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The effect of asymmetric information on product market outcomes[☆]

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ABSTRACT

We explore how asymmetric information in financial markets affects outcomes in product markets. Difference-in-difference tests around brokerage house merger/closure events (which increase asymmetric information through reductions in analyst coverage) indicate worse industry-adjusted sales growth for shocked firms than for their peers. Our results are consistent with Bolton and Scharfstein's (1990) tradeoff between investor agency concerns and predation risk. Further support is found in stronger treatment effects among firms with ex ante greater agency concerns, financing constraints, asymmetric information, and those operating in ex ante more competitive (fluid) product market spaces. Our results are concentrated in industries where we can clearly identify either net firm entry or exit.

1. Introduction

Does a firm's financial information environment affect its outcomes in product markets? The answer to this question is crucial to both the finance and industrial organization literatures. It has potential implications for corporate disclosure policy as well as industry competitive dynamics and equilibria. It also speaks to the relevance of finance for real outcomes. And while theoretical work suggests a link, to date empirical evidence is lacking.

On the theory side, Bolton and Scharfstein (1990) posit that asymmetric information (between investors and managers) makes it difficult for investors to verify actual realized profits.[1] Therefore, greater asymmetric information will cause investors to write optimal contracts designed to prevent resource diversion; however, this necessarily tilts the optimal contract towards inviting greater predation.[2] In particular, when outside investors set tighter performance contingencies to mitigate managerial agency costs, they simultaneously limit the manager's ability to respond to competitive threats, thereby increasing rivals' strategic opportunities to prey.

We test Bolton and Scharfstein's (1990) model, while also recognizing and resolving endogeneity concerns. Specifically, a simple exploration of the relation between

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[1] They provide several examples: whether some expenses represent managerial perquisites, or possible unobserved transfer pricing.

[2] The mechanics of the predation invitation due to an agency-tilted contract, are discussed in detail in Section 2.

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proxies for asymmetric information in financial markets and market share outcomes in product markets is fraught with selection concerns. To illustrate by example, consider less competitive industries. Firms with competitive advantages that create entry barriers are potentially insulated from the adverse effects of asymmetric information on product market outcomes. This could manifest in higher market shares and contemporaneously higher asymmetric information, either because the firm does not find it to be costly (passive higher opacity) or because they actively choose to be opaque precisely to protect competitive advantages.[3] These could lead to a positive observed relation between asymmetric information and market share, despite the theoretically negative one in Bolton and Scharfstein (1990). Indeed, as we show below, the *average* relation between firm asymmetric information and product market share (in a full panel) is positive and significant.

A second endogeneity concern that must be addressed stems from simultaneity bias. In particular, firms with less market share may naturally be harder to understand (e.g., they may be smaller or less well-followed by analysts), implying greater information asymmetry. Thus, even if correlations indicate a negative relation between asymmetric information and product market outcomes, the causality may run in the opposite direction (from product market to financial market). Indeed, recent evidence from the accounting literature suggests that product market characteristics (industry competition) may affect disclosure.[4] All of this suggests that we must utilize an experiment that exogenously shocks asymmetric information in financial markets and study product market share responses to it.

We draw from a large recent literature exploring the causal effects of asymmetric information on various firm and financial market outcomes by studying brokerage house mergers and closures that resulted in analyst coverage declines.[5] Our main tests are difference-in-difference tests, where treated firms are those who lose coverage by at least one analyst due to the brokerage house closure/merger. We compare their change in industry-adjusted sales growth from before to after the event,[6] with the contemporaneous change in market share for a matched sample of untreated firms (matched on firm characteristics likely related to market share outcomes).

We find that firms experiencing a decline in analyst coverage lose market share ex post; their industry-adjusted sales growth declines. Although unshocked peers show univariate declines in industry-adjusted sales growth over the same window, regression tests show no such evidence. More importantly, the change in market share is statistically and economically stronger among treated firms (compared to matched peers) in both univariate and regression tests.[7] The difference-in-differences tests indicate between 4% and 5% worse product market outcomes (market share) due to asymmetric information shocks. Asymmetric information in financial markets negatively affects outcomes in product markets.[8]

Our causal effects are concentrated in logical subsamples: firms where investors face ex ante greater asymmetric information; firms with greater ex ante financial constraints; firms operating in ex ante more competitive product spaces; and firms with ex ante less institutional investor oversight (likely engendering higher agency concerns over resource diversion). Each of these subsample explorations naturally emerges from Bolton and Scharfstein's (1990) theory. We provide details below, but briefly discuss results here.

Our treatment effect (loss of market share because of the analyst shock) is stronger when firms are already followed by fewer analysts and they have more opaque financial statements. In other words, the interpretive value of analysts is high, but there are few of them and the loss of one more is significant. Our results are also concentrated among firms with no payout (dividends or repurchases) or no credit rating. Such results are consistent with the "long-purse" notion that Bolton and Scharfstein (1990) predicate their theory upon. Market share loss by shocked firms is also larger for firms operating in more competitive or *fluid* product spaces (Hoberg, Phillips, and Prabhala, 2014). By construction, these spaces are meant to represent more easily contestable markets (see Baumol, Panzar, Willig, 1982). Predation is easier (less costly) in these cases, so the shock should have a greater effect, ceteris paribus. Finally, we see more pronounced treatment effects in firms with less institutional investor oversight. Chen, Harford, and Li (2007) identify such firms as having greater agency concerns over misuse of resources. Without such concern, the tension in Bolton and Scharfstein (1990) is moot. Overall, heterogeneity in the treatment effect is consistent with implications from Bolton and Scharfstein (1990).

We also explore industry dynamics, segmenting our analysis by whether there is entry or exit in treatment firm industries (by combining Census and Compustat data). The treatment effect is pronounced in industries with either clear indications of entry or clear indications of exit. The latter suggests that shrinking industry revenues are fought over by remaining incumbents, but that treatment firms are compromised by stronger investor conditioning on agency, and this invites predation by unshocked competitors. The former (significant treatment effect in industries with entry) suggests new entrants capture some of the forgiven market share by treated firms.

[3] We use the terms opacity and asymmetric information interchangeably throughout the manuscript.

[4] Tariff reductions (exogenous increases in competition) varyingly increase (Balakrishnan and Cohen, 2014; Young, 2014) or decrease (Lin, Officer, and Zhan, 2014) accounting quality.

[5] The studied outcomes include asset prices (Kelly and Ljungqvist, 2012), analyst bias (Hong and Kacperczyk, 2010), innovation (He and Tian, 2013), corporate investment and financing (Derrien and Kecskés, 2013), financial reporting quality (Irani and Oesch, 2013), and governance outcomes (Chen, Harford, and Lin, 2015).

[6] Which we treat as "market share change," as do Fresard (2010) and Campello (2003).

[7] Also, importantly, our data satisfy the ex ante parallel trends assumption that is required for efficacy of the difference-in-differences approach. See Section 3 for details.

[8] We conduct placebo tests by arbitrarily shifting the event dates by 2 years forward and 2 years back. We find no significant effect for the treated firms in these tests.

Finally, we consider a couple of alternative perspectives. Firms may provide information through other channels such as managerial earnings forecast provision or obtaining a credit rating. However, difference-in-differences tests do not show significant gaps between treatment and peer firms' changes in managerial earnings forecast provision behavior. Nor do we see meaningful differences between treatment and peer firms in the incidence of credit rating procurement. Consistent with no substitute information provision, the coverage shock engenders increases in analyst forecast dispersion about earnings.[9] Equity investors are more compromised in their information sets about relevant cash flows.

We also explore the alternative interpretation that shocks to asymmetric information may raise the cost of capital sufficiently to make investments in market share negative net present value (NPV). However, Hubbard (1998) shows that discount rate increases should optimally lead firms to raise current prices and thus profit margins. We see no evidence of changes in profit margins among treatment firms, nor do these differ from matched peers.

Overall, we offer the following incremental contributions. We illustrate another link between finance and real outcomes; asymmetric information in financial markets plays an important role in competitive product market outcomes. We provide empirical support for Bolton and Scharfstein's (1990) tradeoff between investor concerns over resource diversion and competitor predation. We highlight the difficulty in establishing a causal relationship because of selection and simultaneity concerns, and offer a viable natural experiment to alleviate the concerns thereof. Finally, we illustrate various conditions, both firm-specific and industry-wide, that enhance the treatment effect.

The remainder of this paper is organized as follows. Section 2 discusses Bolton and Scharfstein's (1990) theoretical work linking asymmetric information in financial markets with competitive outcomes in product markets. We highlight key elements of the theory that link to our heterogeneity in treatment effect tests. We also briefly position our work in the empirical literature relating asymmetric information to various other corporate outcomes. We describe our data in Section 3. Results on the relation between asymmetric information and market share (product market outcomes) are presented in Section 4. Section 5 pursues two alternative perspectives; one where asymmetric information shocks may be mitigated by new information provision; the other is the possibility that discount rate rises explain market share losses. We conclude in Section 6.

2. Theoretical underpinnings and related literature

In this section we review the model by Bolton and Scharfstein (1990) and introduce testable implications. We also place our work in the context of other empirical work linking asymmetric information shocks with corporate outcomes.

2.1. Theoretical motivation

The notion that financial constraints enable predation by peers requires a wedge to inhibit capital raising as a response to predatory threat. In other words, why do not investors simply provide financing when predation is a concern? Bolton and Scharfstein (1990) raise this question and show one potential countervailing wedge is the concern that unrestricted resource provision carries diversion (agency) concerns.

They begin with endogenously derived financial constraints. Given investors' resource diversion concerns with managers, it is optimal to commit to funding termination if a firm's performance is poor. However, asymmetric information makes contracting on realized profit imperfect.[10] Thus, the optimal contract imperfectly increases the sensitivity of (re)financing decisions to firm performance. Investors cut off funding too readily in the face of reduced profit outcomes. This increased sensitivity comes with a cost—it enables predation. Competitors recognize that aggressive pricing[11] can lower a prey's profit, which can cause funding termination and the prey's premature exit from the product market space. In turn, the predator benefits from a less competitive product market. Overall, the investor faces a tradeoff. A contract that tilts more towards protecting against resource diversion encourages predation, but more forgiving funding continuation enables greater resource diversion.

Evaluating the empirical validity of this theory is complicated by endogenous relationships. Our use of the natural experiments of brokerage mergers/closures that exogenously reduce analyst coverage has the potential to break that endogenous link. Given evidence (discussed below) that these shocks raise asymmetric information, we can test the theory's implication that heightened agency concerns (driven by the greater difficulties contracting on reported profits) tilt the optimal contract away from addressing predation concerns. In other words, we expect an analyst shock to cause contract tightening by investors and therefore greater predation by the shocked firm's competitors, leading to market share loss for the shocked firm. This forms the basis of our Hypothesis 1 (offered in alternative form).

Hypothesis 1 (H1). Firms that lose coverage (due to brokerage house merger/closure) subsequently lose market share.

The key theoretical tension between agency concerns over resource diversion, and predation concerns, offers another avenue to assess the empirical validity of the model. Specifically, in the absence of agency concerns, investors can tilt the optimal contract completely towards preventing predation; they can (re)finance regardless of firm performance. Thus, the effect of coverage shocks on market share outcomes should only be observed when agency

[9] But we do not find evidence of increased dispersion in *revenue* forecasts.

[10] Given firms' varying expense structures, legal reporting requirements cannot encompass all eventualities. Apparently justifiable expenses may turn out to be managerial perquisites. Or firms may allocate costs to joint venture partners in different ways.

[11] Or some other costly activity such as advertising or product innovation perhaps enabled through research and development (R&D).

concerns are present. Given that agency concerns are likely never irrelevant, we can say more generally that the treatment effect should be more pronounced when agency concerns over resource diversion are heightened. This forms the basis of our Hypothesis 2 (offered in alternative form).

Hypothesis 2 (H2). *Firms with greater agency concerns over resource diversion will lose more market share due to the coverage shock than firms with less agency concern.*

Bolton and Scharfstein (1990) appeal to the "long-purse" literature as motivation for their model. Specifically, the extant literature suggesting cash-rich firms prey on their financially constrained counterparts begs the question of why investors do not provide finance to constrained firms when predation is a concern. While their model answers that question, it also highlights the importance of conditioning on financial constraints. Constrained firms require external resources for investment (including fighting for market share), and so are more sensitive to investor refinancing decisions. The shock should therefore engender worse market share outcomes in this subsample (Hypothesis 3, offered in alternative form).

Hypothesis 3 (H3). Financially constrained firms will lose more market share due to the coverage shock than unconstrained firms.

The tradeoff between predation risk and agency concerns will also depend on the degree of competition the firm faces. To measure the potential for competitive threats, we use the construct of fluidity derived by Hoberg, Phillips, and Prabhala (2014). Fluidity is built to measure the degree to which rivals offer similar products. More fluid (or similar) products suggest that rival threats through mimicry of product (or service) will be more pronounced. This aligns with theories of contestable markets found in Baumol *et al.* (1982), wherein contestability is increasing in ease of entry. In other words, predation risk will be higher (because of easier rival entry) when contestability (i.e., fluidity) is higher. This implies Hypothesis 4 (offered in alternative form).

Hypothesis 4. Firms competing in more "fluid" product spaces will lose more market share due to the coverage shock than firms competing in less "fluid" environments.

Finally, Bolton and Scharfstein's (1990) tension relies on investors' inability to perfectly contract on observable profit. When asymmetric information between these investors and managers is low, the loss of one analyst is unlikely to raise resource diversion concerns among investors as much as when asymmetric information is already elevated. Thus, we have Hypothesis 5 (offered in alternative form).

Hypothesis 5. Firms with ex ante greater asymmetric information will lose more market share due to the coverage shock than firms with less ex ante asymmetric information.

2.2. Evidence on asymmetric information effects in real outcomes

Our work relies on using shocks to analyst coverage as instruments for increases in asymmetric information. This approach was first advocated by Hong and Kacperczyk (2010). They collect a sample of brokerage house mergers and use these to identify plausibly exogenous declines in analyst coverage of firms previously covered by both houses. Then they test whether these declines associate with increases in forecast bias. They do, consistent with reduced information production by analysts.[12] As long as financial statements are not completely transparent, the reduced coverage causes increased asymmetric information between managers and investors.

Hong and Kacperczyk's (2010) experimental design sparked numerous papers using their shock or seeking similar ones. Kelly and Ljungqvist (2012) collect a sample of brokerage house closures that resulted in over 4,000 coverage terminations (affecting over 2,000 firms). Their asset pricing tests also provide evidence that analysts are crucial to information availability.

The two measures of shocks to analyst coverage (brokerage house mergers and closures) are subsequently combined and employed by various papers to study numerous corporate decisions. Derrien and Kecskés (2013) document that capital and acquisition expenditures as well as R&D decline in response to the shocks, as do financing and cash holdings. Irani and Oesch (2013) show that firms increase their earnings management through discretionary accruals in response to such shocks. Chen, Harford, and Lin (2015) show that analysts matter for governance because they monitor other costly forms of managerial behavior. In particular, they show that brokerage closure/merger shocks cause increases in expropriative behavior. Li, Lin, and Zhan (2015) show that treated firms tilt their borrowing away from public debt and towards bank debt, consistent with public debtholders being more sensitive to asymmetric information than bank lenders.[13] Finally, He and Tian (2013) show that reductions in coverage cause increases in innovation, implying a dark side to analyst coverage. However, this inference is questioned by Clarke, Dass, and Patel (2015), who show that the restraining effect of coverage on innovation is limited to those firms with poor innovation efficiency (patents that were never cited). Overall, the ex ante literature supports our use of coverage shocks to instrument asymmetric information increases.

[12] Fong, Hong, Kacperczyk, and Kubik (2014) use the same sample and show that coverage loss associates with more bias in credit ratings, again consistent with analysts providing valuable information to financial market participants.

[13] See also Purnanandam and Rajan (2016).

3. Data and methods

3.1. Baseline sample

We begin by obtaining annual financial data from Compustat during the period of 1981–2011[14] for firms with positive assets, sales, and equity. We exclude financial and utility firms (Standard industrial classification (SIC) codes 6000–6999 and 4900–4999). The analyst coverage data are from the Institutional Brokers' Estimate System (I/B/E/S) database summary file. Stock return data are from the Center for Research in Security Prices (CRSP). After requiring the availability of different measures of market share growth and control variables in our baseline regression, we have a baseline sample with 56,345 firm-year observations.

We define three measures of product market outcomes: *sales growth, market share growth (SIC)*, and *market share growth (FF). Sales growth* equals the percentage change in sales from t-1 through t. It is computed as $(\text{Sale}_t - \text{Sale}_{t\text{-}1})/\text{Sale}_{t\text{-}1}$. *Market share growth (SIC)* equals the above defined sales growth, minus the industry median of the same, where industry is defined by four-digit SIC code. For this construction, we require at least ten firms within the industry-year. This follows both Campello's (2003, 2006) and Fresard's (2010) calculation of market share changes. Third, we estimate *market share growth (FF)* just like the SIC version, but defining industry via the Fama-French 49 industry classification. We again require at least ten firms within the industry-year.

Our explanatory variables are defined as follows. We represent analyst coverage with the average (per month) of the number of analysts providing current-fiscal-year earnings per share (EPS) forecasts during the firm's fiscal year. *LnCoverage* is defined as the natural logarithm of one plus average analyst coverage. Our vector of control variables that could affect market share growth according to Fresard (2010), is represented in our baseline regression model (below) with the vector Z. The controls include *market-to-book* (total assets minus book equity plus market equity, all divided by total assets), *cash* (cash and short-term investments divided by total assets), *lnAssets* (natural logarithm of total book value of assets), and *leverage* (long-term debt plus short-term debt, all scaled by total assets). Considering the endogeneity issue between market share growth and cash holdings (Fresard, 2010), we control for two-year lagged cash holdings in our baseline regression model. Also, following Fresard (2010) we include both one-year lagged leverage and two-year lagged leverage as controls. All variables are defined in the Appendix.

Table 1 presents descriptive statistics for this baseline sample. Many of the variables are ratios, which can take on extreme values when the denominator is small. We therefore winsorize each variable at the 1% and 99% levels and concentrate our discussion on medians. We first observe that while raw percentage sales growth is positive for the median firm in the baseline sample (0.093), it is smaller than own-industry median sales growth (under either definition of industry). Our two *market share growth* (i.e., industry-adjusted sales growth) measures, are −0.004 and −0.007, respectively, for SIC- and FF- based industry definitions. Combined, these two results suggest growing industries in terms of revenue, in the baseline sample.

Our (inverse, level-based) proxy for asymmetric information, *LnCoverage*, is highly skewed, with a mean of 7.1 and median of 4.5. This has implications for our matching procedure described below. Our other control variables are quite close in measure to those in He and Tian (2013). Overall, our baseline sample appears comparable with other work in this general area.

Table 1
Descriptive statistics.
The table presents descriptive statistics for firm-years with positive assets, sales, and equity over the period of 1981–2011. We exclude financial and utility firms (Standard industrial classification (SIC) codes 6000–6999 and 4900–4999). After requiring the availability of different measures of market share growth and control variables in our baseline regression, we have a baseline sample with 56,345 firm-year observations. The variable definitions are detailed in the Appendix.

	N	Mean	Median	Standard deviation
Sales growth	56345	0.156	0.093	0.426
Market share growth (SIC)	56345	0.040	−0.004	0.406
Market share growth (FF)	56345	0.047	−0.007	0.412
Coverage	56345	7.054	4.455	7.082
LnCoverage	56345	1.763	1.696	0.790
LnAssets	56345	5.851	5.631	1.933
Leverage	56345	0.201	0.176	0.179
Market-to-book	56345	1.970	1.463	1.630
Cash	56342	0.185	0.098	0.208
Capital expenditure	56345	0.071	0.050	0.070
R&D	56345	0.053	0.008	0.092
Acquisition	56345	0.022	0.000	0.059
ROA	56222	0.100	0.127	0.171

3.2. Baseline regression model

We use the following ordinary least squares (OLS) regression model to examine the correlation between analyst coverage and firms' market share growth. Given endogeneity concerns, we do not necessarily expect to observe the negative economic relationship hypothesized above between asymmetric information and product market outcomes. In the context of the below regression model, the negative economic relationship would imply a positive β coefficient.

$$\begin{aligned} \textit{Market share growth}_{i,t} &= \alpha + \beta \textit{LnCoverage}_{i,t-1} + \gamma Z_{i,t-1} \\ &+ \textit{Year}_t + \textit{Firm}_i + \varepsilon_{i,t}. \end{aligned} \tag{1}$$

In Eq. (1), i indexes firms and t indexes years. We define the dependent variable in the three different ways described above: *sales growth, market share growth (SIC)*, and *market share growth (FF)*. We also include year, firm, and event fixed effects in our regression model, and standard errors are clustered at the firm level. The results are presented in Table 2. We discuss those results in Section 4.

[14] Our quasi-natural experiment sample is over years 1984 to 2008 and we examine three years before and three years after the event, therefore we define our baseline sample period over years 1981 to 2011.

Table 2
Baseline regression of market share growth on analyst coverage.
The table presents baseline regression results over for the sample described the period of 1981–2011 and in Table 1. All variable definitions are detailed in the Appendix. In all specifications, we control for firm fixed effects and year fixed effects. *T*-statistics are reported in parentheses beneath the coefficients. We report *t*-statistics controlling for firm-clustered standard errors.

Dependent variable	Sales growth (1)	Market share growth (SIC) (2)	Market share growth (FF) (3)	Sales growth (4)	Market share growth (SIC) (5)	Market share growth (FF) (6)
LnCoverage $_{t-1}$	−0.087***	−0.080***	−0.085***	−0.039***	−0.037***	0.044***
	(−13.48)	(−12.88)	(−13.44)	(−5.24)	(−5.25)	(−5.56)
Market-to-book $_{t-1}$				0.052***	−0.040***	0.048***
				(15.04)	(13.33)	(14.14)
Cash $_{t-2}$				0.348***	0.337***	0.344***
				(11.86)	(11.68)	(11.92)
LnAssets $_{t-1}$				−0.079***	−0.071***	−0.073***
				(−10.86)	(−10.03)	(−10.34)
Leverage $_{t-1}$				0.232***	0.250***	0.243***
				(6.94)	(7.65)	(7.37)
Leverage $_{t-2}$				−0.191***	−0.218***	−0.211***
				(−5.45)	(−6.36)	(−6.11)
Sales growth $_{t-1}$				−0.021*		
				(−1.74)		
Sales growth $_{t-2}$				−0.046***		
				(−5.14)		
Market share growth (SIC) $_{t-1}$					−0.022*	
					(−1.84)	
Market share growth (SIC) $_{t-2}$					−0.045***	
					(−4.95)	
Market share growth (FF) $_{t-1}$						−0.020*
						(−1.70)
Market share growth (FF) $_{t-2}$						−0.046***
Constant	0.374***	0.222***	0.238***	0.505***	0.330***	(−5.13)
	(28.89)	(18.05)	(18.98)	(15.99)	(10.71)	(11.16)
Observations	56,345	56,345	56,345	56,345	56,345	56,345
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Cluster by firm	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted *R*-squared	0.176	0.152	0.155	0.219	0.191	0.197

* Statistical significance at the 10% level.
** Statistical significance at the 5% level.
*** Statistical significance at the 1% level.

3.3. Quasi-natural experiments sample

In response to endogeneity concerns in analyzing the effect of analyst coverage on firms' product market outcomes, we adopt the quasi-natural experiments used in Hong and Kacperczyk (2010), Kelly and Ljungqvist (2012), and others. These are brokerage mergers and brokerage closures that generate exogenous variation in firms' analyst coverage. We conduct difference-in-differences tests surrounding the brokerage merger/closure events.

Our treatment sample is a combination of firms affected by brokerage merger events from Hong and Kacperczyk (2010) and firms affected by brokerage merger/closure events from Kelly and Ljungqvist (2012). After excluding four overlapping events from two difference sources, our sample includes 54 merger and closure events from 1984 to 2008. For brokerage merger events which are listed in Hong and Kacperczyk (2010), we obtain the list of firms from Kacperczyk's website,[15] and we exclude sample firms with a stop indicator equal to one (those firms which have a coverage drop prior to the merger event).

For the remaining 39 events (18 acquisitions and 21 closures) covered in Kelly and Ljungqvist (2012), we follow the procedure in He and Tian (2013) to determine firms whose analyst coverage is affected by the merger/closure events. For brokerage closures, we obtain the firms which are covered by the analysts who work for these brokerage houses and who also provide their last earnings forecasts over the window (−15, +3) months around the closure date. In other words, these are analysts who disappear from IBES thereafter. For brokerage mergers, we first select firms that are covered by both the bidder and target brokerage houses over the window (−15, +3) months around the merger date. We then identify firms that lose at least one analyst from either the bidder or target due to the merger event. Specifically, at least one analyst from either the bidder or target provides her last earnings forecast for the firm over the window (−15, +3) months around the merger date. Additionally, if the bidder brokerage house provides its last forecasts for the company over the window (−15, +3) months around the merger date, i.e., stops covering the company after the event, the company is also excluded from the sample (as in He and Tian, 2013).

[15] http://pages.stern.nyu.edu/~sternfin/mkacperc/public_html/~mkacperc.htm.

We examine the effect of exogenous shocks to analyst coverage on market share growth for treatment firms as follows: we average market share growth over the three years before [−3, −1] and three years after [+1, +3] the brokerage merger/closure date, and calculate the difference between the two averages. Because of the difficulty in precisely recognizing the event date (brokerage closures/mergers often span several months), we follow the literature (He and Tian, 2013) in constructing our "event window." Specifically, for test variables measured on an annual basis we identify a 12-month "disappearance period" as six months prior to and after (i.e., symmetrically around) the reported merger/closure date. Given this event year (0), firms included in our treatment sample must also have non-missing matching variables for year −1.

To identify matching firms, we first construct 81 conditionally sorted *SIZE*/*BM*/*MOM*/*NOAN* (market capitalization, book-to-market, momentum, number of analysts) portfolios at the fiscal year-end before the event. The portfolios are constructed by ranking firms into dependent terciles on the basis of the four listed variables, in the order presented. We measure the market capitalization (*SIZE*) at the month-end, three months away from the event date. Book value of equity (the numerator in *BM*) is from the latest fiscal year-end during the window of −15 to −3 months prior to the event date. Momentum (*MOM*) is actually average monthly returns during the window of −15 to −3 months prior to the event date. Number of analysts (*NOAN*) is the average monthly number of analysts during the same window of −15 to −3 months prior to the event date.

We require our candidate control firms to be "unshocked" over the entire product market outcome measurement window of [−3, +3] for the treatment firm. We match our treatment firms with candidate controls in the same *SIZE*/*BM*/*MOM*/*NOAN* portfolio. We then choose the five matching firms with the closest change in sales growth from year −3 to year −1 (Sales growth_{-1} – Sales growth_{-3}). This matching choice is designed to help satisfy the parallel trends assumption that is implicit in the difference-in-differences approach. We further restrict matching firms to have analyst coverage differences between the treatment and control firms (at year −1) of no more than three analysts. This is in deference to the skewness observed in our distribution of analyst coverage numbers. Finally, we average (across these five closest matches) to obtain a 'control measure' for any variable of interest. Overall, we have a matched treatment-control (main) sample with 1,024 pairs.

4. Results

4.1. Baseline regressions

We begin with OLS regressions examining the correlation between analyst coverage (*LnCoverage*) and market share growth. Again, these are panel regressions, subject to significant endogeneity concerns. Table 2 has six columns of results; the first three of which contain only the coverage explanatory variable (but using the three different dependent variables of sales growth and two versions of market share growth), while the last three also include the controls listed above. Under the hypothesis that asymmetric information encourages contracts that tilt away from concerns with predation (because they are more concerned with resource diversion), we expect β to be positive and significant (because *LnCoverage* is inversely related to asymmetric information).

In columns 1–3, the coefficient β is significantly negative; higher coverage associates with worse market share outcomes. This is inconsistent with our main hypothesis and likely due to endogeneity concerns. Perhaps larger increases in market share are only seen among the smallest firms and these are known to be less well-followed by analysts. Or maybe analyst coverage has a dark side like He and Tian (2013) propose, and the lack of innovation compromises eventual growth in market share. Either way, the well-known difficulties in establishing causality via panel regression are likely to be present in Table 2.

These inferences (and associated concerns) persist in columns 4–6. The coefficient on *LnCoverage* remains significantly negative. The coefficients on the controls are consistent with those found in Fresard (2010). Larger firms see smaller market share growth and financial resources (cash) enhance it. Leverage lagged one or two years also has the same direction effect on market share that is seen in Fresard (2010). Overall, our baseline results are largely consistent with both the prior literature and with an endogenous relationship between asymmetric information and product market outcomes.

4.2. Difference-in-differences

To formally test Hypothesis 1 we conduct difference-in-differences tests that alleviate endogeneity concerns. The validity of the experiment relies on limited ex ante heterogeneity between treatment and control firms and also common ex ante time trends in the test variable for both groups. For the former, as long as treatment and control firms are reasonably similar ex ante, it is unlikely that variation in firm characteristics drive either the shock or the treatment outcome. For the latter, parallel trends in the dependent variable mitigate the concern that the outcomes were diverging anyway, regardless of treatment.

Table 3, Panel A speaks to the ex ante similarity in characteristics (between treatment and control firms) assumption. Specifically, we calculate the mean of our matching variables as well as several other firm characteristics, for both treatment and control firms.[16] We then test for significance of the difference in the mean values across the two groups. With one exception, pre-event differences between treatment and control firms are insignificant. This supports our use of the difference-in-differences. The exception is the count variable for analyst coverage. The ex ante mean for treatment firms is 15.1 and the corresponding mean for controls is 14.8. The difference of 0.3 analysts is statistically significant. We submit though, that this is

[16] Some of these characteristics are used in our heterogeneity of treatment effect tests, and are discussed later.

Table 3
Difference-in-differences tests on market share growth.

The table reports results from and diagnostics for difference-in-differences tests on the effect of analyst coverage shocks on firms' market share growth. Our treated sample is a combination of firms affected by brokerage merger events from Hong and Kacperczyk (2010) and firms affected by brokerage merger/closure events from Kelly and Ljungqvist (2012). Our sample includes 54 merger and closure events from 1984 to 2008. For brokerage merger events which are listed in Hong and Kacperczyk (2010), we use the data provided on the author's website[a] and exclude sample firms with a stop indicator equal to one given those firms have a drop in coverage prior to the merger event. For the remaining 39 events (18 acquisitions and 21 closures) covered in Kelly and Ljungqvist (2012), we follow the procedure in He and Tian (2013) to extract firms whose analyst coverage shrinks due to merger/closure events. For brokerage closures, we select firms that are covered by analysts who work for these brokerage houses and who also provide their last earnings forecasts over the window (−15, +3) months around the closure date. For brokerage mergers, we require firms to be covered by both bidder and target brokerage houses over the window (−15, +3) months around the merger date and that they lose at least one analyst from either bidder or target due to the merger event.

To find matching firms, we first construct 81 portfolios based on MKT/BM/RET/NOAN (market capitalization, book-to-market, return, number of analysts) as of the year before the event. Firms must have positive assets, sales, equity, and exist for at least three years in Compustat. The portfolios are constructed by ranking firms into dependent terciles on the basis of market capitalization, book-to-market, return, number of analysts in that order. These four measures are defined in the variable definition Appendix. We require candidate control firms to be firms unaffected by the exogenous shock of analyst coverage and with no missing variables three years before and three years after the merger/closure event for different measures of market share growth. We match our treated firms with candidate controls in the same MKT/BM/RET/NOAN portfolio, then choose the top five matching firms with closest change of sales growth from year $t-3$ to year $t-1$ (Sales growth $t-1$–Sales growth $t-3$). We further restrict analyst coverage difference between treatment and control firms at year $t-1$ to be within three analysts.

We present the pre-event characteristics for treatment and control firms, and their differences, in Panel A. We present difference-in-differences test results in Panel B. All variables are detailed in the Appendix. Event-clustered t-statistics are reported in parentheses.

Panel A: pre-event differences

	N	Treat	Control	Differences	T-statistics
Market capitalization	1024	9224	10940	−1716	−0.88
Book-to-market	1024	0.489	0.483	0.006	0.59
Return	1024	0.016	0.015	0.000	0.41
Coverage	1024	15.112	14.804	0.308	7.41***
LnAssets	1024	7.592	7.464	0.129	1.34
Leverage	1021	0.205	0.193	0.013	0.98
Cash	1023	0.168	0.149	0.019	1.14
Capital expenditure	1024	0.067	0.072	−0.005	−1.45
R&D	1024	0.039	0.039	0.001	0.11
Acquisition	1024	0.028	0.027	0.001	0.42
ROA	1021	0.146	0.156	−0.010	−1.64
Payout	1024	0.747	0.732	0.015	0.60
Credit rating	1024	0.548	0.531	0.018	0.49
Opacity (low coverage subsample)	206	0.301	0.354	−0.053	−1.35
Fluidity	773	0.453	0.468	−0.016	−0.43
Independent, long-term, dedicated, top 5 inst. holding	1023	0.396	0.384	0.012	0.60

Panel B: difference-in-differences (DiD) estimators

	N	Mean treatment difference (after–before)	Mean control difference (after–before)	Mean DiDs (treat-control)
Sales growth	1024	−0.125***	−0.079***	−0.045***
		(−6.07)	(−6.62)	(−2.77)
Market share growth (SIC)	1024	−0.083***	−0.030***	−0.054***
		(−6.88)	(−3.76)	(−4.80)
Market share growth (FF)	1024	−0.089***	−0.038***	−0.051***
		(−6.53)	(−4.60)	(−3.98)

[a] http://pages.stern.nyu.edu/~sternfin/mkacperc/public_html/~mkacperc.htm.
* Statistical significance at the 10% level.
** Statistical significance at the 5% level.
*** Statistical significance at the 1% level.

economically small. Moreover, the significance of the difference is driven by high coverage firms. As we show below, our results are pronounced among low coverage firms (with more opaque financial statements), so the slight (though significant) difference in ex ante coverage is arguably of limited concern.

To test the parallel trends assumption, we point to Figs. 1–3. For each of the market share change variables we see the pre-event trend is similar for treatment and control firms. Specifically, the difference in market share changes between treatment and control firms is flat over years [−3, −1].[17] We also see preliminary evidence of causal declines in market share due to asymmetric information shocks between [−1, +1], and then the trends mirror each other afterwards (difference is flat in [2, 3]). Panel B of Table 3 formalizes the hinted- at causal effect seen in Figs. 1–3.

Our univariate difference-in-differences test results are presented in Panel B. We begin with the raw percent-

[17] Statistical tests confirm the lack of significant differences.

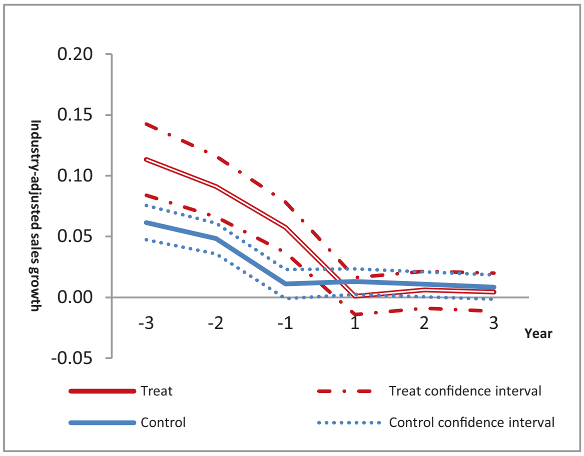

Fig. 1. Market share growth (SIC). The sample is described in the legend of Table 3. The figure shows industry-adjusted sales growth, by year for treatment and control firms. Industry is defined by four-digit SIC code. Dotted lines define two standard errors away from the mean.

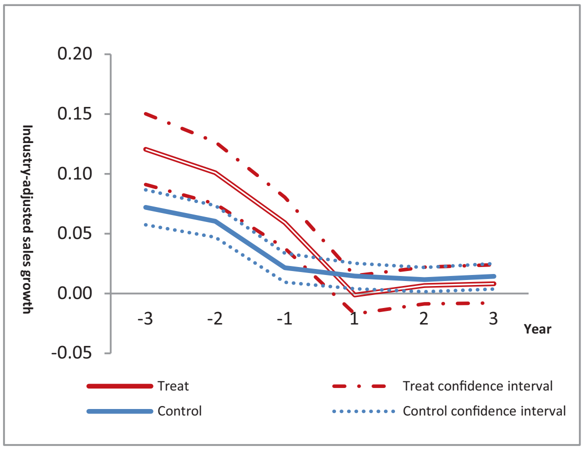

Fig. 2. Market share growth (FF). The sample is described in the legend of Table 3. The figure shows industry-adjusted sales growth, by year for treatment and control firms. Industry is defined by FF-49. Dotted lines define two standard errors away from the mean.

age sales growth numbers. On average, treated firms lose 12.5 percentage points of sales growth, while their peers (matched on *SIZE*/*BM*/*MOM*/*NOAN*) lose 7.9 percentage points. The difference between these time-series changes is roughly 4.5% and significant at the 1% level. In short, sales growth slows significantly due to the shock. But does this translate into declines in *industry-adjusted* sales growth, especially relative to matched peers?

Our market share change variable is industry (median)-adjusted sales growth. For the average treatment firm, we see a decline in the firm's sales growth relative to their industry's median firm sales growth, of 8.3% (8.9%) when industry is defined using SIC code (FF-49). This indicates treated firms lose market share.

But how does that compare with industry-adjusted sales growth changes among controls that are matched on SIZE/BM/MOM/NOAN? For each of the market share change variables, we see larger declines in market share (industry-

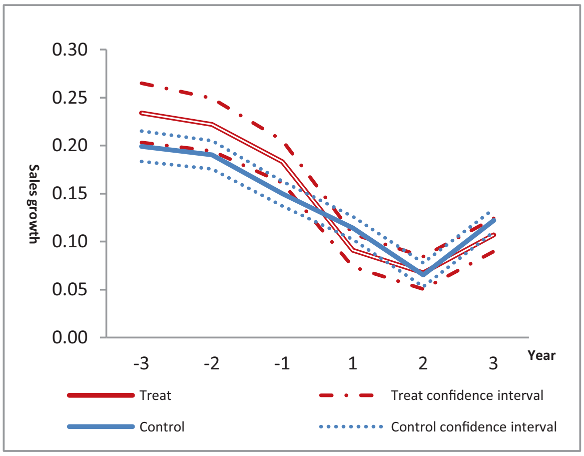

Fig. 3. Sales growth. The sample is described in the legend of Table 3. The figure shows (unadjusted) sales growth, by year for treatment and control firms. Dotted lines define two standard errors away from the mean.

adjusted sales growth) among treated firms than among matched control firms. When the industry is defined by four-digit SIC code, treatment firms lose 8.3% market share while control firms lose 3%. The difference-in-differences shows a 5.4% gap in lost market shares; the shock to asymmetric information causes 5.4% worse industry-adjusted sales growth than we would otherwise expect to occur (what we observe among matched firms). When industry is defined via FF-49, the difference-in-differences estimator indicates a 5.1% gap in lost market shares. Overall, shocks to coverage that increase asymmetric information *cause* declines in market share, consistent with our Hypothesis 1.

4.2.1. Difference-in-differences regressions

As a further control for alternative underlying mechanisms besides asymmetric information shocks, we conduct difference-in-differences regressions. These are presented in Table 4. For these tests we create two dummy variables—*Treat* and *Post*—that are defined as follows:

Treat = 1 for all firms that are in the treatment group, regardless of time.

Post= 1 for all firms, in the three years following any shock, regardless of treatment.

These two variables allow us to define the interactive variable *Treat***Post* equal to one for treatment firms in the (three-year) post-shock window.

We include the two stand-alone dummies as well as the interactive in a panel regression very similar to that found in Table 2. Besides the inclusion of these three new variables, the other main differences in the two tables' setups are the sample and lag structure of the variables. Table 4 samples only on treatment and control firms, so as to highlight the difference-in-differences nature of the tests. Because of this, some of the lags of variables seen in Table 2 are no longer necessary. Specifically, we only include single lags of cash and leverage, and we do not include any lags of the dependent variable. In prior work

Table 4
Difference-in-differences regression tests controlling for firm characteristics and fixed effects.
The table presents regression-based difference-in-differences test results controlling for firm characteristics and fixed effects. The sample includes the treated firms as well as each control firm using the sample criteria described in Table 3. All variable definitions are detailed in the Appendix. We control for firm fixed effects, year fixed effects, and event fixed effects. Firm-clustered *t*-statistics are reported in parentheses beneath the coefficients.

Dependent variable	Sales growth (1)	Market share growth (SIC) (2)	Market share growth (FF) (3)	Sales growth (4)	Market share growth (SIC) (5)	Market share growth (FF) (6)
Treat*Post	−0.038**	−0.044***	−0.041***	−0.039***	−0.045***	−0.041***
	(−2.49)	(−3.18)	(−2.88)	(−2.75)	(−3.41)	(−3.12)
Treat	0.023*	0.029**	0.023*	0.030**	0.035***	0.030**
	(1.75)	(2.51)	(1.93)	(2.42)	(3.11)	(2.57)
Post	0.011	0.018	0.012	0.028**	0.032**	0.027**
	(0.83)	(1.42)	(0.91)	(2.19)	(2.58)	(2.16)
LnAssets_{t-1}				−0.126***	−0.106***	−0.114***
				(−7.88)	(−6.94)	(−7.52)
$\text{Market-to-book}_{t-1}$				0.035***	0.026***	0.030***
				(6.88)	(5.50)	(6.28)
Cash_{t-1}				0.112	0.115*	0.112
				(1.51)	(1.67)	(1.61)
Leverage_{t-1}				0.083	0.083*	0.083
				(1.56)	(1.71)	(1.64)
Constant	0.245***	0.092***	0.101***	0.826***	0.577***	0.628***
	(15.98)	(6.17)	(6.64)	(10.29)	(7.54)	(8.21)
Observations	22,834	22,834	22,834	22,834	22,834	22,834
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Cluster by firm	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted *R*-squared	0.230	0.188	0.185	0.286	0.233	0.237

* Statistical significance at the 10% level.
** Statistical significance at the 5% level.
*** Statistical significance at the 1% level.

that uses full panel regressions (see Fresard, 2010), the additional lags are included in an attempt to mitigate endogeneity problems. Our difference-in-differences design already controls for endogeneity.

We present six regressions in Table 4, mirroring the layout in Table 2. Specifically, the first three regressions only include the variables *Treat*, *Post*, and *Treat**Post*. The latter three regressions include these as well as the other controls. Each regression in a trio varies by the dependent variable. Again, we include firm and year fixed effects, and we cluster standard errors at the firm level.[18]

The results uniformly confirm our earlier inferences. The coefficient on *Treat**Post* is significantly negative in all six specifications, and it is remarkably stable. The loss of market share is significantly worse among treated firms in the post-event window, and the economic magnitude is roughly 4%. Interestingly, the effects of cash and leverage appear weaker in this difference-in-differences regression framework than in the full panel regressions of Table 2. Perhaps recognition and control of asymmetric information explains some of the prior results documented in the literature.

We conduct placebo tests to ensure our results are actually due to the event. For these tests we arbitrarily move the event window forward two years and then (in separate tests) back two years and re-estimate the regressions in Table 4. This results in 12 regressions (two placebo windows with six specifications each). Out of the 12 regressions, we estimate an insignificant coefficient on *Treat**Post* in 11 cases, and one significant coefficient with a *t*-statistic of −1.71. We interpret these tests as indicating that our results are indeed driven by the analyst loss event and not due to other factors like systematic differences between treatment and control firms.[19] Overall, these results further corroborate that asymmetric information in financial markets has significant effects on outcomes in product markets.

The economics of the market share outcomes merit discussion. First, it is important to highlight that we are focusing on industry-adjusted sales growth; negative values for the dependent variable imply the industry median firm had greater percentage sales growth than the "observation" firm (treated or matched peer) during that window. But as we saw in Table 1, the baseline sample shows positive sales growth, suggesting growing industries. So the negative coefficient on *Treat**Post* need not imply reduced sales due to the shock. Rather it indicates that industry-adjusted sales growth was slower than it otherwise would have been without the shock.

[18] Our results are also robust to including industry-time fixed effects. These vary by industry and year, and capture time-varying industry fixed effects.

[19] Additionally, given the sparse sample of analyst shocks prior to 1997, we re-estimate these regressions sampling over 1997–2011. Our results are robust.

Second, the stability in coefficients on *Treat*Post* (roughly −4%) and their proximity to the univariate results in Panel B of Table 3, suggest a robust economic effect. From Table 1 we see the average and median *Sales growth* is 15.6% and 9.3%, respectively. Thus the −4% treatment effect equates to a large fraction of the typical firm's sales growth.

4.3. Alternative explanations

One concern with our inferences based on the above results is that peer firms show significant market share change in the univariate tests. Despite the fact that these are statistically smaller than the market share changes for treatment firms, doubt is raised by the simple fact that the control group also appears to be affected by the shock.

Table 5 Panel A explores this possibility further. We run regressions similar to those in Table 4, but using only the control firm observations. Since we sample strictly on control firms, we need not include the treatment dummy nor its interactive with the post dummy. All we need to include to ascertain whether the control firms are (indeed) affected by the shock, is the post dummy. We find that the coefficient on it is always *positive*, and it is insignificant in all but one specification. We therefore conclude that the univariate negative effect of the shock on control firms is not robust. It appears the shock only affects treatment firms.

Another alternative explanation for our results is that the shock affects treatment firms' control variables, and the changes in control variables cause the loss in market share. To tests this alternative, we run regressions similar to those in Table 4, but the dependent variable is one of our four control variables (assets, market-to-book, cash, or leverage). Again, the explanatory variable of interest is the interactive *Treat*Post*. Table 5 Panel B specifications 1 through 4 only include regressors for this interactive, its two components as stand-alone dummies, and an intercept. Specifications 5 through 8 additionally include lagged dependent variables as regressors.

In all but one specification, the coefficient on *Treat*Post* is insignificant. The sole case of significance is when the dependent variable is leverage (with no lagged dependent variables as controls), and the coefficient is positive. In this case, treated firm leverage appears to increase because of the shock. However, this is of little concern since our earlier results (Table 4) indicate that leverage has a positive effect on market share—not negative. In other words, the effect of changes in the control variable leverage due to the shock appears to counteract our main result that the shock to asymmetric information causes reductions in market share.

4.4. Heterogeneity in the treatment effect

This section explores the testable implications in Hypotheses 2–5 (H2–H5). Specifically, we test whether our treatment effect is more pronounced when agency concerns are high, when financial constraints are present, when competition is more pronounced, and when asymmetric information concerns are elevated. We present univariate triple-diffs, wherein we compare the difference-in-differences results for two subsamples created by sampling on each of the above four general constructs.[20]

4.4.1. Conditioning on agency (H2)

Chen, Harford, and Li (2007) find that firms with more oversight from select institutional owners show greater agency concerns.[21] To identify this select monitoring group, they begin by picking the top five institutional owners of a firm. They further condition on only those institutional owners (among the top five) that are also independent, dedicated, and long-term. Finally, firms are ranked into quintiles according to total ownership by these institutions. Firms in the top quintile of such ownership are deemed to have the lowest agency concerns (greatest institutional investor oversight).

We impute this quintile cutoff (conditional on independent, long-term, dedicated ownership) in our own sample, and find that it translates into ownership of roughly 10% of shares outstanding. Therefore, we form two groups of firms above and below this cutoff. There are 619 treatment observations with less than 10% ownership by independent, long-term, and dedicated institutional owners, and 405 firms with greater than 10%.[22] We expect more pronounced treatment effects in the subsample of 619 (less institutional investor oversight).

The results are reported in Panel A of Table 6. We see the treatment effect is statistically and economically stronger for the low institutional monitoring sample (−7%) compared to the high monitoring sample (-2%). The economic difference in market share loss is roughly 5%, significant at the 1% level. The stronger treatment effect among firms with less institutional investor oversight is consistent with Bolton and Scharfstein (1990). Given that the tension between agency concerns and predation concerns drives their model, greater institutional monitoring will lead to less agency concerns and a weaker treatment effect.

4.4.2. Conditioning on financial constraints

Given the "long-purse" motivation in Bolton and Scharfstein (1990), we create subsamples of treatment firms based on whether they are financially constrained or not, to formally test H3. Extant literature offers numerous proxies for financial constraints. We focus on existence of payout and credit rating because of Bolton and Scharfstein's (1990) emphasis on financing. Positive payout suggests reduced need for it. Existence of a credit rating suggests access to multiple capital markets. Both are indicators of less financially constrained firms. H3 predicts stronger treatment effects among their counterparts (financially constrained firms).

Panel B-1 of Table 6 presents difference-in-differences results for the two subsamples of positive and zero payout treatment firms. Positive payout firms show weak to

[20] Regression tests (available upon request) indicate robustness, except when constraints are proxied with absence of credit rating.

[21] Specifically, they show that these firms are less likely to divert resources on poor acquisition decisions.

[22] Note that our relatively large proportion of treatment observations (40%) with high institutional investor oversight is likely due to the selection effect of requiring analyst coverage. This typically results in larger firms that have greater institutional ownership.

Table 5
Difference-in-differences regression tests for controlling firms and firm characteristics.
The table presents regression tests based on the difference-in-differences sample for control firms in Panel A and for firm characteristics of both treated and control firms in Panel B. The sample is obtained as described in Table 3. All variable definitions are detailed in the Appendix. We control for firm fixed effects, year fixed effects, and event fixed effects. Firm-clustered t-statistics are reported in parentheses beneath the coefficients.

Panel A: regression for controlling firms

Dependent variable	Sales growth (1)	Market share growth (SIC) (2)	Market share growth (FF) (3)	Sales growth (4)	Market share growth (SIC) (5)	Market share growth (FF) (6)
Post	0.003	0.012	0.005	0.022	0.027**	0.022
	(0.20)	(0.90)	(0.35)	(1.56)	(2.07)	(1.61)
LnAssets_{t-1}				−0.112***	−0.092***	−0.103***
				(−5.73)	(−4.98)	(−5.54)
$\text{Market-to-book}_{t-1}$				0.037***	0.028***	0.032***
				(5.83)	(4.92)	(5.43)
Cash_{t-1}				0.043	0.051	0.042
				(0.50)	(0.63)	(0.52)
Leverage_{t-1}				0.098	0.106*	0.105
				(1.44)	(1.69)	(1.62)
Constant	0.247***	0.084***	0.095***	0.745***	0.494***	0.555***
	(13.05)	(4.55)	(4.89)	(8.15)	(5.70)	(6.41)
Observations	16,776	16,776	16,776	16,776	16,776	16,776
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Cluster by firm	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.247	0.208	0.200	0.295	0.246	0.244

Panel B: regression for firm characteristics

Dependent variable	LnAssets (1)	Market-to-book (2)	Cash (3)	Leverage (4)	LnAssets (5)	Market-to-book (6)	Cash (7)	Leverage (8)
Treat*Post	−0.044	−0.085	−0.007	0.010*	−0.010	−0.069	−0.004	0.002
	(−1.60)	(−1.06)	(−1.35)	(1.73)	(−0.99)	(−1.29)	(−1.24)	(0.62)
Treat	0.096**	0.133	0.006	−0.012*	0.008	0.151***	0.002	−0.002
	(2.46)	(1.53)	(0.97)	(−1.83)	(0.68)	(2.71)	(0.70)	(−0.69)
Post	0.043***	−0.046	−0.005	0.003	0.004	0.074	−0.001	0.003
	(2.77)	(−0.77)	(−1.15)	(0.74)	(0.37)	(1.39)	(−0.31)	(0.93)
LnAssets_{t-1}					0.814***	−0.747***	−0.021***	0.003
					(90.79)	(−9.33)	(−5.74)	(0.84)
$\text{Market-to-book}_{t-1}$					0.048***	0.286***	0.001	−0.002**
					(11.78)	(8.70)	(0.73)	(−1.99)
Cash_{t-1}					−0.080*	0.517	0.4***	−0.033***
					(−1.88)	(1.60)	(26.48)	(−3.08)
Leverage_{t-1}					−0.249***	−0.321*	−0.035***	0.572***
					(−6.43)	(−1.83)	(−2.83)	(38.45)
Constant	5.499***	1.556***	0.153***	0.140***	1.167***	5.004	0.196***	0.051***
	(67.06)	(14.69)	(16.29)	(11.69)	(22.51)	(10.95)	(9.59)	(2.85)
Observations	22,834	22,784	22,832	22,803	22,834	22,784	22,832	22,803
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster by firm	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.950	0.524	0.805	0.747	0.985	0.618	0.856	0.836

* Statistical significance at the 10% level.
** Statistical significance at the 5% level.
*** Statistical significance at the 1% level.

insignificant treatment effects, between 0.5% and 2% market share losses compared to control firms. Zero payout firms show strong treatment effects between 15% and 16%. The difference between the two subsamples' difference-in-differences (i.e., the triple-diff) is significant at the 1% level.

Panel B-2 of Table 6 focuses on subsamples of treatment firms with or without a credit rating. Rated firms show significant treatment effects of roughly 3%. Unrated firms show more than twice the effect, between 8% and 8.5%. The triple-diff indicates unrated (more constrained) firms experience stronger treatment effects.

4.4.3. Conditioning on competition

Bolton and Scharfstein's (1990) tension between agency and predation concerns requires a potential competitive threat. Baumol, Panzar, and Willig, (1982) characterize perfectly contestable markets (where potential new entrants can serve the same market demands, without restriction)

Table 6

Difference-in-differences estimators and the firm's pre-event environment.

Panel A reports the difference-in-differences test results on market share growth when conditioning on pre-event institutional holdings. The sample is the same as described in Table 3. The panels present the effect of change in firm's market share growth around the event of brokerage merger/closure for firms with good or bad governance. Good and bad governance are defined based on whether the top-five independent-long-term-dedicated institutional ownership is greater than or equal to 10% in Panel A. Panel B reports the difference-in-differences test results on market share growth when conditioning on pre-event financial constraints. The proxy used for financial constraints is payout in Panel B-1 and credit rating in Panel B-2. In Panel B-1, financially constrained firms are defined as firms with zero payout. In Panel B-2, financially constrained firms are defined as those without an S&P long-term credit rating. Payout and credit rating are explained in the Appendix. Panel C reports the difference-in-differences test results on market share growth when conditioning on pre-event competition. High and low competitions are defined as having above/below the median product market fluidity as defined by Hoberg, Phillips, and Prabhala (2014). See the Appendix for further definitions. Panel D reports the difference-in-differences test results on market share growth when conditioning on pre-event analyst coverage less than or equal to ten and based on whether the firm's Opacity is high or low. High/low is defined as having a value of Opacity above/below the median value of Compustat firms, excluding financials and utilities, in a given year. Opacity is explained in the Appendix. Event-clustered *t*-statistics for Panels A, B, C and *t*-statistics for Panel D are reported in parentheses.

Panel A: difference-in-differences estimators conditional on independent-long-term-dedicated institutional ownership (≥10%)

	N	Sales growth	Market share growth (SIC)	Market share growth (FF)
Bad governance (low ownership)	619	−0.071***	−0.074***	−0.071***
		(−3.59)	(−5.01)	(−4.37)
Good governance (high ownership)	405	−0.006	−0.024*	−0.021
		(−0.38)	(1.82)	(−1.46)
Difference		−0.065***	−0.050***	−0.050***
		(−3.60)	(−2.96)	(−2.88)

Panel B-1: difference-in-differences estimators conditional on payout dummy

	N	Sales growth	Market share growth (SIC)	Market share growth (FF)
Financially constrained (without payout)	259	−0.16***	−0.154***	−0.159***
		(−4.12)	(−4.42)	(−4.57)
Financially unconstrained (with payout)	765	−0.004	−0.020*	−0.015
		(−0.31)	(−1.89)	(−1.25)
Difference		−0.161***	−0.134***	−0.144***
		(−3.76)	(−3.51)	(−3.83)

Panel B-2: difference-in-differences estimators conditional on credit rating dummy

	N	Sales growth	Market share growth (SIC)	Market share growth (FF)
Financially constrained (without credit rating)	462	−0.084***	−0.085***	−0.079***
		(−3.30)	(−4.06)	(−3.69)
Financially unconstrained (with credit rating)	562	−0.013	−0.028***	−0.029*
		(−0.71)	(−3.30)	(−1.75)
Difference		−0.071**	−0.057**	−0.050*
		(−2.41)	(−2.29)	(−1.91)

Panel C: difference-in-differences estimators conditional on competition

	N	Sales growth	Market share growth (SIC)	Market share growth (FF)
High fluidity	353	−0.116***	−0.114***	−0.115***
		(−3.70)	(−4.78)	(−4.80)
Low fluidity	425	−0.017	−0.027*	−0.025
		(−0.82)	(−1.81)	(−1.35)
Difference		−0.098**	−0.087***	−0.090***
		(−2.51)	(−2.84)	(−2.75)

Panel D: difference-in-differences estimators conditional on opacity (within low coverage subsample)

	N	Sales growth	Market share growth (SIC)	Market share growth (FF)
High opacity	63	−0.110**	−0.095*	−0.103*
		(−2.02)	(−1.90)	(−1.98)
Low opacity	144	0.047**	0.019	0.036*
		(2.25)	(0.99)	(1.90)
Difference		−0.157***	−0.114**	−0.139**
		(−2.71)	(−2.13)	(−2.51)

* Statistical significance at the 10% level.
** Statistical significance at the 5% level.
*** Statistical significance at the 1% level.

as highly competitive. Hoberg, Phillips, and Prabhala (2014) construct a product *fluidity* measure to empirically capture this contestability. We use *fluidity* to define high vs. low competitive threat subsamples of treatment firms. According to H4, we expect stronger treatment effects among firms operating in more fluid product spaces.

Panel C of Table 6 presents difference-in-differences results for subsamples of treatment firms operating in more and (separately) less fluid product market spaces. The sample of firms operating in more competitive spaces shows significant treatment effects on the order of 11.5%. There is no significant treatment effect among firms operating in less fluid markets. The difference between the two (i.e., triple-diff) is significant, consistent with H4.

4.4.4. Conditioning on asymmetric information

The importance of the analyst shock to financial contracting (and thus predation risk) depends on investor concerns about resource diversion. These concerns are pronounced when asymmetric information is elevated. More transparent financial statements reduce such concerns, as does a large analyst following. Given this substitutability between ample analyst coverage and transparent financial reports, we characterize elevated asymmetric information concerns as situations when firms are less well-followed *and* have more opaque financial statements.[23]

To test whether treatment effects are stronger under higher ex ante asymmetric information, we condition on low analyst following (number of analysts less than or equal to ten), and then form subsamples based on financial statement opacity. Following H5, we expect treatment effects to be stronger in the low analyst coverage high opacity (of financial statements) subsample, than in the low coverage low opacity subsample.

Table 6 Panel D presents our results. Among low coverage firms with more opaque financial statements, the treatment effect is large (market share losses between 9.5% and 11%) and significant. By contrast, low coverage low opacity firms show rather weak treatment effects (and market share gains if anything). The triple-diff indicates much worse market share outcomes for the high asymmetric information subsample. Overall, the results in Table 6 corroborate the model by Bolton and Scharfstein (1990) and support H2–H5. Treatment effects are pronounced where the theory predicts.

4.5. Industry entry and exit, and their influence on treatment effects

Shocked firms may lose market share to either incumbents or new entrants. Discerning between the two is complicated by the lack of individual firm-level data on private firms' revenues. However, we can begin to analyze industry dynamics and their influence on market share by sorting our sample into four groups that broadly measure entry or exit or mixed entry/exit by private and public firms generally.

To facilitate this analysis, we augment our Compustat sample with Census Bureau data. From 1998 to 2012 we obtain data on the total number of firms in each North American Industry Classification System (NAICS) industry. Given the number of Compustat firms[24] in that industry, we can infer the number of private firms in the industry. Then we (separately) calculate—using only private or only public firm counts—the percentage change in number of private and public firms in the industry, around the event. Specifically, the percentage change is calculated as the difference between the three-year average number of public (or private) firms after the event and the three-year average number of public (private) firms prior to the event, divided by the three-year average number prior to the event.

Given this information, we can now form groups to detect industry entry or exit. We label these groups one through four and define them as follows:

1. Clear exit; when the percentage change in private firms and the percentage change in public firms are both below their respective sample medians (across all events)
2. Public entry, private exit; when the percentage change in public firms is above the sample median, but the percentage change in private firms is below the sample median
3. Private entry, public exit; when the percentage change in private firms is above the sample median, but the percentage change in public firms is below the sample median
4. Clear entry; when the percentage change in private firms and the percentage change in public firms are both above their respective sample medians.

We use above and below the median percentage growth rates in number of firms, rather than simple increases or decreases to indicate entry or exit because most industries show increases in firms during our sample period. Following our approach allows us to have meaningful sample sizes in each group. Even still, we lose substantial observations to lack of Census Bureau data.

4.5.1. Results

Table 7 Panel A presents difference-in-differences estimates of market share changes due to coverage shocks, for the above four subsamples.[25] Groups one and four—those with clear indications of either exit or entry—show significant treatment effects (about 7% each). Neither of the "mixed" groups show this. The market share losses due to asymmetric information shocks are concentrated in observations where the firm's industry was either clearly shrinking or clearly growing.

What does this mean? In the case of group one (clear exit from the industry), we conclude that shrinking markets lead incumbents to fight over limited share. Given fixed adjustment costs (to either capital or labor), it may

[23] We construct financial statement opacity using the technique of Lee and Masulis (2009). We describe our calculations in the variable definition Appendix.

[24] Which we treat as public firms like Badertscher, Shroff, and White, (2013) do.

[25] Given Census Bureau sorting of industries based on NAICS, we add another difference-in-differences test using market share change calculated as industry-adjusted sales growth using four-digit NAICS to form the industry. These results are reported in the last column of Table 7 Panel A.

Table 7
Entry and exit.
The table presents the difference-in-differences (DiD) test results on market share growth around the event of brokerage merger/closure conditioning on industry entry or exit in Panel A and presents the difference-in-differences test results on changes in industry revenues around the event for industries with clear entry and clear exit in Panel B. The sample is the same as described in Table 3 with the additional restriction that the observations can be mapped to the census data which we use to calculate the following additional variables. For each four-digit NAICS industry, the percentage change of firms around the event is calculated as the three-year average number of firms after the event, minus the three-year average number of firms before the event, all divided by the three-year average number of firms before the event. We calculate the percentage change of firms for both public firms and private firms within the four-digit NAICS industry. In our difference-in-differences sample, if the percentage change of firms around the event is above the median, then the group is classified as high. If the percentage change of number of firms is below the median, then the group is classified as low. When both private and public firm percentage changes around the event are high, we classify the industry as a clear entry industry. When both private and public firm percentage changes around the event are low, we classify the industry as a clear exit industry. In Panel B, we examine the percentage change of industry revenues around the event for clear entry and clear exit industries. See the Appendix for further definitions. *t*-statistics are reported in parentheses.

Panel A

		N	Sales growth	Market share growth (SIC)	Market share growth (FF)	N	Market share growth (NAICS)
Private firms change (%) low	Public firms change (%) low	129	−0.074***	−0.074***	−0.070***	125	−0.090***
			(−2.74)	(−2.85)	(−2.67)		(−3.04)
	Public firms change (%) high	114	−0.012	0.013	−0.003	106	0.010
			(−0.43)	(0.53)	(−0.14)		(0.37)
Private firms change (%) high	Public firms change (%) low	113	−0.028	−0.048	−0.041	110	−0.042
			(−0.59)	(−1.05)	(−0.88)		(−0.94)
	Public firms change (%) high	130	−0.072***	−0.079***	−0.070***	128	−0.078***
			(−2.71)	(−3.10)	(−2.70)		(−2.93)

Panel B

Private firms change (%) low and public firms change (%) low

Census firm revenues (NAICS industry)

		Treatment firms	Control firms	DiD (treat − control)
Percentage change of revenues ((after−before)/before)	65	9.041***	24.192***	-15.152***
		(2.89)	(8.63)	(−4.13)

Private firms change (%) high and Public firms change (%) high

Census firm revenues (NAICS industry)

		Treatment firms	Control firms	DiD (treat − control)
Percentage change of revenues ((after−before)/before)	71	40.797***	24.777***	16.020***
		(10.40)	(14.08)	(3.92)

* Statistical significance at the 10% level.
** Statistical significance at the 5% level.
*** Statistical significance at the 1% level.

be optimal to expend resources to preserve or even gain market share. The asymmetric information increase leads investors to tighten performance contingencies on shocked firms, and competitors optimally prey more. There is no entry of new competitors taking up the forgiven market share of the shocked firm; it is strictly incumbent predation.

In the case of group four (clear entry into the industry), the ex ante theoretical effect is perhaps less clear. To wit, there are potentially competing effects at work. New entry can represent a new competitive (predation) threat to the shocked firm. The usual Bolton and Scharfstein (1990) inference would apply here. However, there is an alternative. New entry may be procyclical (Bustamante and Donangelo, 2015)—the industry is growing. The additional investment opportunities may draw potential predators' attention and resources away from preying on the shocked firm. Thus it is an empirical question, which effect dominates. The data tell us that Bolton and Scharfstein's (1990) implication holds sway— shocked firms lose significant market share. Overall, analysis of entry and exit confirms the empirical content of the theoretical tradeoff between agency and predation concerns.

Underlying the above logic is the presumption that industries with clear exit are shrinking industries, while industries with clear entry are growing ones. We test this presumption in Table 7 Panel B. Again using Census Bureau data, we obtain industry receipts data. These data are only available in 2002, 2007, and 2012 around our sample period. To fill in the missing years, we interpolate assuming an even geometric growth rate year-over-year between actual data points for industry receipts. Even still, our sample shrinks further because the first year of available data on industry receipts is 2002.

Each treatment firm (observation) belongs to an industry, so we are able to calculate percentage growth in total receipts from that firm's NAICS industry. Then we simply conduct difference-in-differences analysis of percentage changes in industry receipts.[26] Shrinking industry observations indeed show significantly negative difference-in-differences in industry percentage revenue changes around the shock (of roughly 15%). Growing industry observations are the corollary; significantly positive difference-in-differences in industry percentage revenue changes around the shock (of roughly 16%). We confirm the premise above that treatment firm industries with clear entry are growing while treatment firm industries with clear exit are shrinking.

5. Alternative perspectives

This section considers several alternative ways to corroborate our results and interpretations. In Section 5.1, we explore whether the increased asymmetric information due to the coverage shock is mitigated through the introduction of other sources of information that are valuable to investors. In particular, we ask whether *managers* offer forecasts more often, and we also look for new information in debt markets through introductions of credit ratings. We also check the net effect of the shock on analysts' dispersion of earnings and revenues forecasts. Finally, we revisit our market share treatment effects for the sample where forecast dispersion clearly rises due to the shock.

In Section 5.2 we consider an alternative interpretation to our main results. Instead of tension between agency and predation concerns driving the results, perhaps the increase in asymmetric information simply increases the firm's cost of capital. In such cases, investment in market share-generating activities may become unprofitable. We rely on a separate theoretical prediction by Hubbard (1998) to address this possibility.

5.1. Substitute sources of information

Analysts are not the only providers or interpreters of information. Managers often provide their own forecasts and the loss of coverage may encourage them to do so more often. We collect information on managers' provision of forecasts for our treatment and peer firms in the years surrounding the shock.[27] We conduct the usual difference-in-differences test. Our analysis variable is the average per year (over the three-year window either preceding or following the shock) of the number of manager forecasts per fiscal year for a firm.

The results are presented in Table 8, Panel A. Managers provide just under one forecast per fiscal year on average in the pre-shock period, while their matched peers provide just over one forecast per fiscal year on average in the same window. Managers increase their provision of forecasts in the post-shock window, but we see this effect among both treatment and peers. For shocked firms, the per-fiscal-year average is 1.6 forecasts in the post-shock window, while for peers it is 1.78 forecasts. Though both increases are significant, there is no statistical difference between the two. We conclude that managers do not (differentially) step in to replace the lost analyst information.

As an alternative to internal provision of information, we also examine whether other external evaluators change their provision of information due to the shock. In Panel B we present the numbers of treatment firms and peer firms that fall into four possible groupings of long-term debt credit rating provision. The four groupings are:

(1) firms that went from having no credit rating pre-shock to having a rating post-shock
(2) firms that went from having a credit rating pre-shock to having no rating post-shock
(3) firms that had no credit rating pre-shock and post-shock
(4) firms that had a credit rating pre-shock and post-shock.

The groupings of greater interest are (1) and (2), because they represent potential changes in information provided via credit ratings. More treatment firms pick up a long-term debt rating than lose (117 firms and 19 firms, respectively, in those groups). However, peers show very similar values (105 and 11, respectively). These counts suggest little difference between treatment and peer firms in the obtaining or loss of credit ratings around the shock. It does not appear that ratings information is differentially provided for treatment vs. peer firms due to the shock, so no substitution for the lost information (coverage loss) is likely to happen via public debt rating.

Given the drop in (analyst coverage) information production for our treatment sample, we double-check that a typical indicator of increased asymmetric information appears in our data: increases in analyst forecast dispersion. We again conduct difference-in-differences analysis but we do so using two separate test variables; *earnings* forecast dispersion and *revenues* forecast dispersion. The latter is perhaps more relevant for debtholders. We report the results in Table 8, Panel C. We find that earnings forecast dispersion rises in response to the shock, but revenue forecast dispersion does not.

Finally, we partition our treatment sample based on whether forecast dispersion rose or fell in response to the shock, and re-run our difference-in-differences tests and report the results in Table 8, Panel D. We do this segmentation separately for earnings forecast dispersion changes and for revenue forecast dispersion changes. When earnings forecast dispersion rose due to the shock, our market share loss treatment effect measures are significant (7%). This differs statistically and economically from the treatment effect for the subsample where earnings forecast dispersion fell after the shock. We see similar results[28] when we partition based on whether revenue forecast dispersion increases or decreases due to the shock. Overall, there is little evidence to suggest strategic information production to counteract the market share treatment effects of the coverage shocks.

[26] We do a difference-in-differences rather than simple time-series difference because of general economic growth over our sample window.

[27] Data come from Thomson First Call.

[28] With one exception, when market share change uses SIC code-based industries in the calculation.

Table 8

Tests on factors influencing firms' information environment.

The table reports the test results on factors influencing firms' information environment. Panel A presents difference-in-differences tests of the number of managerial earnings forecasts per firm-fiscal year. Per year averages are over the three years pre- (or post-) event. The sample is 776 treatment firms and their SIZE/BM/MOM/NOAN matched peers for which we have management earnings forecast data from Thomson's First Call dataset. Panel B presents simple counts of the number of firms with and without long-term credit ratings before and after the event. Panel C presents difference-in-differences (DiD) tests on analyst forecast dispersion around the event of brokerage merger/closure, where forecasts are about EPS, and separately about revenues. EPS forecast dispersion and Revenue forecast dispersion are explained in the Appendix. Panel D reports the difference-in-differences test results on market share growth when conditioning on change of analysts' EPS forecasts dispersion around the event of brokerage merger/closure in Panel D-1 and conditioning on change of analysts' revenue forecasts dispersion around the event in Panel D-2. If the change of analysts' EPS /Revenue forecasts dispersion from the three-year average pre-event to the three-year average post-event is greater than zero, then the variable changes of analysts' EPS/Revenue is defined as increase, otherwise, it is defined as decrease. Further explanations of analysts' EPS/Revenue forecasts dispersion are detailed in the Appendix. Event-clustered *t*-statistics are reported in parentheses.

Panel A: number of management earnings forecasts per fiscal year

	N	Pre-event	Post-event	Diff	(*t*-stat)
Treatment	776	0.98	1.59	0.61	(8.14)
Control	776	1.07	1.78	0.71	(13.13)
Difference-in-differences				−0.10	(−1.17)

Panel B: number of firms with or without long-term credit ratings

Existence of credit rating?

Pre-event	Post-event	Number of treatments	Number of peers
No	Yes	117	105
Yes	No	19	11
No	No	400	417
Yes	Yes	488	491

Panel C: Analysts' forecast dispersion

	N	Mean treatment difference (after–before)	Mean control difference (after–before)	Mean DiDs (treat-control)
EPS forecast dispersion	306	0.0035***	0.0015***	0.0020*
		(3.65)	(4.15)	(1.90)
Revenue forecast dispersion	181	0.0056**	0.0040**	0.0016
		(2.29)	(2.15)	(0.85)

Panel D-1: difference-in-differences estimators conditional on change of analysts' EPS forecast dispersion

	N	Sales growth	Market share growth (SIC)	Market share growth (FF)
EPS forecast dispersion increase	232	−0.066***	−0.073***	-0.074***
		(−4.05)	(−4.55)	(−4.54)
EPS forecast dispersion decrease	74	0.005	−0.015	−0.008
		(0.18)	(−0.68)	(−0.34)
Difference		−0.071**	0.058	−0.066**
		(−2.19)	(−2.12)	(−2.27)

Panel D-2: difference-in-differences estimators conditional on change of analysts' revenue forecast dispersion

	N	Sales growth	Market share growth (SIC)	Market share growth (FF)
Revenue forecast dispersion increase	129	-0.059***	-0.066***	-0.072***
		(−3.02)	(−3.34)	(−3.60)
Revenue forecast dispersion decrease	52	0.024	−0.024	−0.005
		(0.84)	(−0.85)	(−0.17)
Difference		0.083**	0.041	0.067**
		(−2.32)	(−1.15)	(−2.00)

* Statistical significance at the 10% level.

** Statistical significance at the 5% level.

*** Statistical significance at the 1% level.

Table 9
Test of alternative explanation.
This table reports the difference-in-differences (DiD) test results on profit margin. *Gross percentage profit margin* is defined in the Appendix. Sample is 1,024 treatment observations and their SIZE/BM/MOM/NOAN matched peers as described in the legend of Table 3. Event-clustered *t*-statistics are reported in parentheses.

Difference-in-differences tests on gross profit margin				
	N	Mean treatment difference (after–before)	Mean control difference (after–before)	Mean DiDs (treat-control)
Gross profit margin (%)	1024	−0.027 (−1.46)	−0.057 (−1.69)	0.0290 (0.82)

* Statistical significance at the 10% level.
** Statistical significance at the 5% level.
*** Statistical significance at the 1% level.

5.2. A cost of capital alternative argument

Up to now, we have interpreted our results as consistent with Bolton and Scharfstein's (1990) tradeoff between resource diversion concerns (pronounced when asymmetric information is higher) and predation concerns. An alternative interpretation may be that the positive shocks to asymmetric information increase the cost of capital for firms and this reduces optimal investment in market share. Given that both interpretations are possible under the presented evidence, it is important to examine an independent implication.

We turn to the theoretical work of Hubbard (1998) to explore an independent implication. He argues that a higher cost of capital discounts future profits more heavily, leading the firm to raise current prices. We therefore present difference-in-differences tests of percentage gross profit margin[29] in Table 9. If our results thus far are due to higher discount rates discouraging investment in market share, then we should also observe increased profit margins caused by the shock. They are not. Profit margins do not change significantly among treatment firms and they decline marginally among their peers. The difference between these two effects (i.e., the difference-in-differences) is not significant. Gross profit margin changes do not support the discount rate explanation for our results under the implication of Hubbard (1998).

6. Conclusions

The literature on finance and product market interactions is long and varied. However, the empirical role of asymmetric information in this relation is to date unclear. This is somewhat surprising given the theoretical financial contracting tightrope that investors navigate, between agency concerns over resource diversion and predation concerns (see Bolton and Scharfstein, 1990).

We propose to test their model. Our identification strategy exploits the now well-worn sample of brokerage house mergers and closures that exogenously shock analyst coverage, to conduct difference-in-differences tests on market share outcomes. Our results indicate that increases in asymmetric information cause reductions in market share ex post. Asymmetric information in financial markets compromises market share outcomes in product markets.

We confirm our empirical support of Bolton and Scharfstein (1990) through analysis of treatment effects for varying subsamples. The tension of their model predicts that firms in the following categories are likely to experience bigger market share losses in the wake of a coverage shock: ex ante more financially constrained firms, firms with greater ex ante investor agency concerns, firms operating in more fluid product markets, and firms with greater ex ante asymmetric information problems. We find all of these to be the case. We also show that these treatment effects are pronounced in firms that belong to either clearly shrinking industries or clearly growing industries.

Firms do not appear to significantly counteract these effects. We do not observe alternative sources of information emerging. Treatment firm managers do not provide earnings forecasts significantly more often compared to their peers. Nor do we see significant increases in the procurement of credit ratings on debt. Earnings forecast dispersion generally increases due to the shock. Finally, our results do not seem to be explained by an increased cost of capital (due to higher asymmetric information) argument. Overall, we conclude that asymmetric information in financial markets compromises firms' competitive status in product markets.

Appendix. Variable definitions

Acquisition is cash acquisition (AQC) scaled by total assets. If AQC is missing, we set it to zero. (Data source: Compustat)

Book-to-market (BM) equals book value of equity divided by market value of equity. Following Fama and French (2008), book value of equity equals total assets (AT) minus liabilities (LT), plus deferred taxes and investment tax credit (TXDITC) (if available), minus the value of preferred stock. The value of preferred stock is estimated by liquidating value (PSTKL), redemption value (PSTKRV), or total value of preferred stock (PSTK) depending on the availability (Data source: Compustat). Market value of equity is the product of stock price (PRC) and shares outstanding (SHROUT) divided by 1,000 (Data source: CRSP). Market value of equity is measured three months prior to the brokerage merger/closure date. Book value of equity is

[29] Sales minus cost of goods sold (COGS), all divided by sales.

measured during the period of 15 months to three months before the brokerage merger/closure date.

Capital expenditure equals capital expenditures (CAPX) scaled by total assets. If CAPX is missing, we set it to zero (Data source: Compustat).

Cash equals cash plus short-term investments (CHE), all divided by total assets (Data source: Compustat).

Competition is defined according to Hoberg, Phillips, and Prabhala's (2014) product market fluidity measure. Using the product market fluidity sample provided by the authors, we rank firms in each year into two groups based on the median of fluidity. Firms with above median fluidity are defined as high competition and firms below the median are defined as low competition (Data source: http://alex2.umd.edu/industrydata/industryconcen.htm).

Coverage equals the monthly average of the number of analysts providing current-fiscal-year EPS forecasts, measured monthly over the fiscal year starting from the month after last fiscal year-end to the month of current fiscal year end (Data source: IBES Summary).

Credit rating equals one if the firm has Standard & Poor's (S&P) Domestic Long Term Issuer Credit Rating (Splticrm) from "AAA" to "C"; else credit rating equals zero.

EPS forecast dispersion is computed by the standard deviation of analysts' EPS forecasts scaled by price (PRICE from IBES) at the month of the forecast fiscal year-end. We use EPS forecasts for current year (FPI = 1) from IBES detail file and exclude the estimates which are stopped or excluded or stale forecasts. We keep the last forecast for each analyst and then calculate the standard deviation of the forecasts for each fiscal year. We drop the standard deviation of forecasts if there are less than three analysts following the firm for the forecast year (Data source: IBES).

Gross percentage profit margin is revenues (SALE) minus COGS, all divided by revenues.

Independent_Long-term_Dedicated is the percent of shares owned by independent, long-term, and dedicated investors who are among the topfive largest institutional investors. We follow Chen, Harford, and Li (2007) to define investor type: independent, long-term and dedicated.

Leverage equals long-term debt (DLTT) plus short-term debt (DLC), all scaled by total assets (Data source: Compustat).

LnAssets is the natural logarithm of total book value of assets (AT) at the end of year (Data source: Compustat).

LnCoverage is the natural logarithm of one plus Coverage (Data source: IBES Summary).

Market capitalization (MKT) equals the product of stock price (PRC) and shares outstanding (SHROUT) divided by 1,000. Market capitalization is measured three months prior to the brokerage merger/closure date (Data source: CRSP).

Market share growth (FF) equals the change in sales from $t-1$ through t, minus the industry median change in sales. Industry is defined by Fama-French 49 industry classification. We require at least ten firms within the industry-year.

Market share growth (SIC) equals the change in sales from $t-1$ through t, minus the industry median change in sales. Industry is defined by four-digit SIC code. We require at least ten firms within the industry-year, following Campello's (2003, 2006) and Fresard's (2010) calculations of market share changes.

Market-to-book equals total assets minus book equity plus market equity (shares outstanding times fiscal year-end stock price (PRCC_F*CSHO)), all divided by total assets. Book value of equity is following Fama and French (2008). It equals totals assets (AT) minus liabilities (LT), plus deferred taxes and investment tax credit (TXDITC) (if available), minus the value of preferred stock. The value of preferred stock is estimated by liquidating value (PSTKL), redemption value (PSTKRV), or total value of preferred stock (PSTK) depending on the availability (Data source: Compustat).

Number of analysts (NOAN) equals the monthly average number of analysts providing current-fiscal-year EPS forecasts, measured monthly during the period of 15 months to three months before the brokerage merger/closure date (Data source: IBES Summary).

Opacity captures a firm's financial reporting quality (also known as *accruals quality*). We follow Lee and Masulis (2009) and Billett and Yu (2016) and use the modified Dechow and Dichev (2002) model (hereafter DD), as applied in Francis, LaFond, Olsson, Schipper, (2005), to measure Opacity, measured by the standard deviation of firm's residuals from estimating the following regression equation: $TCA_{j,t} = c + \emptyset_1 CFO_{j,t-1} + \emptyset_2 CFO_{j,t} + \emptyset_3 CFO_{j,t+1} + \emptyset_4 \Delta SALES_{j,t} + \emptyset_5 PPE_{j,t} + v_{j,t}$ $TCA_{j,t} = c + \emptyset_1 CFO_{j,t-1} + \emptyset_2 CFO_{j,t} + \emptyset_3 CFO_{j,t+1} + \emptyset_4 \Delta SALES_{j,t} + \emptyset_5 PPE_{j,t} + v_{j,t}$ from $t-4$ to t. $TCA_{j,t}$ is total current accruals for firm j in year t. $TCA_{j,t} = \Delta CA_{j,t} - \Delta CL_{j,t} - \Delta Cash_{j,t} + \Delta STDEBT_{j,t}$, CA = current assets (ACT), CL = current liabilities (LCT), Cash = cash and short-term investments (CHE), STDEBT = debt in current liabilities (DLC), $CFO_{j,t}$ is firm j's cash flow from operations in year t, $CFO_{j,t} = IB_{j,t} - TA_{j,t}$, IB = net income before extraordinary items (IB), $TA_{j,t} = \Delta CA_{j,t} - \Delta CL_{j,t} - \Delta CASH_{j,t} + \Delta STDEBT_{j,t} - DEPN_{j,t}$, DEPN = depreciation and amortization (DP), $\Delta SALES_{j,t} = SALES_{j,t} - SALES_{j,t-1}=$, $SALES_{j,t}$ = sales revenue for firm j in year t (SALE), $PPE_{j,t}$ = total property, plant, and equipment for firm j in year t (PPENT). All variables are scaled by the average value of total assets (computed as the average of total assets at the beginning and end of year t). The above regression equation is estimated by running separate industry-year regressions for each of the 49 Fama and French industries with at least 20 firms in a given year. We take a given firm's residuals from five industry-year regressions (years $t-4$ to t), and define Opacity as the standard deviation of those residuals.

Payout equals one if the firm had positive payout (dividend (DV) or repurchase (PRSTKC)) during the year.

R&D is research and development expenditures (XRD) scaled by total assets. If R&D is missing, we set it to zero (Data source: Compustat).

Return (RET) equals the average monthly return during the period of 15 months to three months before the brokerage merger/closure date (Data source: CRSP).

Revenue forecasts dispersion is computed by the standard deviation of analysts' sales forecasts scaled by market value of equity (price multiplied by shares outstanding (PRICE*SHOUT from IBES)) at the month of the forecast fiscal year-end. We use sales forecasts for current year (FPI = 1) from IBES detail file and exclude the estimates which are stopped or excluded or stale forecasts. We keep the last forecast for each analyst and then calculate the standard deviation of the forecasts for each fiscal year. We drop the standard deviation of forecasts if there are less than three analysts following the firm for the forecast year (Data source: IBES).

ROA equals operating income before depreciation (OIBDP), scaled by total assets (Data source: Compustat).

Sales growth equals the change in sales from $t-1$ through t, computed as $(Sale_t - Sale_{t-1})/Sale_{t-1}$ (Data source: Compustat).

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