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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 their 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 their investment and financing by 1.9% and 2.0% of total assets, respectively, compared to similar firms that do not lose an analyst.

IT IS WELL KNOWN that the global financial crisis of the late 2000s caused the worst economic contraction since the 1930s. As a result, there has been a surge of interest in the real effects of financial shocks (for example, see the survey paper by Bond, Edmans, and Goldstein (2012)). In this paper, we study the effects on corporate policies of one particular financial shock: a decrease in analyst coverage. We examine the effect of analyst coverage on corporate policies using two natural experiments, namely, broker closures as in Kelly and Ljungqvist (2012) and broker mergers as in Hong and Kacperczyk (2010). Both broker closures and broker mergers cause analysts to be terminated and analyst coverage to decrease for the firms previously covered by these analysts.

We hypothesize that this decrease in analyst coverage increases information asymmetry and thus increases the cost of capital.[1] As a result of the increase

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[1] For theoretical evidence that an increase in information asymmetry increases the cost of capital, see, for example, 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

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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 (2012). They provide empirical evidence that a decrease in analyst coverage (from broker closures) causes an increase in information asymmetry (see also Brennan and Subrahmanyam (1995) and Ellul and Panayides (2009)) as well as in the cost of capital.[2] In this paper, we provide empirical evidence for the second part of our hypothesis: a decrease in analyst coverage—through the resulting increase in information asymmetry and thus the cost of capital—causes a decrease in investment and financing.

In our tests, we use 52 broker closures and broker mergers between 1994 and 2008 that cause 1,724 firms to lose an analyst. We focus on 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 by industry, total assets, Q, cash flow, and analyst coverage. In doing so, we minimize the possibility that cross-sectional or time-series effects confound our results. We show that, before the loss of an analyst, 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 (2012), we find that, around the decrease in analyst coverage, stock prices decrease significantly more for our treatment firms than our control firms (by 1.05% 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 1.9% and 2.0% of total assets, respectively, compared to our control firms. Capital expenditures decrease by 0.67%, research and development expenditures by 0.21%, and acquisitions expenditures by 0.97%. Similarly, firms that lose an analyst decrease net total debt issuance and equity issuance by 1.07% and 0.90%, respectively. Their cash holdings decrease by 1.1% of total assets. 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 in support of the parallel trends assumption underlying our difference-in-differences approach:

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

the corporate policies of our treatment firms and control firms only diverge from each other after analyst coverage decreases for our treatment firms.

We also find that the real effects of analyst coverage 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 2.4% and 5.7%, respectively, for the smallest firms, 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 brokers and analysts to affect corporate policies, they must produce research that is relevant to investors and firms. 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 analysts' expectations), we find that the quality of our brokers and analysts is actually slightly above average.

To check the robustness of our results, we first 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 similar for firms for which the broker that disappears is not an underwriter (the vast majority of our sample). In additional robustness tests, we examine whether our results are driven by the clustering in time of broker disappearances, 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. In each of these tests, we find that our results continue to hold.

We contribute to the recently renascent literature on the real effects of a variety of shocks originating in the credit market and the stock market. For example, Sufi (2009) and Tang (2009) study the effect of information asymmetry on corporate policies using the introduction of credit ratings on syndicated loans and the refinement of credit ratings on corporate bonds, respectively. Edmans, Goldstein, and Jiang (2012) study the effect on takeovers, that is, acquisitions expenditures, of downward pressure on stock prices caused by forced selling by mutual funds. Duchin, Ozbas, and Sensoy (2010) study the effect of the financial crisis on corporate investment. In this paper, we study the real effects of a change in analyst coverage. More recent papers in the same spirit as ours study the decrease in investment and financing caused by a decrease in short sales constraints (Grullon, Michenaud, and Weston (2011)) and an increase in product market competition (Frésard and Valta (2012)).

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. Extensive evidence shows that analysts' reports impact stock prices (see, for example, Womack (1996), Barber et al. (2001), Jegadeesh et al. (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., Moyer, Chatfield, and Sisneros (1989) and Chung and Jo (1996)). Analysts sometimes issue biased reports to investors (e.g., Lin and McNichols (1998) and Michaely and Womack (1999)), but they are generally incentivized to produce accurate information (e.g., 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 to sell assets, raise capital, and/or cut its dividend,[4] all of which it did 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 Computer, CVS, Gateway, Global Crossing, Motorola, PeopleSoft, and Qwest Communications (Dyck, Morse, and Zingales (2010)). However, even in these cases, analysts may simply have 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 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 to provide large sample evidence that analyst coverage causes corporate policies.

The rest of this paper is organized as follows. Section I presents the sample and data. Section II presents the main results. Section III presents evidence on broker and analyst quality. Section IV presents robustness tests. Section V concludes.

[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 natural experiments like we do.

[4] Lewis, Michael, 2008, The rise and rise of Meredith Whitney, *Bloomberg*, April 8.

I. 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. We identify 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 and corporate policies "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, and year +1 is the year immediately after the broker disappearance date plus three months.[5] Our list of broker disappearances includes all of Hong and Kacperczyk's (2010) broker mergers that are during our sample period (13 out of 15) and all of Kelly and Ljungqvist's (2012) broker closures.

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 covers a firm if there is at least one earnings estimate in I/B/E/S by him for that firm during the year before the broker disappearance date. Similarly, we assume that an analyst disappears if there is no earnings estimate by him in I/B/E/S during the year after the broker disappearance date. Following Kelly and Ljungqvist (2012), we only retain firms for which the estimate is not "stopped" in I/B/E/S before the broker disappearance date; this eliminates the possibility that the analyst terminates coverage of firms for which he anticipates specific corporate policies (e.g., a decrease in investment and financing). For broker closures, we retain firms for which the analyst disappears from I/B/E/S during the year after the broker disappearance date, that is, there is no earnings estimate by the analyst in 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.

We retain publicly traded U.S. operating firms that are not financials or utilities that have been traded for at least 1 year before the broker disappearance

[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, 2005, respectively.

date, and that have Compustat data in both 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 that candidate control firms have the same two-digit SIC code as our treatment firms. We also require candidate control firms to be in the same total assets quintile, *Q* quintile, and cash flow quintile as our treatment firms.[6] We then retain candidate control firms that have the smallest difference in number of analysts compared to the corresponding treatment firms. We break any remaining ties based on the smallest differences in total assets, *Q*, and cash flow. To this end, we compute the difference between treatment firms and control firms for each of total assets, *Q*, and cash flow. 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.

In summary, our treatment firms and control firms are matched by industry, total assets, *Q*, cash flow, and analyst coverage. Our matching is similar in spirit to that of Kelly and Ljungqvist (2012) and Hong and Kacperczyk (2010), who match by market capitalization, book-to-market, momentum, and analyst coverage. However, since our purpose is to ensure that our treatment firms and control firms are similar in terms of the standard determinants of the corporate policies that we study, especially in terms of their investment opportunities, we match by industry, total assets, *Q*, and cash flow. We also match by analyst coverage because we hypothesize that analyst coverage affects corporate policies.[7] Our sample comprises 1,724 treatment firms and the same number of control firms. Correspondingly, we have 52 broker disappearances, of which 19 are the result of broker closures and 33 are the result of broker mergers.[8]

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 1^{st} and 99^{th} percentiles.

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 over time although there is some clustering in 2000 and 2001 and there are no broker disappearances in 1995 and 1996. Firms that lose an analyst, on the other hand, are strongly clustered in time: 734 observations (43% of our sample) are in 2000 and 2002, and a further 820 observations (48% of our sample) are in 1997, 2001, 2005, 2007, and 2008. A small number of broker disappearances account for a large number of firms that lose analyst coverage: for example, Credit Suisse First Boston's

[6] We use the standard definitions of *Q* and cash flow. Specifically, we define *Q* as market-to-book of assets, and we define cash flow as earnings before extraordinary items plus depreciation and amortization all divided by total assets.

[7] The results of the paper are similar if we match by market capitalization, book-to-market, and momentum instead of total assets, *Q*, and cash flow.

[8] Our sample differs from the samples of Kelly and Ljungqvist (2012) and Hong and Kacperczyk (2010) for several reasons: our use of I/B/E/S as the source of broker disappearances (the former use Reuters Estimates and the latter use SDC combined with I/B/E/S), our focus on analyst disappearances (as opposed to broker disappearances), and our exclusion of financials and utilities.

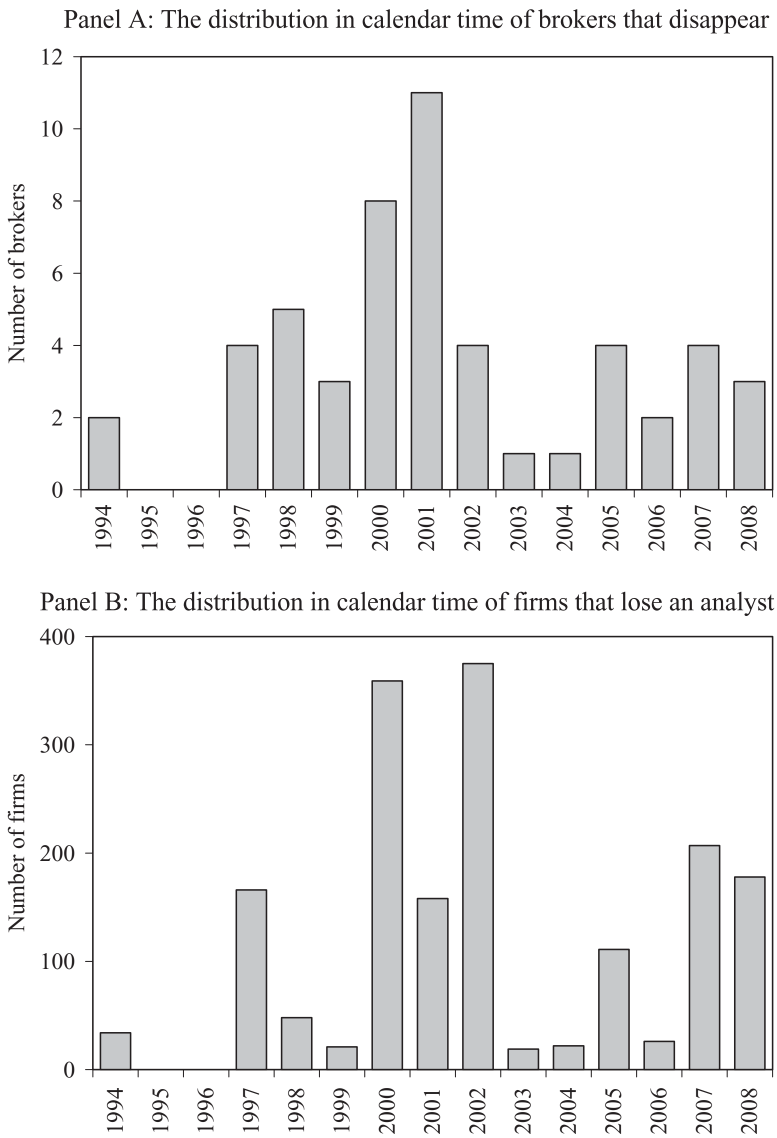

Figure 1. The distribution in calendar time of brokers that disappear and firms that lose an analyst. This figure presents the distribution of brokers and firms in the sample in calendar time. The sample comprises 1,724 treatment firms that lose an analyst between 1994 and 2008 because of broker closures and broker mergers. These firms are publicly traded U.S. operating firms, are not financials or utilities, and have been traded for at least 1 year.

acquisition of Donaldson, Lufkin & Jenrette in October 2000 accounts for 144 firms (8% of our sample), and the top 15, 20, and 25 (of 52) brokers account for a respective 63%, 76%, and 84% of our observations. Our difference-in-differences approach ensures that time-series effects cannot explain our results. However, in the section 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. (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 influence 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 as well as equity), payouts (dividends and share repurchases), and the change in cash holdings. The construction of these variables is detailed in Panel A of Appendix Table A.1.

For our natural experiments to be valid, they must meet two conditions: relevance and exogeneity. While the exogeneity condition is inherently untestable, we provide supportive evidence based on analysts' expectations. We defer doing so, however, 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 0.98 analysts more than our control firms (with a t-statistic of -8.06). Thus, broker disappearances are associated with a decrease in analyst coverage of roughly one analyst. This relation is what we expect given how we construct our sample.

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 in coverage between treatment firms and control firms is roughly horizontal before the decrease in analyst coverage (years -3 through -1) and decreases by roughly one analyst between month -3 and month $+3$ (by 0.98 analysts to be precise). These 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 -1.05% (with a t-statistic of -1.73). Our stock price decreases are similar to those of Kelly and Ljungqvist (2012).

We next 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 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 total assets, Q, cash

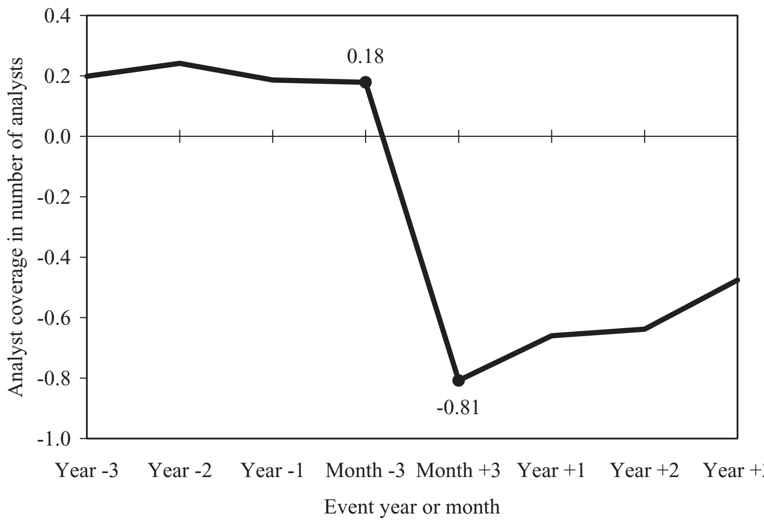

Figure 2. The difference between treatment firms and control firms in analyst coverage in event time. This figure presents the mean difference in analyst coverage between treatment firms and control firms during the 3 years before and the 3 years after the decrease in analyst coverage. The sample comprises 1,724 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, total assets, Q, cash flow, 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 1 year.

flow, and analyst coverage. We measure all variables during the year ending three months before the broker disappearance date.

Table I 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 corporate policy variables. The differences are not economically or statistically significant. We also examine the distribution of matching variables for our treatment firms and control firms using a graphical approach. The results, presented in the Internet Appendix, show that once again our treatment firms are very similar to our control firms for all of our matching variables.[9]

Next, we examine the evidence based on analysts' expectations in support of 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 correct only if the disappearance of brokers and analysts is not caused by changes in corporate policies (reverse causality) or something correlated with them (omitted variables). 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. We do so by comparing

[9] The Internet Appendix may be found in the online version of this article.

Table I

Descriptive Statistics

This table presents descriptive statistics that compare treatment firms and control firms. The sample comprises 1,724 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, total assets, Q, cash flow, 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 1 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 a more favorable recommendation), long-term earnings growth rate estimates for the next 5 years, and price targets for the next year measured as a percent of the stock price. All variables are measured during the year 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								
Total assets ($M)	588	586	2,329	2,246	9,016	7,867	0.658	0.554
Q	1.28	1.32	1.81	1.88	2.94	3.06	0.143	0.217
Cash flow	4.82%	5.36%	9.28%	9.74%	14.17%	14.86%	0.143	0.118
Number of analysts	11.0	10.0	18.0	18.0	24.0	25.0	0.918	0.583
Investment variables								
Capital expenditures	2.60%	2.47%	4.82%	4.65%	8.50%	8.23%	0.261	0.372
Research and development exp.'s	0.00%	0.00%	0.23%	0.18%	5.78%	5.29%	0.812	0.699
Acquisitions expenditures	0.00%	0.00%	0.00%	0.04%	2.14%	2.22%	0.261	0.670
Total investment	6.13%	6.16%	10.52%	10.18%	17.33%	16.88%	0.433	0.670
Financing variables								
Change in short-term debt	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.927	0.974
Change in long-term debt	−1.05%	−0.98%	0.00%	0.00%	3.21%	3.25%	0.781	0.641
Equity issuance	0.19%	0.22%	0.77%	0.81%	2.28%	2.29%	0.433	0.350
Total financing	−0.20%	−0.15%	2.14%	2.10%	6.54%	7.15%	0.759	0.860

(*Continued*)

Table I—*Continued*

	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		
Payout variables								
Dividends	0.00%	0.00%	0.00%	0.00%	1.35%	1.36%	0.340	0.810
Share repurchases	0.00%	0.00%	0.14%	0.08%	3.14%	3.17%	0.233	0.670
Total payouts	0.00%	0.00%	1.23%	1.15%	5.28%	5.05%	0.586	0.836
Change in cash holdings	−1.10%	−0.84%	0.15%	0.26%	2.35%	2.66%	0.610	0.117
Analysts' expectations variables								
Earnings estimates	2.6%	2.7%	4.9%	4.8%	7.1%	6.8%	0.584	0.203
Investment recommendations	3.5	3.5	3.8	3.9	4.2	4.2	0.185	0.181
Long-term earnings growth rate est.'s	11.4%	12.1%	15.3%	16.1%	22.9%	23.0%	0.028	0.015
Price targets	12.3%	11.1%	29.6%	24.2%	60.6%	50.3%	0.001	0.000

analysts' expectations for the two groups of firms using 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 5 years. Finally, we use price targets for the next year measured 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.[10]

Table I presents the results.[11] Earnings estimates and investment recommendations are not significantly different. Long-term earnings growth rate estimates are significantly more pessimistic for our treatment firms than our control firms (e.g., a median of 15.3% versus 16.1%). The opposite is true for price targets for the next year, which are significantly more optimistic for our treatment firms than our control firms (e.g., a median of 29.6% of the stock price vs. 24.2%). Statistically, two differences are significant but in opposite directions, and two differences are not significant. Economically, all of the differences are small. Overall, we conclude that analysts' expectations are similar for our treatment firms and control firms. The results are consistent with broker closures and broker mergers being exogenous to corporate policies.

II. Main Results

A. 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.

Table II presents the results.[12] (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 0.67% of total assets, research and development expenditures by 0.21%, and acquisitions expenditures by 0.97% after the loss of an analyst. Total investment decreases by 1.92%. For financing, the change in short-term debt is insignificant, long-term debt decreases by 1.07% of

[10] Data for price targets begin in July 1999 and thus are only available for roughly half of our observations.

[11] The results are similar for all 3 years before the broker disappearance date.

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

Table II

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 1,724 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, total assets, Q, cash flow, 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 1 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 vs. Controls)	t-statistic for Difference-in-Differences
Investment				
Capital expenditures	− 0.01%	0.62%	− 0.67%***	− 4.73
Research and dev. exp.'s	0.40%	0.60%	− 0.21%**	− 2.27
Acquisitions expenditures	0.09%	1.11%	− 0.97%***	− 3.36
Total investment	0.41%	2.41%	− 1.92%***	− 5.18
Financing				
Change in short-term debt	0.06%	0.06%	0.00%	0.02
Change in long-term debt	− 0.17%	0.97%	− 1.07%***	− 2.79
Equity issuance	− 1.13%	− 0.10%	− 0.90%***	− 2.88
Total financing	− 1.34%	0.89%	− 2.02%***	− 4.00
Payouts				
Dividends	0.10%	0.12%	− 0.02%	− 1.12
Share repurchases	0.15%	0.08%	0.08%	0.48
Total payouts	0.30%	0.23%	0.11%	0.61
Change in cash holdings	− 0.06%	1.03%	− 1.12%***	− 2.62

total assets, and equity issuance decreases by 0.90%. Total financing decreases by 2.02%. For payouts, dividends and share repurchases are insignificant individually and collectively. Finally, cash holdings decrease by 1.12% 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 total investment, for example). Indeed, we do not also have to control for cross-sectional and time-series factors that affect both analyst coverage and corporate policies because we use a difference-in-differences approach. For example, the 2.41% increase in total investment for control firms may be caused by an increase in investment opportunities for all firms. However, total investment increases by only 0.41% for treatment firms because not only do their investment opportunities increase but their analyst coverage decreases. In fact, the investment of our treatment firms and control firms is growing at similar rates before the broker disappearance rate.[13] However, at the broker disappearance date, this growth slows markedly for our treatment firms, whereas it continues for our control firms. The net effect is the difference-in-differences that we document.

Following an increase in information asymmetry, firms should alter their financing mix 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 (lower risk short-term debt first, then higher risk long-term debt), and equity only as a last resort. Table II suggests that this is indeed how firms behave. In particular, equity issuance decreases by 0.9% of total assets, and net total debt issuance decreases by 1.1%, but this decrease in net total debt issuance is driven entirely by the decrease in higher risk net long-term debt issuance since lower risk net short-term debt issuance is virtually unchanged.[14] Finally, firms use more cash to finance their investments, so their cash holdings decrease by 1.1% of total assets.

We also examine the evolution of corporate policies around a decrease in analyst coverage. Figure 3 presents the difference in corporate policy variables between treatment firms and control firms during the 3 years before and the 3 years after the loss of an analyst. Panel A shows that investment is roughly horizontal before year −1 and decreases mainly between year −1 and year +1. This is the case for all of our investment variables except research and development expenditures, which decrease relatively little. Panel B paints a similar picture for financing. Financing is roughly horizontal before year −1 and decreases mainly between year −1 and year +1. This is the case for all of our financing variables except for short-term debt, which does not change much.[15] Panel C shows that the variation in payouts is an order of magnitude smaller than the variation in investment and financing, so the evolution of payouts is insignificant overall. Finally, Panel D shows that the change in cash holdings decreases slightly between year −3 and year −1, and then decreases significantly between year −1 and year +1.

[13] Between year −3 and year −2, our treatment firms and control firms increase their total investment by 2.4 and 2.0 percentage points, respectively. Between year −2 and year −1, the corresponding figures are 1.2 and 1.3 percentage points, respectively.

[14] The relatively greater decrease in debt issuance than equity issuance—1.1% versus 0.9%—appears to be related to debt capacity. To proxy for debt capacity, we follow Almeida and Campello (2007) and Hahn and Lee (2009) and use asset tangibility. We sort firms into halves based on asset tangibility before the decrease in analyst coverage. We find that firms with low debt capacity decrease long-term debt issuance by 1.06%, and they decrease equity issuance by a mere 0.36%. By contrast, firms with high debt capacity decrease long-term debt issuance by 1.08%, but they decrease equity issuance by a substantially greater 1.45%. In other words, firms far from their debt capacity are able to use relatively more (cheaper) debt than equity, whereas firms close to their debt capacity are forced to use relatively more (dearer) equity than debt.

[15] The decrease in financing between year −1 and year +1 is largely offset by an increase in financing between year +1 and year +2 after which financing is roughly horizontal. The same is true for the change in cash holdings. This is consistent with firms softening the blow of the loss of an analyst with other mechanisms that decrease information asymmetry such as voluntary corporate disclosures (see Balakrishnan et al. (2011)).

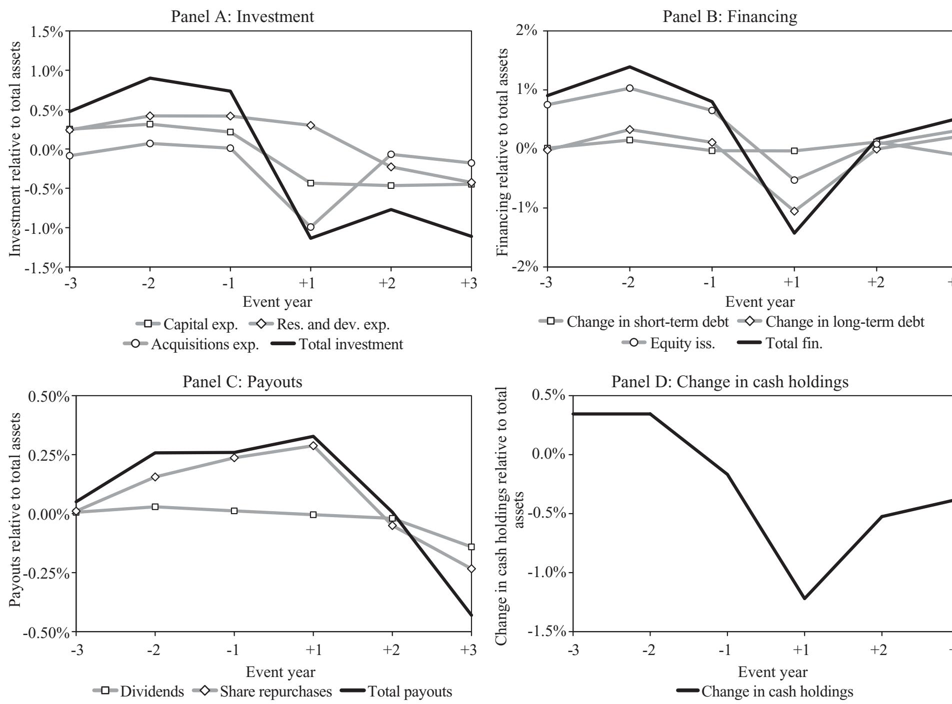

Figure 3. The difference between treatment firms and control firms in corporate policies in event time. This figure presents the mean difference in corporate policy variables between treatment firms and control firms during the 3 years before and the 3 years after the decrease in analyst coverage. The sample comprises 1,724 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, total assets, Q, cash flow, 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 1 year. All corporate policy variables are scaled by total assets.

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 they are changes that occur only after 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 II, we compute the difference between our treatment firms and control firms between year −3 and year −1. We find that none of the differences are statistically significant at the 5% level (and only one difference—for research and development expenditures—is significant at the 10% level). We conclude that the changes in corporate policies are not part of long-term trends before analyst coverage decreases.

We hypothesize that the decreases in investment and financing are caused by the increase in information asymmetry that results from the decrease in analyst coverage. If this is the case, then the firms with the biggest decrease in financing should also be the firms with the biggest decrease in investment. We test this hypothesis 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 0.442 (with a *p*-value of 0.000). We also compute the difference-in-differences in total investment for each quintile of the difference-in-differences in total financing. The results, presented in the Internet Appendix, show that the change in investment is monotonically increasing in the change in financing. Overall, the firms that decrease investment the most are also the firms that decrease financing the most.

By way of comparison, we consider 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 indicating that a decrease in information asymmetry causes an increase in investment. In the same spirit, Tang (2009) finds that credit rating refinements, which decrease information asymmetry, cause a 20 basis point decrease in the cost of debt for firms that are upgraded compared to firms that are downgraded. As a result, firms increase debt financing by roughly 2% of total assets, decrease equity financing by 1% of total assets, and decrease cash holdings by 2% of total assets. Firms also increase capital expenditures by roughly 2% of total assets and increase asset growth by three percentage points.

There is additional evidence from the recent financial crisis. Campello, Graham, and Harvey (2010) provide survey evidence that firms that were financially constrained were significantly more affected by the crisis than firms that were not financially constrained: the former compared to the latter planned to decrease capital expenditures, technology expenditures, and cash holdings (i.e., increase internal financing) by roughly 9, 13, and 12 percentage points, 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 0.42 percentage points less (58% less) for firms with a one-standard-deviation increase in cash holdings. Almeida et al. (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 financially constrained in this sense. Our results are of comparable magnitudes: for total investment, total financing, and cash holdings, the mean differences-in-difference relative to their standard deviations are −12.5%, −9.6%, and −6.3%, respectively. Overall, compared to the results in the literature, our results are economically plausible if somewhat smaller.

B. The Real Effects of Analyst Coverage Conditional upon Market Capitalization and Analyst Coverage

We 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 simple. One analyst is relatively more important for a firm covered by 5 analysts than for a firm covered by 25 analysts: the disappearance of an analyst causes a bigger increase in information asymmetry when there are few analysts remaining that cover the firm than when there are many analysts remaining. Similarly, if smaller firms have more information asymmetry than bigger firms, then the disappearance of an analyst causes a bigger increase in information asymmetry for smaller firms than bigger firms because analyst coverage is more important for the former than the latter.

For each corporate policy variable, we run one pooled regression 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 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.

Table III presents the results for the interaction term for each quintile. Panel A shows that total investment and total financing decrease by 2.4% and 5.7%, respectively, in the bottom quintile of market capitalization, whereas the corresponding figures are insignificant for the biggest firms (in the top two quintiles). Similarly, the decrease in cash holdings is bigger in the bottom quintile of market capitalization than in the top quintile by 1.8 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.

C. The Real Effects of Analyst Coverage Conditional upon the Change in Information Asymmetry

We hypothesize that a decrease in analyst coverage affects corporate policies by causing an increase in information asymmetry. If this is the case, then changes in corporate policies 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 (2012): the bid-ask spread, the Amihud (2002) illiquidity 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 following Kelly and Ljungqvist (2012). We measure the change in information asymmetry using difference-in-differences. We verify that, as in Kelly and Ljungqvist (2012), there is an economically and statistically significant increase in information asymmetry for firms that lose an analyst.

Table III

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 1,724 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, total assets, Q, cash flow, 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 1 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 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. All conditioning variables are measured during the year before the decrease in analyst coverage. 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 Differences-in-Differences Conditional upon Market Capitalization

	Q1 (smallest)	Q2	Q3	Q4	Q5 (biggest)
Investment					
Capital expenditures	− 0.83	− 1.01	− 0.74	− 0.53	0.05
Res. and dev. expenditures	0.07	− 0.22	− 0.21	0.12	− 0.27
Acquisitions expenditures	− 1.25*	− 0.96	− 1.17*	− 1.03	− 0.62
Total investment	− 2.44**	− 2.34**	− 2.03*	− 1.47	− 0.79
Financing					
Change in short-term debt	0.31	− 0.50**	0.20	0.08	− 0.03
Change in long-term debt	− 2.09**	− 1.91**	− 1.98**	− 0.23	0.46
Equity issuance	− 4.28***	− 0.16	− 0.75	− 1.10	0.51
Total financing	− 5.69***	− 2.62**	− 2.58**	− 1.16	1.16
Payouts					
Dividends	− 0.08	0.03	− 0.04	0.08	− 0.05
Share repurchases	− 0.03	0.05	0.16	0.09	0.03
Total payouts	− 0.05	0.19	0.08	0.16	0.00
Change in cash holdings	− 1.91**	− 0.81	− 1.19	− 1.35	− 0.11

Panel B: Mean Differences-in-Differences Conditional upon Analyst Coverage

	Q1 (least)	Q2	Q3	Q4	Q5 (most)
Investment					
Capital expenditures	− 0.75	− 1.05	− 0.58	− 0.49	− 0.43
Res. and dev. expenditures	0.11	− 0.38	− 0.05	− 0.33	0.00
Acquisitions expenditures	− 1.61***	− 1.58**	− 0.83	− 0.29	− 0.74
Total investment	− 2.69**	− 3.00**	− 1.47	− 1.04	− 1.24
Financing					
Change in short-term debt	0.01	0.52**	− 0.15	− 0.42	− 0.04
Change in long-term debt	− 0.83	− 2.38***	− 0.14	− 1.00	− 1.47*
Equity issuance	− 4.07***	− 0.05	− 1.49*	− 0.73	0.74
Total financing	− 4.76***	− 1.70	− 1.66	− 1.94	− 0.83

(*Continued*)

Table III—*Continued*

Panel B: Mean Differences-in-Differences Conditional Upon Analyst Coverage					
	Q1 (least)	Q2	Q3	Q4	Q5 (most)
Payouts					
Dividends	− 0.04	− 0.03	− 0.02	− 0.00	0.01
Share repurchases	0.29	− 0.19	0.33	− 0.27	0.06
Total payouts	0.34	− 0.21	0.27	− 0.34	0.23
Change in cash holdings	− 1.76**	− 0.90	− 1.45	0.53	− 1.49

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

Table IV presents the results. The effect of the loss of an 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 3.2 and 7.3 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 in information asymmetry decrease their cash holdings 3.5 percentage points more 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.

D. The Real Effects of Analyst Coverage Conditional upon Financial Constraints

We examine how the real effects of analyst coverage depend on financial constraints. When analyst coverage decreases for a firm, information asymmetry increases, and thus the firm's cost of external financing increases. Consequently, its optimal amount of investment and its optimal amount of external financing both 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, it is for firms that are more financially constrained that the real effects of analyst coverage should be bigger.

To test this hypothesis, we condition upon proxies for financial constraints. We use two proxies for financial constraints that are standard in the literature: one based on Almeida, Campello, and Weisbach (2004) and another based on Rajan and Zingales (1998). Almeida, Campello, and Weisbach (2004) use four

Table IV

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 1,724 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, total assets, Q, cash flow, 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 1 year. For each corporate policy variable, a mean difference-in-differences-in-differences is computed: the difference between the year after the decrease in analyst coverage and the year before, the difference between treatment firms and control firms, and the 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 construction of the information asymmetry variables is detailed in Panel B of Appendix Table AI. 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.

Differences-in-Differences-in-Differences		Big Change in Bid-Ask Spread ($N = 521$) vs. Small ($N = 522$)	Big Change in Amihud Illiquidity Measure ($N = 567$) vs. Small ($N = 568$)	Big Change in Returns Ratio ($N = 571$) vs. Small ($N = 573$)	Big Change in Earnings Announcement Surprise ($N = 539$) vs. Small ($N = 540$)	Big Change in Earnings Announcement Volatility ($N = 556$) vs. Small ($N = 557$)
Investment						
Capital expenditures	Mean:	−1.11%***	−1.69%***	−0.92%**	−0.24%	−0.67%*
	t-stat:	(−2.76)	(−4.50)	(−2.47)	(−0.63)	(−1.82)
Research and dev. exp.'s	Mean:	−0.44%*	−0.47%*	−0.14%	−0.26%	−0.12%
	t-stat:	(−1.74)	(−1.93)	(−0.57)	(−1.08)	(−0.49)
Acquisitions expenditures	Mean:	−1.61%**	−2.13%***	−1.54%**	−1.83%**	−0.48%
	t-stat:	(−2.18)	(−2.87)	(−2.05)	(−2.52)	(−0.65)
Total investment	Mean:	−3.16%***	−4.86%***	−2.91%***	−2.59%***	−1.32%
	t-stat:	(−3.19)	(−4.97)	(−2.99)	(−2.70)	(−1.38)

(*Continued*)

Table IV—*Continued*

Differences-in-Differences-in-Differences		Big Change in Bid-Ask Spread ($N = 521$) vs. Small ($N = 522$)	Big Change in Amihud Illiquidity Measure ($N = 567$) vs. Small ($N = 568$)	Big Change in Returns Ratio ($N = 571$) vs. Small ($N = 573$)	Big Change in Earnings Announcement Surprise ($N = 539$) vs. Small ($N = 540$)	Big Change in Earnings Announcement Volatility ($N = 556$) vs. Small ($N = 557$)
Financing						
Change in short-term debt	Mean:	−0.07%	−0.26%	−0.50%	0.03%	0.07%
	t-stat:	(−0.22)	(−0.89)	(−1.48)	(0.11)	(0.22)
Change in long-term debt	Mean:	−2.36%**	−2.14%**	−1.50%	−2.20%**	−1.47%
	t-stat:	(−2.34)	(−2.10)	(−1.55)	(−2.21)	(−1.49)
Equity issuance	Mean:	−5.06%***	−5.09%***	−3.79%***	−2.51%***	−0.33%
	t-stat:	(−5.60)	(−5.92)	(−4.78)	(−3.06)	(−0.38)
Total financing	Mean:	−7.31%***	−7.70%***	−5.60%***	−5.00%***	−1.01%
	t-stat:	(−5.36)	(−5.70)	(−4.41)	(−3.84)	(−0.76)
Payouts						
Dividends	Mean:	−0.10%**	−0.13%***	−0.07%	−0.11%**	−0.02%
	t-stat:	(−2.08)	(−2.85)	(−1.57)	(−2.44)	(−0.47)
Share repurchases	Mean:	−0.34%	−0.07%	0.01%	−0.39%	0.27%
	t-stat:	(−0.87)	(−0.19)	(0.02)	(−1.02)	(0.65)
Total payouts	Mean:	−0.34%	−0.07%	−0.12%	−0.50%	0.17%
	t-stat:	(−0.83)	(−0.18)	(−0.28)	(−1.22)	(0.37)
Change in cash holdings	Mean:	−3.47%***	−3.08%***	−3.39%***	−1.83%	−2.03%*
	t-stat:	(−2.93)	(−2.68)	(−3.07)	(−1.64)	(−1.74)

proxies to classify firms as constrained or unconstrained: total payout ratio, total assets, bond rating status, and commercial paper rating status.[16] We construct their four proxies, and then classify as constrained firms that are constrained based on all four of their proxies and as unconstrained firms that are unconstrained based on all four of their proxies. In this way, we are able to use all of the information in their four proxies as well as summarize this information succinctly in a single composite proxy.[17]

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 use both a firm-level version as well as an industry-level version of this proxy. For the former, we classify firms in the bottom (top) half of the cash flow–investment gap as constrained (unconstrained). For the latter, we classify industries in the bottom (top) quartile of the cash flow–investment gap as constrained (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 verify that our conditioning variables are not significantly different for our treatment firms and control firms.) We use a triple difference approach: we compare the mean difference-in-differences for each corporate policy variable (as in Table II) for firms that are financially constrained and firms that are not financially constrained.

Table V presents the results. The effect of the loss of an analyst on corporate policies is bigger for firms that are financially constrained. By way of example, using the firm-level proxy from Rajan and Zingales (1998), total investment and total financing decrease 5.0 and 4.4 percentage points more, respectively, for firms that are financially constrained than for firms that are not financially constrained. Overall, the results suggest that the real effects of analyst coverage are indeed bigger for financially constrained firms.

III. Broker and Analyst Quality

If the brokers and analysts that disappear affect the corporate policies of the firms that they cover, then it must be the case that they produce research that is relevant to investors and firms. Our finding that these brokers and analysts do have significant real effects indirectly suggests that they do not produce low-quality research. We now directly examine whether this is the case.

[16] These four proxies are standard in the literature. For example, for dividend payer status, 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).

[17] If we use their four proxies individually, the results are similar.

Table V

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 1,724 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, total assets, Q, cash flow, 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 1 year. For each corporate policy variable, a mean difference-in-differences-in-differences is computed: the difference between the year after the decrease in analyst coverage and the year before, the difference between treatment firms and control firms, and the difference between firms that are financial constrained and firms that are not financially constrained. All corporate policy variables are scaled by total assets. The construction of the financial constraints variables is detailed in Panel C of Appendix Table AI. 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

Differences-in-Differences-in-Differences	Constrained ($N = 358$) vs. Unconstrained ($N = 435$) Based on the Composite Proxy from Almeida, Campello, and Weisbach (2004)		Constrained ($N = 862$) vs. Unconstrained ($N = 862$) Based on the Firm-Level Proxy from Rajan and Zingales (1998)		Constrained ($N = 1{,}034$) vs. Unconstrained ($N = 294$) Based on the Industry-Level Proxy from Rajan and Zingales (1998)	
	Mean	t-statistic	Mean	t-statistic	Mean	t-statistic
Investment						
Capital expenditures	– 0.39%	– 0.89	– 1.46%***	– 5.17	– 1.23%***	– 4.26
Research and dev. exp.'s	0.09%	0.32	– 0.16%	– 0.87	– 0.26%	– 1.36
Acquisitions expenditures	– 0.41%	– 0.51	– 3.19%***	– 5.59	– 0.26%	– 0.40
Total investment	– 0.81%	– 0.74	– 4.98%***	– 6.79	– 1.86%**	– 2.36

(*Continued*)

Table V—*Continued*

Differences-in-Differences-in-Differences	Constrained ($N = 358$) vs. Unconstrained ($N = 435$) Based on the Composite Proxy from Almeida, Campello, and Weisbach (2004)		Constrained ($N = 862$) vs. Unconstrained ($N = 862$) Based on the Firm-Level Proxy from Rajan and Zingales (1998)		Constrained ($N = 1{,}034$) vs. Unconstrained ($N = 294$) Based on the Industry-Level Proxy from Rajan and Zingales (1998)	
	Mean	*t*-statistic	Mean	*t*-statistic	Mean	*t*-statistic
Financing						
Change in short-term debt	0.33%	0.79	− 0.57%**	-2.19	0.11%	0.33
Change in long-term debt	− 1.49%	− 1.29	− 2.64%***	− 3.46	− 1.21%	− 1.41
Equity issuance	− 2.71%***	− 2.73	− 1.06%*	− 1.69	− 0.64%	− 1.01
Total financing	− 3.65%**	− 2.43	− 4.39%***	− 4.36	− 1.89%*	− 1.76
Payouts						
Dividends	− 0.05%	− 1.03	− 0.06%	− 1.56	0.07%	1.50
Share repurchases	0.34%	0.76	− 0.07%	− 0.22	0.43%	1.09
Total payouts	0.18%	0.40	− 0.09%	− 0.27	0.56%	1.33
Change in cash holdings	− 2.51%*	− 1.89	− 0.49%	− 0.57	− 1.25%	− 1.40

First, we examine the quality of the research produced by our brokers. Typically, research is a cost center that is supported by revenues from underwriting, trading, and market making. The closures and mergers of brokers in our sample appear to be motivated by the general business strategy of the broker as we discuss in the Internet Appendix. 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 for 60% of our sample firms.[18]

Next, we examine the earnings estimate accuracy of our brokers and analysts. This standard measure of research quality is known 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 of zero to one—of a broker across all firms that the broker covers. The resulting measure captures the mean accuracy of a broker relative to other brokers who cover the same firms that the broker covers. We construct this measure analogously for analysts.

Panel A of Table VI presents the mean and median accuracy of our brokers and analysts as well as the mean accuracy weighted by the number of sample firms covered by the broker or analyst. In our sample, 71 unique brokers disappear as well as 373 unique analysts.[19] The accuracy of our brokers is slightly above average: the mean and median accuracy are 0.52 and 0.54, respectively. This is also the case for our analysts: the mean and median accuracy are 0.54 and 0.55, respectively.

Moreover, the literature finds that analysts that are not promoted or ones that 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 this very low-accuracy group: only 6% of our analysts are in the bottom quartile.

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 to the mean expectations of all other analysts that cover treatment firms. We use the same measures of analysts' expectations as in Table I.[20]

[18] 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.

[19] 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.

[20] Most analysts produce earnings estimates. However, not all analysts produce recommendations, growth rate estimates, and price targets. Moreover, data for price targets begin in July 1999. Consequently, for three of our four measures of analysts' expectations, data are only available for roughly half of our observations.

Table VI

Broker and Analyst Quality

This table presents results on the quality of brokers and analysts. Panel A presents the relative earnings estimate accuracy of brokers and analysts that disappear. Panel B presents analysts' expectations for analysts that disappear compared to other analysts. The sample comprises 1,724 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, total assets, Q, cash flow, 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 1 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 5 years, and price targets for the next year measured 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 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	0.520	0.539	0.535
Analysts	373	0.536	0.546	0.554

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	1,635	3.4%	3.8%	0.226
Investment recommendations	966	3.7	3.8	0.003
Long-term earn.'s gr. rate est.'s	698	18.4%	19.5%	0.043
Price targets	804	56.7%	60.3%	0.563
	Median			
	Number of Observations	Analysts that Disappear	Other Analysts	p-value of Test of Equality
Earnings estimates	1,635	4.7%	4.9%	0.235
Investment recommendations	966	4.0	3.8	0.000
Long-term earn.'s gr. rate est.'s	698	15.0%	16.5%	0.022
Price targets	804	26.2%	28.3%	0.441

Panel B of Table VI presents the results.[21] The earnings estimates of analysts that disappear are similar to the earnings estimates of other analysts. While we interpret the results of the rest of the table with caution because of the small sample size, the results are similar. Mean recommendations are more pessimistic but median recommendations are more optimistic. Long-term earnings growth rate estimates are more pessimistic using both the mean and the median, but the differences between treatment firms and control firms are small. Price targets are similar using both the mean and the median. Overall, the expectations of analysts that cover treatment firms and disappear are similar to the mean expectations of all other analysts that cover treatment firms.

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

IV. Robustness Tests

A. Underwriting Relationships

We examine whether our results can be explained by underwriting relationships. Fernando, May, and Megginson (2012) 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 consider a firm as having an underwriting relationship with a broker if the broker that disappears underwrites an offering during the 3 years ending three months before the broker disappearance date.[22] We consider two types of underwriting relationships: debt and equity. We examine the mean difference-in-differences for each corporate policy variable (as in Table II) for firms with underwriting relationships and firms without underwriting relationships.

Table VII presents the results. Firms without debt or equity underwriting relationships account for 89% of our firms. The results for these firms (the first set of results) are similar to—if slightly smaller in economic magnitude than—the results in Table II. As for the results for firms with underwriting relationships, we interpret them with caution because of the small sample size, but they do provide interesting insights. Firms that lose a debt underwriter substitute equity financing for long-term debt financing (second set of results): they decrease long-term debt issuance by 2.8%, whereas they increase their equity issuance by 0.5%. By contrast, firms that lose an equity underwriter decrease both long-term debt and equity financing (third set of results), by 2.0% and 4.8%, respectively.

We examine not only actual underwriting relationships but also potential underwriting relationships. We now consider a firm to have an underwriting

[21] The results are similar for all 3 years before the broker disappearance date.

[22] The results for 1-year and 5-year windows are similar.

Table VII

Underwriting Relationships

This table presents the change in corporate policies caused by the loss of an analyst conditional upon underwriting relationships. The sample comprises 1,724 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, total assets, Q, cash flow, 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 1 year. For each corporate policy variable, the mean difference-in-differences is computed conditional upon the broker not being an underwriter for the firm, conditional upon the broker being a debt underwriter for the firm, and conditional upon the broker being an equity underwriter for the firm. All corporate policy variables are scaled by total assets. A broker is an underwriter for the firm if it underwrites an offering during the 3 years before the broker disappearance date. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Broker Is Not Underwriter ($N = 1,537$)		Broker Is Debt Underwriter ($N = 136$)		Broker Is Equity Underwriter ($N = 58$)	
Differences-in-Differences	Mean	t-statistic	Mean	t-statistic	Mean	t-statistic
Investment						
Capital expenditures	−0.64%***	−4.35	−1.20%**	−2.12	−1.16%	−1.00
Research and development expenditures	−0.17%*	−1.75	−0.56%**	−2.16	−0.39%	−0.42
Acquisitions expenditures	−0.85%***	−2.82	−2.37%***	−2.88	−1.35%	−0.60
Total investment	−1.71%***	−4.41	−4.20%***	−3.79	−3.92%	−1.21
Financing						
Change in short-term debt	0.00%	0.01	−0.16%	−0.29	0.25%	0.49
Change in long-term debt	−0.81%**	−2.01	−2.77%**	−2.03	−1.98%	−0.58
Equity issuance	−0.95%***	−2.93	0.54%	0.78	−4.76%*	−1.92
Total financing	−1.87%***	−3.54	−2.26%	−1.39	−5.50%	−1.30
Payouts						
Dividends	−0.01%	−0.72	−0.11%	−1.33	0.01%	0.37
Share repurchases	0.09%	0.52	−0.09%	−0.15	0.29%	0.35
Total payouts	0.14%	0.77	−0.35%	−0.54	0.35%	0.40
Change in cash holdings	−1.08%**	−2.42	1.60%*	1.76	−8.31%**	−2.08

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 during the year ending three months before the broker disappearance date. These underwriting relationships do not explain our results either: the results for the 80% of our firms that have such an underwriting relationship are not significantly different from the results for the 20% of our firms without such a relationship. Overall, the results suggest that underwriting relationships affect corporate policies but they do not explain the effect of analyst coverage on corporate policies.

B. 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 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 repeat Table II for the top 15, 20, and 25 brokers ranked by the number of firms that lose analyst coverage, which collectively account for 1,089, 1,307, and 1,455 observations (63%, 76%, and 84% of our sample), respectively. The results are similar. Second, we repeat Table II 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 repeat Table II for the group of brokers that disappear in 2000, 2001, 2002, and 2008 (1,070 observations or 62% of our sample) separately from the group of brokers that disappear in the other years in our sample (654 observations or 38% of our sample). We find that our results are similar for both groups. This is also the case if we use the NBER definitions of economic contractions and economic expansions.

In our second group of robustness tests, we examine how our results are affected by our matching methodology. First, 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 total assets, Q, cash flow, 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 repeat Table II for this sample and present the results in the Internet Appendix. We again find that the results are similar to the results in Table II.

Second, 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 over time using the same control firms as in our main sample. We have one "before" observation and one "after" observation for each treatment firm and 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 total assets, Q, cash flow, and analyst coverage. In the third specification, we add control variables interacted with the "after" dummy variable. We present the results in the Internet Appendix. In each of the three specifications, the results are again similar to the results in Table II.

Third, we consider whether our results are driven by life cycle differences between our treatment firms and control firms. To do so, we compute the ages of our treatment firms and control firms, where age is measured as the number of years since the firm became publicly traded. We find that the age of our treatment firms and control firms is similar: the mean (median) age is 22.3 (14.1) years for our treatment firms versus 22.5 (14.5) years for 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 (54% of our sample) or broker mergers (46% of our sample). We repeat Table II separately for each of these two groups of broker disappearances. We find that our results are similar for both groups. Overall, our results are not driven by either broker closures or broker mergers alone.

V. 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 internal financing, the optimal amount of external financing decreases as well. In short, a decrease in analyst coverage causes a decrease in investment and financing.

The literature already provides empirical evidence that exogenous decreases in analyst coverage cause an increase in information asymmetry and the cost of capital. What we contribute to the literature is 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.

Using a difference-in-differences approach, we find that firms that lose an analyst significantly decrease their 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 intermediaries that significantly affect corporate policies. These results extend our understanding of the real effects of financial shocks.

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Appendix

Table A.1
Details of Corporate Policy, Information Asymmetry, and Financial Constraints Variables

Panel A: Corporate Policy Variables

Variable	Definition in Terms of 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	DLCCH + DLTIS-DLTR + SSTK
Payouts	
Dividends	DV
Share repurchases	PRSTKC
Total payouts	DV + PRSTKC
Change in cash holdings	CHECH

Panel B: Information Asymmetry Variables

– Bid-ask spread: 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
– Amihud illiquidity measure: Mean during the year of the daily absolute value of the stock return divided by the dollar value of trading volume
– Ratio of zero and missing returns days to total days: Number of trading days with zero or missing returns during the year divided by the number of trading days during the year
– Earnings announcement surprise: Mean during the year of the quarterly absolute value of the difference between actual earnings and expected earnings divided by the stock price
– Earnings announcement volatility: Mean during the year of the quarterly volatility of the three-day market reaction to earnings announcements

(*Continued*)

Table A.1—*Continued*

Panel C: Financial Constraints Variables
– Composite proxy for financial constraints based on Almeida, Campello, and Weisbach (2004): Firms are first classified as constrained or unconstrained based on four proxies: dividend payer status, total assets, bond rating status, and commercial paper rating status. Firms that do not pay dividends are classified as constrained and firms that pay dividends as unconstrained. Firms in the bottom half of total assets are classified as constrained and firms in the top half as unconstrained. Firms with debt and no bond rating and firms with debt and a noninvestment grade bond rating are classified as constrained, whereas firms with debt and an investment grade bond rating and firms with no debt and no bond rating are classified as unconstrained. This classification is used for both bonds and commercial paper. 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.
– Firm-level proxy for financial constraints based on 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
– Industry-level proxy for financial constraints based on Rajan and Zingales (1998): Industries in the bottom quartile of the cash flow–investment gap are classified as constrained and industries in the top quartile are classified as unconstrained

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's web site:

Appendix S1: Internet Appendix