Monitoring of Managers and CSR

- CSR As a Governance Problem

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Abraham Hvidberg Paaske

Supervisor: Georg Wernicke

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Abstract

This thesis analyzes Corporate Social Responsibility (CSR) as a governance problem by examining the level of CSR activity of firms that experience a reduction in the level of managerial monitoring, measured as the number of security analysts who follow the firm. If CSR rating of firms can be used as a proxy for the CSR expenditures of firms, then following the overinvestment hypothesis on CSR, one would observe a higher level of CSR expenditures in the firms who's managers are unexpectedly less monitored, implying that managers obtain utility from investing in CSR, at shareholders' expense. The thesis starts with an overview of different perspectives of CSR, and how this thesis fits with the existing literature. To measure the effects of a reduction in managerial monitoring, a natural experiment is undertaken. Brokerage house mergers are used as an exogenous event causing a reduction in analyst coverage. Following the merger of brokerage houses, it is expected that a number of analysts employed at said brokerage houses will be found redundant in the new merged entity, causing some firms to be followed by fewer analysts than before the merger. Using a difference- in differences methodology, I compare the difference in the observed CSR rating of companies affected by the brokerage house mergers, compared to those companies that are not affected by the brokerage house mergers. To measure the level of CSR activities of companies, I use data from Kinder, Lydenberg, and Domini (KLD). The results of the natural experiment, if valid, indicate that firms experiencing a reduction in analyst' monitoring receive a significantly higher "community involvment" score in the year after the exogenous reduction in analyst coverage compared to the group of control firms who do not experience a reduction in analyst coverage. This effect is however only observed in the group of firms with a below-median level of analyst coverage in the year before the merger. Some caveats, mentioned in the text, must also be considered.

Introduction

"The directors of such [joint-stock] companies, however, being the managers rather of other people's money than of their own, it cannot well be expected, that they should watch over it with the same anxious vigilance with which the partners in a private copartnery frequently watch over their own. Like the stewards of a rich man, they are apt to consider attention to small matters as not for their master's honour, and very easily give themselves a dispensation from having it. Negligence and profusion, therefore, must always prevail, more or less, in the management of the affairs of such a company."

With this quote from Adam Smith's *The Wealth of Nations* (1776), Jensen and Meckling begin their (1976) paper on agency costs and the theory of the firm, illustrating that the nature of the agency problem was identified already by Adam Smith in 1776: Managers may not watch over the company's funds with the same "anxious vigilance" compared to if it had been their own money.

Jensen and Meckling (1976) show that the agency problem of firms with a separation of management and ownership has a cost, and that this cost can be reduced through increased voluntarily disclosure by the managers or by increased monitoring of the managers' behavior by the owners.

If it is expected that managers will watch over the company's fund's with greater care and take greater care to make sure that the funds are well invested if they are more closely monitored by the owner's of said funds; then it should be interesting to examine whether the managers will spend more or less funds on Corporate Social Responsibility (CSR) activities if the level of monitoring changes.

What is Corporate Social Responsibility?

There exists no exact definition of what the concept of CSR encompasses, and it appears to be interpreted in many different ways. Any analysis of CSR should therefore clarify its explicit meaning (Bénabou and Tirole 2009). Bénabou and Tirole (2009) suggest three different views of CSR, and consecutive explanations for why we observe the phenomenon.

Win-Win View

The first view is the "win-win" view, as proposed by Porter and Kramer (2006). It suggests that CSR is a way for firms to increase their profitability. Firms can "do well by doing good". Bénabou and Tirole (2009) maintain that there are two ways in which this "promise of a free lunch" could be possible: One is that CSR efforts help firms maintain a long-term focus, and thereby correct for a short-termism observed in many companies. For example, a firm may choose to cut back on emission control in order to increase short-term profits, but this might increase the probability of a future public-relations scandal in the future. CSR policies can therefore help the company maintain a long-term profitability focus. The second possibility is through "strategic CSR", in which CSR could for example be used as a way for companies to market themselves as more "green" or "socially responsible". Flammer (2013) find evidence indicating that increase firms to increase their CSR efforts. This effect is especially pronounced in business-to-consumer industries.

Investors may have increasingly started to agree with this view of CSR. Ioannou and Serafeim

(2013) find evidence that investor preferences have shifted over the last 15 years from viewing CSR as an increased agency cost towards a more value-creating activity and thereby view CSR more favorably.

However, the strategic use of CSR may also stem from less noble motivations, as Bénabou and Tirole (2009) notes, the use of CSR could be a way for firms to appease the public and lawmakers in order to reduce the probability of stricter regulation of the industry in the future. CSR could also be used as a way to hurt the competition through pushing for stricter environmental or other pro-social regulation that the firm knows will particularly stifle other firms in the industry.

Delegated Philanthropy View

The second view of CSR is what Bénabou and Tirole (2009) term the "delegated philanthropy view". This view posits that other stakeholders of the firm are interested in promoting social good, and therefore accept a lower financial return from the firm. Some investors may prefer to invest in firms they deem to be socially responsible, even though it does not offer the highest immediate return. Some customers may prefer to deal with companies they know are socially responsible. As Bénabou and Tirole (2009 p.10) writes: *"[..] stakeholders have some demand for corporations to engage in philanthropy on their behalf. The corresponding CSR profit sacrifice is then passed through to stakeholders at their demand.* "They note that one then needs to explain why one would want corporations to do good on their behalf compared to through other charitable organizations with a specific mission to relieve social issues. One explanation could be that it is because the socially responsible firm has lower transaction costs in doing the social work compared to a charitable organization. The example given by Bénabou and Tirole (2009) is that

instead of charitable organizations working to improve the living conditions of poor coffee farmers, the customer, a corporation with a "fair-trade" focus, decides to pay above-market prices to the farmers in order to improve their living conditions. A different explanation for the delegated philanthropy view is that the philanthropic activity of corporation offers no advantage in efficiency of promoting social welfare compared to charitable donations, but that it is simply a reaction to consumer and other stakeholder demands.

Insider-initiated philanthropy view

The third view of CSR posited by Bénabou and Tirole (2009) is that of insider-initiated philanthropy. This perspective of CSR is that CSR is not primarily motivated by a desire to appease stakeholders or to give the corporation a strategic advantage, but is primarily motivated by managers' desire to be involved in philanthropy, and will reduce the profitability of the firm. This can for example be by giving to charitable organizations. Sometimes this happens to be charity organizations where managers or board members of the corporations have personal affiliations.

As Bénabou and Tirole (2009) writes, the insider-initiated view of philanthropy poses some corporate governance concerns. In order for the management to be able to donate to projects they themselves deem to be charitable at the expense of profits and thereby shareholders, a certain level of management entrenchment must be expected. Kacperczyk (2009) finds that increased takeover protection leads to firms increasing their community and environmental efforts, indicating that an increased level of management entrenchment leads to a higher level of philanthropic spending by managers. However, Kacperczyk (2009) also finds this increase in takeover protection is associated with a higher level of long-term firm value, which is interpreted to mean that this increased managerial entrenchment actually helps protect the firm against the short-termism of the stock market, and thus supports the "win-win" perspective on CSR. On a related note, Cespa and Cestone (2007) find that CSR activities can be used as an entrenchment strategy by managers in itself. They argue that inefficient managers have a motivation to engage in CSR activities that gain the support of specific stakeholders. Those stakeholders that are positively affected by these CSR activities are then more likely to voice their concern and support the incumbent manager in the event of a potential takeover threat. Cespa and Cestone (2007) argues that this helps explain the emergence of specialized institutions to monitor CSR activities of firms, as this provides a standardized way of measuring CSR activities and thereby reduces the probability of stakeholders supporting the incumbent management in the event of a takeover, because the stakeholders are not reliant on the incumbent managers for the CSR activities of the firm to be continued after the takeover.

It is this insider-initiated philanthropy understanding of CSR that has been criticized, most famously by Milton Friedman (1970), and the view of CSR that this thesis seeks to investigate further.

Legitimacy theory

Another way to explain the phenomenon of CSR is through legitimacy theory (Dowling and Pfeffer 1975). Legitimacy theory provides a framework for analyzing the congruence of values of organizations and their surrounding society, and posits that organizations seek to increase their legitimacy in the society they are a part of. CSR activities can thereby be explained as a way for firms to increase their legitimacy in their society. Legitimacy theory can be said to be an extension of the concept of the social contract by Jean-Jacques Rousseau applied to organizations.

Is CSR appropriate?

Conceptually separate, but related to, the discussion of why we observe the phenomenon of CSR, there is a discussion of the appropriateness of CSR, and whether it is value-adding, value-destroying, or in fact irrelevant to value-adding. (Jo and Harjoto 2011)

Milton Friedman has quite directly opposed the notion of CSR, arguing that "*The Social Responsibility Of Business Is To Increase Its Profits*", as is the title of his 1970 article in The New York Times. In his article he states that if managers of corporations divert resources to projects they themselves deem to be "socially responsible", they are essentially imposing a tax on their shareholders. This is an argument against what Bénabou and Tirole (2009) term "Insider-Initiated Philanthropy". Friedman writes that it should be up to the shareholders as private citizens themselves to decide which, if any, social improvement programs they wish to support. As Karnani (2011) also points out, allowing managers of companies to become redistributors of funds to social programs is an undemocratic process, and undermines the system already in place for social redistribution of resources; through the government.

Aneel Karnani (2011) argues that there is no need for the notion of CSR. He writes that if markets are working, then the interests of the firm and society are perfectly aligned. However, in the case of market imperfection, for example in the case of externalities, the interests of the firm are not aligned with that of the surrounding society, and there exists a tradeoff between the profits of the firm and the welfare of society. In these cases, government regulation of the market is the most effective way of correcting the failed markets. Calling upon companies to invest in CSR in order to self-regulate the imperfect market will not adequately address the problem of the failed market.

Aneel Karnani writes that the most socially responsible action a firm can undertake is to maximize its profits. When all actors pursue this strategy in a well functioning market, the utility of all market participants will be maximized. In cases of market imperfection, these should be corrected by government regulation, and not through industry self-regulation such as CSR, as this is not the most effective way of addressing the failed market.

Freeman (1984) argues that managers have a broader responsibility beyond their fiduciary duty to their shareholders. He argues that the modern corporation is different from that of the traditional corporation. The traditional corporation would only process goods from a supplier and then sell these processed goods to their customers. The modern corporation however, is much more complex and thus has a broader set of constituencies, stakeholders, in which the corporation has a responsibility to serve their interests. Stakeholders can be anyone who has a claim on the organization, be it customers, the local community, or the government. Evan and Freeman (1988, p 103) argue that: *"The task of management in today's corporation is akin to that of King Solomon. The stakeholder theory does not give primacy to one stakeholder group over another, though surely there will be times when one group will benefit at the expense of others. In general, however, management must keep the relationship between stakeholders in balance. When these relationships become imbalanced, the survival of the firm will be in jeopardy." Under a stakeholder view, investing in CSR activities may be appropriate, because it appeases a certain group of stakeholders. However, a strict interpretation of the stakeholder view implies that the interests of the owners of the corporation ranks no higher than any other stakeholder of the*

organization. Following the theory of the firm of Jensen and Meckling (1976), a company with managers akin to that of King Solomon, placing no primacy to the interests of the owners, will face higher costs of capital. If investors know that their interests will not have primacy over other stakeholder, they will most likely require a higher rate of return on their investment, or invest somewhere else, forcing the price of capital for the firm upwards.

Jensen (2002) with his enlightened value maximization theory agrees that attention to stakeholders is important in order to stay competitive, but that the manager's ultimate responsibility is to maximize the value of the firm. Instructing managers to balance the interests of all stakeholders provide no clear guidance for behavior. Metrics such as the "balanced scorecard" approach provides no clear indication of whether a company is performing better or worse. If the management of a firm uses a stakeholder approach as a rationale for investing in CSR, it can at worst dilute the accountability of the managers. As described by Thomsen and Conyon (2012 p.114) in relation to corporate governance and CSR: "*too many objectives for the firm are, in fact, no objective at all*".

As the above discussion illustrates, the issue of CSR is complex and multifaceted, and any discussion of CSR's appropriateness must always first address what is specifically meant by the term. No resolution to the question of CSR's value-adding characteristics has been universally accepted. Nevertheless, the "Insider-Initiated Philanthropy" view of CSR implies a corporate governance problem, in which managers impose a tax on the shareholders, as pointed out by Friedman (1970), and which is the perspective that is further examined in this thesis.

Security analysts' role

Jensen and Meckling's (1976) theory of agency costs suggests that security analysts play a key role in the monitoring of the management. Security analysts perform a form of monitoring of the management to the benefit of existing and potentially new shareholders, and thereby help reduce the agency costs of firms that have a separation of ownership and control. Security analysts supply earnings forecasts of firms on a timely basis. They are often industry experts, have close and frequent contact with corporate insiders, and thus act as information intermediaries between the company and the market. Security analysts are often employed by brokerage houses, consulting services firms, or other independent research services (Jensen and Meckling 1976).

Chung and Jo (1996) find evidence that analyst coverage helps reduce the agency costs of the separation of ownership and control by effectively disseminating information in the market and making it easily available to investors. This reduces the amount of "private information" held only by the managers. Chung and Jo (1996) contends that analysts help discipline managers and make them less likely to spend corporate funds on activities that enrich the managers at the expense of shareholders. Chen, Harford, and Lin (2013), find that the agency costs increase in firms that experience an unexpected reduction in the number of analysts following the firm.

Hypotheses

Barnea and Rubin (2010) find that increased level of insider ownership of corporations is associated with a lower social rating of firms. Assuming that a higher CSR rating is associated with higher CSR expenditures, then the implication Barnea and Rubin suggest is that owners with a personal affiliation to the company invest more in CSR activities when they do not have to bear all of the costs themselves. CSR can thus be viewed as a conflict between shareholders. Shareholders with a personal affiliation to the firm would like to be perceived as good citizens and thereby derive utility from being associated with a firm with a high CSR rating, at the expense of the minority shareholders. This implication suggests that CSR activities may not be value-adding and thereby adds support to what is termed the overinvestment hypothesis of CSR – meaning that companies overinvest in CSR activities because they bring reputational benefits to the insiders at the expense of minority shareholders.

If CSR activities can be viewed as an agency problem, then it would be interesting to measure what happens to the level of CSR expenditure if the level of managerial monitoring changes. Following Barnea and Rubin (2010), who find that insiders are more inclined to spend resources on CSR activities when they do not have to bear all the costs themselves, managers, a form of insiders, may become inclined to spend corporate funds on charitable projects if they are suddenly faced with less monitoring.

Thus, if CSR activities provide reputational benefits to the managers at the expense of shareholders, we would expect to see a negative relationship between the level of monitoring by analysts and the level of CSR activities. Since CSR is a broad area with little consensus of what constitutes the boundaries of CSR, I choose to primarily focus on the level of philanthropic expenses following a reduction in analyst coverage, in line with the "Insider Initiated Philanthropy" view of CSR by Bénabou and Tirole (2009). I expect that philanthropy is the area of CSR where corporate managers are most likely to gain any personal or reputational benefits, since donations will often involve personal appearances by the managers to cut ribbons or partake in other ceremonial activities. The effect of a philanthropic donation is directly

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measurable by the manager and the results are immediate compared to for example an emissions reduction program, which may take many years to implement and measure the effects of. This leads to hypothesis 1:

Hypothesis 1: Ceteris paribus, a company's philanthropic expenses will increase following an exogenous reduction in analyst coverage.

Although I expect the effect to be most noticeable in the level of philanthropic expenses, it is not unreasonable to expect an increase in the overall CSR activity of a company following a reduction in analyst coverage as well. Hypothesis 2 is therefore also included:

Hypothesis 2: Ceteris paribus, a company's CSR activity will increase following an exogenous reduction in analyst coverage.

If one finds evidence to support these hypotheses, then it will add support to the overinvestment hypotheses mentioned by Barnea and Rubin (2010) and by Jo and Harjoto (2011)

Methodology

Chung and Jo (1996) find that analysts are more likely to cover "high quality" companies, since these companies are easier to market to their clients. There is therefore a possibility that brokerage houses select companies to cover based on the company's financial performance and the markets expectation of their performance. In addition, Cochran and Wood (1984) found that there is a correlation between the level of firms' CSR activities and the financial performance of the firms'. Together, these two observations means that the study of the CSR rating in relation to the analyst coverage might then suffer from an issue of endogeneity since the financial performance may be affecting both the CSR activities of the firm, as well as the level of analyst coverage, and it will be difficult to isolate the effect of the reduction in analyst coverage alone.

To overcome this potential endogeneity I employ a difference in difference methodology, using a reduction in analyst coverage as a treatment effect. A difference in differences methodology is classified as a quasi-experiment, or natural experiment (Wooldridge 2008). Ideally, one would have undertaken a controlled experiment in a laboratory in order to determine the causal relationship between events, however this is not possible when dealing with socioeconomic real life events such as CSR. Instead, one can only observe what happens when one of two otherwise identical groups are exposed to a "treatment effect". In both groups, one compares the difference in a variable of interest before and after the "treatment effect", and then compares the difference between the differences in the treatment and control group, thus the name "difference in differences". Using a difference in differences methodology allows for one to draw clearer inferences of causality compared to when not using a control group, as one then cannot know the outcome of the variable of interest had the treatment effect not occurred.

An assumption of the difference in differences methodology is that the treatment and control group have similar characteristics and that their development with regards to Y had followed the same path had the treatment effect not occurred. That is, that the two groups have "parallel trends" (See Figure 1) (Abadie 2005).

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To estimate the difference in differences estimator using OLS, the data is usually broken down into four groups as Table 1 illustrates, and the following model is estimated:

$$y = \beta_0 + +\beta_1 \times TREATED + \beta_2 \times POST + \beta_3 \times POST \times TREATED + \varepsilon_1$$

Where TREATED is a dummy variable equal to one if the observation is in the treatment group. POST is a dummy variable equal to one if the observation is in the post-treatment period. The interaction between the two dummy variables is the difference in differences estimator.

Formally:
$$\hat{\beta}_3 = (\hat{y}_{POST=0,TREATED=1} - \hat{y}_{POST=0,TREATED=0}) - (\hat{y}_{POST=1,TREATED=1} - \hat{y}_{POST=1,TREATED=0})$$

Table 1 lays out the differences in differences estimation methodology. The rightmost bottom

cell is the difference in differences estimator.

Dependent variable: y _{gt}	g=0 (Treatment group)	g=1(Control group)	Difference
t=0	y0,0	y1,0	y0,0- y1,0
t=1	Y0,1	Y1,1	y0,1 -y1,1
Difference	Y0,0- Y0,1	y1,0- y1,1	$=(y_{0,0}, y_{0,1})-(y_{1,0} - y_{1,1})$

Table 1: Difference in differences estimator

(Adapted from Wooldridge (2008) p. 453)

Treatment effect

The treatment effect in this experiment is an exogenous reduction in analyst coverage caused by the merger of two brokerage houses. Following the merger of two brokerage houses it is expected that there will be some number of analysts found to be redundant in the new merged brokerage house. The new merged entity will most likely not employ two analysts to cover the same stock if only one analyst was covering the stock in their respective brokerage houses before the merger. The companies that were covered by these two analysts should therefore experience a reduction in coverage following the merging of the two brokerage houses. This reduction in analyst coverage should be exogenous of the covered firms' financial performance.

To use brokerage house closures and mergers as an exogenous shock to analyst coverage has been increasingly used in the later years to study the effect of changes in the level of monitoring of managers. Irani and Oesch (2013) find that companies' financial reporting quality decrease following a decrease in coverage. Hong and Kacperczyk (2010) find that the optimism bias of analysts' earnings forecasts increases following brokerage house mergers, indicating a reduction in the quality of available information. Kelly and Ljungqvist (2012) find that the information asymmetry between shareholders and insiders increases as a result of the coverage reduction and that this therefore affects the stock price negatively. Chen, Harford, and Lin (2013) find that the agency problem between shareholders and managers increase due to the coverage reduction and thus adds to Kelly and Ljungqvist (2012), claiming that another reason for the witnessed drop in stock price is because the market prices in the increased agency problem caused by the reduction in analyst coverage.

A potential issue with using the brokerage house mergers as an exogenous shock in monitoring is Chung and Jo's (1996) finding that brokerage houses are more likely to cover "high quality" firms. As a result of this, there will most likely be a higher probability of "high quality" firms being covered by both of the two merging brokerage houses, and thereby a higher probability of "high quality" firms being selected as being part of the treatment sample. The selection of the treatment sample can therefore no longer be assumed to be unbiased. Because the selection of the treatment sample may be biased, I may violate the "parallel trend" assumption. Industry fixed effects and firm characteristics are added as explanatory variables to the model in an attempt to control for differences in firm characteristics and industries in the sampling.

Data

To track the number of analysts following firms I use the Thomson Reuters Institutional Brokerage Estimate System (I/B/E/S). This is a comprehensive database attempting to track all estimates made by financial analysts. The database contains data as far back as 1976 and collects data from more than 930 brokerage houses. Every brokerage house and analyst is assigned a unique code, which allows users to track the employment history of individual analysts. The unique code given to every analyst makes it possible to track how many analysts are covering a given firm in a given period of time.

To measure the CSR activity of companies I use data from Kinder, Lydenberg, and Domini (KLD). KLD data is also used in a similar difference in differences study on CSR by Flammer (2013). KLD has historical data back to 1991, but their coverage has increased considerably after 2003. Before 2001 they covered approximately 650 firms. After 2003 they cover approximately 3100 firms. KLD provides approximately 80 indicators in seven quantitative issue areas: Community, Corporate Governance, Diversity, Employee Relations, Environment, Human Rights and Product. For each quantitative issue area, they mark every firm they rate with a "strength" point and/or "concern" point.

To measure the level of philanthropic activities of a company, I use the sum of community strength indicators in the KLD dataset, named *com_str_num*. There are eight possible indicators a company may receive a "strength" point for: Charitable Giving, Innovative Giving, Non-US Charitable Giving, Support For Housing, Support For Education, Indigenous Peoples Relations, Volunteer Programs, and Other Strengths. The maximum com_str_num value for a company is therefore eight, and the minimum is zero.

To measure the overall CSR activity of a company I use the sum of all the quantitative issue area strengths, except the corporate governance issue area. I include only the strengths and not the weaknesses because the motivation to undertake a "positive" CSR effort by the managers of a firm may be conceptually different from that which will cause a company to receive a "concern" score from KLD, and thus the two concepts should therefore not necessarily be measured on the same continuum. I do not include the corporate governance issue area, as it is not an issue area related directly to external social contributions made by the firm.

The KLD data is dichotomous and not continuous. This may have some consequences for the precision of the data. For example, when measuring the amount of donations given by a company, KLD has only one dichotomous variable indicating whether the company has donated over a certain amount consistently for the last three years. A company that donates consistently close to this number, but not enough to fulfill the requirement, will be placed in the same category as those firms who have not donated anything. This coarseness of the data can also make it difficult to use the data for estimation purposes. One must either construct composited scores using sums of the dichotomous data, or use estimation methods designed for dichotomous outcomes such as logit or probit.

Before 1995, KLD only provides the official stock exchange ticker and company name as identifying information in their database. The company name and official stock exchange ticker may not be consistent over time and may therefore not be reliable identifiers when tracking the firm over a longer period of time. I therefore use the I/B/E/S ticker to track companies over time.

For the firm-specific control variables I use the Compustat database of historical accounting data. I use the same firm-specific control variables as Flammer (2013), which she writes are widely used in the economics and finance literature to construct samples of similar firms. I construct variables for firm size, the amount of cash holdings, the cash flow of the company, and firm leverage. Flammer (2013) also includes price-to-book ratio, but because Compustat does not have sufficient data coverage of the market price of stocks, I do not include this as a control variable. Firm size is estimated using the natural logarithm of the book value of total assets. Cash holdings are estimated using the ratio of the book value of cash and short-term investments to the book value of total assets. Cash flow is estimated using the ratio of book value of sales to the book value of total assets^{*}. Leverage is estimated as the ratio of the book value of the long-term debt to the book value of total assets.

The KLD data recorded with one observation per firm per calendar year, whereas the release of Compustat data is based on when the financial year of the company ends. Companies may have financial years that do not align with the calendar year. If I use Compustats' annual financial year accounting data, I may introduce a layer of noise to the analysis since firms' accounting data will not necessarily be comparable at the same point in time. In an attempt to overcome this, I use Compustat's quarterly data and use data from the last four quarters to construct a calendar year income statement item estimate. For every company, I identify the last observation in the calendar year. For income statement items, I sum the values of the last four quarters to get the year-to-date value based on a calendar year. By using this method, the firm's accounting data can better be compared to each other across time and the KLD and Compustat datasets sample collection times are in closer vicinity to each other. For balance sheet items I use the value of the last quarter in the calendar year

Model

Since the variables from KLD are count data and therefore not necessarily normally distributed,

^{*} Flammer (2013) uses net income before extraordinary items divided by total assets to measure cash flow. However, the data availability of net income before extraordinary items is poor in Compustat, so I use Net Sales instead.

I use the change in the KLD score from t-1 to t+1 instead, where t is the year of the merger. This approach is similar to that of Flammer (2013), who also uses a difference in differences methodology with KLD data. The following model is estimated:

$$\Delta CSR_{i} = \beta_{0m} + \alpha_{t} + \beta_{1} \times TREATED_{i} + \gamma' X_{i,t-1} + \varepsilon_{it}$$

Where ΔCSR_i is the change in the CSR rating for firm i from year t-1 to t+1, where t is the year of the merger.

Formally: $\Delta CSR_i = CSR_{i,t+1} - CSR_{i,t-1}$

 β_{0m} is a the intercept term for every year where a merger occus, thus adding merger fixed effects. α_t is 2-digit SIC industry fixed effects. TREATED is the difference in differences estimator and is the variable of interest. It is a dummy variable equal to one if the observation is in the treatment group and zero if the observation is in the control group. If the TREATED dummy variable is significant, it means that the treatment group has had a different evolution compared to the control group. $X_{i,t-1}$ is a vector of firm control variables, measured in the year prior to the merger. The control variables are firm size, leverage, cash holdings, and cash flow. ε_{it} is the error term.

Identification of treatment sample

To find the brokerage house mergers in the I/B/E/S database, I use the list provided by Irani and Oesch (2013), as they have published the I/B/E/S brokerage house codes of the merging brokerage houses, allowing me to use the same list. Irani and Oesch's (2013) list contains 13

mergers, spanning from the year 1994 to the year 2005.

All stocks that are covered by the acquiring brokerage house 365 days preceding the merger are marked by a dummy variable equal to one if the stock is covered by the acquiring brokerage house. All stocks covered by the acquired brokerage house 365 days preceding the merger are marked by a different dummy variable equal to one if the stock is covered by the brokerage house being acquired. These dummy variables form two lists of stocks that are covered by the respective merging brokerage houses. Those stocks that are found in both of these lists, and are also found to be covered by the surviving brokerage house in the 365 days after the merger are marked as the overlapping stocks and form the treatment group. Following a similar approach as Irani and Oesch's (2013), I use all other stocks in the sample as my control group.

Table 2 lists the results of the process of finding overlapping stocks. The table lists the number of stocks covered by the affected brokerage houses and the number of overlapping stocks for every merger. The corresponding numbers from Irani and Oesch (2013) and Hong and Kacperczyk (2010) are included for comparison. The observant reader will notice that the number of my overlapping stocks sometimes deviates somewhat from Irani and Oesch's numbers. I suspect the reason for this is that Irani and Oesch use a somwhat different methodology in that they focus on the fiscal-years of companies when identifying overlapping observations.

Having identified the overlapping stocks for one merger, I repeat this procedure for all 13 mergers, creating 13 dummy variables, each equal to one if the stock is in the treatment group for the merger in question, and zero if it is not a stock affected by the merger and thus part of the control group.

Table 2: Overlapping stocks

								Stock co	overage
				Staalt aa		Stock c	overage d Oscab	Hong ar	nd mente
	Brokerage house	I/B/E/S		SLOCK CO	verage	IT AITT AIT	iu Oescii	Kacperc	ZYK
#	Acquiring house on first line	identifier	Merger date	#	Overlap	#	Overlap	#	Overlap
1	Paine Webber	189	31 Dec 1994	805	331	816	257	659	234
	Kidder Peabody	150		695		722		545	
2	Morgan Stanley	192	31 May 1997	1068	355	1081	283	739	251
	Dean Witter Reynolds	232		559		553		470	
3	Smith Barney (Travelers)	254	28 Nov 1997	1389	512	1367	422	914	327
	Salomon Brothers	242		937		936		721	
4	EVEREN Capital	829	9 Oct 1998	266	20	249	17	178	17
	Principal Financial Securities	495		102		212		142	
5	DA Davidson & Co	79	17 Feb 1998	101	17	108	13	76	8
	Jensen Securities	932		68		73		53	
6	Dain Rauscher	76	6 April 1998	446	40	459	37	360	26
	Wessels Arnold & Henderson	280		180		201		135	
7	First Union	282	1 Oct 1999	452	42	417	30	274	21
	EVEREN Capital	829		289		277		204	
8	Paine Webber	189	6 Dec 2000	762	29	758	28	516	28
	JC Bradford	34		171		229		182	
9	Credit Suisse First Boston	100	15 Oct 2000	1255	539	1359	452	856	307
	Donaldson Lufkin and Jenrette	86		1066		1021		595	
10	UBS Warburg Dillon Read	85	10 Dec 2000	910	388	936	296	596	213
	Paine Webber	189		758		730		487	
11	JP Morgan	873	31 Dec 2000	732	109	721	88	415	80
	Chase Manhattan	125		617		598		487	
12	Fahnestock	98	18 Sep 2001	154	9	161	7	117	5
	Josephtal Lyon & Ross	933		122		121		91	
13	Janney Montgomery Scott	142	22 Mar 2005	148	10	162	8	116	10
	Parker/Hunter	860		65		64		54	

The dataset is then merged with the KLD data using the official ticker identifier found in the KLD dataset and in the I/B/E/S dataset, retaining only observations with a valid link to KLD. The original I/B/E/S dataset has 91 159 unique firm-year observations based on official ticker and calendar year, in the years between and in 1990 and 2009. Of these, 25 249 are succesfully matched to KLD using the official ticker and year. Four of these observations are found to have duplicate values of official tickers per I/B/E/S ticker and, and are therefore dropped. The final merged dataset has 25 245 unique observations using I/B/E/S ticker and calendar year. See Table 3 for an overview of the observations, grouped by calendar year, before and after merging with KLD.

Having created a KLD dataset with dummy variables indicating the treatment sample of the 13 individual mergers, this dataset is imported 13 times into a new empty dataset to form a pooled dataset, where only the relevant observations for each individual merger is kept. For every merger all observations one year before the merger, and three years after the merger are kept. Mergers occurring in the same year are combined, leaving 7 unique merger-years. By doing this, a firm-year observation may be in the dataset multiple times, but with a different value of years relative to the unique merger it is associated to. I assign every firm-year-observation a unique numeric id based on its I/B/E/S ticker and the combined merger the observation is associated to.

Having created the pooled dataset, keeping only relevant observations for every period, I am left with 42 565 observations in the pooled dataset. I then proceed to match the pooled dataset to Compustat to add control variables, keeping only observations with a match to Compustat. After matching to Compustat, there are 32 325 observations in the dataset, of which 18 820 are unique observations based on I/B/E/S ticker and calendar year. Furthermore, I remove all

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observations for firms that do not have observations in at leat the year before the merger and the year after. I am now left with 19 374 observations in the pooled dataset, of which 13 164 of these are unique observations based on I/B/E/S ticker and calendar year. Of these unique observations in the final dataset, 12 596 observations are in the control group, and 2285 are in the treatment group. See Table 3, Table 4, and Table 5, for an overview of the number of observations after merging the different datasets.

	Observations	Observations
	before matching	after matching to
Year	to KLD	KLD
1991	3,591	608
1992	3,843	610
1993	4,230	615
1994	4,913	610
1995	5,415	614
1996	6,036	616
1997	6,456	618
1998	6,356	624
1999	6,076	631
2000	5,674	632
2001	4,703	1,070
2002	4,483	1,078
2003	4,457	2,760
2004	4,796	2,891
2005	4,963	2,866
2006	5,115	2,829
2007	5,230	2,800
2008	4,822	2,773
Total	91,159	25,245

Table 3: Overview of analyst estimates in I/B/E/S detail history file by year before and after merging with KLD

				after removing
				firms without
		Number of	after merging	observations in
		observations in	with Compustat	complete 3-year
	Year	pooled dataset	data	period
	1993	615	323	305
	1994	610	327	305
	1995	614	343	305
	1996	1,232	712	625
	1997	1,854	1,116	957
	1998	1,872	1,152	1,015
	1999	2,524	1,636	1,367
	2000	3,160	2,195	1,751
	2001	4,280	3,248	1,429
	2002	3,234	2,580	1,106
	2003	5,520	4,328	764
	2004	5,782	4,668	2,294
	2005	2,866	2,324	1,898
	2006	2,829	2,398	1,898
	2007	2,800	2,455	1,714
	2008	2,773	2,520	1,641
-	Total	42,565	32,325	19,374

Table 4: Distribution of observations across years in pooled dataset after merging different datasets.

Table 5: Distribution of observations over years and treatment group in pooled dataset

Year	Control group	Treatment group
1993	213	92
1994	213	92
1995	213	92
1996	360	265
1997	681	276
1998	827	188
1999	944	423
2000	1,328	423
2001	1,168	261
2002	859	247
2003	520	244
2004	2,260	34
2005	1,868	30
2006	1,868	30
2007	1,685	29
2008	1,612	29
Total	16,619	2,755

Year relative to		
mergers	Control group	Treatment group
-1	3,444	558
0	3,444	558
1	3,444	558
2	3,198	545
3	3,089	536
Total	16,619	2,755

Table 6: Distribution of observations over years relative to merger in pooled dataset

Table 7 lists the descriptive statistics of the observations in the remaining pooled dataset. The statistics are split by the treatment and control group. The variables Δ KLD_2, Δ KLD_3, KLD_4, and Δcom str num 2, Δcom str num 3, Δcom str num 4, are the change in values from year t-1 to t+1, t+2, t+3 of the overall KLD score and the community strenghts score. The control variables are only the observations from year t-1, where t is the year of the merger. Comparing the size variable of the treated and control firm, it appears as if the firms in the treamment group have higher values of size and cash flow compared to the control group. This may indicate somewhat of a sample selection bias, as mentioned in the methodology section, caused by selecting a treatment group based on firms with overlapping coverage. The firms which are covered by both brokerage houses are more likely to be larger firms which are easier to market to clients, causing a higher probability of large firms to enter as part of the treatment sample. Comparing the change in overall KLD score and the community strenghts variable, com str num, it appears as if the treatment group has increased by more in both overall KLD score and in the community strenghts score. The control group has a mean value of Δ KLD 2 of 0.237, whereas the treatment group has a value of 0.364. For the community strengths, the control group has a mean value of Δ com str num 2 of 0.011, whereas the treatment group has a mean value of Δcom str num 2 of 0.031.

					25 th		75 th	
Variable	Mean	Min	Max	Ν	percentile	Median	percentile	Std. dev.
Control group								
KLD	1.800	0.000	21.000	16619	0.000	1.000	3.000	2.210
com_str_num	0.262	0.000	5.000	16619	0.000	0.000	0.000	0.618
$\Delta Coverage_2$	0.355	-24.000	32.000	3444	-2.000	0.000	3.000	4.310
ΔKLD_2	0.237	-11.000	9.000	3444	0.000	0.000	1.000	1.050
$\Delta com_str_num_2$	0.011	-2.000	2.000	3444	0.000	0.000	0.000	0.350
ΔKLD_3	0.365	-11.000	10.000	3198	0.000	0.000	1.000	1.250
$\Delta com_str_num_3$	0.012	-2.000	2.000	3198	0.000	0.000	0.000	0.400
ΔKLD_4	0.463	-11.000	10.000	3056	0.000	0.000	1.000	1.430
$\Delta com_str_num_4$	0.004	-2.000	3.000	3056	0.000	0.000	0.000	0.456
Size _{t-1}	7.730	-0.816	14.200	3433	6.530	7.670	8.840	1.760
Cash Flow t-1	0.914	-0.266	11.600	3401	0.397	0.793	1.230	0.763
Cash Holdings t-1	0.136	0.000	0.982	3429	0.020	0.056	0.173	0.181
Leverage t-1	0.185	0.000	1.690	3414	0.035	0.157	0.281	0.178
E-Index t-1	2.430	0.000	6.000	2006	1.000	3.000	3.000	1.330
Treatment group								
KLD	2.820	0.000	13.000	2755	1.000	2.000	4.000	2.360
com_str_num	0.511	0.000	4.000	2755	0.000	0.000	1.000	0.810
$\Delta Coverage_2$	-1.250	-20.000	21.000	558	-4.000	-1.000	2.000	5.110
ΔKLD_2	0.364	-4.000	5.000	558	0.000	0.000	1.000	1.220
$\Delta com_str_num_2$	0.031	-2.000	2.000	558	0.000	0.000	0.000	0.506
ΔKLD_3	0.490	-4.000	9.000	545	0.000	0.000	1.000	1.460
$\Delta com_str_num_3$	0.035	-2.000	2.000	545	0.000	0.000	0.000	0.592
ΔKLD_4	0.679	-5.000	11.000	533	0.000	1.000	2.000	1.680
$\Delta com_str_num_4$	0.039	-2.000	2.000	533	0.000	0.000	0.000	0.652
Size t-1	8.880	3.110	13.500	555	7.960	8.900	9.750	1.470
Cash Flow t-1	0.970	0.066	17.100	551	0.479	0.818	1.140	1.110
Cash Holdings t-1	0.090	0.000	0.903	551	0.015	0.039	0.100	0.130
Leverage t-1	0.192	0.000	0.757	552	0.082	0.175	0.291	0.136
E-Index t-1	2.390	0.000	6.000	96	1.000	3.000	3.000	1.420

Table 7: Descriptive statistics of observations in pooled dataset.

 Δ Coverage, Δ KLD and Δ com_str_num are the changes in values from year t-1 to t+1,2, and 3, respectively, where t is the year of the merger. (The number indicates time span for which the change is calculated.) All control variables are values measured at the year before the merger (t-1). Cash is the ratio of the book value of cash and short-term investments to the book value of total assets. Cash flow is the ratio of book value of sales to the book value of total assets. Leverage is the book value of the long-term debt to the book value of total assets. E-Index is the entrenchment index from Bebchuck et al. (2009).

Confirm validity of experiment

Before estimating the model using the KLD data, I confirm the validity of the experiment by estimating the following difference in differences model, which should indicate whether the firms affected by the mergers experience a reduction in analyst coverage or not, using only observations found in t -1 and t + 1, where t is the year of the merger.

$$\Delta COVERAGE_i = \beta_{0m} + \alpha_i + \beta_1 TREATED_i + \varepsilon_{it}$$

Where $\Delta COVERAGE$ is the change in number of analysts following firm i from year t-1 to t + 1 where t is the year of the merger. β_{0m} is the intercept term for every combined merger. α_i is 2digit SIC industry fixed effects. TREATED is a dummy variable equal to one if the observation is in the treatment group and zero if it is a part of the control group. TREATED is the difference in differences estimator, and indicates whether the treated firms experience a reduction in analyst coverage consequent to the merger. ε_{it} is the error term.

Figure 2 illustrates the evolution of the average coverage in the treatment and control group over the years. There appears to a spike in analyst coverage of the treated firms in 2004, before rapidly declining. Figure 3 shows the same mean of analyst coverage, only now over the years relative to the mergers. There appears to be a decline in coverage for the treated firms in the two years following the mergers, indicating that the mergers may have caused a reduction in analyst coverage for the treated firms.



Figure 2: Mean coverage over calendar years in pooled dataset

Figure 3: Mean coverage over years relative to merger in pooled dataset



The regression results of the regression on analyst coverage are presented in Table 8. The difference in differences estimator TREATED is significant at the 5% level, also when including industry fixed effects. The results indicate that the treated firms do experience a reduction in analyst coverage of approximately 0.7 less than the control group following the mergers. This difference does however not appear to be significant once firm control variables are added to the model. This means that the validity of the experiment must be questioned. Once the control variables are added, the magnitude of the difference in change between the treatment and control group is reduced, indicating that the treatment sample does not experience a reduction in analyst coverage compared to the control group. However, the plots and the simple regressions indicate that the treatment group has a reduction in coverage compared to the control group. I therefore choose to proceed with the experiment, with the caveat that the results may not be valid.

Variable	$\Delta COVERAGE_2$	$\Delta COVERAGE_2$	$\Delta COVERAGE_2$
TREATED	-0.730	-0.656	-0.315
	(2.44)**	(2.17)**	(1.02)
Size	-	-	-0.339
	-	-	(6.70)***
Leverage	-	-	1.666
	-	-	(3.43)***
Cash Holdings	-	-	2.208
	-	-	(4.13)***
Cash Flow	-	-	0.054
	-	-	(0.61)
Constant	-1.045	-1.113	0.302
	(3.90)***	(1.89)*	(0.44)
Merger Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	No	Yes	No
R^2	0.05	0.08	0.10
Ν	4,002	4,002	3,922

Table 8: Regression results of change in analyst coverage

OLS regression with robust standard errors used to obtain estimates. Estimated coefficients of merger and industry fixed effects are not included in this table.

* *p* < 0.1; ** *p* < 0.05; *** *p* < 0.01

Results of natural experiment

Figure 4 and Figure 5 plots the evolution of the average KLD score and the average community strength score score of the pooled sample over calendar years. There appears to be a sharp decrease in the com_str_num and overall KLD score from the year 2002 to 2004. The reason for this might be that KLD increased their coverage considerably in 2003, and thereby introduced many smaller firms with a lower score, thus decreaseing the mean value of the sample in the years after 2003. Ignoring this reduction, there appears to be an upward trend in the overall KLD score, both before 2003 and after 2003. For the community strengths score, there does not appear to be an as convincing trend as in the overall KLD score.

Figure 6 and Figure 7 displays the evolution of the overall KLD score and the community strength score for the treated and control firms over the years relative to the merger. The figures reveal that the firms in the treatment group appear to have a higher overall KLD and community strengths score compared to the control group of firms. The overall KLD score appears to have an upward trend for both the treatment group and control group, but there does not appear to be any noticable difference in the evolution of the treatment group compared to the control group. For the community strengths score, com_str_num, there does appear to be somewhat of a larger increase in the score of the treatment group compared to the control group in the year after the merger, before decreasing somewhat again two years after the merger.

Figure 4: Evolution of average KLD score over calendar years.





Figure 5: Evolution of community strengths score over calendar years in sample.

Figure 6: Evolution of KLD score, split by treatment group and control group, over years relative to merger







The regression results of the natural experiment are presented in Table 9. White's heteroskedasticity-robust standard errors are used in all regressions. The TREATED variable has a positive sign in all the models, indicating that the treated firms have a higher overall KLD and

community strenghts score one year after the merger. However, the TREATED variable is not significant in any of the models.

Variable	ΔKLD_2	ΔKLD_2	$\Delta com_str_num_2$	$\Delta com_str_num_2$
TREATED	0.058	0.040	0.041	0.041
	(0.83)	(0.57)	(1.49)	(1.46)
Size	0.129	0.164	0.014	0.019
	(8.45)***	(9.75)***	(2.67)***	(3.41)***
Leverage	-0.182	-0.265	0.015	0.010
	(2.05)**	(2.66)***	(0.44)	(0.28)
Cash Holdings	0.431	0.248	0.083	0.062
	(4.08)***	(2.10)**	(2.43)**	(1.61)
Cash Flow	0.057	0.050	0.001	0.004
	(2.11)**	(1.30)	(0.08)	(0.30)
Constant	-0.588	-0.561	-0.040	0.046
	(4.12)***	(2.69)***	(0.77)	(0.52)
Year Fixed Effects	Yes	Yes	Yes	Yes
Industry Fixed Effects	No	Yes	No	Yes
R^2	0.04	0.07	0.02	0.03
Ν	3,922	3,922	3,922	3,922

Table 9: Regression results of natural experiment. t-statistics in parenthesis.

OLS regression with robust standard errors used to obtain estimates. Estimated coefficients of merger and industry fixed effects are not included in this table.

* *p* < 0.1; ** *p* < 0.05; *** *p* < 0.01

Following Irani and Oesch (2013), I split the sample into those firms with "high coverage" and "low coverage" in the year prior to the merger. The hypothesis is that firms with fewer analysts following them should experience a more pronounced effect after experiencing a drop of one analyst covering them. If a firm has above median coverage in the year preceding the merger I mark the firm being a high-coverage firm. If the dummy variable is equal to one, the observation is in the high coverage group. Figure 8 and Figure 9 displays the evolution of the overall KLD score and the community strengths score over years relative to the mergers, split by treatment group and also split by whether the observation is in the high coverage group or in the low coverage group.

Figure 8: Evolution of overall KLD score over years relative to merger, split by treatment group and high/low coverage



Figure 9: Evolution of community strenghts score over years relative to merger, split by treatment group and high/low coverage



I repeat the regressions of the natural experiment, only this time splitting the sample into high and low coverage firms. The results of the regressions are presented in Table 10. In these regressions, there appears to be a significant effect on the community strengths score in the low coverage group. In the low-coverage group of firms, the treatment group appears to have a 0.078 higher community strengths score in the year after the merger compared to the control group. This difference in differences estimator is significant at the 5% level.

	ΔKI	.D_2	$\Delta com_str_num_2$		
	Low Coverage	High Coverage	Low Coverage	High Coverage	
TREATED	-0.036	0.023	0.078	0.044	
	(0.36)	(0.18)	(2.09)**	(0.93)	
Size	0.115	0.180	0.007	0.030	
	(4.90)***	(6.32)***	(0.87)	(3.13)***	
Cash holdings	-0.049	-0.410	0.115	-0.087	
	(0.43)	(2.35)**	(2.49)**	(1.47)	
Cash flow	0.260	0.145	0.046	0.062	
	(1.62)	(0.77)	(0.93)	(1.00)	
Leverage	0.092	0.054	0.014	0.005	
	(2.17)**	(1.01)	(1.04)	(0.25)	
Constant	-0.337	-0.595	0.019	0.036	
	(1.24)	(1.86)*	(0.36)	(0.25)	
Year Fixed Effects	Yes	Yes	Yes	Yes	
Industry Fixed Effects	Yes	Yes	Yes	Yes	
R^2	0.07	0.09	0.05	0.05	
Ν	1,953	1,969	1,953	1,969	

Table 10: Results of splitting sample into high and low coverage. t-statistics in parenthesis.

OLS regression with robust standard errors used to obtain estimates. Estimated coefficients of merger and indsiustry fixed effects are not included in this table.

* p < 0.1; ** p < 0.05; *** p < 0.01

Is the effect sustained?

I repeat the experiment using the change in community strengths scores from t-1 to t+2 and t+3 as the dependent variable, where t is the year of the merger. I use only the low-coverage group as the sample for the regression. The results of these regressions are presented in Table 11. Two years after the mergers, the effect is still significant at the 5% level, however, three years after the merger, there does not appear to be any significant difference between the treatment group and control group in the low coverage sample. The explanation for this might be that other governance mechanisms come into play at a later point and thereby substitute for the reduction in analyst coverage. It is also unknonwn whether new analysts start following firms in the treatment sample two or three years after the reduction in analyst coverage.

	$\Delta com_str_num_3$	$\Delta com_str_num_4$
TREATED	0.089	0.012
	(2.09)**	(0.26)
Size	0.009	-0.010
	(0.95)	(0.89)
Cash holdings	0.063	0.065
	(1.37)	(1.23)
Cash flow	0.013	-0.024
	(0.23)	(0.40)
Leverage	0.025	0.000
	(1.53)	(0.02)
Constant	-0.037	0.045
	(0.56)	(0.50)
Year Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
R^2	0.05	0.00
Ν	1,800	1,703

Table 11: Results of regressions of com_str_num in years two and three years after the mergers

OLS regression with robust standard errors used to obtain estimates. Estimated coefficients of merger and industry fixed effects are not included in this table.

* *p*<0.1; ** *p*<0.05; *** *p*<0.01

Does corporate governance play a role?

Since, as mentioned by Bénabou and Tirole (2009), a certain level of managerial entrenchment is expected for managers to spend corporate funds on charitable projects they themselves deem appropriate, it would be interesting to measure whether the level of managerial entrenchment affects the results in any way. Irani and Oesch (2013) present two hypotheses on the relationship between analyst coverage and other corporate governance mechanisms. Their first hypothesis is that analyst monitoring and other internal corporate governance mechanisms such as the board of directors serve much of the same function and thereby act as substitutes to each other. Both analysts and internal governance mechanisms provides incentives for the managers to refrain from rent-seeking behavior. The second hypothesis is that security analysts instead complement the existing internal corporate governance mechanisms, providing relevant private information to the board of directors, so that the internal corporate governance functions are more effective. Irani and Oesch find evidence for the substitution hypothesis; that analyst coverage has a substitution effect to internal corporate governance mechanisms.

If analyst coverage and other internal corporate governance mechanisms act as substitutes, and managerial philantrophy can be viewed as managerial rent-seeking behavior, then one would expect to see a higher level of spending on community charitable programs in the years after the mergers in poorly goverened firms in the treatment sample compared to the other firms in the treatment sample. To test this, I use approximately the same empirical setup as Irani and Oesch (2013), using a dummy variable to mark firms with a high level of managerial entrenchment and introducing an interaction term to the original regression:

$$\Delta CSR_{i} = \beta_{0m} + \alpha_{t} + \beta_{1} \times TREATED_{i} + \beta_{2} \times hiEntr + \beta_{3} \times TREATED \times hiEntr + \gamma' X_{i,t-1} + \varepsilon_{it}$$

To measure managerial entrenchment, I use the Entrenchment index constructed by Bebchuk, Cohen, and Ferrell (2009 p. 1). The index is constructed based on information on "*staggered boards, limits to shareholder bylaw amendments, poison pills, golden parachutes, and supermajority requirements for mergers and charter amendments*". The index provides a score from zero to six on the level of managerial entrenchment in the firm. Using this information, I construct a dummy variable "hiEntr" equal to one if the Entrenchment score of the firm is equal to four or more in the year before the merger. The Entrenchment index has observations spanning from 1990 to 2002, but the coverage is less than that of the KLD sample, so a number of observations is lost. Using the sample of low-coverage firms, there are 1 953 observations with a valid observation of $\Delta \text{com_str_num_3}$ and the "hiEntr" dummy variable. I do however confirm that the difference in difference estimator is significant in the low coverage group of the sample with valid observations of the "hiEntr".

The results of the regression using the "hiEntr" interaction term are presented in Table 12. One year after the merger, the interaction term is greater in magnitude and significant at the 10% level, whereas none of the other dummy variables appear to be significant. This indicates that firms that are poorly goverened spend more on community charitable projects following a reduction in analyst coverage compared to the other firms in the treatment sample, which might indicate that community charitable projects are not value adding for the firm. However, two years after the merger, the interaction term is no longer significant.

	$\Delta com_str_num_2$	$\Delta com_str_num_3$
TREATED	0.067	0.064
	(1.82)*	(1.52)
hiEntr	0.018	0.013
	(0.70)	(0.48)
TREATED x hiEntr	0.199	0.475
	(0.93)	(2.13)**
Size	0.007	0.009
	(0.81)	(0.94)
Cash holdings	0.115	0.061
	(2.49)**	(1.32)
Cash flow	0.050	0.020
	(1.02)	(0.36)
Leverage	0.013	0.025
	(1.01)	(1.55)
Constant	0.012	-0.053
	(0.21)	(0.80)
Year Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
R^2	0.05	0.06
Ν	1,953	1,800

Table 12: Regression results when including E-index interaction term. Low-coverage sample used.

OLS regression with robust standard errors used to obtain estimates. Estimated coefficients of merger and industry fixed effects are not included in this table.

* *p* < 0.1; ** *p* < 0.05; *** *p* < 0.01

Robustness checks

Placebo merger dates

As a robustness check, I estimate the orignal model but this time use merger dates that are hypothetical, creating a set of placebo mergers. To do this, I move the year of the mergers to two years before the actual merger. If the increase in community strenghts score is indeed caused by a reduction in analyst coverage, then I should observe no effect when running a placebo experiment, where the merger dates are not related to an actual merger The results of the

placebo-regressions are presented in Table 13. White's heteroskedasticity-corrected standard

errors are used in all regressions.

Variable	Δ KLD_2	ΔKLD_2	ΔKLD_2	$\Delta com_str_num_2$	$\Delta com_str_num_2$	$\Delta com_str_num_2$
	Overall	Low coverage	High Coverage	Overall	Low coverage	High Coverage
TREATED	-0.033	0.018	-0.092	0.017	0.011	0.026
	(0.46)	(0.14)	(0.84)	(0.55)	(0.19)	(0.51)
Size	0.088	0.108	0.060	0.002	0.006	-0.005
	(4.80)***	(4.49)***	(1.82)*	(0.21)	(0.61)	(0.44)
Leverage	-0.444	-0.379	-0.638	-0.147	-0.109	-0.222
	(3.02)***	(2.17)**	(2.52)**	(2.57)**	(1.51)	(2.37)**
Cash Holdings	0.414	0.230	0.445	0.010	-0.058	0.041
	(2.12)**	(0.90)	(1.52)	(0.12)	(0.57)	(0.34)
Cash Flow	-0.020	-0.008	-0.036	-0.020	-0.013	-0.026
	(0.48)	(0.22)	(0.58)	(1.87)*	(0.76)	(1.83)*
Constant	-0.148	-0.316	0.204	0.152	0.086	0.250
	(0.83)	(1.56)	(0.56)	(2.28)**	(0.96)	(1.79)*
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed	No	No	No	No	No	No
Effects						
R^2	0.02	0.03	0.02	0.02	0.02	0.02
Ν	2,687	1,382	1,305	2,687	1,382	1,305

Table 13: Placebo regressions. Merger dates set to two years before the actual mergers

OLS regression with robust standard errors used to obtain estimates. Estimated coefficients of merger and industry fixed effects are not included in this table.

* *p* < 0.1; ** *p* < 0.05; *** *p* < 0.01

In the placebo experiment, the change in the treatment group is never significantly different from the control group, for neither the overall group nor the high or low coverage group. This is a good confirmation of the validity of the experiment. This indicates that the results observed in the original experiment are not the result of randomness, but that the reduction in analyst coverage may in fact have contributed to the increase in KLD score.

Results by merger

As another robustness check, I run the regressions for every merger to determine if there are any specific mergers that appear to be driving the results. The results are presented in Table 14.

	Coefficient of		Coefficient of		
Combined merger	TREATED,		TREATED,		
number	ΔKLD_2	t-value	$\Delta com_str_num_2$	t-value	
1	0.397	(2.36)**	0.143	(1.86)*	
2	-0.071	(0.54)	0.036	(0.62)	
3	0.202	(0.63)	0.019	(0.12)	
4	-	-	-	-	
5	0.169	(1.33)	0.059	(1.61)	
6	-0.253	(3.00)***	-0.094	(3.00)***	
7	-0.267	(1.38)	-0.002	(0.04)	

Table 14: Individual combined mergers' regressions on $\Delta \text{com_str_num_2}$ and $\Delta \text{KLD_2}$. Industry fixed effects are not included in the estimation of these models.

OLS regression with robust standard errors used to obtain estimates.

* *p*<0.1; ** *p*<0.05; *** *p*<0.01

For the change in community strenghts score, on a merger-by-merger basis, it appears that there is a positive sign on the difference in difference estimator for combined merger one, two, three, and five, but it is only significant, at the 10% level, in the first merger. In the last two mergers, the difference in differences estimator has a negative sign, and it is significant at the 1% level in combined merger number six. Merger number four does not appear to have enough observations to compute a valid difference in difference estimator.

For the change in overall KLD score, on a merger-by-merger basis, the coefficient of the difference in differences estimator has a positive sign in three of the regressions, and a negative sign in three of the regressions. Combined merger number four does not have enough observations to compute a valid estimate for the difference in differences coefficient. Of the valid estimates, two of them are significant. In the first merger, the difference in differences estimator has a value of 0.397, significant at the 5% level. In combined merger number six, the difference in difference in difference in difference in difference in the term of the term of the term of the difference in the difference in the difference in the term of term of the term of term of the term of the term of term of term of term of term of the term of the term of te

Prais-Winsten model

I estimate a baseline model of comparing changes in KLD rating directly to the level of analyst coverage in the year before the KLD score is given, ignoring the issue of endogeneity. Using only unique firm-year observations in the sample, I estimate the following regression, with a Prais-Winsten transformation as described by Gujarati and Porter (2009, p.325):

$$CSR_{it} - \rho CSR_{i,t-1} = \beta_{0t}(1-\rho) + \alpha_i + \beta_1 (COVERAGE_{i,t-1} - \rho COVERAGE_{i,t-2}) + \gamma' X_{it} + \nu_{it}$$

COVERAGE is the number of analysts following firm i in calendar year t. β_{0t} is the intercept term for every calendar year. α_i is 2-digit SIC industry fixed effects. X_{it} is a vector of the change in firm control variables from year t-1 to t, adjusted for ρ . I use a Prais-Winsten transformation in an attempt to correct for serial correlation and also in an attempt to overcome the limitation of the non-normally distributed dependent variable. ρ is a value between zero and one and is estimated using the single lag of the OLS residuals. v_{it} is the error term. The results of the regression are presented in Table 15. White's heteroskedasticity-robust standard errors are used in all regressions.

Variable	ΔKLD	Δcom_str_num
$\Delta COVERAGE_{t-1}$	0.010	0.003
	(3.21)***	(2.57)**
ΔSize	0.472	0.110
	(16.48)***	(11.58)***
ΔLeverage	-0.011	-0.034
	(0.15)	(1.66)*
∆Cash Holdings	0.117	0.096
	(1.09)	(3.12)***
∆Cash Flow	0.232	0.067
	(5.29)***	(5.20)***
Constant	-1.622	-0.171
	(3.71)***	(1.11)
Year Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
R^2	0.06	0.09
Ν	10,283	10,283
ρ	0.959	0.870
Durbin-Watson (orignial)	0.147	0.201
Durbin-Watson (transformed)	1.623	1.572

Table 15: Results of baseline OLS Prais-Winsten model.

Prais-Winsten regression procedure in Stata used to obtain estimates, with robust standard errors. Estimated coefficients of merger and industry fixed effects are not included in this table.

* *p* < 0.1; ** *p* < 0.05; *** *p* < 0.01

The regression results of this baseline model indicate that the change in overall KLD score and community strengths score appears to be somewhat affected by the change in analyst coverage in the preceding period. The magnitude of the effect appears to be quite low, but highly significant, at the 1% level in both the model with the overall KLD score, and also the community strenghts score. The estimator has a positive sign, the opposite of what the hypotheses in this thesis proposes. As mentioned in the methodology section, it is unknown whether analyst coverage has an endogenous relationship to firm financial performance, or firm size, so the results of this model is

as a confirmation of the appropriateness of using a difference in differences methodology instead of a regular OLS model.

Conclusion

This thesis used a natural experiment to test whether an exogenous decrease in the level of managerial monitoring affects the CSR rating of companies. If the CSR rating of companies increases after a reduction in monitoring, and if the CSR rating can be used as a proxy for the level of CSR expenditures of companies, it could indicate that CSR activities are, overall, not value-adding activities, since a higher level of monitoring should discipline managers to withhold from spending resources on non value-adding activities. This is consistent with the overinvestment hypothesis on CSR.

The results of the natural experiments, if valid, do appear to add support for hypothesis 1 in this this thesis. Using a difference in differences methodology, the treatment group of firms, with a low initial level of analyst coverage is found to have a significantly higher "community strength" KLD score in the year following the reduction in analyst coverage, compared to the control group. However, the regression comparing the level of analyst coverage before and after the merger is not robust to adding firm control variables – questioning the validity of the experiment.

When introducing the level of managerial entrenchment as an interaction dummy term, the effect is only significant in the group of firms with initial low coverage, and intitial high level of managerial entrenchment. This provides indications that firms with higher levels of managerial entrenchment are more inclined to become involved in community charity projects following a

reduction in analyst coverage compared to both the other firms in the treatment group, as well as the control group.

Limitations and areas for further research

As mentioned in the methodology section, there are a number of sources of error that could impact the validity and results of the natural experiment in this thesis. Firstly, the selection criteria used in the construction of the treatment sample might cause the treatment and control groups to have different characteristics, thus potentially violating the "paralell trend" assumption of the difference in differences methodology. The descriptive statistics in this thesis indiates that the two groups are markedly different in both size and the level of cash holdings. Any subsequent research using the same methodology should attempt to construct two otherwise identical samples of firms.

Secondly, the coarseness of the KLD data may make it difficult to measure the CSR actitities accurately enough to capture the possible effect of a reduction in analyst coverage. As mentioned in the methodology section, KLD uses a threshold before a company is marked as a "high donations" company, which could mean that changes in observations that are not close to this threshold will not be recorded by KLD. Subsequent research could attempt to use Asset4's Environmental, Social, and Governance data, which provide more fine-grained data on the CSR activities of a large sample of companies, such as the dollar amount of donations. However, the majority of Asset4's ESG coverage is in the years after 2005. Brokerage house mergers and the brokerage house's I/B/E/S codes must therefore be identified for years 2005 and onwards in order to conduct a natural experiment using this data.

Thirdly, as noted by Kelly and Ljungqvist (2012), the methodolgogy used by Hong and Kacperczyk (2010) and Irani and Oesch (2013) (and thereby also this thesis) to identify stocks that lose coverage as a result of a brokerage house merger, also captures some possible endogeneous changes in coverage. Specifically two instances; those instances where a stock is decided to be dropped in the course of the preceding year leading up to the merger date. This decision to drop coverage could potentially be endogenous to the firm being covered, and should therefore not be treated as a qualifier for being in the treatment sample. Kelly and Ljungqvist (2012) writes that they find many instances of coverage of stocks in Hong and Kacperczyk's (2010) sample that were in fact terminated months before the merger, and should therefore not have been included in the treatment group. Secondly, the methodology used to find overlapping stocks does not capture those instances where the stock loses coverage as a result of the coverage, but where coverage is reinstated in the subsequent months after the merger.

Finally, subsequent research could also look further into whether the effect is different in different sets of firms with different characterics, e.g. Flammer (2013) found that the effect of increased competition on the CSR rating was more pronounced in firms that had a lower financing constraints and thus had more available cash to invest. It is reasonable to assume that similar mechanisms could be observed with regards to CSR expenditures.

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Appendix I: Stata script

//Script created as part of Master's Thesis at Copenhagen Business School 2013, by Abraham Paaske

//Start with the I/B/E/S Detail History file //Generate Stata formatted dates tostring actdats, gen(actdatsstr) format(%20.0f)
replace actdats = date(actdatsstr,"YMD") format actdats %d drop actdatsstr tostring fpedats, gen(fpedatsstr) format(%20.0f) replace fpedats = date(fpedatsstr,"YMD")
format fpedats %d drop fpedatsstr tostring anndats, gen(anndatsstr) format(%20.0f)
replace anndats = date(anndatsstr,"YMD") format anndats %d drop anndatsstr //Sort sort ticker actdats acttims //Drop observations with analyst code equal to zero, as this is most likely a placeholder code drop if analys==0 $% \left(\frac{1}{2}\right) =0$ //Drop duplicates. There resulting dataset now has one estimate per analyst per stock per day duplicates drop ticker estimator analys actdats, force //Drop unused variables drop fpi measure value usfirm fpedats acttims anndats //Generate variable for year based on the date of the estimate gen Year=vear(actdats) //Generate numeric id variable based on the I/B/E/S TICKER sort ticker egen numid = group(ticker) order numid //Find analyst coverage of stock by year by Year numid analys, sort: egen noccur = count(analys)
by Year numid analys: gen byte first = 1 if noccur[_n+1] != noccur by Year numid: egen COVERAGE = count(first) drop noccur first label variable COVERAGE "Number of analysts following the stock in the current calendar year." rename actdats actdate save "ibeswip_2.dta", replace
use "ibeswip_2.dta", clear //Set dates of mergers
global date1 = date("31/12/1994","DMY")
global date2 = date("31/05/1997","DMY")
global date3 = date("28/11/1997","DMY")
global date4 = date("09/10/1998","DMY")
global date5 = date("06/04/1998","DMY")
global date6 = date("06/04/1998","DMY")
global date7 = date("06/12/2000","DMY")
global date8 = date("10/12/2000","DMY")
global date10 = date("10/12/2000","DMY")
global date11 = date("18/09/2001","DMY")
global date13 = date("22/03/2005","DMY") //Set dates of mergers //Robustness check: Move years of merger to two years before actual merger //- Remove comment indicators from this section to set placebo merger dates to two years before the mergers /* /*
global date1 = date("31/12/1992","DMY")
global date2 = date("31/05/1995","DMY")
global date3 = date("28/11/1995","DMY")
global date4 = date("09/10/1996","DMY")
global date5 = date("17/02/1996","DMY")

```
global date6 = date("06/04/1996","DMY")
global date7 = date("01/10/1997","DMY")
global date8 = date("06/12/1998","DMY")
global date9 = date("15/10/1998","DMY")
global date10 = date("10/12/1998","DMY")
global date11 = date("31/12/1998","DMY")
global date12 = date("18/09/1999","DMY")
global date13 = date("22/03/2003","DMY")
*/
*/
//Generate matrix with IBES identifiers of brokerage houses and merger date
matrix input M =
(189,150,$date1,.,..\192,232,$date2,.,..\254,242,$date3,.,..\829,495,$date4,.,..\79,932,$date5,.,..\76,280,$d
ate6,.,.\282,829,$date7,....\189,34,$date8,....\100,86,$date9,....\85,189,$date10,....\873,125,$date11,....
\98,933,$date12,....\142,860,$date13,....)
matrix colnames M = h1 h2 mDate h1Cov h2Cov overlap
mat list M
//Set date range and remove unneccessary observations to preserve memory.
keep if Year > 1990 & Year < 2009
sort estimator analys actdate
forvalues i=1/13{
            //Set date variables to be used based on information from the previously defined matrix
            local mDate = M[`i',3]
            //Generate local variables with the brokerage house IBES codes
            local h1 = M[`i',1]
local h2 = M[`i',2]
            //Mark stocks covered by house 1 in pre_event period
            gen preCov_h1_m`i' = 0
replace preCov_h1_m`i' = 1 if (actdate <= `mDate' & actdate >= `mDate' - 365) & estimator == `h1'
levelsof numid if preCov_h1_m`i' == 1, local(levels)
foreach l of local levels{
                        replace preCov_h1_m`i' = 1 if numid == `l'
            }
            //Mark stocks covered by house 2 in pre_event period
            gen preCov_h2_m`i' = 0
replace preCov_h2_m`i' = 1 if (actdate <= `mDate' & actdate >= `mDate' - 365) & estimator == `h2'
levelsof numid if preCov_h2_m`i' == 1, local(levels)
foreach l of local levels{
                        replace preCov_h2_m`i' = 1 if numid == `l'
            }
            //Mark the overlapping stocks
            gen TREATED_m`i' = 0
             levelsof numid if preCov_h1_m`i' == 1 & preCov_h2_m`i' ==1 , local(levels)
            foreach l of local levels{
                        replace TREATED_m`i' = 1 if numid == `l'
            ł
            //Are all the treated stocks covered by the remaining brokerage house in the year after the merger?
            //If not, remove the observation from the treatment group.
            levelsof numid if TREATED_m`i'== 1 & (actdate >= `mDate' & actdate <= `mDate' + 365), local(levels)
replace TREATED_m`i' = 0</pre>
            foreach l of local levels{
                        replace TREATED_m`i' = 1 if numid==`l'
            }
            //Add info to the previously defined matrix
            distinct numid if preCov_h1_m`i'==1
matrix M[`i',4] = r(ndistinct)
            distinct numid if preCov h2 m`i'==1
            matrix M[`i',5] = r(ndistinct)
            distinct numid if TREATED_m`i' ==1
            matrix M[`i',6] = r(ndistinct)
}
mat list M
forvalues i=1/13{
drop preCov_h1_m`i' preCov_h2_m`i'
save "ibeswip_3.dta", replace
use "ibeswip_3.dta", clear
```

sort numid Year oftic

```
//Remove duplicates based on official ticker to match with KLD
//The resulting dataset now has one observation per official stock ticker per year
//Observations with blank values of offical ticker is removed prior to removing duplicates
sort ticker actdate estimator analys oftic cusip cname
drop analys estimator analys actdate
drop if oftic == ""
duplicates drop oftic Year, force
//Change variable names from uppercase to lowercase
rename oftic OFTIC
rename cusip CUSIP
rename cname CNAME
rename ticker TICKER
//Count how many observations there are before merging
unique OFTIC Year
table Year
//Merge with KLD dataset
merge 1:1 OFTIC Year using "KLD_nodupoftic.dta", keep(match) nogenerate
sort numid Year OFTIC
duplicates drop numid Year, force
//Count how many observations there are after merging
unique numid Year
table Year
//Generate variables to be used in merged dataset
gen ev_num =
gen rel_year = .
gen PRE = 0
gen MID = 0
gen POST = 0
gen TREATED = 0
//Save first instance of data
save "wip_4.dta", replace
use "wip_4.dta", clear
//Clear memory and start building merged dataset
clear
//Start building merged dataset
forvalues i=1/13{
            //Insert baseline dataset
            append using "wip_4.dta", generate(event`i')
            //Set merger number based on import number
replace ev_num = `i' if event`i'==1
            drop event`i'
            //Get year of merger
local mDate = M[`i',3]
local mYear = year(`mDate')
           //Populate rel_year and dummy variables
replace rel_year = -1 if Year == `mYear'-1 & ev_num==`i'
replace rel_year = 0 if Year == `mYear' & ev_num==`i'
replace rel_year = 1 if Year == `mYear'+1 & ev_num==`i'
replace rel_year = 2 if Year == `mYear'+2 & ev_num==`i'
replace rel_year = 3 if Year == `mYear'+3 & ev_num==`i'
            replace PRE = 1 if Year==`mYear'-1 & ev_num==`i
replace MID = 1 if Year==`mYear' & ev_num==`i'
            replace POST = 1 if Year==`mYear'+1 & ev_num==`i'
            //Remove observations that are not in the 3-year span of the merger in question
            drop if rel_year == . & ev_num==`i'
}
//Collapse ev_num if merger happened in same year
gen ev_num_c = .
replace ev_num_c = 1 if ev_num==1
replace ev_num_c = 2 if ev_num==2 | ev_num==3
replace ev_num_c = 3 if ev_num==4 | ev_num==5 | ev_num==6
replace ev_num_c = 4 if ev_num==7
```

replace ev_num_c = 5 if ev_num==8 | ev_num==9 | ev_num==10 | ev_num==11
replace ev_num_c = 6 if ev_num==12
replace ev_num_c = 7 if ev_num==13 //Mergers that happened in the same year are combined replace TREATED = 1 if TREATED_m1==1 & ev_num_c==1 forvalues i = 2/3{ replace TREATED = 1 if TREATED_m`i'==1 & ev_num_c==2 forvalues $i = 4/6{$ replace TREATED = 1 if TREATED_m`i'==1 & ev_num_c==3 } replace TREATED = 1 if TREATED_m7==1 & ev_num_c==7 forvalues i = 8/11{ replace TREATED = 1 if TREATED_m`i'==1 & ev_num_c==5 } replace TREATED = 1 if TREATED_m12==1 & ev_num_c==6
replace TREATED = 1 if TREATED_m13==1 & ev_num_c==7 //Create unique numid/event sort ev_num_c numid Year
egen numid_u = group(numid ev_num_c) sort numid_u Year //Group by collapsed mergers
duplicates drop numid_u rel_year, force //Import controls from COMPUSTAT //Check how many observations I have before the import unique numid_u Year table Year merge m:1 OFTIC Year using "COMPUSTAT_qtr7.dta", keep(match) nogenerate //Check how many observations I have after the import unique numid Year table Year //Find median and 25th percentile coverage by ev_num_c Year, sort: egen qrtCov = pctile(COVERAGE), p(25) by ev_num_c Year, sort: egen medianCov = pctile(COVERAGE), p(50) //Generate "high coverage" dummy variables gen hiCov = 0 replace hiCov = 1 if COVERAGE > qrtCov gen hiCov50 = 0 replace hiCov50 = 1 if COVERAGE > medianCov //Set hiCov for all post-years based on year t-1
levelsof numid_u if hiCov==1 & rel_year==-1, local(levels) replace hiCov=0 foreach l of local levels{ replace hiCov=1 if numid_u==`l' levelsof numid_u if hiCov50==1 & rel_year==-1, local(levels) replace hiCov50=0 foreach l of local levels{ replace hiCov50=1 if numid_u==`l' } //Add Entrenchment Index to dataset and create dummy variables based on values i year t-1
merge m:1 CUSIP Year using "E-Index.dta", keep(match master) nogenerate gen hiEntr = 0 replace hiEntr = 1 if EIndex >3 replace hiEntr = . if EIndex == levelsof numid_u if hiEntr==1 & rel_year==-1, local(levels) replace hiEntr=0 foreach l of local levels{ replace hiEntr=1 if numid_u==`l' } //Generate overall KLD score egen KLD = rowtotal(div_str_num emp_str_num env_str_num hum_str_num pro_str_num com_str_num) //Define variables to calculate lags and average values for global vars = "KLD com_str_num size leverage cashhold cashflow COVERAGE" //Create lags foreach i of global vars{ forvalues x=1/4{

```
by numid_u (rel_year), sort: gen `i'_lag`x' = `i'[_n-`x']
         }
}
//Create average values
foreach i of global vars{
         egen `i'_3avg = rowmean(`i' `i'_lag1 `i'_lag2)
egen `i'_2avg = rowmean(`i' `i'_lag1)
}
//Add average values to varlist
global vars = "KLD com_str_num size leverage cashhold cashflow COVERAGE KLD_2avg com_str_num_2avg size_2avg
leverage_2avg cashhold_2avg cashflow_2avg COVERAGE_2avg KLD_3avg com_str_num_3avg size_3avg leverage_3avg
cashhold_3avg cashflow_3avg COVERAGE_3avg"
//Remove all firm-observations without an observation in year t-1
gen drop = 1
levelsof numid_u if rel_year==-1, local(levels)
foreach l of local levels{
         replace drop = 0 if numid_u == `l'
drop if drop == 1
drop drop
//Create change-in values of all relevant variables
foreach i of global vars{
    gen `i'_lavg = `i'
          forvalues x=0/3{
                   local n = x'+1
                   by numid_u (rel_year), sort: gen `i'_change`n' = `i' - `i'[_n-`n'] if rel_year==`x'
         }
}
//Remove all firm-observations without an observation in at least the year before the merger and the year after
aen drop = 1
levelsof numid_u if KLD_change2 != ., local(levels)
foreach l of local levels{
          replace drop = 0 if numid_u == `l'
}
drop if drop == 1
drop drop
//Check how many observations I have after removing firms without coverage before and after the the mergers
unique numid Year
table Year
//See how they are distributed over treatment/control group table Year TREATED
table rel_year TREATED
unique numid Year if TREATED==0
unique numid Year if TREATED==1
//Descriptive statistics
tabstat KLD com_str_num COVERAGE_change2 KLD_change2 com_str_num_change2 KLD_change3 com_str_num_change3
KLD_change4 com_str_num_change4, statistics( mean min max count p25 median p75 sd ) by(TREATED) nototal
columns(statistics) longstub format(%9.3g)
tabstat size cashflow cashfold leverage EIndex if rel_year==-1, statistics( mean min max count p25 median p75 sd )
by(TREATED) nototal columns(statistics) longstub format(%9.3g)
//Create graphs
global vars = "com_str_num KLD COVERAGE"
by numid Year, sort: egen noccur = count(numid)
by numid Year: gen byte first = 1 if noccur[_n+1] != noccur
drop noccur
graph drop _all
foreach i of global vars{
         //Create comparison graph with high/low coverage
mean `i' if TREATED==0 & hiCov50==0, over(rel_year)
         mat b = e(b)
         mat list b
         svmat b
         mean `i' if TREATED==1 & hiCov50==0, over(rel_year)
         mat d = e(b)
mat list d
         symat d
         mean `i' if TREATED==0 & hiCov50==1, over(rel_year)
         mat e = e(b)
         mat list e
         svmat e
```

mean `i' if TREATED==1 & hiCov50==1, over(rel_year) mat f = e(b)mat list f symat f mean rel_year, over(rel_year)
mat c = e(b)' mat list c svmat c //Create comparison graph
mean `i' if TREATED==0, over(rel_year) mat b = e(b)mat list b svmat b mean `i' if TREATED==1, over(rel_year) mat d = e(b)
mat list d symat d mean rel_year, over(rel_year)
mat c = e(b)' mat list c svmat c twoway (connected b1 c1) (connected d1 c1) ,xlabel(-1(1)3) ytitle(Mean) xtitle(Year relative to merger) title(Mean of `i') legend(order(1 "Control" 2 "Treatment group")) scheme(s1mono) name(comparison_graph_`i') drop b1 c1 d1 //Create year-over-year graph without splitting by treatment
mean `i' if first==1, over(Year) mat b = e(b)mat list b svmat b mean Year if first==1, over(Year)
mat c = e(b)'
mat list c svmat c twoway (connected b1 c1) ,xlabel(1993(1)2008) ytitle(Mean) xtitle(Year) title(Mean of `i') legend(order(1
"Control" 2 "Treatment group")) scheme(s1mono) name(yoy_`i') drop b1 c1 drop first //Confirm validity of experiment reg COVERAGE_change2 i.ev_num_c TREATED, robust outreg using "../Output/coverage_noind.doc", replace starlevel(10 5 1) reg COVERAGE_change2 i.ev_num_c i.sic2 TREATED, robust outreg using ".,/Output/coverage withind.doc". replace starlevel(10 5 1) reg COVERAGE_change2 i.ev_num_c i.sic2 TREATED size_lag2 leverage_lag2 cashhold_lag2 cashflow_lag2, robust outreg using "../Output/coverage_withind_withcontrols.doc", replace starlevel(10 5 1) //Difference in differences regressions //Without industry fixed effects
reg KLD_change2 i.eu_num_c TREATED size_lag2 leverage_lag2 cashhold_lag2 cashflow_lag2, robust
outreg using "../Output/KLD_change_noindustry.doc", replace starlevel(10 5 1)
reg com_str_num_change2 i.ev_num_c TREATED size_lag2 leverage_lag2 cashhold_lag2 cashflow_lag2, robust outreg using "../Output/com_change_noindustry.doc", replace starlevel(10 5 1) //With industry fixed effects //with industry fixed energy inter energy interest //Split into high- and low coverage //High coverage: //Without industry fixed effects reg KLD_change2 i.Year TREATED size_lag2 leverage_lag2 cashhold_lag2 cashflow_lag2 if hiCov50 == 1, robust

outreg using "../Output/KLD_change_hiCov_noindustry.doc", replace starlevel(10 5 1)
reg com_str_num_change2 i.Year TREATED size_lag2 leverage_lag2 cashhold_lag2 cashflow_lag2 if hiCov50 == 1, robust outreg using "../Output/com_change_hiCov_noindustry.doc", replace starlevel(10 5 1) //With industry fixed effects
reg KLD_change2 i.sic2 i.Year TREATED size_lag2 leverage_lag2 cashhold_lag2 cashflow_lag2 if hiCov50 == 1, robust
outreg using "../Output/KLD_change_hiCov_withindustry.doc", replace starlevel(10 5 1)
reg com_str_num_change2 i.sic2 i.Year TREATED size_lag2 leverage_lag2 cashhold_lag2 cashflow_lag2 if hiCov50 == 1, robust outreg using "../Output/com_change_hiCov_withindustry.doc", replace starlevel(10 5 1) //Low coverage: //Low Coverage. //Without industry fixed effects reg KLD_change2 i.Year TREATED size_lag2 leverage_lag2 cashhold_lag2 cashflow_lag2 if hiCov50 == 0, robust outreg using "../Output/KLD_change_lowCov_noindustry.doc", replace starlevel(10 5 1) reg com_str_num_change2 i.Year TREATED size_lag2 leverage_lag2 cashhold_lag2 cashflow_lag2 if hiCov50 == 0, robust outreg using "../Output/com_change_lowCov_noindustry.doc", replace starlevel(10 5 1) //With industry fixed effects reg KLD_change2 i.sic2 i.Year TREATED size_lag2 leverage_lag2 cashhold_lag2 cashflow_lag2 if hiCov50 == 0, robust outreg using "../Output/KLD_change_lowCov_withindustry.doc", replace starlevel(10 5 1) reg com_str_num_change2 i.sic2 i.Year TREATED size_lag2 leverage_lag2 cashhold_lag2 cashflow_lag2 if hiCov50 == 0, robust outreg using "../Output/com_change_lowCov_withindustry.doc", replace starlevel(10 5 1) //Merger-by-merger regressions forvalues i = 1/7{ //Without industry fixed effects quietly reg KLD_change2 TREATED size_lag2 leverage_lag2 cashhold_lag2 cashflow_lag2 if ev_num_c==`i', robust outreg using "../Output/KLD_change_noindustry_m`i'.doc", replace starlevel(10 5 1)
quietly reg com_str_num_change2 TREATED size_lag2 leverage_lag2 cashhold_lag2 cashflow_lag2 if ev_num_c==`i', robust outreg using "../Output/com_change_noindustry_m`i'.doc", replace starlevel(10 5 1) } //Is the effect sustained? reg com_str_num_change3 i.Year i.sic2 TREATED size_lag3 leverage_lag3 cashhold_lag3 cashflow_lag3 if hiCov50 == 0, robust outreg using "../Output/com_change_lowCov_withindustry_lag3.doc", replace starlevel(10 5 1) reg com_str_num_change4 i.Year TREATED size_lag4 leverage_lag4 cashhold_lag4 cashflow_lag4 if hiCov50 == 0, robust outreg using "../Output/com_change_lowCov_withindustry_lag4.doc", replace starlevel(10 5 1) //Does corporate governance matter? reg com_str_num_change2 i.sic2 i.Year TREATED##hiEntr size_lag2 leverage_lag2 cashhold_lag2 cashflow_lag2 if hiCov50 == 0, robust outreg using "../Output/com_change_lowCov_withindustry_withEntr_lag2.doc", replace starlevel(10 5 1) reg com_str_num_change3 i.sic2 i.Year TREATED##hiEntr size_lag3 leverage_lag3 cashhold_lag3 cashflow_lag3 if hiCov50 == 0, robust outreg using "../Output/com_change_lowCov_withindustry_withEntr_lag3.doc", replace starlevel(10 5 1) //Regression diagnostics //Are the residuals normally distributed?
reg com_str_num_change2 i.sic2 i.Year TREATED size_lag2 leverage_lag2 cashhold_lag2 cashflow_lag2 if hiCov50 == 0, robust predict res, r predict yhat qnorm res, scheme(s1mono) name(qnorm) hist res, scheme(s1mono) normal name(res_hist) twoway (scatter res yhat), scheme(s1mono) name(res_scatter) //Test residuals for normality sktest res //Is the mean different from zero? ttest res == 0 //Variance inflation factor vif //Baseline regression - Prais-Winsten transformation duplicates drop numid Year, force xtset numid Year prais KLD i.Year i.sic2 COVERAGE_lag1 size leverage cashhold cashflow, robust outreg using "../Output/KLD_base.doc", replace starlevel(10 5 1) prais com_str_num i.Year i.sic2 COVERAGE_lag1 size leverage cashhold cashflow, robust outreg using "../Output/com_str_base.doc", replace starlevel(10 5 1)

Appendix II - Test of normality of residuals

I obtain the residuals from estimating the following model using OLS with robust standard errors, on the low coverage sample of firms:

$$\Delta CSR_i = \beta_{0m} + \alpha_t + \beta_1 \times TREATED_i + \gamma' X_{i,t-1} + \varepsilon_{it}$$

I plot the residuals against the inverse normal distribution. I also produce a histogram of the residuals, with an overlay of the normal distribution. See Figure 10 and Figure 11.



Figure 10: Residuals vs. inverse normal distribution





Skewness and kurtosis test for normality:

Table 16: Test of normality of residuals

				Joint		
Variable	Obs	Pr(Skewness)	Pr(Kurtosis)	adj chi2(2)	Prob>chi2	
res	3.90E+	03 0.000	0.000	o.	0.0000	

As can be read from the table, the null hypothesis – that the residuals are normally distributed must be rejected.

t-test of residual mean different from zero:

Variable	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf. Interval]			
res	3922	0.0025583	0.0059571	0.3730703	-0.0091211	0.0142376		
mean = mea	an(res)					t = 0.4294		
Ho: mean =	= 0					of freedom = 3921		
Ha: mean <	C 0		Ha: mean != 0			Ha: mean > 0		
Pr(T < t) =	0.6662		Pr(T > t) = 0.6676			$\Pr(T > t) = 0.3338$		

Table 17: t-test of hypothesis that mean of residuals is equal to zero

As can be read from the table, one cannot reject the null hypothesis that the mean of the residuals is zero.