cash holdings are often at the risk of being tunneled out of firms for private benefits (e.g., Frésard and Salva (2010)). If CSR is an agency problem and analysts act as monitors, I expect that the loss of analysts leads to a larger increase in CSR in firms with more abundant cash flows and cash holdings. The results presented in the first four rows of Table 3, Panel C support this conjecture. Specifically, the DiD estimate is significantly larger among the subsample of treatment firms with higher cash flows (rows 1 and 2) and higher cash holdings (rows 3 and 4), compared to their control counterparts before the shock. These results are consistent with the agency view of CSR. However, they should be interpreted with caution because cash and cash flows can be correlated with other non-agency related firm characteristics, which might not have been adequately controlled for.

Next, I examine if the negative effect of analyst following on CSR activities depends on the number of analysts following the firm. Intuitively, loss of analyst coverage should have a stronger effect on CSR in firms which are followed by very few analysts to begin with. In rows 5 and 6 of Table 3, panel B, I show that the DiD estimate for two years after the shock ($t - 1$ to $t + 2$) is larger and statistically significant among matched treatment firms with below-median initial coverage (i.e., 7.8) compared to those with above-median coverage. However, there is no significant difference in DiD between these subsample for $t - 1$ to $t + 1$. This finding suggests that the impact of coverage loss on CSR is more permanent among firms with less initial coverage.

The next test attempts to answer whether the increase in CSR scores after the brokerage mergers/closures hinges on actual loss of analysts. For the main DiD test, I focus on the loss of analysts 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 (which is often 1) 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 compared to controls is 1, the realized loss is not always 1. In some cases, such offsetting effects might dampen the impact of the loss of an analyst on CSR. The last two rows of panel C show that the effect on CSR due to brokerage mergers/closures is more pronounced among treatment firms for which the *realized* difference in analyst coverage is negative, relative to the control firms. In other words, firms are more likely to increase spending on CSR because of exogenous loss of analyst coverage if such loss is not offset by a gain in analysts for other reasons.

4.2.3. Robustness

In this section, I perform a number of robustness checks of my main DiD results to address the concern that the observed DiD results might be an artifact of a specific matching scheme, or a specific way of defining CSR, or is driven by a single outlier event of brokerage closure/merger.

Panel A of Table 4 reports the results of DiD estimation with two 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 and Fama-French 48 industry. As shown in row 1 of Table 5, even with this crude matching scheme, my main DiD results continue to hold although the magnitude of the treatment becomes smaller. This result suggests that the conclusions drawn from the main matched-sample DiD analysis is not an artifact of the specific matching scheme.

Second, in addition to the main 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 economic magnitudes of the effects of the shock are 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 similar to the main analysis.

Third, I employ an alternative way of calculating CSR scores (*Scaled CSR*), which adjusts for the fact 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

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 an alternative definition of CSR. *Crude Match* matches each treatment firm with candidate control firm only on the basis of the fiscal year of brokerage closure/merger and industry. *CSR Match* requires the treatment and control firms to be in the same tercile of CSR scores before the shock. *Scaled CSR* is obtained by scaling the number of each firm's CSR strengths and concerns, respectively, by the maximum number of strengths and concerns that *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. ***, **, 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	0.14***	4.13	0.11***	2.61
2. CSR match	0.23**	2.15	0.34***	2.65
3. Scaled CSR	0.066***	2.66	0.073**	2.47
Panel B: Year-by-year DiD				
Year by year	DiD $(t-1, t+1)$	t-Stat	DiD $(t-1, t+2)$	t-Stat
2001	0.17	0.79	0.43	0.90
2002	0.56**	2.37	0.79**	2.47
2003	0.972*	1.93	−0.238	−0.33
2004	0.397	1.55	0.463	1.43
2005	0.1659	1.01	0.3364	1.60
2006	−0.2857	−0.35	−0.80	−0.78
2007	0.1805	1.62	0.2225	1.91*
2008	0.469	0.18	−0.70	−1.33
Omit 2001 and 2002	0.18**	2.22	0.22**	2.18

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.

$$Scaled\ CSR_{it} = \Sigma(CSR\ Strength_{it}/Max\ CSR\ Strengths_t) - \Sigma(CSR\ Concerns_{it}/Max\ CSR\ Concerns_t) \tag{3}$$

The results of DiD estimates of scaled CSR, reported in row 3, support 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 be 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 the observed differences are driven by one particular year of a 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 larger in magnitude and statistically stronger in the years 2002 and 2003. However, my full sample results are not driven only by these years. As shown in the last row of panel B, the main results hold if I exclude all observations for 2002 and 2003.

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.

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 as described in Section 4.3 of the text. 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. *** indicates the statistical significance level of 1%.

	(1)	(2)	(3)
	First stage: Coverage	Second stage: CSR_{t+1}	Second stage: CSR_{t+2}
Coverage (Instrumented)		*−0.389**** *(−9.96)*	*−0.288**** *(−9.16)*
ExpFollow	0.427*** (22.41)		
Firm controls	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	15,034	15,034	15,034
R^2	0.520	0.063	0.074

4.3. More on identification: instrumental variables 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.[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:

$$ExpFollow_{i,t,j} = \left(Broker\ Size_{j,t}/Broker\ Size_{j,0}\right) \times Follow_{i,0} \tag{4}$$

and

$$ExpFollow_{i,t} = \sum_{j=1}^{N} ExpFollow_{i,t,j} \tag{5}$$

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 instrument 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 highly 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 (more negative) than those obtained from regressions without the correction for endogeneity reported in Table 2. These differences indicate the direction of bias in the regression estimates if the endogeneity in analyst coverage is not controlled for. The implication of this upward bias in the coefficient of *Coverage* (i.e., less negative in OLS) is that some omitted variables simultaneously affect analyst coverage and CSR *in the same direction*. Firms' time-varying investment opportunity can be an example of such a variable. For example, a favorable change in a firm's investment opportunity might attract more analyst coverage (e.g., McNichols and O′Brien (1997)), and, in the meantime, draw scrutiny of local community and CSR activists and make the firm more socially responsible. Another example can be managers' ethical values, to the extent they are time-varying within a firm. More ethical managers likely behave in a more socially responsible manner. They are also likely to be more honest and transparent to investors, and produce higher quality financial reports. This, in turn, could attract more analysts because they can now analyze the firm more easily and make more accurate forecasts. 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 instrumental variable based on exogenous events of change in brokerage size helps eliminate the correlation between analyst coverage and the firm's unobserved omitted variables. As a result, endogeneity in *Coverage* is removed and the coefficient estimates move closer to their true values by turning more negative.

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[10] Yu (2008) and He and Tian (2013) provide several real-world examples that support this view. 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.

[11] I estimate this regression using Stata module "xtivreg2" written by Schaffer (2007).

Overall, my evidence from DiD estimates and the instrumental variables technique suggests there is a negative causal relation between firms' analyst coverage and CSR.

5. Possible underlying channels

In this section, I discuss some potential underlying channels through which analysts cause a decrease in a firm's CSR activities. Many prior studies have documented several ways analysts affect firm policies. So, I follow He and Tian's (2013) approach and focus on discussing the effect of analyst coverage on potential mechanisms, and the effect of mechanisms on CSR, which have been established by prior literature. I then reconcile these earlier findings with the findings of the present study. I provide new evidence when the existing literature does not offer an obvious link.

5.1. Decrease in managerial ownership

I examine managerial ownership as a potential channel through which analysts affect CSR. Managers derive utility from at least two sources: 1) pecuniary benefits from cash compensation and an increase in their wealth invested in the firm's equity and options, and 2) non-pecuniary benefits from managerial perks and pet projects. Many agency models predict that low managerial ownership and imperfect monitoring lead to managers who maximize a combination of private benefits and firm value. Cheng et al. (2014) argue that CSR activities fetch private benefits to managers at the cost of shareholders. To support their view of CSR as an agency problem, Cheng, Hong and Shue show that an exogenous increase in managerial ownership leads to a decline in CSR. Kelly and Ljungqvist (2012) find that firms that lose analysts due to exogenous reasons experience significant stock price declines with no short-term reversal. Kelly and Ljungqvist (2012) do not show a direct effect of analyst disappearance on managerial wealth. However, I expect that this stock price decline should lead to a decline in managers' firm-specific wealth, especially if they have significant ownership stake in the firm initially.

To test this conjecture, I first calculate the changes in market values of equity of the treatment and control firms one year around the shocks. As shown in the first row of panel A of Table 6, treatment firms lose about 9% of their equity value compared to the control firms a year after the brokerage closures/mergers. This result complements those of Kelly and Ljungqvist (2012),

Table 6

Potential mechanisms. Panel A of this table reports the results from difference-in-differences (DiD) estimation on how exogenous shock to analyst coverage affects the value of firms' equity, and CEOs' firm-related wealth. The sample covers 278 treatment and matched control pairs from fiscal year 2001 to 2008. Construction of the treatment and control samples is described in Section 4.2.1 of the text in detail. Panel B conducts DiD analysis of CSR from years $t-1$ to $t+1$ on the matched sample described in Section 4.2.1 (same as in Table 3) in a multivariate framework that controls for the dependence of DiD estimator on possible mechanisms. The sample includes observations with non-missing mechanism variables. Column 1 regresses *CSR* on a treatment dummy (*Treated*), and a dummy to indicate post event period $t+1$ (*After*) and the interaction between *Treated* and *After*. Column 2 regresses *CSR* on *Treated*, *After* and *Treated* ∗ *After* and controls for *Real EM*, the interaction *CEO Firm Wealth* ∗ *High CEO Ownership* and its main effects, and the interaction of each of these channels with *After* (*Channels* ∗ *After*). ***, **, and * indicate statistical significance levels of 1%, 5% and 10%, respectively. In panel B, standard errors are clustered at each pair of treatment and control group.

Panel A: Changes in equity value and CEOs' firm related wealth

	DiD	t-Stat
Diff. Log(Market Cap.)$_{(t-1,\,t+1)}$	−0.0899**	−2.19
Diff. Log(CEO Firm Wealth)$_{(t-1,\,t+1)}$	−0.0908	−1.34
Among Initial CEO Ownership ≥ Median (0.86%)		
Diff. Log(CEO Firm Wealth)$_{(t-1,\,t)}$	−0.1761**	−2.09
Diff. Log(CEO Firm Wealth)$_{(t-1,\,t+1)}$	−0.2518*	−1.65

Panel B: DiD tests with controls for potential mechanisms

	(1)	(2)
	CSR	CSR
Treated ∗ after	0.172*	0.141
	(1.73)	(1.28)
Treated	−0.202	−0.207
	(−1.39)	(−1.37)
After	0.156**	0.334*
	(2.24)	(1.82)
Real EM		−0.427**
		(−2.25)
CEO firm wealth ∗ high CEO ownership		−0.007**
		(−1.98)
High CEO ownership		−0.208
		(−1.13)
CEO firm wealth		0.007**
		0.141
Channels ∗ after		Yes
N	790	790

who show price drops a few days around these events. I then calculate the difference in CEO's firm-related wealth, which is the value of their stock and option portfolio (see Daniel et al. (2016)), among the treatment and control firms. In a DiD setting that uses the entire sample of matched firms, I find that CEOs of treatment firms lose an average of about 9.1% more wealth after the shock, but the difference is not statistically significant (second row of Table 6, Panel A). However, among firms with above-median initial CEO ownership (i.e. 0.86%), treatment firms' CEOs lose about 17.6% of their firm-related wealth in the same year of analyst disappearance (i.e., from $t - 1$ to t), and about 25.2% of their wealth between $t - 1$ and $t + 1$, compared to the control firms' CEOs, as shown in the last two rows of Table 6, Panel A.

The above DiD analysis shows that an exogenous decrease in analyst coverage decreases firm equity and, as a result, decreases the ownership stake of CEOs, especially those with sizeable initial ownerships. This finding, coupled with Cheng et al.'s (2014) finding, points to the possibility that a decline in CEOs' firm ownership may be a mechanism through which loss in analyst coverage causes an increase in CSR.

5.2. Discretionary spending

The pressure from analysts to manage real earnings by reducing discretionary spending (Irani and Oesch (2016)), part of which contributes to better CSR, is another potential mechanism through which analysts reduce CSR.

Greater analyst following has two effects that can compound the problem of managers. Analysts put pressure on managers to meet their earnings benchmarks but make accruals-based earnings management, one popular tool managers use to meet those benchmarks, more difficult (Yu (2008)). So, one plausible way to meet analyst expectation is to cut back on discretionary real spending as a part of real earnings management. Di Giuli and Kostovetsky (2014) find that increasing CSR scores requires extra (discretionary) spending that likely goes to charitable giving, pollution control, investment in community etc. but does not contribute to revenues.[12] Other studies (e.g., Hong and Andersen (2011) and Kim et al. (2012)) have also documented a negative relationship between CSR scores and real earnings management, i.e., a positive relation between discretionary spending and CSR scores. Moreover, my own analysis of the economic mechanisms, discussed later in Table 6 panel B, also shows this relationship. Irani and Oesch (2016) find that after the exogenous loss of analysts, managers spend more on discretionary expenses (i.e., manage real earnings less). In sum, after the loss of analysts, not having to engage in real earnings management by cutting discretionary expenses may lead to more spending on CSR.

5.3. Direct vs. indirect effect

I proposed two potential mechanisms through which financial analysts might curb CSR activities. However, it is still unclear whether analyst coverage has a direct effect on CSR or only an indirect effect through analysts' effect on the underlying channels already well-known in the literature. In this section, I jointly test the effect of the two potential underlying mechanisms and attempt to tease out the residual effect of analysts on CSR. To do so, I employ a methodology similar to He and Tian's (2013) and directly control for the economic mechanisms in a DiD regression as follows, using the matched sample described in Section 4.2.1.

$$CSR_{i,t} = \alpha + \beta_1 Treated \times After + \beta_2 Treated + \beta_3 After + \gamma_1' Channels_{i,t} + \gamma_2' Channels_{i,t} \times After + \varepsilon_{i,t} \tag{6}$$

where i indexes firm and t indexes pre- or post-event period ($t - 1$ and $t + 1$). *Treated* is a dummy variable that equals 1 for treatment firms and zero for control firms. *After* is a dummy variable that equals one for post-event period ($t + 1$), and zero otherwise. *Channels* is a vector of variables that proxy for my two underlying channels. The first channel is discretionary spending as a part of real earnings management (*Real EM*), which I calculate as the sum of negative abnormal discretionary expenses and abnormal production costs, as defined by Irani and Oesch (2016). The second channel is the interaction of above-median CEO Ownership and CEO firm wealth (*High CEO Ownership* × *CEO Firm Wealth*) consistent with the findings of Section 5.1 that analysts affect firm-specific wealth mainly of the CEOs with higher ownership stakes. Since this channel is represented by an interaction variable, I also include the two main effects. To more cleanly identify the residual effect of *Treated* × *After*, I also include the interaction $Channels_{i,t} \times After$ for each channel, which controls the possibility that the observed DiD effect is due to some confounding events that might have affected the underlying channels *regardless of* broker closure/mergers.

The key variable of interest is the DiD estimator β_1, which represents any residual treatment effect of analyst following on CSR after controlling for the two economic mechanisms.

I report the estimates from Eq. (6) in panel B of Table 6. In column 1, I estimate the model without any mechanism variable to obtain a benchmark estimation. The coefficient of β_1 is 0.172, which is statistically significant at 10% level. Note that this point estimate is different, and its statistical significance is weaker, compared to the DiD estimate of 0.232 in Table 3 Panel C. This is because I restrict the sample only to firms with non-missing mechanism variables for this analysis. In column 2, I estimate regression 6 and control for all mechanism variables. The estimate of β_1 continues to remain positive but the magnitude decreases to 0.141, and turns statistically insignificant at conventional levels. The difference in point estimates between (1) and (2) reflect

[12] Most firms do not directly report expenses related to CSR. Di Giuli and Kostovetsky (2014) argue that such expenses become a part of (extra) SG&A expense, so they use SG&A expense to assign dollar amounts to CSR scores. They estimate that one standard deviation increase in CSR score is associated with 6.4% increase in SG&A, which equals to an extra \$44 million for the mean firm, and an extra \$201 million for the mean firm in S&P 500.

Table 7
Exploring earnings target channel: Importance of negative EPS surprises. This table reports the results from DiD estimation on whether the effect of analyst coverage on CSR depends on a firm's likelihood of missing analysts' quarterly EPS targets, and whether the exogenous coverage loss leads to an increase in the likelihood of missing the targets. The sample covers 278 treatment and matched control pairs from fiscal year 2001 to 2008. Construction of the treatment and control samples is described in Section 4.2.1. Treatment firms in "*missers*" group experience a negative quarterly earnings surprise (i.e., Actual EPS < Consensus) during the year immediately before the coverage shock, $t-1$ (alternatively, during the year $t-1$ or $t-2$). "*non-missers*" are the treatment firms not in the "*missers*" group. ***, **, and * indicate statistical significance levels of 1%, 5% and 10%, respectively.

	DiD	t-Stat
Diff. in CSR$_{(t-1,\,t+1)}$ conditioned on:		
No negative EPS surprises in $t-1$ ("*non-missers*")	0.243***	2.71
At least one negative EPS surprise in $t-1$ ("*missers*")	0.186	1.25
No negative EPS surprises in $t-1$ and $t-2$ ("*non-missers*")	0.221**	2.19
At least one negative EPS surprise in $t-1$ and/or in $t-2$ ("*missers*")	0.246*	1.79
Diff. in # negative EPS surprises$_{(t-1,\,t+1)}$	0.097	1.26

an approximately 18% drop from the benchmark DiD estimate. This result suggests that the two proposed mechanisms explain about one fifth of the total effect of analyst coverage on CSR. The mechanism variables seem to affect CSR as predicted by theory. Specifically, CSR is higher when real earnings management is lower and when CEO's wealth is more sensitive to firm performance because they have invested more dollars in the firms and hold larger fractions of the firms.

Interestingly, the coefficient on *Treated* × *After* in column 2 is not statistically significant, but its size is still about 82% of that from column 1. Lack of strong statistical significance makes the interpretation of the residual effect difficult. However, it suggests that there exists a large (albeit imprecisely estimated) direct effect of analyst coverage on CSR after accounting for the (more precise) indirect effect via my two proposed economic mechanisms.[13]

6. Alternative explanation: "earnings target" story

An alternative interpretation of my findings is that financial analysts may not care about CSR but care about firms' earnings. Because it is important for managers to meet analysts' expectations (Graham et al. (2005)), earnings targets set by these analysts prevent managers from making wasteful spending, including CSR expenses. Therefore, this "earnings target" story might also be important in explaining the effect of analyst coverage on CSR, especially because my monitoring channels explain only about one fifth of the analysts' effect.[14,15] In this section, I examine whether and how a firm's ability to meet earnings targets matters for the relation between analyst coverage and CSR.

He and Tian (2013, Internet appendix) show that the failure to meet an earnings target leads to a larger negative stock price reaction when a firm is followed by more analysts. This happens because greater analyst coverage facilitates faster and more complete price adjustment. An implication of He and Tian's (2013) finding for this study is that firms may spend more on CSR after the coverage loss because now they will be penalized less for missing earnings targets. I build on this finding by He and Tian (2013) to test the following two predictions implied by the earnings target story:

One likely prediction of the earnings target story is that the effect of analyst coverage on CSR should be stronger among firms which are more prone to missing analysts' earnings target. I employ a firm's recent history of missing quarterly earnings (EPS) targets as a proxy for its likelihood of doing the same in the near future.[16] Accordingly, I partition my original matched sample into two groups: 1) firms which experienced a negative quarterly earnings surprise (i.e., Actual EPS < Consensus) during the year immediately before the coverage shock, $t-1$ (referred to as "*missers*" hereafter), and 2) firms which did not have a negative surprise in $t-1$ ("*non-missers*"). The DiD estimates presented in the first two rows of Table 7 show that the increase in the treatment group's CSR is more pronounced among the *non-missers* sample than among the *missers* sample. This result is not consistent with the earnings target story. I also do not find much support for this story when I construct the *missers* and *non-missers* samples based on negative surprises during the two years before the shock ($t-1$ and $t-2$). As shown in Table 7, the DiD estimates of CSR in both sub-samples are about the same.

Another potential implication of the earnings target story is that the treatment firms miss analysts' expectations more often after the coverage loss because they spend more on CSR. The DiD estimate presented in the last row of Table 7 reveals that

[13] However, as He and Tian (2013) point out, this residual effect might not represent analysts' *direct* effect but their effect via other economic mechanisms that this study did not uncover. This is admittedly a significant possibility because many recent studies have found numerous ways analysts affect corporate policies/outcomes. Although I explored several other economic mechanisms (with less success), exploring all the possible mechanisms is formidably challenging.

[14] A few points are worth-mentioning here. The anecdotes presented in the introduction of this paper suggest that financial analysts do not view CSR positively. However, my conclusions do not hinge on analysts caring about CSR per se, but caring about any firm policy (including CSR) which does not contribute to performance. Moreover, earnings target explanation and monitoring explanations are not mutually exclusive. While financial analysts often behave myopically and focus too much on short-term earnings (e.g., Cheng et al. (2007); He and Tian (2013)), earnings targets also serve as a tool to keep managers focused on value creation (e.g., Chung and Jo (1996); Knyazeva (2007) and Chen et al. (2015)).

[15] As shown in panel B of Table 6, although the residual (direct) effect of analyst coverage on CSR is large, it is also noisy (large standard error). As a result, it is likely difficult to find more channels that systematically explain this effect, at least in my sample.

[16] This assumption seems reasonable because I find that the time-series of negative earnings surprises exhibits a strong positive serial correlation (Pearson correlation = 0.66, p < 0.0001).

the treatment firms experience a positive but statistically insignificant change in the frequency of negative surprises after the shock. So, despite spending more on CSR, the treatment firms do not miss earnings targets significantly more. This happens possibly because the loss of analyst coverage also makes it easier for firms to do accrual-based earnings management (Yu (2008); He and Tian (2013)), which can help cover up the extra spending on CSR.[17]

Overall, I do not find strong evidence to support the view that the effect of analyst coverage on CSR is driven by firms' concern of missing short-term earnings targets. However, admittedly, these results do not completely rule out the importance of earnings target because recent studies have uncovered the impact of analyst coverage on a wide variety of firm policies, a fact that makes it difficult to ascertain the partial effect of either CSR or analyst coverage on earnings.

7. 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 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 a positive effect on a firm's existing shareholders by showing that they curtail spending potentially wasteful for shareholders, these results are agnostic to the issue of whether CSR is good for society as a whole and to how financial analysts affect the welfare of stakeholders other than shareholders.

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[17] Analysts may also revise their forecasts in anticipation of increased CSR spending (Ioannou and Serafeim (2015)).

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