The Real Effects of 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 2.35% and 2.62% of total assets, respectively. These results are significantly stronger when the decrease in analyst coverage is more costly – for smaller firms, for firms with less analyst coverage, and for firms that lose a more influential analyst – as well as for firms that are more financially constrained.

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1. Introduction

A long line of literature finds that equity research analysts produce information that matters to investors and firms. The existing research focuses on the financial effects of analysts,[1] and there is a dearth of direct evidence on the real effects of analysts. This paper focuses on how analyst coverage affects firms' investment and financing policies. An almost insurmountable impediment to this research is the problem of causality: does analyst coverage cause corporate policies or vice versa? In this paper, we overcome this obstacle using two natural experiments to identify changes in analyst coverage that are exogenous to corporate policies.

We hypothesize that a decrease in analyst coverage increases information asymmetry and thus increases the cost of capital.[2] 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 (2010). They provide empirical evidence that exogenous decreases in analyst coverage (from broker closures) cause an increase in information asymmetry (see also Brennan and

[1] 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 Givoly and Lakonishok (1979) 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)), and they increase the investor recognition of these firms (see Merton (1987)). Analysts sometimes issue biased analyst reports to investors (e.g., see Lin and McNichols (1998) and Michaely and Womack (1999)). However, analysts are generally incentivized to produce information that is valuable to investors (e.g., see Hong and Kubik (2003) and Mikhail, Walther, and Willis (1999)).

[2] 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), Korajczyk, Lucas, and McDonald (1991), Korajczyk, Lucas, and McDonald (1992), 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 (e.g., Merton (1987)) or systematic risk (e.g., Easley and O'Hara (2004)).

Subrahmanyam (1995) and Ellul and Panayides (2009)) as well as the cost of capital.[3] In this 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.

We examine the effect of analyst coverage on corporate policies using two natural experiments: broker closures like Kelly and Ljungqvist (2010), 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 identify such decreases in analyst coverage using broker disappearances in I/B/E/S. For broker mergers, we also require that both the target broker and the acquirer broker cover the firm before the merger and that exactly one of their analysts disappear. We use broker closures and broker mergers because the resulting decrease in analyst coverage is exogenous to corporate policies. We provide empirical support for our claim in our robustness tests.

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 compare the changes in corporate policies of these 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. Moreover, consistent with Kelly and Ljungqvist (2010), we find that around the decrease in analyst coverage stock prices decrease significantly more for

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

our treatment firms than our control firms (e.g., 1.00 percentage points and 2.87 p.p. more during the two-month and six-month event windows, respectively).

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 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 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. We also find that the decrease in analyst coverage causes firms to switch to financing that is less sensitive to information asymmetry: they 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 that lose a more influential analyst. For example, total investment and total financing decrease by 4.88% and 9.51%, respectively, for firms in the smallest quintile of market capitalization, whereas the corresponding effects are economically small 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 classify our firms as financially constrained or unconstrained, and we test whether our results are stronger for firms that are more financially constrained. To this end, we use several proxies: the total payout ratio, bond rating status, and the cash flow-investment gap. We find that the real effects of analyst coverage are indeed bigger for firms that are more financially constrained.

We perform numerous robustness tests of our results. Perhaps the most important of these examines the exogeneity of broker closures and broker mergers to corporate policies. We examine analysts' expectations as well as realized future returns to test whether the brokers and analysts that disappear cover the wrong firms and/or their expectations diverge from the consensus. To this end, we compare consensus expectations for treatment firms and control firms as well as the expectations of analysts that disappear and the consensus expectations of those that do not. We also compare realized future returns for treatment firms and control firms. The evidence suggests that decreases in analyst coverage caused by broker disappearances are indeed exogenous to corporate policies. 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.

As we later argue in greater detail, the evidence suggests that the broker closures and brokers mergers in our sample do not involve research departments that produce low quality research. Our broker terminations appear to be motivated by business strategy considerations of the broker. For example, before Prudential Financial Inc. closed its research department because its other businesses were uncompetitive, it was one of the leading brokers in research according to Institutional Investor magazine (ranking just after Goldman Sachs and just before Deutsche Bank). Similarly, before their merger, both Credit Suisse First Boston and Donaldson, Lufkin & Jenrette were among the leading brokers (ranking #4 and #5, respectively).

The evidence also suggests that our analysts that disappear do not produce low quality research. We measure the quality of research produced by an analyst using earnings estimate accuracy; this standard measure is shown in the literature to explain analyst promotions and demotions (e.g., Mikhail, Walther, and Willis (1999), Hong and Kubik (2003), and Wu and Zang (2009)). We find that the earnings estimate accuracy of analysts that disappear is in fact slightly above average, and very few of our analysts are in the very low accuracy group that the literature shows are the analysts that are not promoted or are demoted. In summary, the evidence suggests that the brokers and analysts that disappear do not produce low quality research.

We contribute to the literature on analyst coverage and corporate policies specifically as well as the literature on information asymmetry and corporate policies generally. To our knowledge, ours is the first paper to show that changes in analyst coverage cause changes in corporate policies. Ours is also one of the few papers that study analyst coverage and corporate policies comprehensively.[4] Finally, recent research has challenged the received wisdom that analysts affect stock prices. Altınkılıç and Hansen (2009) and Altınkılıç, Balashov, and Hansen (2010), respectively, find that analysts' recommendation changes and earnings estimate changes

[4] 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.

have no price impact once other contemporaneous news is taken into account. We show that analysts are important information producers: the information that they produce has economically significant real effects.

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.[5] Our list of 52 broker

[5] 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 (2010) 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 (2010), 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 exactly 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.[6] 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.[7] 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 (2010) 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, and accounting data are from Compustat. 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

[6] 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. For remaining ties, we repeat this procedure for total assets.

[7] 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.

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.

A valid instrument must meet two conditions: relevance and exogeneity. First, 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 1.22 analysts and by 0.27 analysts for our control firms, so analyst coverage of our treatment firms decreases by 0.95 analysts more than for 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 six months centered on the broker disappearance month, the mean and median difference in stock returns between our treatment firms and control firms is -2.87 p.p. and -0.95 p.p., respectively (with a t-statistic and a z-statistic of -3.24 and -1.94, respectively). Our stock price decreases are somewhat bigger than those of Kelly and Ljungqvist (2010), but we also use a much wider window (six months versus three days) because our broker disappearance dates are less accurate than theirs. (During the two months centered on the broker disappearance month, our results are similar to theirs: our mean and median difference in returns is -1.00 p.p. and -0.51 p.p., respectively.) Second, although the exogeneity condition is inherently untestable, we summarize the evidence (based on analysts' expectations and future realized returns) in the section on robustness tests that supports our claim that decreases in analyst coverage caused by broker disappearances are exogenous to corporate policies.

To evaluate how well our control firms match our treatment firms, we compute the 25^{th} percentile, the median, and the 75^{th} percentile of our matching variables as well as total assets and our corporate policy variables for both groups of firms. We also test the equality of the medians as well as the distributions of all of these variables (using the Kolmogorov-Smirnov test for the latter). By construction, all of our control firms have the same two-digit SIC code as our treatment firms, so our treatment firms and control firms are well matched by industry. The other matching variables are market capitalization, book-to-market, momentum, and analyst coverage. We examine corporate policy variables for investment and financing. For investment, we use capital expenditures (Compustat variable CAPX), research and development expenditures (XRD), and acquisitions expenditures (AQC), as well as total investment for which we use the sum of the foregoing three variables (CAPX plus XRD plus AQC). For financing, we use the change in short-term debt (DLCCH), the change in long-term debt (DLTIS minus DLTR), equity issuance (SSTK), and total financing (DLTIS minus DLTR plus DLCCH plus SSTK). For payouts, we use dividends (DV), share repurchases (PRSTKC), and total payouts (DV plus PRSTKC). Finally, we use use the change in cash holdings (CHECH). All corporate policy variables are scaled by total assets. We measure matching variables during the year ending three months before the broker disappearance date; we measure other variables, investment variables, and financing 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 our matching variables but also for total assets and our corporate policy variables. Differences in our 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.

[Insert Figure 3 about here]

We also examine graphically the distribution of our matching variables for our treatment firms and control firms. Figure 3 presents the results. Once again, our treatment firms are very similar to our control firms for market capitalization, book-to-market, and momentum, but our treatment firms are covered by more analysts than our control firms. In the section on robustness tests, we correct for this bias. (We find that our results are similar.)

The main difference in corporate policy variables is that treatment firms have higher median total investment by roughly 1 percentage point mostly because their median research and development expenditures are higher by roughly 0.5 p.p. and their median capital expenditures are higher by roughly 0.25 p.p.. Also, while treatment firms have higher median total financing, the difference is only 0.25 p.p., and the differences in the individual components of financing are not economically or statistically significant. Overall, our treatment firms and control firms are very similar.

3. Main Results

3.1. The Real Effects of Analyst Coverage

In this section, we examine the effect of a decrease in analyst coverage on corporate policies. We begin with a graphical depiction of the evolution of corporate policies during the years before and after the decrease in analyst coverage. Then we formally test whether changes in corporate policies caused by a decrease in analyst coverage are statistically significant. Throughout our analysis, we study the difference between treatment firms and control firms unless otherwise indicated.

[Insert Figure 4 about here]

Figure 4 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 the difference between treatment firms and control firms in investment, financing, payouts, and the change in cash holdings.

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 but financing is less well behaved than investment. Equity issuance increases and decreases between year -3 and year -1 to end roughly unchanged over these three years; between year +1 to year +3, it is roughly horizontal. By contrast, the change in long-term debt is roughly horizontal between year -3 and year -1; it increases and decreases to end roughly unchanged over these three years. The change in short-term debt is insignificant. Since total financing is the sum of equity issuance and net total debt issuance, it exhibits an increase-decrease pattern during both three-year periods. 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 the evolution of payouts. Dividends are roughly horizontal between year -3 and year +3. Share repurchases increase between year -3 and year -1 and then decrease between year -1 and year +1 before increasing again between year +2 and year +3. Total payouts follow the pattern of share repurchases. However, the evolution of payouts overall is insignificant because the maximum time variation in payouts is an order of magnitude smaller than the time variation in investment and financing. Finally, Panel D shows the evolution of the change in cash holdings. The change in cash holdings increases and decreases between year -3 and year -1, it decreases between year -1 and year +1, and then it increases between year +1 and year +3 to end roughly unchanged between year -3 and year +3.

Figure 4 also shows that the changes in corporate policies between year -1 and year +1 are not part of long-term trends in corporate policies but instead are changes that occur only when analyst coverage decreases, i.e., between year -1 and year +1. This result supports the parallel trends assumption underlying our difference-in-differences approach.

Next, we formally test whether changes in corporate policies caused by a decrease in analyst coverage are statistically significant. 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 for this estimate we also compute the t-statistic.

[Insert Table 2 about here]

Table 2 presents the results. (Totals do not always 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.

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.

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 of 0.71%, 0.64%, and 0.94%, respectively, of total assets correspond to a mean decrease of $12.9 million, $11.6 million, and $17.0 million, respectively. In other words, for the typical firm, total investment decreases by $41.5 million. For financing, the mean decreases in net total debt issuance and equity issuance of 1.55% and 1.12%, respectively, of total assets correspond to a mean decrease of $28.1 million and $20.3 million, respectively. In other words, for the typical firm, total financing decrease by $48.4 million.

We also test the parallel trends assumption that the corporate policies of our treatment firms and control firms only diverge from each other when analyst coverage decreases for our treatment firms, i.e., between year -1 and year +1. 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 and between year +1 and year +3. We find (results not tabulated) that only three differences are statistically significant at the 5% level: research and development expenditures and total investment between year -3 and year -1, and dividends between year +1 and year +3. Even in these three cases (out of twenty-four), the differences are small in economic magnitude compared to the differences that we observe between year -1 and year +1, which is consistent with the patterns in Figure 4. We conclude that most of the divergence between the corporate policies of our treatment firms and control firms occurs when analyst coverage decreases for our treatment firms between year -1 and year +1.

We hypothesize that both the decrease in investment and financing are caused by the increase in information asymmetry that results from the decrease in analyst coverage. 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 sort the difference-in-differences in total financing into quintiles, and we compute the mean and median difference-in-differences in investment for each quintile.

[Insert Figure 5 about here]

Figure 5 presents the results. The change in investment is monotonically increasing in the change in financing: firms in the bottom quintile of difference-in-differences in financing decrease investment by a mean and median of 13.9% and 10.3%, respectively, whereas firms in the top quintile of difference-in-differences in financing increase investment by a mean and median of 6.8% and 4.0%, respectively. The results suggest that the firms that decrease investment the most are indeed the firms that decrease financing the most.

We examine whether the loss of an analyst causes firms to use 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 1.12% of total assets, and net debt issuance decreases by 1.55%, but this decrease in net debt issuance is driven by the decrease in higher risk long-term debt issuance, which drops by 1.62% of total assets, while lower risk short-term debt issuance is virtually unchanged. Finally, firms increase their use of cash by 1.04% of total assets.

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, for firms with less analyst coverage, and for firms that lose a more influential analyst. 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.

We compute the mean difference-in-differences for our corporate policy variables for each quintile of our three conditioning variables (size, analyst coverage, and analyst influence). Specifically, we sort our treatment firms into quintiles based on market capitalization, analyst coverage, and analyst influence. We measure market capitalization and analyst coverage using only treatment firms, and we measure them during the year before the decrease in analyst coverage. To proxy for the influence of the analyst that disappears, we use the market reaction to the loss of an analyst: the market reaction should be more negative if the analyst that disappears is more influential to the firm. We measure the market reaction as the treatment firm's return minus the control firm's returns both during the six months centered on the broker disappearance date.[8]

[Insert Table 3 about here]

[8] Another natural proxy for analyst influence is whether the analyst that disappears is a star analyst according to the annual Institutional Investor magazine ranking of analysts. If star analysts produce more information, then the disappearance of a star analyst causes a bigger increase in information asymmetry than the disappearance of a non-star analyst. Unfortunately, almost none of our observations are associated with terminated analysts that are stars. While it is not surprising that other brokers inevitably hire the star analysts of brokers that disappear, it makes it difficult for us to condition upon star analyst status.

Table 3 presents the results. Panel A shows that total investment and total financing decrease by 4.88% and 9.51%, respectively, in the bottom quintile of market capitalization whereas the corresponding figures are economically small in the top quintile. Similarly, the decrease in the change in cash holdings is bigger in the bottom quintile of market capitalization – 3.08% – than in the top quintile. Panel B shows that the results for analyst coverage are also significantly bigger in the bottom quintile than in the top quintile. Panel C shows that the results are similar for analyst influence. Overall, the results suggest that a decrease in analyst coverage affects corporate policies mostly for smaller firms, firms with less analyst coverage, and firms that lose a more influential analyst.

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

In this section, 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 total payout ratio (e.g., Fazzari, Hubbard, and Petersen (1988)) and bond rating status (e.g., Whited (1992), Kashyap, Lamont, and Stein (1994), and Gilchrist and Himmelberg (1995)).[9,10] We compute these variables and classify firms as financially constrained or not like Almeida, Campello, and Weisbach (2004) and Denis and Sibilkov (2010). Specifically, we classify firms in the bottom three deciles of the total payout ratio (the ratio of dividends plus share repurchases to operating income) as constrained and firms in the top three deciles as unconstrained. We classify firms that have long-term debt but no bond rating as constrained, and otherwise we classify them as unconstrained.

We also use another proxy for financial constraints: the cash flow-investment gap defined as cash flow minus investment. At one extreme, a firm with less cash flow than investment (i.e., a negative gap) must finance its investment externally. At the other extreme, a firm with more cash flow than investment (i.e., a positive gap) can finance its investment internally. The negative gap firm is more sensitive to the cost of capital than the positive gap firm. Therefore, in response to an increase in the cost of capital caused by a decrease in analyst coverage, the negative gap firm should decrease investment and financing by more than the positive gap firm. We compute the cash flow-investment gap 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

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

[10] A third standard proxy for financial constraints is total assets (e.g., Gilchrist and Himmelberg (1995) and Hadlock and Pierce (2010)). Since Table 3 shows that our results are stronger for firms with lower market capitalization, we do not repeat this analysis for size in this section.

are not significantly different.) As in Table 2, we compute the mean difference-in-differences change for each of our corporate policy variables (i.e., the difference between the year after versus the year before and the difference between our treatment firms versus our control firms).

[Insert Table 4 about here]

Table 4 presents the results. The effect of a loss of analyst on corporate policies is bigger for firms that are financially constrained. This is the case whether we proxy for financial constraints using the total payout ratio (Panel A), bond rating status[11] (Panel B), or the cash flow-investment gap (Panel C). By way of example, using the total payout ratio, total investment and total financing decrease by 2.43 p.p. and 5.07 p.p. more for firms that are financially constrained than for firms that are not financially constrained. Similarly, financially constrained firms use up 2.47 p.p. more of their cash holdings than financially unconstrained firms. Overall, the results suggest that the real effects of analyst coverage are indeed bigger for financially constrained firms.

4. Robustness Tests

4.1. The Exogeneity of Analyst Terminations

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). We can think of two channels through which these alternative interpretations can operate: the

[11] We also use commercial paper rating status as a proxy for financial constraints (e.g., Calomiris, Himmelberg, and Wachtel (1995)). Following Almeida, Campello, and Weisbach (2004) and Denis and Sibilkov (2010), we classify firms that have short-term debt but no commercial paper rating as constrained, and otherwise we classify them as unconstrained. We find that the results are similar (not tabulated).

brokers and analysts that disappear cover the wrong firms and/or their expectations diverge from the consensus. We explain each of these alternative interpretations and test them empirically.

We begin with the first channel: the brokers and analysts that disappear cover the wrong firms. These brokers and their analysts choose to cover firms for which investment and financing is expected to decrease, so the profitability of providing research for these firms decreases, 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 note that we match our treatment firms to similar control firms and show that our corporate policy variables are similar the year before the decrease in analyst coverage for both groups of firms. We also note that our matching variables include scaled price variables (market capitalization, book-to-market, and momentum) that presumably capture the market's expectations about corporate policies. However, it is possible that our treatment firms and control firms differ based on characteristics not captured by our matching variables but that affect future corporate policies. We test this prediction by comparing analysts' expectations as well as realized future returns for our treatment firms and control 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 measure all analysts' expectations variables during the year ending three months before the broker disappearance date. We note that data for analysts' expectations variables are not available for all firms and all analysts.[12]

[Insert Table 5 about here]

Panel A of Table 5 presents the results.[13] Mean and median earnings estimates for the next fiscal year are significantly more pessimistic for treatment firms than for control firms (2.9% of the stock price versus 4.1% using the mean, and 4.6% versus 4.9% using the median). Mean investment recommendations are the same for both groups as are median recommendations (3.8 on a five-point scale). Long-term earnings growth rate estimates for the next five years are actually more optimistic for treatment firms than for control firms both using the mean (19.1% versus 18.6%) and the median (16.3% versus 15.8%), although the differences are not statistically significant. Price targets for the next year are significantly more optimistic for treatment firms than for control firms both using the mean (37.2% of the stock price versus 31.2%) and the median (24.5% versus 22.0%). Overall, we conclude that analysts' expectations are similar for our treatment firms and control firms.

We now turn from expectations before the decrease in analyst coverage to realizations thereafter. To this end, we 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

[12] When we compare treatment firms to control firms, data are generally available for all variables except for price targets; price targets data begin in July 1999 and thus are only available for roughly 70% of our observations. When we compare analysts that disappear to other analysts, 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.

[13] We also redo the results for the previous two years, and we find that the results are similar for all three years before the decrease in analyst coverage. In light of our finding in Table 3 that our results are stronger for smaller firms, we also redo the results in Table 5 for firms in the bottom quintile of market capitalization, and we again find that they are similar.

treatment firms and 1.41%, 0.82%, and 0.88%, respectively, for control firms. In other words, our treatment firms have similar realized future returns to our control firms. (The slightly better performance of treatment firms compared to control firms is consistent with a slight increase in the cost of capital caused by the loss of an analyst.)

We now turn to the second channel: the expectations of brokers and analysts that disappear diverge from the consensus. It does not matter whether the affected brokers and analysts correctly expect the investment and financing of the firms that they cover to decrease or whether they incorrectly fail to expect it: what matters is that their expectations differ from the expectations of their competitors. Therefore, the brokers' client firms and/or client investors take their business elsewhere, the profitability of providing research decreases, and the brokers and analysts disappear. We test this prediction by comparing the expectations of analysts that cover treatment firms and disappear and the consensus expectations of all other analysts that cover treatment firms. We redo Panel A of Table 5 for both groups.

Panel B of Table 5 presents the results. 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 consensus expectations of all other analysts that cover treatment firms are similar.

In summary, we find no evidence that the brokers and analysts that disappear cover the wrong firms and/or their expectations diverge from the consensus. This reinforces our claim that the disappearance of brokers and analysts is not caused by changes in corporate policies or something correlated with them.

4.2. Robustness Tests of the Main Results

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 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.

[Insert Table 6 about here]

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.[14] We redo Table 2 for this sample. Panel A of Table 6 presents the results, which 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 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. Panel B of Table 6 presents the results, which are again similar to the results in Table 2.[15]

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

[14] 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).
[15] The results for the mean treatment difference are not identical in Table 2 and Panel B of Table 6 because the two samples are slightly different: in Panel B of Table 6, we include the 5% of the sample with the biggest differences in market capitalization, book-to-market, momentum, and analyst coverage that we exclude in Table 2.

between the "treatment firm" dummy variable and the "after" dummy variable.[16] 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. Panel C of Table 6 presents the results for the difference-in-differences estimates (the other estimates are not tabulated). 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

[16] The results of this baseline specification should be the same as the results in Table 2. Discrepancies arise because in Table 2 we winsorize the difference-in-differences sample (treatment firms after minus before all minus control firms after minus before) whereas in the pooled regressions we winsorize the pooled sample (treatment firms before and after and control firms before and after).

results 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.[17] Overall, our results are not driven by either broker closures or broker mergers alone.

4.3. The Quality of Research Produced By the Brokers and Analysts That Disappear

For the coverage of the brokers and analysts in our sample to affect corporate policies, it must be the case these brokers and analysts do not produce low quality research. If they did, then their disappearance would not decrease information asymmetry, thus it would not decrease the cost of capital and thereby affect corporate policies. That we find that these brokers and analysts do have significant real effects implies 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. In fact, the brokers involved are often ranked among the leaders in research according to Institutional Investor magazine.

We illustrate our point with several examples of some of the broker disappearances in our sample each of which causes a large number of firms to lose analyst coverage. We begin with broker closures. Before Prudential closed its research department in June 2007, it was ranked #12

[17] 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.

between Goldman Sachs (#11) and Deutsche Bank (#13). However, its other businesses were uncompetitive; investment banking was closed in 2000, retail brokerage was sold in 2003, and when institutional trading was closed in 2007, there was nothing left to support research. Robertson Stephens, another broker in our sample, was a boutique investment bank that focused on technology firms and was one of the leading investment banks in its field. However, after the technology bust, its parent, FleetBoston Financial, closed it in July 2002. This closure of research was therefore motivated by the deterioration of investment banking even though Robertson Stephens produced some of the highest ranked technology research during the technology boom.

Turning to broker mergers, often both the target broker and the acquirer broker are leading brokers in research and the motivation for the merger is totally unrelated to the quality of research. In December 1994, PaineWebber, ranked #8 (just after Credit Suisse First Boston), acquired Kidder, Peabody, ranked #12 (just after Sanford C. Bernstein), which was suffering the revelation of fraud by one of its bond traders. In May 1997, Morgan Stanley, ranked #2, acquired Dean Witter Reynolds, ranked #19 in a merger that sought to combine the more volatile cash flows of an investment bank with the more stable cash flows of a retail brokerage. This merger was also part of the wave of consolidation in the banking industry that also included the next three mergers. In November 1997, Travelers Group, parent of Smith Barney, ranked #5, acquired Salomon Brothers Inc., ranked #6, to combine the retail expertise of the former with the institutional expertise of the latter. Deutsche Bank, ranked #18, acquired Bankers Trust, parent of Alex Brown, ranked #14, to achieve greater scale in investment banking; the following year, it moved up to #13 in the rankings (between Prudential at #12 and Banc of America at #14). The November 2000 acquisition by Credit Suisse First Boston, ranked #4, of Donaldson, Lufkin & Jenrette, ranked #5, aimed to achieve truly massive scale by combining two diversified investment banking giants. Finally, it was the bank run on Bear Stearns, ranked #2, during the credit crisis that led to it being bought at a fire sale price in May 2008 by JPMorgan, ranked #4. In summary, the brokers that disappear do not appear to produce low quality research.

Second, we examine the quality of the research produced by our analysts. We measure the quality of research using earnings estimate accuracy; this standard measure 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)). We use the relative earnings estimate accuracy measure. This standard measure in the literature is constructed by first computing the accuracy rank – on a scale of zero to one – across all analysts that cover a firm and then computing the mean accuracy rank – also on a scale of zero to one – of an analyst across all firms that the analyst covers. The resulting measure captures the mean accuracy of the analyst relative to other analysts who cover the same firms that this analyst covers.

We find that the earnings estimate accuracy of analysts that disappear is in fact slightly above average: the mean and median accuracy of our analysts are 0.51 and 0.52, respectively, whereas the mean and median accuracy of all analysts is 0.50 by construction. 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 (effects in the second quartile are at only marginally significant). Similarly, Wu and Zang (2009) find that turnover is concentrated in the bottom quintile and top quintile of accuracy: there is a slight negative trend in the middle three quintiles, but comparatively little variation in the turnover rate. We find that very few of our analysts fall into the very low accuracy group: only 8% of our analysts have accuracy of less 0.25. In summary, the evidence suggests that the brokers and analysts that disappear do not produce low quality research.

5. Conclusion

In this paper, we study the causal effects of analyst coverage on firms' investment and financing 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.

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. In our empirical analysis, we use two natural experiments to identify changes in analyst coverage that are exogenous to corporate policies: broker closures and broker mergers. For our sample, we use 1,961 firms that lose an analyst because of 52 broker closures and broker mergers between 1994 and 2008. We compare the changes in corporate policies of these treatment firms to those of control firms matched on size, book-to-market, momentum, industry, and analyst coverage.

We find that firms that lose an analyst decrease total investment and total financing by 2.35% and 2.62% of total assets, respectively. Moreover, we examine whether 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 that lose a more influential analyst. We find that this is indeed the case. Furthermore, we examine how the effect of a decrease in analyst coverage depends on financial constraints: analyst coverage should only affect the corporate policies of financially constrained firms. We find that this is in fact case. Finally, we provide empirical support for our claim that our decreases in analyst coverage are exogenous to corporate policies using numerous measures of analysts' expectations as well as future realized returns. Taken as a whole, our results suggest that analysts are important information producers that have economically significant real effects.

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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. Matching 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 dist.
	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
Res. and dev. expenditures	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

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 (yr +1 vs. yr -1)	Mean control difference (yr +1 vs. yr -1)	Mean of diff-in-diffs (treatments vs. 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, Analyst Coverage, and the Market Reaction to the Decrease in Analyst Coverage

This table presents the mean change in corporate policies caused by the loss of an analyst conditional upon market capitalization, analyst coverage, and the market reaction to 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 the difference between treatment firms and control firms. All corporate policy variables are scaled by total assets. The sample is sorted into quintiles based on the value of conditioning variables. Market capitalization and analyst coverage are measured using only treatment firms, and they are measured during the year before the decrease in analyst coverage. The market reaction is computed as the treatment firm's return minus the control firm's return, and it is measured during the six months centered on the broker disappearance date. ***, **, 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	-1.49%***	-0.80%**	-0.64%*	-0.69%***	0.10%
Research and development expenditures	-1.76%***	-0.64%**	-0.49%***	0.09%	-0.39%
Acquisitions expenditures	-1.39%*	-1.42%**	-0.90%	-0.23%	-0.75%
Total investment	-4.88%***	-3.15%***	-1.87%**	-0.91%	-0.93%
Financing					
Change in short-term debt	0.41%	-0.18%	0.18%	-0.12%	0.06%
Change in long-term debt	-3.75%***	-2.78%***	-0.99%	-0.46%	-0.11%
Equity issuance	-6.19%***	0.02%	-0.35%	-0.11%	1.05%**
Total financing	-9.51%***	-2.78%**	-1.13%	-0.61%	0.94%
Payouts					
Dividends	0.05%	0.05%	-0.06%	-0.08%	0.07%
Share repurchases	-0.63%*	-0.43%	0.27%	-0.57%	0.49%
Total payouts	-0.39%	-0.33%	0.07%	-0.76%*	0.46%
Change in cash holdings	-3.08%*	-1.30%	-0.78%	-0.02%	0.00%

Panel B: Mean Difference-in-Differences Conditional upon Analyst Coverage at Year -1					
	Q1 (least)	Q2	Q3	Q4	Q5 (most)
Investment					
Capital expenditures	-1.60%***	0.11%	-0.48%	-0.90%***	-0.55%
Research and development expenditures	-1.45%***	-0.82%***	-0.42%**	-0.13%	-0.21%
Acquisitions expenditures	-1.68%**	-0.55%	-0.95%	-0.33%	-1.12%*
Total investment	-4.93%***	-1.11%	-2.09%***	-1.60%**	-1.67%**
Financing					
Change in short-term debt	0.26%	-0.02%	-0.16%	0.08%	0.15%
Change in long-term debt	-3.68%***	-1.58%*	-1.36%	-0.37%	-0.76%
Equity issuance	-4.46%***	0.16%	-0.77%	-0.18%	0.16%
Total financing	-7.82%***	-1.21%	-2.22%**	-0.63%	-0.43%
Payouts					
Dividends	0.02%	0.08%*	-0.02%	-0.05%	0.00%
Share repurchases	-0.20%	-0.67%*	-0.16%	-0.60%	0.87%*
Total payouts	-0.16%	-0.53%	-0.13%	-0.87%**	0.85%*
Change in cash holdings	-2.64%**	-1.14%	-1.21%	-0.15%	0.25%

Panel C: Mean Difference-in-Differences Conditional upon Market Reaction to the Loss of an Analyst					
	Q1 (smallest)	Q2	Q3	Q4	Q5 (biggest)
Investment					
Capital expenditures	-2.08%***	-0.97%***	-0.52%*	-0.13%	0.19%
Research and development expenditures	-0.73%**	-0.68%***	-0.60%**	-0.19%	-1.01%***
Acquisitions expenditures	-1.92%**	-0.24%	-0.91%	-0.74%	-0.86%
Total investment	-4.91%***	-2.00%**	-1.85%**	-1.18%	-1.78%*
Financing					
Change in short-term debt	0.66%**	-0.14%	0.16%	-0.11%	-0.22%
Change in long-term debt	-4.46%***	-0.61%	-0.99%	-0.67%	-1.34%
Equity issuance	-5.88%***	-1.80%**	0.15%	0.91%	1.06%
Total financing	-9.73%***	-2.62%**	-0.67%	0.25%	-0.27%
Payouts					
Dividends	0.02%	-0.04%	-0.03%	0.02%	0.07%
Share repurchases	-1.26%***	-0.77%*	0.02%	0.72%*	0.42%
Total payouts	-1.23%***	-0.82%*	-0.17%	0.71%*	0.58%
Change in cash holdings	-3.88%***	-0.74%	-0.86%	-0.33%	0.64%

Table 4
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, the mean change from the year before the decrease in analyst coverage to the year after is computed for the difference between treatment firms and control firms. All corporate policy variables are scaled by total assets. 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 that have long-term debt but no bond rating are classified as constrained, and otherwise they are classified as unconstrained. 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.

Panel A: Difference-in-Differences Conditional upon Total Payout Ratio at Year -1				
	Mean for low total payout ratio	Mean for high total payout ratio	Mean for low ratio minus high ratio	t-statistic for low ratio minus high ratio
Investment				
Capital expenditures	-1.30%***	-0.50%***	-0.80%**	-2.30
Research and development expenditures	-1.10%***	-0.58%***	-0.53%	-1.58
Acquisitions expenditures	-1.37%**	-0.58%	-0.78%	-1.04
Total investment	-4.08%***	-1.66%***	-2.43%**	-2.43
Financing				
Change in short-term debt	0.24%	-0.11%	0.35%	1.20
Change in long-term debt	-2.41%***	-1.53%**	-0.88%	-0.90
Equity issuance	-4.15%***	0.52%	-4.66%***	-4.52
Total financing	-6.14%***	-1.07%	-5.07%***	-3.53
Payouts				
Dividends	0.02%	-0.04%	0.06%	1.00
Share repurchases	0.61%***	-2.85%***	3.47%***	7.50
Total payouts	0.69%***	-3.15%***	3.84%***	7.84
Change in cash holdings	-2.38%**	0.08%	-2.47%**	-1.97

Panel B: Difference-in-Differences Conditional upon Bond Rating at Year -1				
	Mean for no bond rating	Mean for bond rating	Mean for no rating minus rating	t-statistic for no rating minus rating
Investment				
Capital expenditures	-0.90%***	-0.61%***	-0.30%	-0.98fs
Research and development expenditures	-0.85%***	-0.57%***	-0.28%	-1.05
Acquisitions expenditures	-0.82%	-1.03%***	0.21%	0.35
Total investment	-2.70%***	-2.24%***	-0.46%	-0.57
Financing				
Change in short-term debt	-0.06%	0.12%	-0.18%	-0.80
Change in long-term debt	-2.53%***	-1.24%***	-1.29%	-1.60
Equity issuance	-3.91%***	0.05%	-3.96%***	-4.62
Total financing	-6.54%***	-1.00%*	-5.54%***	-4.73
Payouts				
Dividends	0.02%	0.00%	0.02%	0.50
Share repurchases	-0.76%**	0.06%	-0.82%**	-2.36
Total payouts	-0.65%**	-0.01%	-0.64%*	-1.73
Change in cash holdings	-3.04%***	-0.15%	-2.89%***	-2.93

Panel C: Difference-in-Differences Conditional upon Cash Flow-Investment Gap at Year -1				
	Mean for low gap	Mean for high gap	Mean for low gap minus high gap	t-statistic for low gap minus high gap
Investment				
Capital expenditures	-1.38%***	-0.03%	-1.35%***	-4.56
Research and development expenditures	-1.10%***	-0.19%	-0.91%***	-3.74
Acquisitions expenditures	-2.77%***	0.89%**	-3.66%***	-6.37
Total investment	-5.35%***	0.64%	-5.99%***	-7.86
Financing				
Change in short-term debt	-0.06%	0.18%	-0.24%	-1.03
Change in long-term debt	-3.56%***	0.28%	-3.83%***	-4.98
Equity issuance	-2.34%***	0.10%	-2.44%***	-3.14
Total financing	-5.93%***	0.63%	-6.56%***	-5.99
Payouts				
Dividends	-0.02%	0.03%	-0.05%	-1.00
Share repurchases	-0.02%	-0.33%	0.31%	0.86
Total payouts	-0.02%	-0.36%	0.34%	0.89
Change in cash holdings	-0.96%	-1.13%**	0.17%	0.18

Table 5
Analysts' Expectations

This table presents comparisons of analysts' expectations. Panel A compares consensus expectations for treatment firms to consensus expectations for control firms. Panel B compares expectations of analysts that cover treatment firms and disappear to consensus expectations of all other analysts that cover treatment 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. Analysts' expectations comprise 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. Consensus expectations are computed as mean expectations. In Panel A, results are presented for an output regardless of how many analysts produce that output. In Panel B, results are only presented for an output if the analyst that disappears produces that output. Analysts' expectations variables are measured during the year ending three months before the broker disappearance date.

Panel A: Analysts' Expectations for Treatment Firms Compared to Control Firms

	Number of observations	Mean: Treatment firms	Mean: Control firms	p-value of test of equality of means	Median: Treatment firms	Median: Control firms	p-value of test of equality of medians
Earnings estimates	1,925	2.9%	4.1%	0.000	4.6%	4.9%	0.029
Investment recommendations	1,891	3.8	3.8	0.164	3.8	3.8	0.142
Long-term earnings growth rate estimates	1,774	19.1%	18.6%	0.150	16.3%	15.8%	0.160
Price targets	1,345	37.2%	31.2%	0.000	24.5%	22.0%	0.056

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

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

Table 6
Robustness Tests of Main Results

This table presents variations of the main results in Table 2 as follows. In Panel A, observations are dropped if the analyst coverage of treatment firms is more than five analysts greater than the analyst coverage of control firms. This results in the same distribution of analyst coverage for treatment firms and control firms. In Panel B, propensity score matching is used to match treatment firms and control firms. Using all firms between 1994 and 2008, a probit regression is run to estimate propensity scores. A dummy variable that equals one for treatment firms and zero for control firms is regressed on market capitalization, book-to-market, momentum, analyst coverage, two-digit SIC code dummy variables, and calendar year dummy variables. Each treatment firm is matched to a control firm in the same industry and same calendar year with the nearest predicted propensity score. In Panel C, pooled regressions are run using treatment firms and control firms before and after the decrease in analyst coverage. The first specification uses 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. The second specification adds control variables for market capitalization, book-to-market, momentum, and analyst coverage. The third specification adds control variables interacted with the "after" dummy variable. Only the difference-in-differences is tabulated.

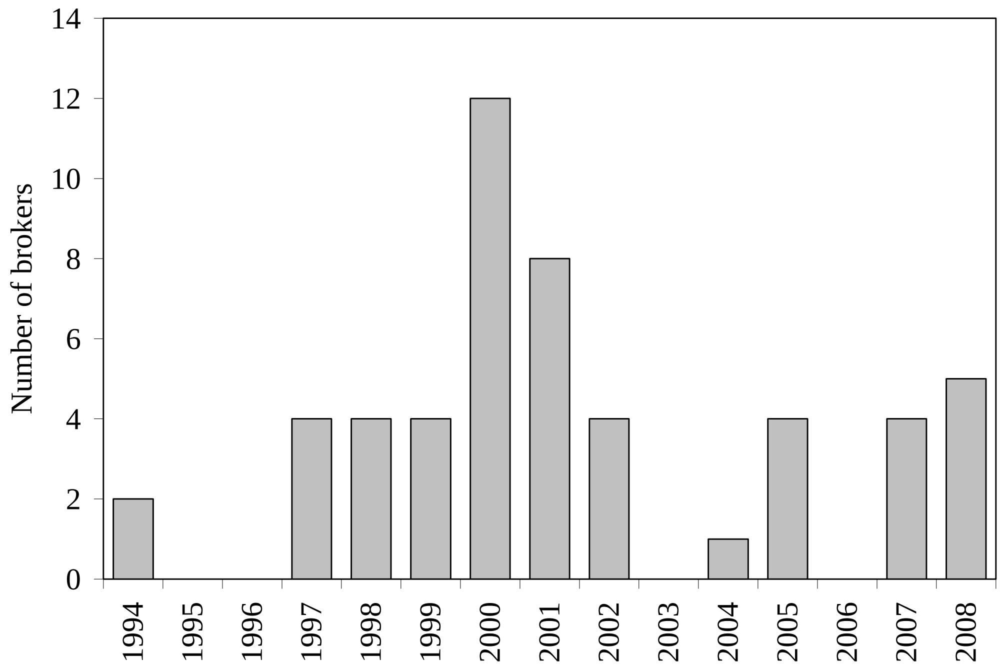
Panel A: Same Distribution of Analyst Coverage for Treatment Firms and Control Firms

	Mean treatment difference (yr +1 vs. yr -1)	Mean control difference (yr +1 vs. yr -1)	Mean of diff-in-diffs (treatments vs. controls)	t-statistic for difference-in-differences
Investment				
Capital expenditures	0.41%	1.04%	-0.68%***	-4.13
Research and development expenditures	0.47%	1.28%	-0.77%***	-5.36
Acquisitions expenditures	0.30%	1.37%	-1.00%***	-3.13
Total investment	1.20%	3.72%	-2.49%***	-5.72
Financing				
Change in short-term debt	-0.15%	-0.10%	0.01%	0.10
Change in long-term debt	-0.04%	1.60%	-1.72%***	-4.01
Equity issuance	-1.84%	-0.58%	-1.16%***	-2.62
Total financing	-1.94%	0.88%	-2.85%***	-4.61
Payouts				
Dividends	0.15%	0.18%	-0.03%	-1.19
Share repurchases	0.13%	0.40%	-0.30%	-1.46
Total payouts	0.26%	0.58%	-0.37%*	-1.73
Change in cash holdings	0.03%	1.13%	-1.28%**	-2.50

Panel B: Treatment Firms and Control Firms Matched Using Propensity Score Matching				
	Mean treatment difference (yr +1 vs. yr -1)	Mean control difference (yr +1 vs. yr -1)	Mean of diff-in-diffs (treatments vs. controls)	t-statistic for difference-in-differences
Investment				
Capital expenditures	0.14%	0.68%	-0.58%***	-3.93
Research and development expenditures	0.38%	1.09%	-0.75%***	-5.98
Acquisitions expenditures	0.04%	1.19%	-1.14%***	-4.36
Total investment	0.58%	3.03%	-2.44%***	-6.60
Financing				
Change in short-term debt	-0.14%	-0.30%	0.22%*	1.96
Change in long-term debt	-0.28%	1.24%	-1.65%***	-4.46
Equity issuance	-1.94%	-0.04%	-1.95%***	-5.84
Total financing	-2.33%	0.91%	-3.33%***	-6.59
Payouts				
Dividends	0.13%	0.15%	-0.03%*	-1.84
Share repurchases	0.19%	0.68%	-0.53%***	-2.86
Total payouts	0.32%	0.85%	-0.57%***	-2.98
Change in cash holdings	-0.05%	1.00%	-1.08%**	-2.53

Panel C: Difference-in-Differences from Pooled Regressions Using Treatment Firms and Control Firms Before and After the Decrease in Analyst Coverage						
	Baseline specification		Plus control variables		Plus control variables interacted with "after" dummy variable	
	Coefficient estimate	t-statistic	Coefficient Estimate	t-statistic	Coefficient Estimate	t-statistic
Dependent variables						
Investment						
Capital expenditures	-0.68%**	-2.49	-0.44%	-1.63	-0.53%*	-1.95
Research and development expenditures	-0.60%*	-1.86	-0.66%**	-2.20	-0.55%*	-1.84
Acquisitions expenditures	-1.06%***	-3.71	-1.04%***	-3.56	-1.10%***	-3.73
Total investment	-2.34%***	-4.67	-2.14%***	-4.44	-2.18%***	-4.52
Financing						
Change in short-term debt	0.04%	0.38	0.06%	0.55	0.05%	0.46
Change in long-term debt	-1.48%***	-4.05	-1.36%***	-3.75	-1.51%***	-4.15
Equity issuance	-1.21%***	-2.85	-1.19%***	-3.23	-1.25%***	-3.37
Total financing	-2.63%***	-4.41	-2.51%***	-4.57	-2.75%***	-4.98
Payouts						
Dividends	0.01%	0.15	0.05%	0.63	0.02%	0.33
Share repurchases	-0.17%	-0.73	-0.04%	-0.17	-0.05%	-0.24
Total payouts	-0.18%	-0.68	-0.01%	-0.03	-0.05%	-0.20
Change in cash holdings	-0.91%**	-2.24	-0.89%**	-2.30	-1.01%***	-2.59
Independent variables						
Constant term	Yes		Yes		Yes	
"Treatment firm" dummy variable?	Yes		Yes		Yes	
"After" dummy variable?	Yes		Yes		Yes	
"Treatment firm" dum. var. × "after" dum. var.?	Yes		Yes		Yes	
Control variables?	No		Yes		Yes	
Control variables × "after" dummy variable?	No		No		Yes	

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



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

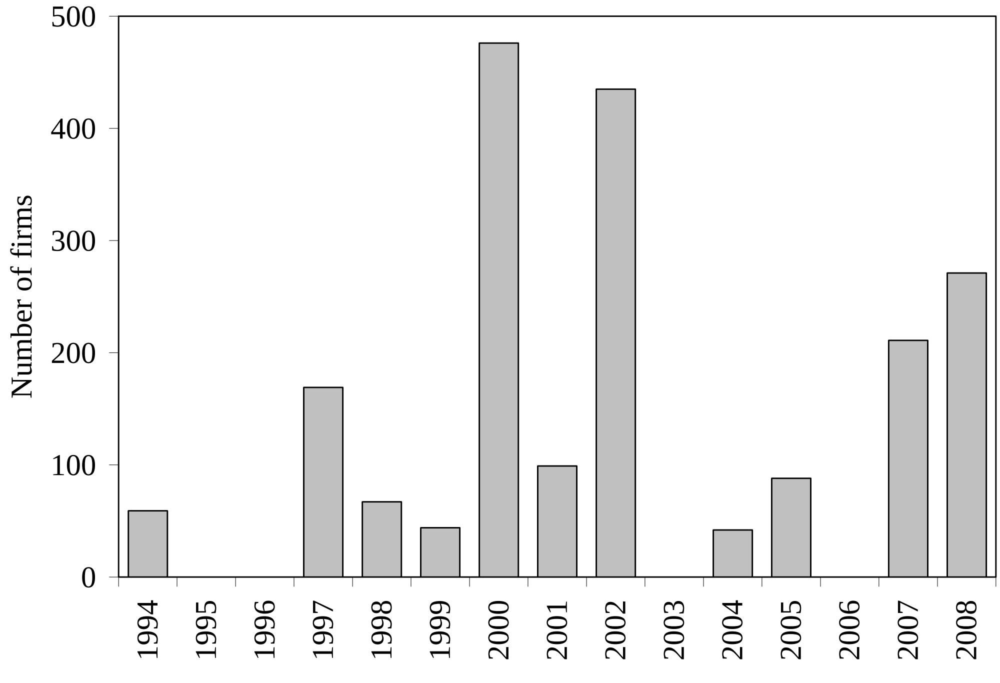


Figure 1. Distribution in calendar time of brokers that disappear and firms that lose an analyst. This figure presents the distribution of brokers and firms in the sample in calendar time. The sample comprises 1,961 treatment firms that lose an analyst between 1994 and 2008 because of broker closures 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.

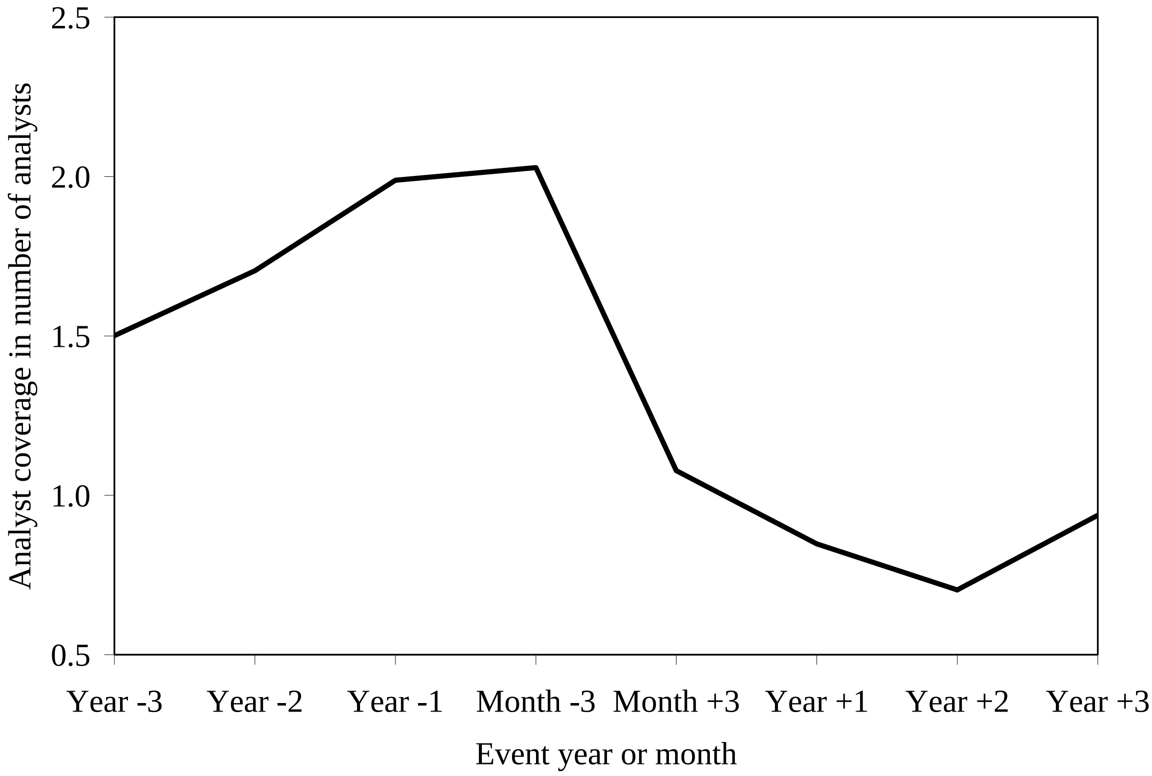


Figure 2. Mean difference between treatment firms and control firms in analyst coverage in event time. This figure presents the difference in analyst coverage between treatment firms and control firms during the three years before and the three years after the decrease in analyst coverage. The sample 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.

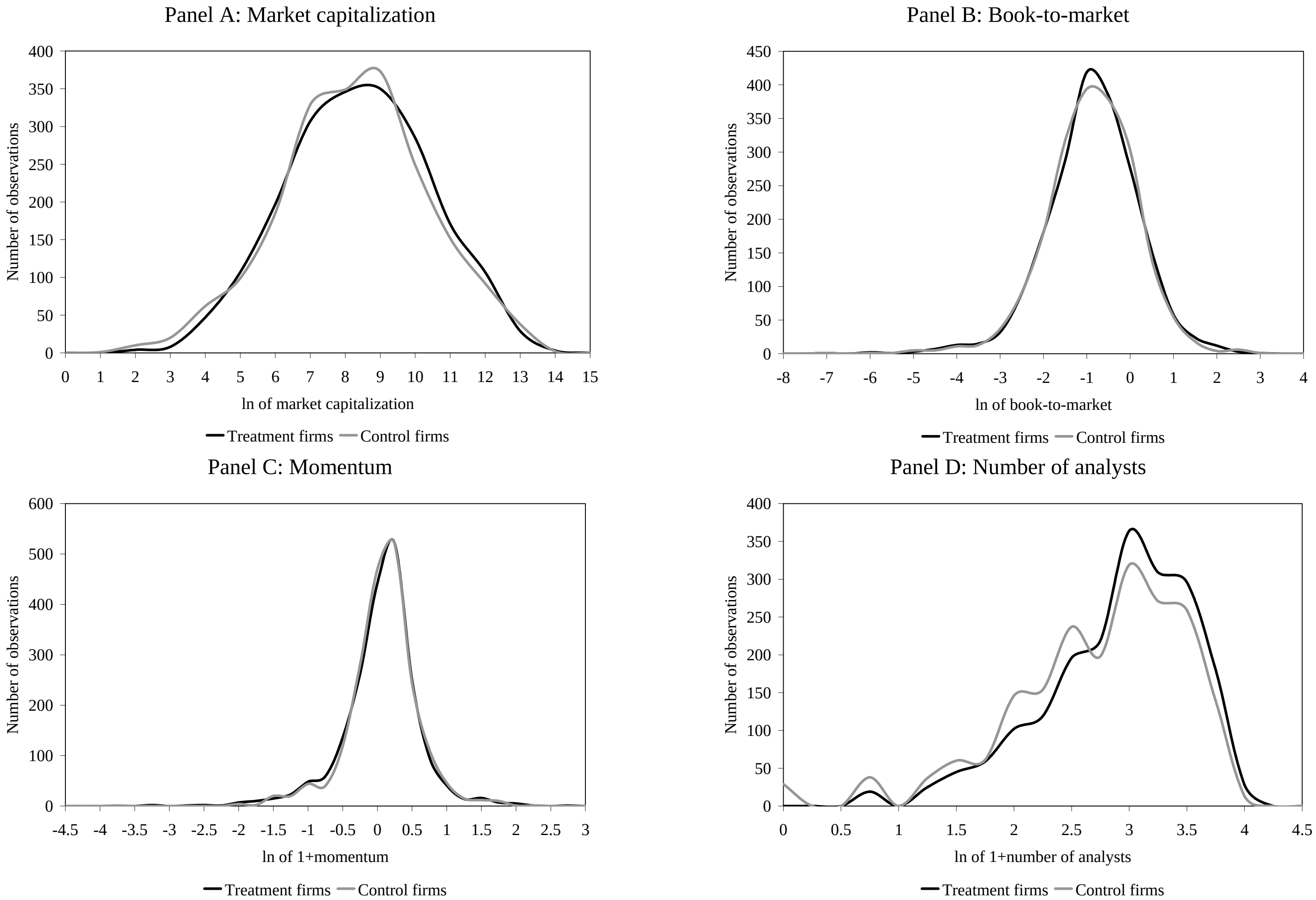


Figure 3. Distribution of matching variables for treatment firms and control firms. This figure presents the distribution of market capitalization, book-to-market, momentum, and number of analysts for treatment firms and control firms. These matching variables are measured during the year ending three months before the broker disappearance date. 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.

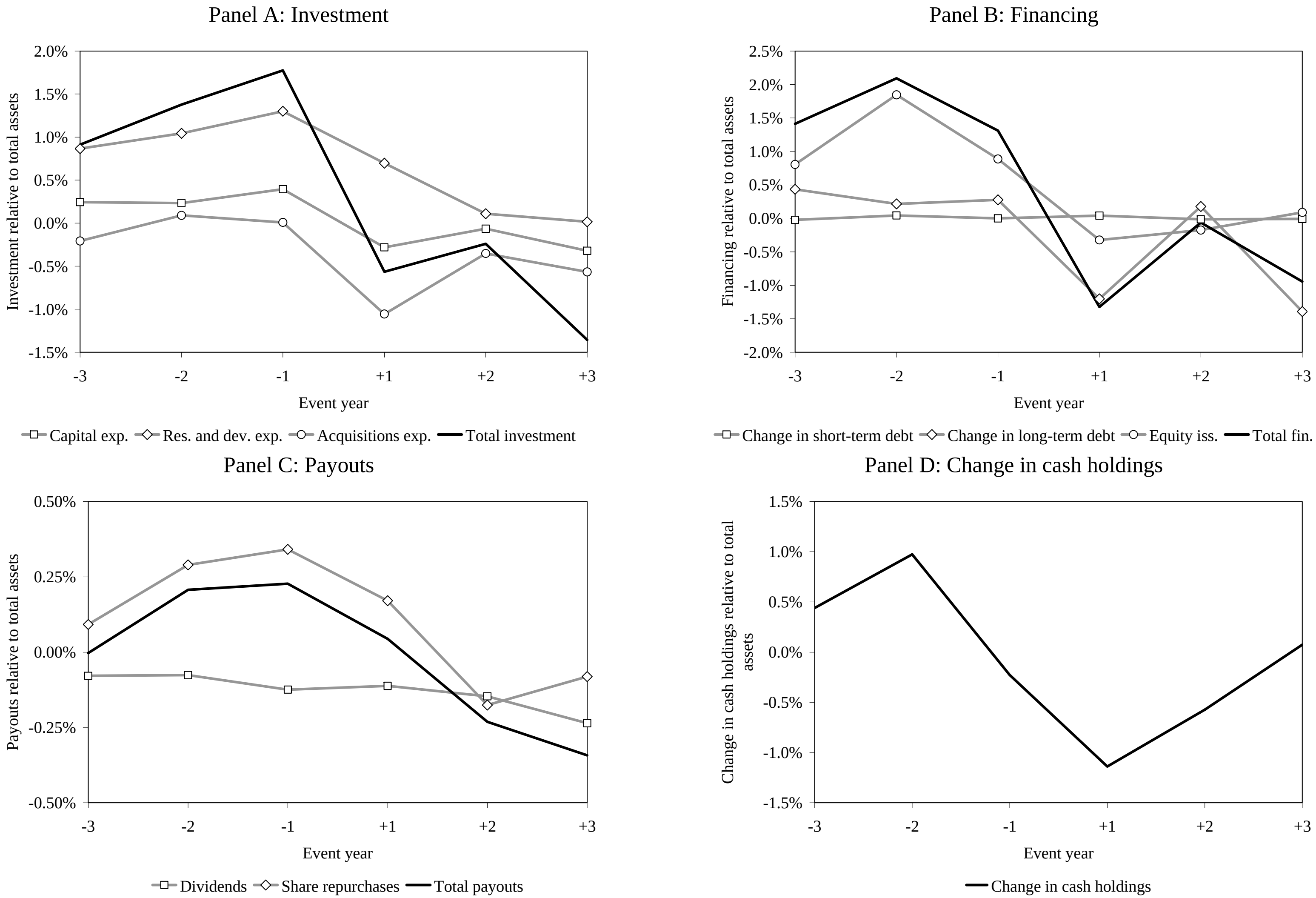


Figure 4. Mean difference between treatment firms and control firms in corporate policies in event time. This figure presents the difference in corporate policy variables between treatment firms and control firms during the three years before and the three years after the decrease in analyst coverage. 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.

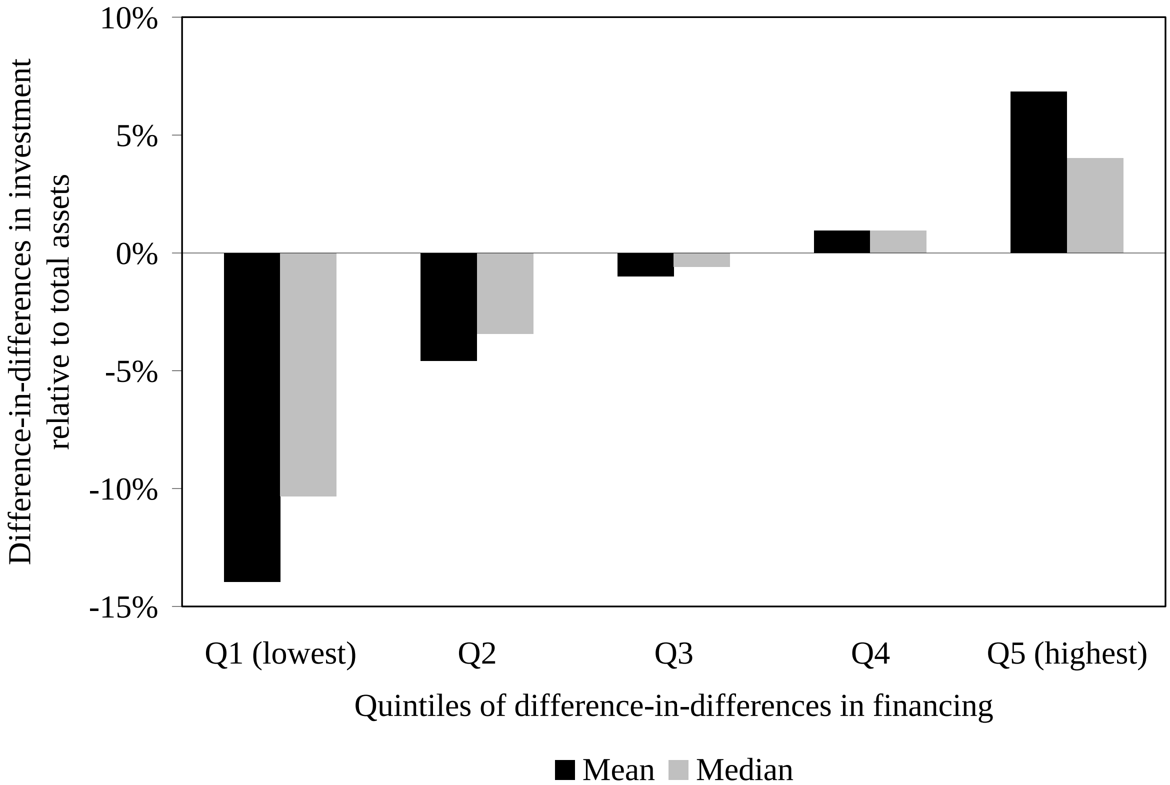


Figure 5. Difference-in-differences in investment as a function of difference-in-differences in financing. This figure presents the difference-in-differences in total investment as a function of the difference-in-differences in total financing. Both investment and financing are measured between the year before and the year after the decrease in analyst coverage and for treatment firms compared to 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.