The Real Effects of Financial Shocks: Evidence from Exogenous Changes in Analyst Coverage

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Abstract

We study the causal effects of analyst coverage on corporate investment and financing policies. We hypothesize that a decrease in analyst coverage increases information asymmetry and thus increases the cost of capital; as a result, firms decrease investment and financing. We use broker closures and broker mergers to identify changes in analyst coverage that are exogenous to corporate policies. Using a difference-in-differences approach, we find that firms that lose an analyst decrease investment and financing by 2.4% and 2.6% of total assets, respectively. These results are significantly stronger for firms that are smaller, have less analyst coverage, have a bigger increase in information asymmetry, and are more financially constrained.

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1. Introduction

It is well known that the global financial crisis of the late 2000s caused the worse economic contraction since the 1930s. As a result, there has been a sudden surge of interest in the real effects of financial shocks (e.g., see Sufi (2009), Duchin, Ozbas, and Sensoy (2010), and Edmans, Goldstein, and Jiang (2011)). In this paper, we study the effects on corporate policies of a specific financial shock: the loss of analyst coverage. We examine the effect of analyst coverage on corporate policies using two natural experiments: broker closures like Kelly and Ljungqvist (2011), and broker mergers like Hong and Kacperczyk (2010). Both broker closures and broker mergers cause analysts to be terminated and analyst coverage to decrease for the firms covered by these analysts.

We hypothesize that a decrease in analyst coverage increases information asymmetry and thus increases the cost of capital.[1] As a result of the increase in the cost of capital, the profitability of projects decreases, so the optimal amount of investment decreases. Similarly, since the cost of external financing increases both in absolute terms and relative to the cost of internal financing, the optimal amount of external financing decreases as well. In summary, a decrease in analyst coverage causes a decrease in investment and financing.

The first part of our hypothesis rests on the foundation laid by Kelly and Ljungqvist (2011). They provide empirical evidence that exogenous decreases in analyst coverage (from broker closures) cause an increase in information asymmetry (see also Brennan and Subrahmanyam (1995) and Ellul and Panayides (2009)) as well as the cost of capital.[2] In this

[1] For theoretical evidence that more information asymmetry increases the cost of capital, see, e.g., Stiglitz and Weiss (1981), Myers and Majluf (1984), Diamond (1985), Merton (1987), Lucas and McDonald (1990), Botosan (1997), and Easley and O'Hara (2004). We are agnostic about whether the relationship between information asymmetry and the cost of capital is driven by idiosyncratic risk or systematic risk.

[2] Using several proxies for information asymmetry based on measures of liquidity and the market reaction to earnings announcements, they show that decreases in analyst coverage cause an increase information asymmetry and

paper, we provide empirical evidence for the second part of our hypothesis: a decrease in analyst coverage – through the increase in information asymmetry and thus the cost of capital – causes a decrease in investment and financing.

In our empirical tests, we use 52 broker closures and broker mergers between 1994 and 2008 that cause 1,961 firms to lose an analyst. We use broker closures and broker mergers because the resulting decrease in analyst coverage is exogenous to corporate policies. We compare the changes in corporate policies of treatment firms to those of control firms matched on industry, size, book-to-market, momentum, and analyst coverage. In doing so, we minimize the possibility that cross-sectional or time-series effects affect our results. We show that before the decrease in analyst coverage our treatment firms are similar to our control firms not just in terms of our matching characteristics but also in terms of corporate policies and analysts' expectations. This is what we expect if broker disappearances are exogenous to changes in corporate policies. Moreover, consistent with Kelly and Ljungqvist (2011), we find that around the decrease in analyst coverage stock prices decrease significantly more for our treatment firms than our control firms (1.01% during the two-month event window).

Proceeding to our main analysis, we find that our treatment firms respond to the loss of an analyst by decreasing total investment and total financing (the year after compared to the year before) by 2.35% and 2.62% of total assets, respectively, compared to our control firms. Capital expenditures decrease by 0.71%, research and development expenditures by 0.64%, and acquisitions expenditures by 0.94%. Similarly, firms that lose an analyst decrease equity issuance and net total debt issuance by 1.12% and 1.55%, respectively. These results are of

a decrease in stock prices, and that bigger increases in information asymmetry are associated with bigger decreases in stock prices.

economically plausible magnitudes: the loss of an analyst causes an average decrease in total investment and total financing of $41.5 million and $48.4 million, respectively.

Moreover, the decrease in analyst coverage causes firms to switch to financing that is less sensitive to information asymmetry: we find that firms decrease their use of equity and (higher risk) long-term debt, they do not change their use of (lower risk) short-term debt, and they increase their use of cash. We also provide evidence to support the parallel trends assumption underlying our difference-in-differences approach: the corporate policies of our treatment firms and control firms only diverge from each other when analyst coverage decreases for our treatment firms.

We also find that the real effects of the loss of an analyst are significantly bigger when the loss of an analyst is more costly: for smaller firms, for firms with less analyst coverage, and for firms with a bigger increase in information asymmetry resulting from the loss of an analyst. For example, total investment and total financing decrease by 4.9% and 9.9%, respectively, for firms in the smallest quintile of market capitalization, whereas the corresponding effects are insignificant for the biggest firms.

Next, we examine how the real effects of analyst coverage depend on financial constraints. If the cost of external financing is irrelevant to both the investment and financing decisions of a firm, then the decrease in analyst coverage should not affect corporate policies. In other words, a decrease in analyst coverage – and, indeed, anything that affects the cost of external financing – only affects corporate policies for firms that are financially constrained. We find that the real effects of the loss of an analyst are indeed bigger for firms that are more financially constrained.

For analysts to affect corporate policies, they must produce research that is relevant. We examine the quality of the research produced by the brokers and analysts that disappear. Using numerous measures of quality (such as historic earnings estimates accuracy and analyst expectations), we find that the quality of our brokers and analysts is actually slightly above average.

We also perform numerous robustness tests of our results. We examine whether it is the loss of an analyst or the loss of an underwriter that causes the firms that they cover to decrease investment and financing. We find that our results are not explained by whether the broker that disappears is an underwriter for the firm. In additional robustness tests, we examine whether our results are driven by the clustering in time of broker closures and mergers; our matching methodology (e.g., we also use propensity score matching); life cycle differences between our treatment firms and control firms; and broker closures compared to broker mergers. We find that our results are robust.

We contribute to the recently renascent literature on the real effects of a variety of different financial shocks. For example, Sufi (2009) studies the effect of credit ratings on corporate policies. Edmans, Goldstein, and Jiang (2011) study the effect of stock mispricing on takeovers. Duchin, Ozbas, and Sensoy (2010) study the effect of the global financial crisis on corporate investment. In this paper, we study the effect of analyst coverage on corporate policies.

In doing so, we also contribute to the literature on analyst coverage and corporate policies specifically as well as the literature on information asymmetry and corporate policies generally. A long line of literature finds that equity research analysts produce information that matters to investors and firms. There is extensive evidence that analysts' reports impact stock prices (e.g., see Womack (1996), Barber, Lehavy, McNichols, and Trueman (2001), Jegadeesh, Kim, Krische, and Lee (2004), and Loh and Stulz (2011) for recommendations, and Stickel (1991) for earnings estimates). By producing information about the firms that they cover, analysts also monitor these firms (e.g., see Moyer, Chatfield, and Sisneros (1989) and Chung and Jo (1996)). Analysts sometimes issue biased reports to investors (e.g., see Lin and McNichols (1998) and Michaely and Womack (1999)), but they are generally incentivized to produce accurate information (e.g., see Hong and Kubik (2003)).

However, there is a dearth of direct evidence on the real effects of analysts. Ours is one of the few papers that study analyst coverage and corporate policies comprehensively.[3] Moreover, to our knowledge, ours is the first paper to show that changes in analyst coverage cause changes in corporate policies. While most practitioners consider it obvious that analysts affect the firms that they cover, there is little evidence that analysts affect firms directly. For example, it is well known that on October 31, 2007 Oppenheimer analyst Meredith Whitney called for Citigroup sell assets, raise capital, and/or cut its dividend (Lewis (2008)) all of which it did do several months later. It is also well known that analysts blew the whistle on corporate fraud occurring at many firms including Amazon, Charter Communications, Compaq Compuer, CVS, Gateway, Global Crossing, Motorola, PeopleSoft, and Quest Communications (Dyck, Morse, and Zingales (2010)). However, even in these cases, analysts may have simply anticipated corporate policies. The most compelling evidence that analysts matter to firms is survey evidence showing that almost 80% of managers admit that they are willing to decrease investment in order to meet analysts' earnings estimates and that 36% of managers rank analysts

[3] We know of only three other papers that study analyst coverage and corporate policies. The most closely related paper, Doukas, Kim, and Pantzalis (2008), finds that firms with greater analyst coverage spend more on capital expenditures (but does not study research and development expenditures and acquisitions expenditures) and raise more total external financing (debt plus equity). The other two papers are less related. Chang, Dasgupta, and Hilary (2006) study analyst coverage and capital structure but they do not study investment at all nor do they directly study financing. Yu (2008) studies analyst coverage and earnings management. None of these papers address the endogeneity of analyst coverage and corporate policies with a natural experiment like ours does.

as the most important economic agents in setting the stock price of their firm (Graham, Harvey, and Rajgopal (2005)). In this paper, we use two natural experiments and thus can provide large sample evidence that analyst coverage causes corporate policies.

The rest of this paper is organized as follows. Section 2 presents the sample and data. Section 3 presents the main results. Section 4 presents robustness tests. Section 5 concludes.

2. Sample and Data

We construct our sample by identifying firms that lose analyst coverage because of broker closures and broker mergers. We then match these treatment firms to similar control firms. This allows us to estimate the difference-in-differences effect of a decrease in analyst coverage: the difference between the year after versus the year before and the difference between our treatment firms versus our control firms.

We use I/B/E/S to identify brokers that disappear between 1994 and 2008, and we determine broker closures using press releases and broker mergers using the Yearbooks published by the Securities Industry Association. We also use these two sources to identify broker disappearance dates. These dates do not always correspond to broker disappearance dates in I/B/E/S. Since we have no means to reconcile the two when they differ, we instead measure analyst coverage "before" the broker disappearance at three months before the broker disappearance date and "after" the broker disappearance at three months after. Hence the end of year -1 and the start of year +1 are actually separated by six months. For Compustat variables, we use six months before and six months after because Compustat data are annual data and we must avoid overlapping Compustat data in year -1 and year +1.[4] Our list of 52 broker

[4] For example, consider a firm with a broker disappearance date of September 30, 2005 and for which the fiscal year ends on December 31. Analyst coverage for year -1 and year +1 is from June 30, 2005 and December 31, 2005, respectively, while Compustat variables for year -1 and year +1 are from December 31, 2004 and December 31, 2006, respectively.

disappearances is similar to those of Kelly and Ljungqvist (2011) and Hong and Kacperczyk (2010) combined; any differences arise from our use of I/B/E/S data rather than the Reuters data used by Kelly and Ljungqvist (2011), and our use of a broader sample of broker mergers than used by Hong and Kacperczyk (2010).

We construct a list of firms covered by brokers during the year before their disappearance dates as well as the analysts working for the brokers. We assume that an analyst disappears if there is no earnings estimate for him in I/B/E/S during the year after the broker disappearance date. For broker closures, we retain firms for which the analyst disappears from I/B/E/S during the year after the broker disappearance date. For broker mergers, we retain firms covered by both the target broker and the acquirer broker before the merger and for which one of their analysts disappears; this eliminates the possibility that only one broker covers the firm before the merger and the analyst is terminated because he anticipates specific corporate policies for the firms that he covers (e.g., a decrease in investment and financing).

We retain publicly traded U.S. operating firms that are not financials or utilities, that have been traded for at least one year before the broker disappearance date, and that have Compustat data both in year -1 and year +1. Since we use both treatment firms and control firms in our empirical analysis, we impose these restrictions on both groups of firms. We require candidate control firms to be in the same market capitalization tercile, book-to-market tercile, and momentum tercile as our treatment firms. We also require that candidate control firms have the same two-digit SIC code as our treatment firms. We then retain candidate control firms that have the smallest difference in number of analysts to the corresponding treatment firms. We break any remaining ties based on the smallest differences in market capitalization, book-to-market, and momentum. To this end, we compute the difference between treatment firms and controls firms for each of market capitalization, book-to-market, and momentum. We compute the rank of the difference for each of these three variables, and we compute the total rank across all three variables. We retain candidate control firms that have the lowest total rank.

Finally, in the sample thus far, treatment firms have slightly higher market capitalization than control firms, slightly higher book-to-market, slightly lower momentum, and slightly higher analyst coverage. We correct these biases by dropping the 5% of the sample with the biggest positive differences in market capitalization, book-to-market, and analyst coverage and the biggest negative difference in momentum. To this end, we compute the difference between treatment firms and control firms for each of market capitalization, book-to-market, momentum, and analyst coverage. We compute the rank of the difference for each of these four variables such that higher ranks are assigned when treatment firms have a higher market capitalization than control firms, higher book-to-market, lower momentum, and higher analyst coverage. We compute the total rank across all four variables, and we drop the 5% of the sample with the highest total rank.

In summary, our treatment firms and control firms are matched by industry, market capitalization, book-to-market, momentum, and analyst coverage. Our matching is similar to that of Kelly and Ljungqvist (2011) and Hong and Kacperczyk (2010) except that we also match by industry to account for industry effects that explain corporate policies. Our sample comprises 1,961 treatment firms and the same number of control firms.

Analyst data are from I/B/E/S, stock trading data are from CRSP, accounting data are from Compustat, and debt and equity underwriting data are from SDC. We winsorize all continuous variables at the 1st and 99th percentiles.

[Insert Figure 1 about here]

We examine the distribution in calendar time of brokers that disappear and firms that lose an analyst. Figure 1 presents these two distributions. Broker disappearances are relatively dispersed through time although there is some clustering in 2000 and there are no broker disappearances in 1995, 1996, 2003, and 2006. Firms that lose an analyst, on the other hand, are strongly clustered in time: 911 observations (46% of our sample) are in 2000 and 2002, and a further 651 observations (33% of our sample) are in 1997, 2007, and 2008. A small number of broker disappearances accounts for a large number of firms that lose analyst coverage: for example, Credit Suisse First Boston's acquisition of Donaldson, Lufkin & Jenrette in October 2000 accounts for 134 firms (7% of our sample), and the top 15, 20, and 25 (of 52) brokers account for 73%, 84%, and 90%, respectively, of our firms. Our difference-in-differences approach ensures that time-series effects cannot explain our results. However, we examine in the section on robustness tests our results separately for the small number of broker disappearances each of which causes a large number of firms to lose analyst coverage. (We find that our results are similar.)

We use a difference-in-differences approach to ensure that the variation in analyst coverage and the variation in corporate policies are not caused by variation in some other variables that affect both analyst coverage and corporate policies. As long as our treatment firms and control firms are similar except for the loss of an analyst for our treatment firms, our approach ensures that the changes in corporate policies that we estimate are caused by changes in analyst coverage. In this case, we do not also have to control for cross-sectional and time-series effects that affect both analyst coverage and corporate policies. We use four groups of corporate policy variables: investment (capital, research and development, and acquisitions expenditures), financing (issuance of short-term and long-term debt and equity), payouts (dividends and share repurchases), and the change in cash holdings. The construction of these variables is detailed in Appendix 1.

A valid instrument must meet two conditions: relevance and exogeneity. The exogeneity condition is inherently untestable, but we provide evidence based on analysts' expectations. However, we defer doing so until we compare our treatment firms and our control firms. Here, we test the relevance condition by computing the decrease in analyst coverage for our sample firms. During the six months centered on the end of the broker disappearance month, analyst coverage of our treatment firms decreases by 0.95 analysts more relative to our control firms (with a t-statistic of -8.22). Thus broker disappearances are associated with a decrease in analyst coverage of roughly one analyst. This is what we expect given how we construct our sample.

[Insert Figure 2 about here]

We also examine the evolution of analyst coverage during the years before and after the decrease in analyst coverage. Figure 2 presents the results. The mean difference between treatment firms and control firms in coverage is roughly horizontal before and after the decrease in analyst coverage (years -3 through -1 and years +1 through +3) and decreases by roughly one analyst between month -3 and month +3 (by 0.95 analysts to be precise). Our decreases in analyst coverage are clearly not part of long-term trends in analyst coverage but instead are one-time decreases.

We also examine stock returns around decreases in analyst coverage. We find that stock prices decrease: during the two months centered on the broker disappearance month, the mean and median difference in stock returns between our treatment firms and control firms is -1.01% and -0.51%, respectively (with a t-statistic and a z-statistic of -2.03 and -1.86, respectively). Our stock price decreases are similar to those of Kelly and Ljungqvist (2011).

We assess how well our control firms match our treatment firms. To this end, we test the equality of the medians as well as the distributions (using the Kolmogorov-Smirnov test) of our matching variables as well as total assets and our corporate policy variables for both groups of firms. By construction, all of our control firms have the same two-digit SIC code as our treatment firms, so they are well matched by industry. The other matching variables are market capitalization, book-to-market, momentum, and analyst coverage. We measure matching variables during the year ending three months before the broker disappearance date; we measure other variables and corporate policy variables during the year ending six months before the broker disappearance date.

[Insert Table 1 about here]

Table 1 presents the results. Our treatment firms are very similar to our control firms during the year before the decrease in analyst coverage. This is the case not just for matching variables but also for total assets and corporate policy variables. Differences in matching variables are not economically or statistically significant with the exception of analyst coverage: treatment firms are covered by two more analysts than control firms. In the section on robustness tests, we correct for this bias. (We find that our results are similar.)

Now, we provide evidence based on analysts' expectations for the exogeneity condition. This matters because we use broker closures and broker mergers to identify changes in corporate policies that are caused by a decrease in analyst coverage. However, this interpretation of our results is necessarily valid only if the disappearance of brokers and analysts is not caused by changes in corporate policies (reverse causality) or something correlated with them (omitted variable bias). Alternatively, perhaps our brokers and their analysts choose to cover firms for which investment and financing are expected to decrease, so the profitability of providing research for these firms decreases,[5] and the brokers that cover these firms close or merge. As a result of their choice to cover these firms, the analysts cannot find work at another broker, and they disappear.

We examine the possibility that our treatment firms and control firms differ based on characteristics that affect future corporate policies but that are not captured by our matching variables by comparing analysts' expectations for the two groups of firms. We use four measures of analysts' expectations. First, we use earnings estimates for the next fiscal year measured as a percent of the stock price. Second, we use investment recommendations measured on a five-point scale. Higher recommendations are more favorable. Third, we use long-term earnings growth rate estimates for the next five years. Finally, we use price targets for the next year measured as the natural logarithm of the price target as a percent of the stock price. We compute all analysts' expectations variables as the mean expectations of all analysts covering the firm, and we measure them during the year ending three months before the broker disappearance date. We note that data are not available for all firms and all analysts.[6]

Table 1 presents the results.[7] Earnings estimates for the next fiscal year are significantly more pessimistic for treatment firms than for control firms (e.g., a median of 4.6% of the stock price versus 4.9%). Investment recommendations and long-term earnings growth rate estimates are not significantly different. Price targets for the next year are significantly more optimistic for treatment firms than for control firms (e.g., a median of 24.5% of the stock price versus 22.0%). Statistically, two differences are significant but in opposite directions, and two differences are not significant. Economically, the differences are small. Overall, we conclude that analysts'

[5] For example, value firms compared to growth firms provide less profit to investment banks from underwriting securities offerings, trading the firm's stock for their investor clients, making a market in the firm's stock, etc.
[6] Data are generally available for all variables except price targets. Price targets data begin in July 1999 and thus are only available for roughly 70% of our observations.
[7] The results are similar for all three years before the broker disappearance date.

expectations are similar for our treatment firms and control firms. This conclusion is supported by comparisons of realized future returns.[8] The results suggest that changes in corporate policies are indeed exogenous to the disappearance of brokers and analysts.

3. Main Results

3.1. The Real Effects of Analyst Coverage

We now examine the effect of a decrease in analyst coverage on corporate policies. For each of our corporate policy variables, we compute the mean change from year -1 to year +1 for our treatment firms (the treatment difference), our control firms (the control difference), and the difference between our treatment firms and control firms (the difference-in-differences). We focus on the mean difference-in-differences, and we also compute its t-statistic.

[Insert Table 2 about here]

Table 2 presents the results.[9] (Totals do not always exactly equal the sum of their components because of winsorizing.) All of the main mean difference-in-differences are economically and statistically significant. For investment, capital expenditures decrease by 0.71% of total assets, research and development expenditures by 0.64%, and acquisitions expenditures by 0.94% after the decrease in analyst coverage. Total investment decreases by 2.35%. For financing, the change in short-term debt is insignificant, the change in long-term debt is -1.62% of total assets, and equity issuance decreases by 1.12%. Total financing decreases by 2.62%. For payouts, dividends and share repurchases are insignificant individually and collectively. Finally, the change in cash holdings is -1.04% of total assets.

[8] To examine realized future returns, we compute mean monthly returns for our treatment firms and control firms over one, two, and three years after the loss of an analyst. We find that mean monthly returns are 1.43%, 0.86%, and 0.96%, respectively, for treatment firms and 1.41%, 0.82%, and 0.88%, respectively, for control firms.

[9] The results are similar if we cluster standard errors by the analyst that disappears, by the broker that disappears, by the broker disappearance date, or by industry.

We note that it does not matter that the treatment difference on its own is not always negative (as is the case for investment, for example). Indeed, we do not also have to control for cross-sectional and time-series effects that affect both analyst coverage and corporate policies because we use a difference-in-differences approach. For example, the 3.26% increase in investment for control firms may be caused by an increase in investment opportunities for all firms. However, investment increases by only 0.84% for treatment firms because not only do their investment opportunities increase but their analyst coverage decreases, so the net effect is the difference: 3.26%-0.84%=2.42%. In other words, all that matters is the difference-in-differences.

Following an increase in information asymmetry, firms should alter their financing decisions not only in terms of the total amount of financing they use but also in terms of how their financing is split between different sources of funds. They should favor those sources of financing that are the least sensitive to information asymmetry: internal financing (cash) first, then debt in increasing order of riskiness (first lower risk short-term debt and then higher risk long-term debt), and equity only as a last resort. Table 2 suggests that this is in fact how firms behave: equity issuance decreases by 1.1% of total assets, and net total debt issuance decreases by 1.6%, but this decrease in net total debt issuance is driven by the decrease in higher risk net long-term debt issuance, which drops by 1.6% of total assets, while lower risk net short-term debt issuance is virtually unchanged.[10] Finally, firms increase their use of cash for financing: they use up cash by 1.0% of total assets (hence the decrease in cash holdings).

[10] The relatively greater decrease in debt issuance than equity issuance – 1.6% versus 1.1% – appears to be related to debt capacity. We sort firms into halves based on leverage before the decrease in analyst coverage. We find that low leverage firms – firms with greater debt capacity – decrease debt issuance by 1.2% whereas high leverage firms – firms with less debt capacity – decrease debt issuance by 2.1%, and the corresponding figures for equity issuance are a decrease of 2.8% for low leverage firms and an increase of 0.5% (not statistically significant) for high leverage firms. In other words, firms far from their debt capacity are able to use relatively more (cheaper) debt than equity, but firms close to their debt capacity are forced to use relatively more (dearer) equity than debt.

The results are also of magnitudes that are economically plausible. Using median total assets of $1,811 million from Table 1, we compute the effect of the loss of an analyst on the corporate policies of the typical firm. The effect is economically significant. For investment, the mean decreases in capital expenditures, research and development expenditures, and acquisitions expenditures correspond to a mean decrease of $13 million, $12 million, and $17 million, respectively. In other words, for the typical firm, total investment decreases by $42 million. For financing, the mean decreases in net total debt issuance and equity issuance correspond to a mean decrease of $28 million and $20 million, respectively. In other words, for the typical firm, total financing decrease by $48 million.

By way of comparison, we consider the recent research on the real effects of financial shocks. Sufi (2009) finds that the introduction of a credit rating causes both asset growth and cash acquisitions to roughly double, and he interprets his findings as showing that a decrease in information asymmetry causes an increase in investment. Similarly, Edmans, Goldstein, and Jiang (2011) find that an interquartile decrease in stock valuation causes the probability of a takeover of the firm to roughly double.

There is additional evidence from the recent financial crisis. Campello, Graham, and Harvey (2010) provide survey evidence that firms that were not financially constrained were not significantly affected by the crisis compared to financially constrained firms: the latter compared to the former planned to decrease capital expenditures, technology expenditures, and cash holdings (i.e., increase internal financing) by roughly 9, 13, and 12 percentage points more, respectively. Duchin, Ozbas, and Sensoy (2010) find that investment decreased by 0.72% of total assets (annualized) for firms with zero cash holdings whereas it decreased by 0.42 percentage points less (58% less) for firms with a one-standard deviation increase in cash holdings.

Almeida, Campello, Laranjeira, and Weisbenner (2011) find that firms with substantial long-term debt maturing during the credit crisis decreased investment by 10% of capital (annualized) more than firms that were not thus financially constrained. Our results are comparable: for total investment, total financing, and cash holdings, the mean difference-in-difference relative to its standard deviation is -13.8%, -10.7%, and -5.1%, respectively. Overall, compared to the results in the literature, our results are economically plausible if somewhat smaller.

[Insert Figure 3 about here]

Next, we examine the evolution of corporate policies around the loss of an analyst. Figure 3 presents the difference in corporate policy variables between treatment firms and control firms during the three years before and the three years after the decrease in analyst coverage: Panel A through Panel D present investment, financing, payouts, and the change in cash holdings, respectively.

Panel A shows that investment is roughly horizontal before year -1 and after year +1 and decreases mainly between year -1 and year +1. This is the case for all four of our investment variables. Panel B paints a similar picture. Equity issuance increases and decreases between year -3 and year -1 to end roughly unchanged over these three years. The changes in short-term and long-term debt are roughly horizontal between year -3 and year -1. Since total financing is the sum of equity issuance and net total debt issuance, it exhibits an increase-decrease pattern between year -3 and year -1. Overall, however, the main effect is a decrease between year -1 and year +1 in equity issuance, debt issuance, and total financing. Panel C shows that the evolution of payouts is insignificant overall because the variation in payouts is an order of magnitude smaller than the variation in investment and financing. Finally, Panel D shows the change in cash holdings increases and decreases somewhat between year -3 and year -1, and then it decreases between year -1 and year +1.

Figure 3 also shows that the changes in corporate policies between year -1 and year +1 are not part of long-term trends in corporate policies before the decrease in analyst coverage but instead are changes that occur only when analyst coverage decreases. This result supports the parallel trends assumption underlying our difference-in-differences approach. We test this assumption as follows: for each corporate policy variable in Table 2, we compute the difference between our treatment firms and control firms between year -3 and year -1. We find (results not tabulated) that only two differences are statistically significant at the 5% level: research and development expenditures and total investment. Even in these two cases (out of twelve), the differences are small in economic magnitude compared to the differences between year -1 and year +1, which is consistent with the patterns in Figure 3. We conclude that the changes in corporate policies are not part of long-term trends when analyst coverage decreases.

We hypothesize that both the decrease in investment and financing are caused by the increase in information asymmetry that results from the loss of an analyst. In this case, the firms that decrease financing the most should also be the firms that decrease investment the most. We test this by examining how the magnitude of the decrease in financing is associated with the magnitude of the decrease in investment. We find a significant correlation of 0.377 (with a p-value of 0.000). Overall, the firms that decrease investment the most are also the firms that decrease financing the most.

We also examine whether our results are stronger when the decrease in analyst coverage is more costly. The loss of an analyst should be more costly for smaller firms and for firms with less analyst coverage. The motivation for these conditioning variables is self-explanatory. For example, one analyst is relatively more important for a firm covered by five analysts than for a firm covered by twenty-five analysts: the disappearance of an analyst causes a bigger increase in information asymmetry when there are few other analysts remaining that cover the firm than when there are many analysts remaining.

For each corporate policy variable, we run one pooled regressions using treatment firms and control firms before and after the decrease in analyst coverage. We have one "before" observation and one "after" observation for each treatment firm and analogously for each control firm. We sort firms into quintiles based on the value of conditioning variables for treatment firms and control firms before the decrease in analyst coverage. For each quintile, we use a constant term, a "treatment firm" dummy variable, an "after" dummy variable, and an interaction between the "treatment firm" dummy variable and the "after" dummy variable. What we are interested in is the interaction term (the difference-in-differences) for each quintile.

[Insert Table 3 about here]

Table 3 presents the results for the interaction term for each quintile. Panel A shows that total investment and total financing decrease by 4.9% and 9.9%, respectively, in the bottom quintile of market capitalization whereas the corresponding figures are insignificant in the top quintile. Similarly, the decrease in the change in cash holdings is bigger in the bottom quintile of market capitalization than in the top quintile by 3.4 percentage points. Panel B shows that the results are similar for analyst coverage. Overall, the results suggest that a decrease in analyst coverage affects corporate policies mostly for smaller firms and firms with less analyst coverage.

3.2. The Real Effects of Analyst Coverage Conditional Upon the Change in Information Asymmetry

We hypothesize that the loss of an analyst affects corporate policies by causing an increase in information asymmetry. If this is the case, then the changes in corporate policies that we find above should be biggest for firms for which information asymmetry increases the most as a result of the loss of an analyst.

To test this, we condition upon proxies for the change in information asymmetry. We use the same five proxies for information asymmetry as Kelly and Ljungqvist (2011): the bid-ask spread, the Amihud liquidity measure, the ratio of zero and missing returns days to total days, the magnitude of earnings announcement surprises, and the volatility of the market reaction to earnings announcements. We also compute these variables like Kelly and Ljungqvist (2011). We measure the change in information asymmetry using difference-in-differences. We classify firms in the top tercile of the change in the information asymmetry proxy as having a big change in information asymmetry, and we classify firms in the bottom tercile as having a small change. Like Kelly and Ljungqvist (2011), we find an economically and statistically significant increase in information asymmetry for firms that lose an analyst (not tabulated).

For all five conditioning variables, we use a triple difference approach: we compare the mean difference-in-differences for each corporate policy variable (as in Table 2) for firms with a big change in information asymmetry and firms with a small change in information asymmetry.

[Insert Table 4 about here]

Table 4 presents the results. The effect of a loss of analyst on corporate policies is bigger for firms with a bigger increase in information asymmetry. This is the case for all five proxies for information asymmetry. By way of example, using the bid-ask spread, total investment and total financing decrease by 2.4 and 7.2 percentage points more, respectively, for firms with a big increase in information asymmetry than for firms with a small increase in information asymmetry. Similarly, firms with a big increase use up 5.1 percentage points more of their cash holdings than firms with a small increase. Overall, the results suggest that the real effects of analyst coverage are indeed bigger for firms with a bigger change in information asymmetry.

3.3. The Real Effects of Analyst Coverage Conditional Upon Financial Constraints

We examine how the real effects of analyst coverage depend on financial constraints. When a firm loses analyst coverage, information asymmetry increases, and thus its cost of external financing increases. Consequently, its optimal amount of investment and its optimal amount of external financing decrease. However, the cost of external financing is irrelevant to firms that have sufficient internal capital to finance their investments. For such financially unconstrained firms, the decrease in analyst coverage should not affect corporate policies. Therefore, the real effects of analyst coverage should be bigger for firms that are more financially constrained.

We condition upon proxies for financial constraints and test whether the real effects of analyst coverage are bigger for firms that are more financially constrained. We use two proxies for financial constraints that are standard in the literature: the composite proxy based on Almeida, Campello, and Weisbach (2004) and the proxy based on Rajan and Zingales (1998).[11]

Almeida, Campello, and Weisbach (2004) use four proxies to classify firms as constrained or unconstrained: total payout ratio, total assets, bond rating status, and commercial paper rating status.[12] We construct their four proxies, and we then classify as constrained firms that are constrained based on all four of their proxies, and we classify as unconstrained firms that

[11] These proxies are arguably exogenous compared to the constituent proxies of the index of financial constraints proposed by Kaplan and Zingales (1997).

[12] These four proxies are standard in the literature. For example, for the total payout ratio, see Fazzari, Hubbard, and Petersen (1988); for total assets, see Gilchrist and Himmelberg (1995) and Hadlock and Pierce (2010); for bond rating status, see Whited (1992), Kashyap, Lamont, and Stein (1994), and Gilchrist and Himmelberg (1995); and for commercial paper rating status, see Calomiris, Himmelberg, and Wachtel (1995).

are unconstrained based on all four proxies. In this way, we are able to utilize all of the information in their four proxies as well as to capture this information succinctly in a single composite proxy.[13]

Rajan and Zingales (1998) use a single proxy to classify firms as constrained or unconstrained: the cash flow-investment gap. We construct this proxy as cash flow minus investment all divided by total assets. For cash flow, we use net income before extraordinary items plus depreciation and amortization. For investment, we use the sum of capital expenditures, research and development expenditures, and acquisitions expenditures. We classify firms in the bottom half of the cash flow-investment gap as constrained, and otherwise we classify them as unconstrained.

We measure all of our conditioning variables using only treatment firms, and we measure them during the year before the decrease in analyst coverage. (We examine whether our treatment firms and control firms are similar for our conditioning variables, and we find that they are not significantly different.) We use a triple difference approach: we compare the mean difference-in-differences for each corporate policy variable (as in Table 2) for firms that are financially constrained and firms that are not financially constrained.

[Insert Table 5 about here]

Table 5 presents the results. The effect of a loss of analyst on corporate policies is bigger for firms that are financially constrained. By way of example, using the cash flow-investment gap from Rajan and Zingales (1998), total investment and total financing decrease by 6.0 and 6.6 percentage points more, respectively, for firms that are financially constrained than for firms that

[13] If we use their four proxies individually, the results are similar (not tabulated).

are not financially constrained.[14] Overall, the results suggest that the real effects of analyst coverage are indeed bigger for financially constrained firms.

4. Broker and Analyst Quality

For the coverage of the brokers and analysts in our sample to affect corporate policies, it must be the case these brokers and analysts produce research that is relevant. Otherwise, their disappearance would not affect corporate policies. That we find that these brokers and analysts do have significant real effects indirectly suggests that they do not produce low quality research, but we now examine this possibility directly.

First, we examine the quality of the research produced by our brokers. Typically for most brokers, research is a cost center for investment banks and it is supported by revenues from underwriting, trading, and market making. The broker closures and brokers mergers in our sample appear to be motivated by the general business strategy of the broker. Moreover, the brokers involved are often research powerhouses. We find that the broker that closes or at least one of the brokers that merge is a leader in research according to Institutional Investor magazine for 58% of our sample firms.[15]

Next, we examine the earnings estimate accuracy of our brokers and analysts. This standard measure of the quality of research is shown in the literature to explain analyst promotions and demotions (see Mikhail, Walther, and Willis (1999), Hong and Kubik (2003), and Wu and Zang (2009)). For brokers, we construct the relative earnings estimate accuracy measure used in the literature by first computing the accuracy rank – on a scale of zero to one – across all brokers that cover a firm and then computing the mean accuracy rank – also on a scale

[14] If we use the cash flow-investment gap at the industry level rather than the firm level, the results are similar (not tabulated).

[15] Each year, Institutional Investor magazine surveys money managers about which analysts they think produced the best research. The best analysts are named "star" analysts, and the brokers with the most star analysts are named "leading" brokers. There are about 15 leading brokers each year.

of zero to one – of a broker across all firms that the broker covers. The resulting measure captures the mean accuracy of the broker relative to other brokers who cover the same firms that this broker covers. For analysts, we construct this measure analogously.

[Insert Table 6 about here]

Panel A of Table 6 presents the mean and median accuracy for our brokers and analysts as well as the mean accuracy weighted by the number of firms covered by the broker or analyst. There are 71 unique brokers that disappear and 430 unique analysts.[16] The accuracy of our brokers is slightly above average: the mean and median accuracy is 0.53 and 0.55, respectively. This is also the case for our analysts: the mean and median accuracy is 0.51 and 0.52, respectively.

Moreover, the literature finds that analysts that are not promoted or are demoted tend to have very low accuracy. For example, Hong and Kubik (2003) find that turnover is concentrated in the bottom quartile of accuracy. Very few of our analysts fall into the very low accuracy group: only 8% of our analysts have accuracy of less 0.25 (not tabulated).

Finally, we examine the quality of the research provided by our brokers and analysts by comparing their expectations to the expectations of their peers. Specifically, we compare the expectations of analysts that cover treatment firms and disappear and the mean of the expectations of all other analysts that cover treatment firms. To examine whether this is the case, we use the same measures of analysts' expectations as in Table 1. We note that data are not available for all firms and all analysts.[17]

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[16] There are 71 unique brokers even though there are 52 broker disappearances because for broker mergers, some analysts that disappear work for one broker while other analysts work for the other broker.

[17] Data are generally available for earnings estimates. Recommendations data are only available for roughly 55% of our observations because not all analysts produce recommendations. For the same reason, growth rate estimates data are only available for roughly 45% of our observations. Finally, partly because price targets data begin in July 1999 and partly because not all analysts produce price targets, price targets data are only available for roughly 45% of our observations.

Panel B of Table 6 presents the results.[18] The expectations of analysts that disappear are similar to the expectations of other analysts for earnings estimates. While we interpret the results for the rest of the table with caution because data are only available for roughly half of our observations, the results are similar. Mean recommendations are more pessimistic but median recommendations are more optimistic. Long-term earnings growth rate estimates are the same using the mean and more pessimistic using the median. Price targets are similar using both the mean and the median. Overall, the expectations of analysts that cover treatment firms and disappear and the mean expectations of all other analysts that cover treatment firms are similar.

In summary, the evidence suggests that the brokers and analysts that disappear do not produce low quality research.

5. Robustness Tests

5.1. Underwriting Relationships

We also examine whether our results can be explained by underwriting relationships. Fernando, May, and Megginson (2011) find that when Lehman Brothers collapsed, firms for which it had underwritten equity lost roughly 5% of their market value. It is possible that our sample firms decrease their investment and financing not because they lose an analyst but because they lose an underwriter. We use a triple difference approach: we compare the mean difference-in-differences for each corporate policy variable (as in Table 2) for firms with underwriting relationships and firms without.

[Insert Table 7 about here]

We consider a firm to have an underwriting relationship with a broker if the broker that disappears underwrites an offering during the three years ending three months before the broker

[18] The results are similar for all three years before the broker disappearance date.

disappearance date. We consider underwriting relationships based on two types of offerings separately: debt and equity. Table 7 presents the results.[19]

Firms with and without past debt underwriting relationships behave similarly with one exception: firms substitute equity financing for debt financing when their debt underwriter disappears (second set of results). However, firms with past equity underwriting relationships clearly decrease both their investment and their financing significantly more than firms without such relationships (third set of results). Of course, these underwriting relationships do not explain our results because only a small proportion of firms have such relationships: 7% for debt underwriting and 4% for equity underwriting. Moreover, the results for firms without such relationships (first set of results) are similar to the results in Table 2.

We examine not only actual underwriting relationships but also potential underwriting relationship. We also consider a firm to have an underwriting relationship with a broker if the broker that closes or either broker that merges underwrites at least one debt or equity offering (for any firm generally and not specifically for the firm in question) during the year ending three months before the broker disappearance date. These underwriting relationships do not explain our results either: the results for the 82% of our firms that have such underwriting relationships are similar to the results for the 18% of our firms without such relationship (not tabulated). Overall, the results suggest that underwriting relationships affect corporate policies but they do not explain the effect of analyst coverage on corporate policies.

5.2. Statistical Robustness

We perform numerous robustness tests of our results. In our first group of robustness tests, we examine our results separately for the small number of broker disappearances each of which causes a large number of firms to lose analyst coverage. This is important because Figure

[19] The results for the one-year and five-year windows are similar (not tabulated).

1 shows that firms that lose analyst coverage are strongly clustered in time. To this end, we perform three analyses. First, we collapse our observations by broker to avoid giving more weight to broker disappearances that cause a large number of firms to lose analyst coverage. For each broker, we use the mean change for each of our corporate policy variables. We redo Table 2 for the top 15, 20, and 25 brokers ranked by the number of firms that lose analyst coverage, which collectively account for 1,436, 1,639, and 1,758 observations (73%, 84%, and 90% our sample), respectively. The results are similar. Second, we redo Table 2 for each of the top 25 brokers separately. We find that the results for our full sample are not driven by one broker or a small number of brokers. Third, we examine whether our results are different for broker disappearances that occur during economic contractions versus economic expansions. To this end, we redo Table 2 for the group of brokers that disappear in 2000, 2001, 2002, and 2008 (1,281 observations or 65% of our sample) separately from the group of brokers that disappear in the other years in our sample (680 observations or 35% of our sample). We find that our results are similar for both groups.

In our second group of robustness tests, we examine how our results are affected by our matching methodology. First, as Table 1 and Figure 2 show, our treatment firms are covered by two more analysts than our control firms. We now correct this bias by dropping observations for which analyst coverage of treatment firms is more than five analysts greater than the analyst coverage of control firms.[20] We redo Table 2 for this sample, and we find that the results are similar to the results in Table 2.

Second, we use propensity score matching as our matching methodology. Using all firms between 1994 and 2008, we run a probit regression to estimate propensity scores. We regress a

[20] As a result, we drop 20% of the sample, and the distribution of analyst coverage is the same for treatment firms and control firms (median of 16 analysts for both groups).

dummy variable that equals one for treatment firms and zero for control firms on market capitalization, book-to-market, momentum, analyst coverage, two-digit SIC code dummy variables, and calendar year dummy variables. We match each treatment firm to a control firm in the same industry and same year with the nearest predicted propensity score. We then redo Table 2 for this sample, and we again find that the results are similar to the results in Table 2.

Third, we run pooled regressions using treatment firms and control firms before and after the decrease in analyst coverage to control for changes in our matching variables before versus after the decrease in analyst coverage using the same control firms as in our main sample. We have one "before" observation and one "after" observation for each treatment firm and analogously for each control firm. We use three specifications. In the first specification, we use a constant term, a "treatment firm" dummy variable, an "after" dummy variable, and an interaction between the "treatment firm" dummy variable and the "after" dummy variable. In the second specification, we add control variables for market capitalization, book-to-market, momentum, and analyst coverage. In the third specification, we add control variables interacted with the "after" dummy variable. In each of the three specifications, the results are again similar to the results in Table 2.

Fourth, we consider whether our results are driven by life cycle differences between our treatment firms and control firms. Suppose that the analysts in our sample are terminated because the firms that they cover are about to begin to terminally decline. Furthermore, suppose that this is the case only for our treatment firms, not our control firms; in other words, our treatment firms and control firms are not properly matched by life cycle. Then the changes in corporate policies that we attribute to a decrease in analyst coverage are actually attributable to life cycle effects. We test this explanation by computing the ages of our treatment firms and control firms where age is measured as the number of years since the firm became a publicly traded firm. We find that the age of our treatment firms and control firms is similar: the mean (median) age is 21.5 (13.6) years for our treatment firms versus 22.5 (14.1) years for our control firms; if anything, our treatment firms are younger than our control firms. These four analyses suggest that our results are not driven by our matching methodology.

In our third and final group of robustness tests, we examine whether our results are driven by broker closures (57% of our sample) compared to broker mergers (43% of our sample). We redo Table 2 separately for each of these two groups of broker disappearances. We find that our results (not tabulated) are similar for both groups (except that equity issuance exhibits only an economically small decrease in the merger group) although they are economically and statistically less significant for mergers than for closures.[21] Overall, our results are not driven by either broker closures or broker mergers alone.

6. Conclusion

In this paper, we study the real effects of financial shocks in the context of analyst coverage and corporate policies. We hypothesize that a decrease in analyst coverage increases information asymmetry and thus increases the cost of capital. Consequently, the profitability of projects decreases, so the optimal amount of investment decreases. Likewise, since the cost of external financing increases both in absolute terms and relative to the cost of external financing, the optimal amount of external financing decreases as well. In short, a decrease in analyst coverage causes a decrease investment and financing.

[21] The difference in significance is driven by size. Firms that lose analyst coverage because of broker mergers are much bigger than firms that lose analyst coverage because of broker closures (the mean and median market capitalization is $10,946 million and $3,321 million, respectively, for mergers, compared to $6,282 million and $1,084 million, respectively, for closures). Moreover, Table 3 shows that our results are bigger for smaller firms. Since broker merger-closure status is correlated with size, it is not surprising that our results are stronger for closures than for mergers.

Since the literature provides empirical evidence that exogenous decreases in analyst coverage cause an increase in information asymmetry and the cost of capital, we provide evidence that a decrease in analyst coverage causes a decrease in investment and financing. We use two natural experiments to identify changes in analyst coverage that are exogenous to corporate policies: broker closures and broker mergers. We compare the changes in corporate policies of these firms that lose an analyst to those of firms matched on size, book-to-market, momentum, industry, and analyst coverage.

We find that firms that lose an analyst significantly decrease investment and financing. Moreover, our results are stronger when the decrease in analyst coverage is more costly: for smaller firms, for firms with less analyst coverage, and for firms with a bigger increase in information asymmetry. Similarly, our results are stronger for firms that are financially constrained. Taken as a whole, our results suggest that analysts are important information producers that significantly affect corporate policies, and, more broadly, our results extend our understanding of the real effects of financial shocks.

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Table 1
Descriptive Statistics

This table presents descriptive statistics that compare treatment firms and control firms. The sample comprises 1,961 treatment firms that lose an analyst between 1994 and 2008 because of broker closures and broker mergers and the same number of control firms matched by industry, size, book-to-market, momentum, and analyst coverage. Both groups of firms are publicly traded U.S. operating firms, are not financials or utilities, and have been traded for at least one year. All corporate policy variables are scaled by total assets. Analysts' expectations variables are computed as the mean expectations of all analysts covering the firm, and they comprise the following: earnings estimates for the next fiscal year measured as a percent of the stock price; investment recommendations measured on a five-point scale a higher value of which means more a favorable recommendation; long-term earnings growth rate estimates for the next five years; and price targets for the next year measured as the natural logarithm of the price target as a percent of the stock price. Matching variables and analysts' expectations variables are measured during the year ending three months before the broker disappearance date. All other variables are measured during the year ending six months before the broker disappearance date.

	25th percentile		Median		75th percentile		p-value of test of equality of medians	p-value of test of equality of distributions
	Treatment firms	Control firms	Treatment firms	Control firms	Treatment firms	Control firms		
Matching variables								
Market capitalization ($M)	632	603	2,680	2,445	11,001	9,724	0.523	0.154
Book-to-market	0.179	0.172	0.339	0.333	0.633	0.633	0.655	0.846
Momentum	-26.54%	-24.44%	-1.47%	-1.18%	25.27%	25.04%	0.898	0.267
Number of analysts	10.0	8.0	17.0	15.0	25.0	23.0	0.000	0.000
Other variables								
Total assets ($M)	460	396	1,811	1,556	7,354	6,118	0.030	0.106
Investment variables								
Capital expenditures	2.46%	2.25%	4.48%	4.23%	8.11%	7.23%	0.094	0.069
Research and development exp.'s	0.00%	0.00%	1.52%	1.05%	7.83%	6.09%	0.114	0.000
Acquisitions expenditures	0.00%	0.00%	0.00%	0.00%	1.92%	1.83%	0.598	0.999
Total investment	6.54%	6.09%	11.18%	10.05%	18.01%	15.70%	0.001	0.000
Financing variables								
Change in short-term debt	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.024	0.410
Change in long-term debt	-0.74%	-0.92%	0.00%	0.00%	2.32%	2.46%	0.801	0.727
Equity issuance	0.23%	0.21%	0.94%	0.83%	2.71%	2.38%	0.057	0.149
Total financing	0.08%	-0.04%	2.32%	2.07%	7.18%	6.84%	0.076	0.208
Payout variables								
Dividends	0.00%	0.00%	0.00%	0.00%	1.21%	1.39%	0.376	0.328
Share repurchases	0.00%	0.00%	0.11%	0.07%	3.19%	3.04%	0.576	0.717
Total payouts	0.00%	0.00%	1.06%	1.18%	5.12%	5.22%	0.453	0.719
Change in cash holdings	-1.36%	-1.01%	0.17%	0.21%	2.76%	2.55%	0.472	0.122
Analysts' expectations variables								
Earnings estimates	2.3%	2.7%	4.6%	4.9%	6.6%	6.8%	0.029	0.010
Investment recommendations	3.5	3.5	3.8	3.8	4.1	4.2	0.142	0.463
Long-term earnings growth rate est.'s	11.6%	12.0%	16.3%	15.8%	23.5%	22.9%	0.160	0.510
Price targets	11.2%	11.0%	24.5%	22.0%	46.6%	40.7%	0.056	0.012

Table 2
The Effect of a Decrease in Analyst Coverage on Corporate Policies

This table presents the change in corporate policies caused by the loss of an analyst. The sample comprises 1,961 treatment firms that lose an analyst between 1994 and 2008 because of broker closures and broker mergers and the same number of control firms matched by industry, size, book-to-market, momentum, and analyst coverage. Both groups of firms are publicly traded U.S. operating firms, are not financials or utilities, and have been traded for at least one year. For each corporate policy variable, the mean change from the year before the decrease in analyst coverage to the year after is computed for treatment firms (the treatment difference), control firms (the control difference), and the difference between treatment firms and control firms (the difference-in-differences). All corporate policy variables are scaled by total assets. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Statistical significance is only tabulated for the mean of the difference-in-differences.

	Mean treatment difference (year +1 vs. year -1)	Mean control difference (year +1 vs. year -1)	Mean of diff-in-diffs (treatments versus controls)	t-statistic for difference-in-differences
Investment				
Capital expenditures	0.26%	0.93%	-0.71%***	-4.76
Research and development expenditures	0.44%	1.10%	-0.64%***	-5.25
Acquisitions expenditures	0.12%	1.17%	-0.94%***	-3.24
Total investment	0.84%	3.26%	-2.35%***	-6.08
Financing				
Change in short-term debt	-0.14%	-0.15%	0.07%	0.59
Change in long-term debt	-0.17%	1.32%	-1.62%***	-4.19
Equity issuance	-1.90%	-0.84%	-1.12%***	-2.87
Total financing	-2.17%	0.41%	-2.62%***	-4.74
Payouts				
Dividends	0.13%	0.15%	0.01%	0.35
Share repurchases	0.21%	0.39%	-0.17%	-0.98
Total payouts	0.35%	0.52%	-0.19%	-1.00
Change in cash holdings	0.03%	0.95%	-1.04%**	-2.27

Table 3
The Effect of a Decrease in Analyst Coverage on Corporate Policies Conditional Upon Market Capitalization and Analyst Coverage

This table presents the mean change in corporate policies caused by the loss of an analyst conditional upon market capitalization and analyst coverage. The sample comprises 1,961 treatment firms that lose an analyst between 1994 and 2008 because of broker closures and broker mergers and the same number of control firms matched by industry, size, book-to-market, momentum, and analyst coverage. Both groups of firms are publicly traded U.S. operating firms, are not financials or utilities, and have been traded for at least one year. For each corporate policy variable, one pooled regression is run using treatment firms and control firms before and after the decrease in analyst coverage. There is one "before" observation and one "after" observation for each treatment firm and analogously for each control firm. Firms are sorted into quintiles based on the value of conditioning variables for treatment firms and control firms before the decrease in analyst coverage. For each quintile, there is a constant term, a "treatment firm" dummy variable, an "after" dummy variable, and an interaction between the "treatment firm" dummy variable and the "after" dummy variable. All corporate policy variables are scaled by total assets. Only the interaction terms (the differences-in-differences) are tabulated. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Mean Difference-in-Differences Conditional Upon Market Capitalization at Year -1

	Q1 (smallest)	Q2	Q3	Q4	Q5 (biggest)
Investment					
Capital expenditures	-1.32**	-0.80	-0.75	-0.79	0.17
Res. and dev. expenditures	-1.55**	-0.95	-0.42	-0.09	-0.20
Acquisitions expenditures	-1.95***	-1.17*	-0.66	-0.59	-1.00
Total investment	-4.90***	-3.08***	-1.74	-1.37	-0.94
Financing					
Change in short-term debt	0.40	-0.16	0.23	-0.26	0.04
Change in long-term debt	-3.66***	-1.64**	-1.58*	-0.05	-0.58
Equity issuance	-6.28***	-0.43	-0.00	0.07	0.45
Total financing	-9.85***	-2.14	-1.33	-0.04	-0.03
Payouts					
Dividends	0.12	0.05	-0.08	0.02	-0.08
Share repurchases	-0.44	-0.55	-0.33	-0.07	0.48
Total payouts	-0.14	-0.44	-0.54	-0.21	0.30
Change in cash holdings	-3.44***	0.28	-0.82	-0.38	-0.11

Panel B: Mean Difference-in-Differences Conditional Upon Analyst Coverage at Year -1

	Q1 (least)	Q2	Q3	Q4	Q5 (most)
Investment					
Capital expenditures	-1.43**	-0.72	-0.57	-0.42	-0.74
Res. and dev. expenditures	-1.39**	-0.75	-0.62	-0.03	-0.64
Acquisitions expenditures	-1.51**	-0.84	-0.82	-0.99	-1.22*
Total investment	-4.55***	-2.19*	-1.74	-1.67	-2.58**
Financing					
Change in short-term debt	0.34	0.21	-0.13	-0.21	0.04
Change in long-term debt	-2.98***	-1.56*	-1.40*	-0.76	-0.76
Equity issuance	-4.94***	-0.97	0.26	-0.24	-0.35
Total financing	-7.80***	-1.89	-1.28	-1.32	-0.87
Payouts					
Dividends	0.07	0.07	-0.04	-0.10	-0.08
Share repurchases	-0.14	-0.70	-0.41	-0.38	0.63
Total payouts	0.02	-0.64	-0.48	-0.56	0.42
Change in cash holdings	-2.11**	-1.59	-0.17	-0.60	0.24

Table 4
The Effect of a Decrease in Analyst Coverage on Corporate Policies Conditional Upon the Increase in Information Asymmetry

This table presents the change in corporate policies caused by the loss of an analyst conditional upon the increase in information asymmetry. The sample comprises 1,961 treatment firms that lose an analyst between 1994 and 2008 because of broker closures and broker mergers and the same number of control firms matched by industry, size, book-to-market, momentum, and analyst coverage. Both groups of firms are publicly traded U.S. operating firms, are not financials or utilities, and have been traded for at least one year. For each corporate policy variable, a mean difference-in-differences-in-differences is computed: the mean difference between the year after the decrease in analyst coverage and the year before; the mean difference between treatment firms and control firms; and the mean difference between firms that have a big change in information asymmetry and firms that have a small change in information asymmetry. All corporate policy variables are scaled by total assets. Firms in the top tercile of the change in information asymmetry are classified as having a big change and firms in the bottom tercile are classified as having a small change. The bid-ask spread is computed as the mean during the year of the daily ask price minus the bid price all divided by the mean of the ask price and the bid price. The Amihud liquidity measure is computed as the mean during the year of the daily absolute value of the stock return divided by the dollar value of trading volume. The ratio of zero and missing returns days to total days is computed as the number of trading days with zero or missing returns during the year divided by the number of trading days during the year. The earnings announcement surprise is computed as the mean during the year of the quarterly absolute value of the difference between actual earnings and expected earnings divided by the stock price. The earnings announcements volatility is computed as the mean during the year of the quarterly volatility of the three-day market reaction to earnings announcements. All conditioning variables are measured as differences-in-differences: the difference between the year after versus the year before and the difference between treatment firms versus control firms. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Diff-in-diffs-in-diffs		Big change in bid-ask spread (N=601) vs. small (N=602)	Big change in Amihud liquidity measure (N=648) vs. small (N=649)	Big change in returns ratio (N=652) vs. small (N=657)	Big change in earnings announcement surprise (N=607) vs. small (N=607)	Big change in earnings announcement volatility (N=633) vs. small (N=634)
Investment						
Capital expenditures	Mean:	-0.57%	-1.83%***	-0.76%**	-0.14%	-0.52%
	t-stat:	(-1.41)	(-4.54)	(-2.01)	(-0.36)	(-1.35)
Research and development expenditures	Mean:	-0.12%	-0.25%	-0.19%	0.49%	0.03%
	t-stat:	(-0.36)	(-0.78)	(-0.63)	(1.56)	(0.11)
Acquisitions expenditures	Mean:	-1.24%*	-1.52%**	-1.46%**	-0.55%	-1.80%**
	t-stat:	(-1.65)	(-1.99)	(-1.99)	(-0.71)	(-2.49)
Total investment	Mean:	-2.37%**	-3.50%***	-2.54%***	0.07%	-2.58%***
	t-stat:	(-2.31)	(-3.45)	(-2.61)	(0.07)	(-2.63)
Financing						
Change in short-term debt	Mean:	0.40%	0.26%	-0.19%	0.35%	-0.08%
	t-stat:	(1.32)	(0.95)	(-0.65)	(1.22)	(-0.27)
Change in long-term debt	Mean:	-2.67%**	-2.54%**	-3.36%***	-0.95%	-1.74%*
	t-stat:	(-2.55)	(-2.46)	(-3.49)	(-0.93)	(-1.70)
Equity issuance	Mean:	-4.66%***	-5.57%***	-4.34%***	-4.86%***	-0.88%
	t-stat:	(-4.03)	(-5.01)	(-4.33)	(-4.56)	(-0.84)
Total financing	Mean:	-7.23%***	-8.22%***	-7.92%***	-5.64%***	-2.77%*
	t-stat:	(-4.67)	(-5.46)	(-5.65)	(-3.76)	(-1.90)
Payouts						
Dividends	Mean:	-0.08%	-0.06%	-0.06%	-0.09%	-0.04%
	t-stat:	(-1.40)	(-1.05)	(-1.13)	(-1.50)	(-0.61)
Share repurchases	Mean:	-0.85%**	-0.30%	-0.46%	-0.74%*	0.37%
	t-stat:	(-2.12)	(-0.73)	(-1.11)	(-1.72)	(0.86)
Total payouts	Mean:	-0.95%**	-0.35%	-0.45%	-0.73%	0.13%
	t-stat:	(-2.22)	(-0.81)	(-1.01)	(-1.60)	(0.28)
Change in cash holdings	Mean:	-5.08%***	-3.42%***	-3.55%***	-2.37%*	1.56%
	t-stat:	(-4.11)	(-2.69)	(-3.14)	(-1.87)	(1.25)

Table 5
The Effect of a Decrease in Analyst Coverage on Corporate Policies Conditional Upon Financial Constraints

This table presents the change in corporate policies caused by the loss of an analyst conditional upon financial constraints. The sample comprises 1,961 treatment firms that lose an analyst between 1994 and 2008 because of broker closures and broker mergers and the same number of control firms matched by industry, size, book-to-market, momentum, and analyst coverage. Both groups of firms are publicly traded U.S. operating firms, are not financials or utilities, and have been traded for at least one year. For each corporate policy variable, a mean difference-in-differences-in-differences is computed: the mean difference between the year after the decrease in analyst coverage and the year before; the mean difference between treatment firms and control firms; and the mean difference between firms that are financial constrained and firms that are not financially constrained. All corporate policy variables are scaled by total assets. For the composite proxy for financial constraints from Almeida, Campello, and Weisbach (2004), firms are first classified as constrained or unconstrained based on four proxies: total payout ratio, total assets, bond rating status, and commercial paper rating status. Firms in the bottom three deciles of the total payout ratio (the ratio of dividends plus share repurchases to operating income) are classified as constrained and firms in the top three deciles as unconstrained. Firms in the bottom three deciles of total assets are classified as constrained and firms in the top three deciles as unconstrained. Firms that have long-term debt but no bond rating are classified as constrained, and otherwise they are classified as unconstrained. Firms that have short-term debt but no commercial paper rating are classified as constrained, and otherwise they are classified as unconstrained. Firms that are constrained based on all four of these proxies are then classified as constrained, and firms that are unconstrained based on all four proxies are classified as unconstrained. For the proxy for financial constraints from Rajan and Zingales (1998), firms in the bottom half of the cash flow-investment gap (cash flow minus investment all divided by total assets) are classified as constrained, and otherwise they are classified as unconstrained. All conditioning variables are measured using only treatment firms, and they are measured during the year before the decrease in analyst coverage. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Constrained (N=207) versus unconstrained (N=638) based on the composite proxy from Almeida, Campello, and Weisbach (2004)		Constrained (N=978) versus unconstrained (N=978) based on the proxy from Rajan and Zingales (1998)	
	Mean of diff-in-diffs-in-diffs	t-statistic for diff-in-diffs-in-diffs	Mean of diff-in-diffs-in-diffs	t-statistic for diff-in-diffs-in-diffs
Investment				
Capital expenditures	-1.10%**	-2.31	-1.35%***	-4.56
Research and development exp.'s	-1.80%***	-3.69	-0.91%***	-3.74
Acquisitions expenditures	-1.42%	-1.54	-3.66%***	-6.37
Total investment	-4.62%***	-3.54	-5.99%***	-7.86
Financing				
Change in short-term debt	0.44%	1.12	-0.24%	-1.03
Change in long-term debt	-2.40%*	-1.92	-3.83%***	-4.98
Equity issuance	-9.72%***	-6.10	-2.44%***	-3.14
Total financing	-11.62%***	-5.83	-6.56%***	-5.99
Payouts				
Dividends	0.04%	0.58	-0.05%	-1.00
Share repurchases	-0.16%	-0.36	0.31%	0.86
Total payouts	-0.02%	-0.04	0.34%	0.89
Change in cash holdings	-4.97%***	-2.94	0.17%	0.18

Table 6
Broker and Analyst Quality

This table presents results on the quality of brokers and analysts. Panel A presents the relative earnings estimate accuracy of analysts and brokers that disappear. Panel B presents analysts' expectations for analysts that disappear compared to other analysts. The sample comprises 1,961 treatment firms that lose an analyst between 1994 and 2008 because of broker closures and broker mergers and the same number of control firms matched by industry, size, book-to-market, momentum, and analyst coverage. Both groups of firms are publicly traded U.S. operating firms, are not financials or utilities, and have been traded for at least one year. Relative earnings estimate accuracy is constructed by first computing the accuracy rank – on a scale of zero to one – across all brokers that cover a firm and then computing the mean accuracy rank – also on a scale of zero to one – of a broker across all firms that the broker covers. Analysts' expectations comprise the following: earnings estimates for the next fiscal year measured as a percent of the stock price; investment recommendations measured on a five-point scale a higher value of which means more a favorable recommendation; long-term earnings growth rate estimates for the next five years; and price targets for the next year measured as the natural logarithm of the price target as a percent of the stock price. Expectations of other analysts are computed as the mean of their expectations. They are only computed for other analysts if they are available for the analyst that disappears. All variables are measured during the year ending three months before the broker disappearance date.

Panel A: Relative Earnings Estimate Accuracy of Brokers and Analysts that Disappear

	Number of observations	Mean	Median	Mean weighted by number of firms covered
Brokers	71	0.528	0.545	0.529
Analysts	430	0.509	0.521	0.517

Panel B: Analysts' Expectations for Treatment Firms for Analysts that Disappear Compared to Other Analysts

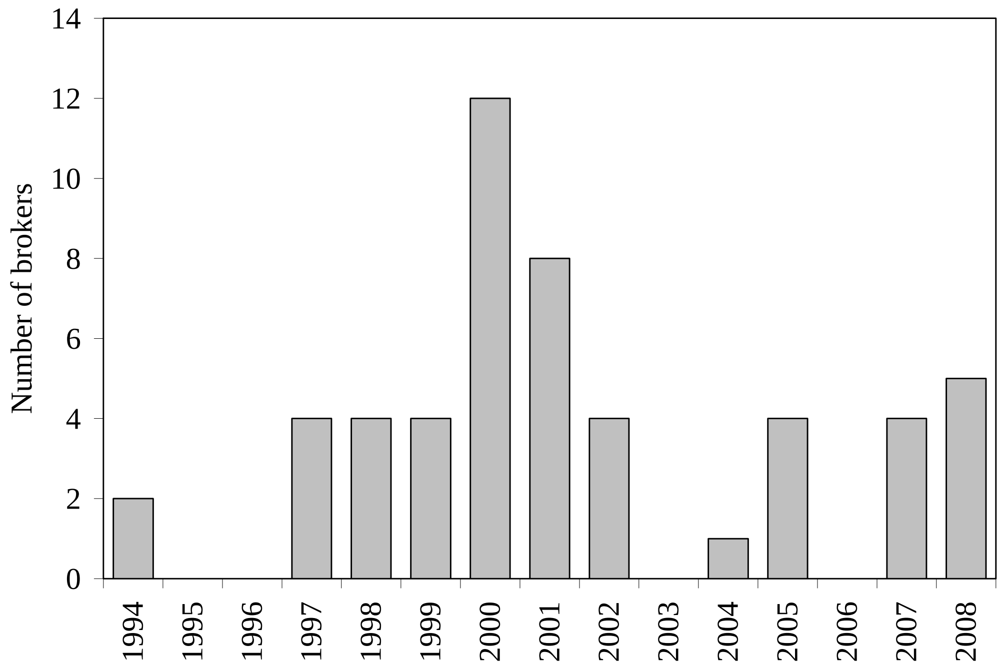
		Mean		
	Number of observations	Analysts that disappear	Other analysts	p-value of test of equality
Earnings estimates	1,821	2.9%	2.9%	0.961
Investment recommendations	1,102	3.7	3.8	0.000
Long-term earnings growth rate est.'s	807	18.7%	19.3%	0.238
Price targets	862	32.4%	34.7%	0.271
		Median		
	Number of observations	Analysts that disappear	Other analysts	p-value of test of equality
Earnings estimates	1,821	4.4%	4.5%	0.507
Investment recommendations	1,102	4.0	3.8	0.000
Long-term earnings growth rate est.'s	807	15.0%	16.7%	0.007
Price targets	862	22.8%	23.4%	0.792

Table 7
Underwriting Relationships

This table presents the change in corporate policies caused by the loss of an analyst conditional upon underwriting relationships. The sample comprises 1,961 treatment firms that lose an analyst between 1994 and 2008 because of broker closures and broker mergers and the same number of control firms matched by industry, size, book-to-market, momentum, and analyst coverage. Both groups of firms are publicly traded U.S. operating firms, are not financials or utilities, and have been traded for at least one year. For each corporate policy variable, the mean difference-in-differences is computed conditional upon the broker not being an underwriter for the firm. Additionally, a mean difference-in-differences-in-differences is computed: the mean difference between the year after the decrease in analyst coverage and the year before; the mean difference between treatment firms and control firms; and the mean difference between firms with underwriting relationships and firms without underwriting relationships. All corporate policy variables are scaled by total assets. A broker is an underwriter for the firm if it underwrites an offering during the three years ending three months before the broker disappearance date. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Difference-in-Differences Conditional Upon Whether Broker that Disappears is Debt or Equity Underwriter for the Firm						
	Broker is not underwriter (N=1,770)		Broker is debt underwriter (N=132) vs. not underwriter (N=1,770)		Broker is equity underwriter (N=67) vs. not underwriter (N=1,770)	
	Mean of diff-in-diffs	t-statistic for diff-in-diffs	Mean of diff-in-diffs-in-diffs	t-statistic for diff-in-diffs-in-diffs	Mean of diff-in-diffs-in-diffs	t-statistic for diff-in-diffs-in-diffs
Investment						
Capital expenditures	-0.76%***	-4.94	0.56%*	1.77	0.87%**	2.54
Research and development expenditures	-0.63%***	-5.01	0.08%	0.36	-0.46%	-1.31
Acquisitions expenditures	-0.82%***	-2.69	-0.10%	-0.19	-3.06%***	-4.49
Total investment	-2.28%***	-5.68	0.65%	0.87	-2.86%***	-2.89
Financing						
Change in short-term debt	-0.01%	-0.06	0.83%***	3.15	0.58%***	2.63
Change in long-term debt	-1.22%***	-3.00	-4.14%***	-5.51	-5.00%***	-5.86
Equity issuance	-0.95%**	-2.40	3.14%***	5.14	-9.97%***	-7.97
Total financing	-2.12%***	-3.70	0.05%	0.05	-14.92%***	-10.33
Payouts						
Dividends	0.01%	0.23	0.01%	0.23	0.07%*	1.65
Share repurchases	-0.24%	-1.29	0.88%**	2.56	0.25%	0.80
Total payouts	-0.26%	-1.31	0.69%*	1.88	0.78%**	2.40
Change in cash holdings	-0.76%	-1.61	0.57%	0.77	-9.59%***	-7.28

Panel A: The distribution in calendar time of brokers that disappear



Panel B: The distribution in calendar time of firms that lose an analyst

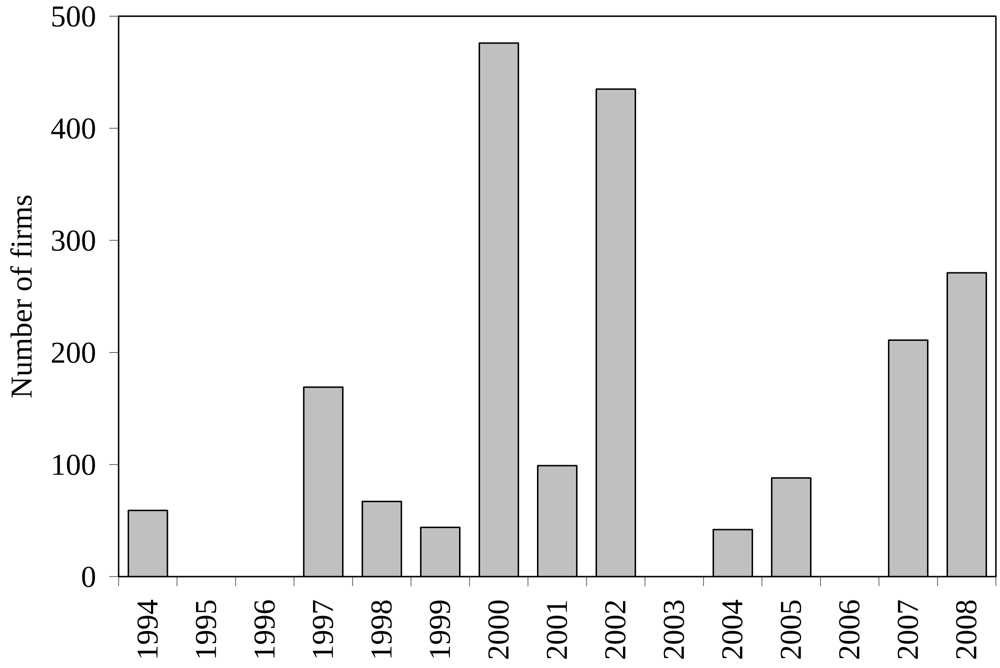


Figure 1. Distribution in calendar time of brokers that disappear and firms that lose an analyst. This figure presents the distribution of brokers and firms in the sample in calendar time. The sample comprises 1,961 treatment firms that lose an analyst between 1994 and 2008 because of broker closures and broker mergers and the same number of control firms matched by industry, size, book-to-market, momentum, and analyst coverage. Both groups of firms are publicly traded U.S. operating firms, are not financials or utilities, and have been traded for at least one year.

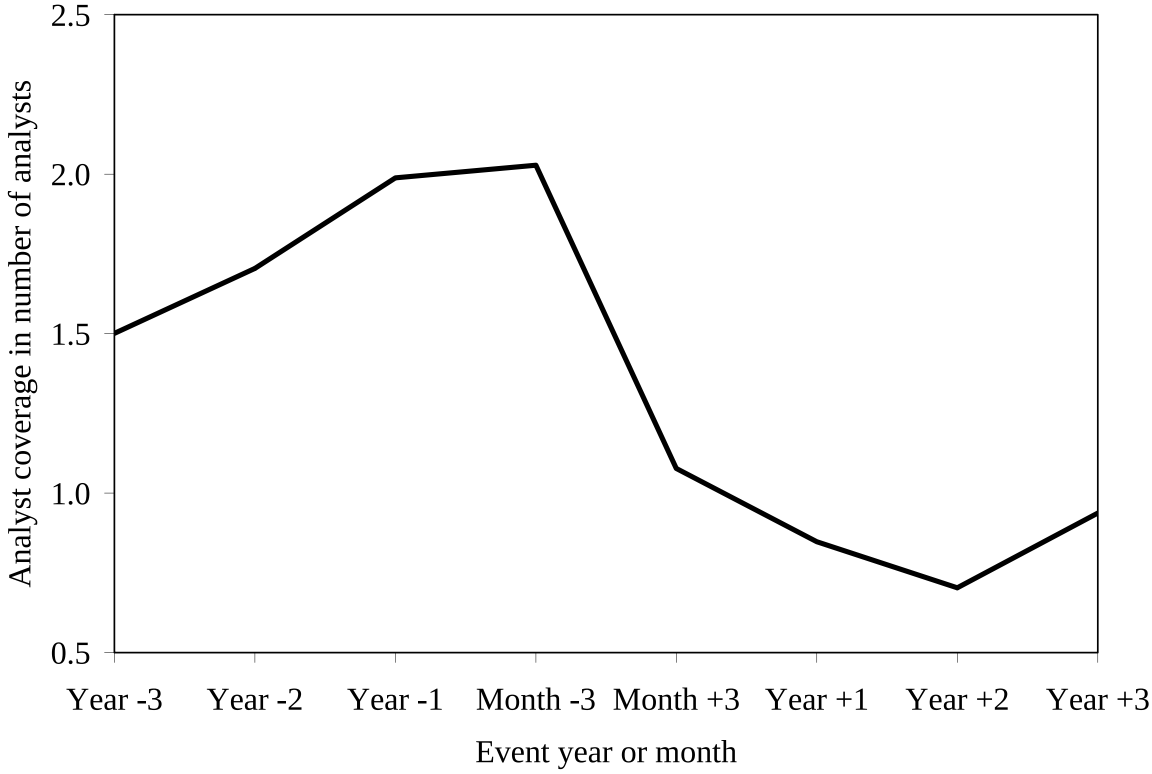


Figure 2. Mean difference between treatment firms and control firms in analyst coverage in event time. This figure presents the difference in analyst coverage between treatment firms and control firms during the three years before and the three years after the decrease in analyst coverage. The sample comprises 1,961 treatment firms that lose an analyst between 1994 and 2008 because of broker closures and broker mergers and the same number of control firms matched by industry, size, book-to-market, momentum, and analyst coverage. Both groups of firms are publicly traded U.S. operating firms, are not financials or utilities, and have been traded for at least one year.

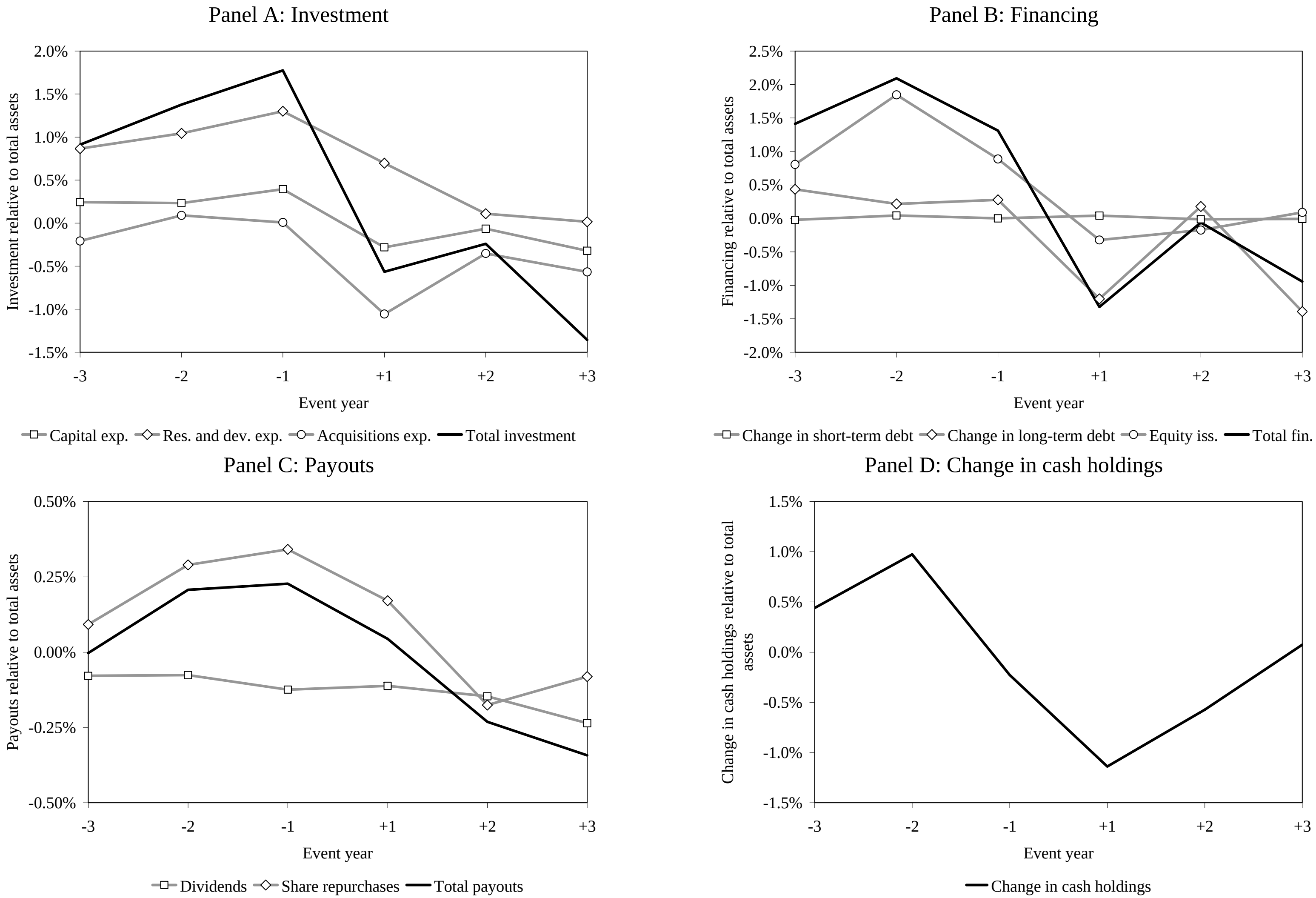


Figure 3. Mean difference between treatment firms and control firms in corporate policies in event time. This figure presents the difference in corporate policy variables between treatment firms and control firms during the three years before and the three years after the decrease in analyst coverage. The sample comprises 1,961 treatment firms that lose an analyst between 1994 and 2008 because of broker closures and broker mergers and the same number of control firms matched by industry, size, book-to-market, momentum, and analyst coverage. Both groups of firms are publicly traded U.S. operating firms, are not financials or utilities, and have been traded for at least one year. All corporate policy variables are scaled by total assets.

Appendix 1
Details of Corporate Policy Variables

Corporate policy variables	Compustat variables (scaled by total assets)
Investment	
Capital expenditures	CAPX
Research and development expenditures	XRD
Acquisitions expenditures	AQC
Total investment	CAPX+XRD+AQC
Financing	
Change in short-term debt	DLCCH
Change in long-term debt	DLTIS-DLTR
Equity issuance	SSTK
Total financing	DLTIS-DLTR+DLCCH+SSTK
Payouts	
Dividends	DV
Share repurchases	PRSTKC
Total payouts	DV+PRSTKC
Change in cash holdings	CHECH