Testing Asymmetric-Information Asset Pricing Models*

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Abstract

We test models of asset pricing under asymmetric information using plausibly exogenous variation in the supply of information caused by the closure or restructuring of brokerage firms' research operations. Consistent with predictions derived from a Grossman and Stiglitz-type model, share prices and uninformed investors' demands fall as information asymmetry increases. Cross-sectional tests support the comparative statics. Prices and uninformed demand experience larger declines, the more investors are uninformed, the larger and more variable is turnover, the more uncertain is the asset's payoff, and the noisier is the better-informed investors' signal. We show that prices fall because expected returns become more sensitive to a liquidity-risk factor.

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The aim of the paper is to empirically examine the fundamental driving forces of asset pricing models under asymmetric information. These are typically noisy rational expectations equilibrium models in which some investors are better informed than others. In equilibrium, prices reveal informed investors' superior information only partially due to the presence of noise in the form of random supply of the risky asset. Random supply might reflect 'noise traders' whose demands are independent of information. Prominent examples of such models include Grossman and Stiglitz (1980), Hellwig (1980), Admati (1985), Wang (1993), and Easley and O'Hara (2004).

To derive empirical predictions representative of these models, Section I adapts the Grossman and Stiglitz model to show that an increase in information asymmetry leads, not surprisingly, to a fall in share price and a reduction in uninformed investors' demand for the risky asset. Simple expressions for equilibrium prices reveal that the channel linking information asymmetry and price is liquidity risk: Price falls because the asset's exposure to liquidity risk increases. The model's comparative statics relate the changes in price and demand to the fraction of investors who are better informed, the supply and liquidity of the asset, the uncertainty about the asset's payoff, and the noisiness of the better-informed investors' signal.

Testing a model of asset pricing with asymmetric information poses a tricky identification challenge. Imagine regressing the change in investor demand on the change in a proxy for information asymmetry, such as the stock's bid-ask spread. Interpreting the coefficient would be hard if, as is likely, there are omitted variables (such as changes in the riskiness of the company's cash flows) which simultaneously affect spreads and demand. Another problem is reverse causality. Spreads may increase precisely because uninformed demand is expected to fall. To overcome simultaneity, we need an instrument for the degree of information asymmetry in the asset market.

The Grossman and Stiglitz model and its descendants suggest what such an instrument might look like: An exogenous change in the cost of information about an asset's payoffs. These models demonstrate how an increase in information cost will increase information asymmetry by inducing fewer investors to purchase information. Exogeneity means, in this case, that the change in information cost must affect prices and demands only via its effect on the asymmetry of information. Cost changes would have to be independent of all other underlying determinants of asset prices and demands, particularly the asset's future payoffs.

Equity research analysts are among the most influential information producers in financial markets. We argue that their presence or absence affects the extent of information asymmetry. Suppose investors are heterogenous in their information costs. All those whose costs exceed the price at which an analyst sells his research will purchase it. If the analyst were to stop selling research, investors would have to fall back on alternative information sources, and for some, the cost of becoming informed would exceed the benefit. (For instance, hedge funds or mutual funds likely have relatively low-cost substitute information sources, but retail investors presumably do not.) Thus, information asymmetry would increase.

Consider the extreme case of a market in which each stock is covered by exactly one analyst. The ideal experiment would be to randomly ban analysts from researching some stocks. Because the cost of information substitutes for buyers of analyst research must (weakly) exceed the analyst's asking price, the average cost of information will increase for the stocks that randomly lose research, and information asymmetry will increase as a result. An econometrician could then measure the effect of information asymmetry on equilibrium asset prices and investor demands.

Our empirical approach is to identify a quasi-experiment of this nature. We compile a list of events in which brokerage firms terminated research coverage of large sets of stocks. Coverage terminations will qualify as a valid instrument if they satisfy two conditions. First, they must correlate with an increase in information asymmetry. This will result if information costs are heterogeneous, as we have argued. Second, they must affect price and demand *only* through their effect on information asymmetry. Most coverage terminations do not satisfy this exclusion restriction. Instead, they typically reflect a negative change in the analyst's view of the company's future prospects and so are endogenous (McNichols and O'Brien (1997)). However, we are able to identify a subset of 14,939 coverage terminations over the period 2000-2005 that are plausibly exogenous in the sense that they result from closures or downsizing of brokerage firms' research operations, rather than an analyst's decision to *selectively* drop coverage on some stocks but continue it on others.[1] We discuss the exogenous shocks that led to this restructuring in Section II, where we also show that sample terminations correlate with increases in information asymmetry while passing various tests of the exclusion restriction, as required for exogeneity.[2,3]

We test the model's predictions in Section III. As predicted, both price and uninformed demand fall following a coverage termination. Cumulative abnormal returns average between -51 and -56 basis points on the day of an exogenous coverage termination, depending on the benchmark. Institutional investors—which are more likely to be better informed—increase their holdings of affected stocks while retail investors—who are more likely to be uninformed—sell. Cross-sectional tests support the comparative statics. Prices and uninformed demand experience larger declines, the more investors are uninformed, the larger and more variable is turnover, the more uncertain is the asset's payoff, and the noisier is the signal. Finally, using standard empirical models of

[1]Coverage terminations should not be confused with 'stopped coverage.' The latter is a temporary suspension of coverage and occurs most frequently when a subject of coverage raises capital with the brokerage firm's help.

[2]Our instrument is similar in spirit to Hong and Kacperczyk's (2007). Like us, these authors exploit exogenous variation in coverage (in their case, variation due to mergers among brokerage firms with overlapping coverage universes), though their focus is different. Their main finding is that reduced coverage leads to more optimistic earnings forecasts as competition among analysts decreases.

[3]We are interested in coverage terminations purely as an instrument with which to identify changes in the supply of information. For studies focusing on terminations per se, see Khorana, Mola, and Rau (2007), Scherbina (2008), Kecskes and Womack (2007), and Ellul and Panayides (2007). Each of these papers uses endogenous terminations.

expected returns, we show that the loading on a factor proxying for liquidity risk increases following exogenous coverage terminations. This supports the liquidity-risk channel of the model.

Our tests contribute to the literature by providing direct evidence of the role of asymmetric information in asset pricing. With one notable exception, discussed below, the literature has mainly focused on the role of liquidity in asset pricing, perhaps because liquidity is considerably easier to measure than information asymmetry. Relevant empirical studies include Pastor and Stambaugh (2003), Amihud and Mendelson (1986), Amihud (2002), Hasbrouck and Seppi (2001), Bekaert, Harvey, and Lundblad (2007), Jones (2002), and Eleswarapu (1997). Following theoretical models such as Amihud and Mendelson (1986), Acharya and Pedersen (2005), and Huang (2003), these studies treat liquidity as exogenous. Our findings suggest that liquidity varies with information asymmetry, consistent with microstructure models such as Kyle (1985) and Glosten and Milgrom (1985). Thus, one fundamental driver of asset prices appears to be information asymmetry, consistent with models such as Grossman and Stiglitz (1980), Hellwig (1980), Admati (1985), Wang (1993), and Easley and O'Hara (2004).

The notable exception is the empirical literature that uses Easley and O'Hara's (1992) *PIN* measure to proxy for information asymmetry. *PIN* is based on the idea that the presence of privately informed traders can be noisily inferred from order flow imbalances. Easley et al. (1996) show that *PIN* correlates with measures of liquidity such as bid-ask spreads, while Easley, Hvidkjær, and O'Hara (2002) find that *PIN* affects expected returns.[4] The advantage of our approach relative to this literature is that it exploits exogenous variation in information asymmetry rather than relying on a proxy for information asymmetry whose potential correlations with unobserved variables are unknown.

[4] *PIN* is controversial. Mohanram and Rajgopal (2006) find that *PIN* is not priced beyond Easley et al.'s (2002) sample period. Duarte and Young (2008) show that *PIN* is only priced to the extent that it proxies for illiquidity.

I. The Model

The setup follows Grossman and Stiglitz (1980). There is a unit mass of investors who have identical initial wealth, W_0, and are risk-averse with CARA utility of consumption $-e^{-C}$.[5] Investors trade in period one and consume in period 2. There is a risk-free asset with gross return R and a risky asset with aggregate supply $X \sim N(\bar{X}, \sigma_x^2)$ and payoff $u = \theta + \eta$, with $\eta \sim N(0, \sigma_u^2)$. Investors know θ and can engage in research at cost $c > 0$ which results in a noisy signal s about the risky asset's payoff innovation, η: $s = \eta + v$, with $v \sim N(0, \sigma_v^2)$. When investor i observes the signal, his information set, $\mathcal{F}^i$, includes both s and the equilibrium price, P, though price information is then redundant. When s is not observed, P contains useful conditioning information for payoff u.

In addition to the investors, there may or may not be an analyst, working for a brokerage firm, who can also produce signal s. We assume the analyst disseminates s (in the form of earnings forecasts, research reports, or trading recommendations) for free. This mirrors institutional practice: Investors are not charged for each analyst report they receive, so at the margin, the cost of observing the analyst's signal is zero. Brokers recoup the cost of producing research through account fees, trading commissions, or cross-subsidies from market-making or investment banking.[6]

In the analyst's presence, s is public information, so information is symmetric. In his absence, we are in the Grossman-Stiglitz (1980) world where investors must decide whether to produce the signal themselves. Grossman and Stiglitz show that in equilibrium, some fraction $0 < \delta < 1$ of the investors become informed. Thus, in the analyst's absence, information is asymmetric.

Following Grossman and Stiglitz (1980), we assume that investors do not observe aggregate supply X. The three random variables of the model (X, η, and v) are assumed to be independent.

[5]The risk aversion coefficient is assumed equal to unity for simplicity and without loss of generality.

[6]We leave the brokerage firm's incentive to disclose the analyst's signal unmodeled. For models that endogenize this decision, see Admati and Pfleiderer (1986), Fishman and Hagerty (1995), or Veldkamp (2006).

A. Equilibrium Effects

To determine the equilibrium effects of a coverage termination, we compare prices and demands for the risky asset in the symmetric information case, which results from an analyst's presence, to the asymmetric information case that occurs in the analyst's absence. The following proposition summarizes the equilibrium changes when the analyst ceases to produce research on the asset.

Proposition 1: *Following a coverage termination, a) the price of the risky asset falls by*

$$\Delta EP \equiv E[P_{asymm} - P_{symm}] = \frac{-\sigma_u^4 \sigma_v^4 \bar{X}\, \sigma_x^2\ (1-\delta)}{R\ (\sigma_u^2 + \sigma_v^2)\left(\sigma_x^2\, \sigma_v^4 + \sigma_v^2\, \delta\, \sigma_u^2\, \sigma_x^2 + \sigma_u^2\, \delta^2 + \sigma_v^2\, \delta^2\right)} < 0 \qquad (1)$$

and b) a measure $1-\delta$ *of investors reduce their demand for the risky asset by selling part of their holdings to investors who choose to become informed. Informed demand increases by the amount*

$$\Delta EID = \frac{(1-\delta)\, \sigma_u^2\, \sigma_v^2\, \bar{X}\, \sigma_x^2}{\sigma_x^2\, \sigma_v^4 + \sigma_v^2\, \delta\, \sigma_u^2\, \sigma_x^2 + \sigma_u^2\, \delta^2 + \sigma_v^2\, \delta^2} > 0. \qquad (2)$$

Proof: See Appendix A.

B. Discussion

CARA utility combined with the Gaussian nature of the random variables implies that investors optimize their demands by trading off mean and variance conditional on their information set. (The details can be found in Appendix A.) A coverage termination changes only the uninformed investors' information set. From their perspective, a coverage termination increases the conditional payoff variance while having a mean zero effect on their expected payoff. This lowers uninformed demand and thus equilibrium price. At a lower price, informed investors, whose payoff beliefs are unchanged, are induced to increase their demand until the market-clearing condition is satisfied.

Why does payoff uncertainty increase from the perspective of the uninformed? In the analyst's absence, the uninformed do not observe s and so base their demand solely on the observed price, P. However, P is not fully revealing, because investors do not observe aggregate supply, X. As a result, the uninformed cannot simply back out the informed investors' signal from observed prices; they cannot tell whether a price change reflects a change in aggregate supply or a change in the signal. Instead, they noisily infer the signal by forming an expectation of payoff u conditional on observed price P. Thus, a coverage termination exposes the uninformed to aggregate supply risk.

More formally, Appendix A shows that, under rational expectations, both the symmetric and asymmetric-information equilibrium prices are linear in the signal and aggregate supply:

$$P_{symm} = \frac{\theta}{R} + \frac{\sigma_u^2}{R(\sigma_u^2 + \sigma_v^2)} s - \frac{\sigma_u^2}{R}(1 - \frac{\sigma_u^2}{\sigma_u^2 + \sigma_v^2})X \tag{3}$$

$$P_{asymm} = c_1 + c_2 s + c_3 X \tag{4}$$

where $c_2 > 0$ and $c_3 < 0$ (the precise expressions can be found in Appendix A). Following Brennan and Subrahmanyam (1996) and Amihud (2002), we can interpret X as trade volume and the coefficient on X as the price impact of trade. As the following corollary shows, the effect of a coverage termination is to increase the price impact of trade:

Corollary 1: *Let the price impact of trade be defined as $\partial P/\partial X$. Then the change in price impact of trade following a coverage termination is given by the quantity*

$$\begin{aligned} \Delta(\partial P/\partial X) &= c_3 + \frac{\sigma_u^2}{R}(1 - \frac{\sigma_u^2}{\sigma_u^2 + \sigma_v^2}) \qquad (5) \\ &= -\frac{1}{R}\left(\frac{\sigma_v^2\ \sigma_u^2\ \left(\delta + \ \sigma_v^2\ \sigma_x^2\right)}{\left(\ \sigma_x^2\ \sigma_v^{2\,2} + \left(\delta^2 + \delta\ \sigma_u^2\ \sigma_x^2\right)\ \sigma_v^2 + \ \sigma_u^2\,\delta^2\right)} + \ \sigma_u^2\left(1 - \frac{\sigma_u^2}{\sigma_u^2 + \ \sigma_v^2}\right)\right) < 0. \end{aligned}$$

Corollary 1 says that price falls as information asymmetry increases because price becomes more sensitive to liquidity risk. Thus, we have:

Corollary 2: *Following a coverage termination, the asset's expected return increases as the asset's exposure to liquidity risk increases.*

In a factor-model setting, for instance, we would thus expect the loading on a factor proxying for liquidity risk to increase following an exogenous coverage termination.

C. Robustness

In common with Grossman and Stiglitz (1980), our model makes two simplifying assumptions: There is a single risky asset, and there is a single signal s which can be produced by both the analyst and investors. Neither assumption is restrictive.

Easley and O'Hara (2004) extend the Grossman-Stiglitz model to multiple assets and show that information risk cannot be diversified away even in large portfolios. Intuitively, the uninformed are at an informational disadvantage for every asset in their portfolio and so information risk is priced in equilibrium in the way we have modeled it.

Multiple signals also do not change the conclusions. Suppose there are N analysts publishing a public signal and M other signals that may be purchased at cost $c_j > 0$. An analyst's departure corresponds to a (weakly) positive shift in the cost schedule for the menu of possible signal combinations. In equilibrium, investors consume all the free signals and a Grossman and Stiglitz solution determines consumption of the costly signals. Any signal combination an investor consumed before the analyst's departure is more expensive now, so fewer signals are consumed. In the new equilibrium, prices drop due to the decrease in the supply of information and the accompanying increase in overall payoff uncertainty following a coverage termination.

D. Testable Implications

To guide our empirical analysis, we derive the following comparative statics.

The more investors become informed following a coverage termination, the fewer investors are affected by the loss of analyst information. Thus, we have:

Implication 1: *The larger the fraction of informed investors among the company's shareholders, the smaller is the negative price impact of a coverage termination and the smaller is the resulting increase in informed demand for the company's stock:*

$$\frac{\partial \Delta EP}{\partial \delta} = \frac{\left(2\,\delta(1-\delta) + \sigma_v^2\ \sigma_x^2\right)\sigma_u^4\,\sigma_v^4\ \bar{X}\ \sigma_x^2}{\left(\sigma_x^2\ \sigma_v^4 + \sigma_v^2\,\delta^2 + \sigma_v^2\,\delta\ \sigma_u^2\ \sigma_x^2 + \sigma_u^2\,\delta^2\right)^2 R} > 0$$

$$\frac{\partial \Delta EID}{\partial \delta} = \frac{-\sigma_v^2\ \sigma_u^2\ \bar{X}\ \sigma_x^2\left(\sigma_x^2\ \sigma_v^4 + \sigma_v^2\ \sigma_u^2\ \sigma_x^2 + \delta(\sigma_v^2 + \sigma_u^2)(2-\delta)\right)}{\left(\sigma_x^2\ \sigma_v^4 + \sigma_v^2\,\delta^2 + \sigma_v^2\,\delta\ \sigma_u^2\ \sigma_x^2 + \sigma_u^2\,\delta^2\right)^2} < 0.$$

Corollary 1 states that a coverage termination increases the (negative) sensitivity of price to aggregate supply because the uninformed have a harder time filtering the signal from the price. Thus, stocks with larger aggregate supply experience larger price and demand changes:

Implication 2: *The greater is mean aggregate supply, the larger is the negative price impact of a coverage termination and the greater is the resulting increase in informed demand for the stock:*

$$\frac{\partial \Delta EP}{\partial \bar{X}} = \frac{-\sigma_x^2\ \sigma_v^4\ \sigma_u^4\,(1-\delta)}{(\sigma_u^2 + \sigma_v^2)\,R\left(\sigma_x^2\ \sigma_v^4 + \left(\delta^2 + \delta\ \sigma_u^2\ \sigma_x^2\right)\sigma_v^2 + \sigma_u^2\,\delta^2\right)} < 0$$

$$\frac{\partial \Delta EID}{\partial \bar{X}} = \frac{(1-\delta)\ \sigma_u^2\ \sigma_v^2\ \sigma_x^2}{\sigma_x^2\ \sigma_v^4 + \sigma_v^2\,\delta^2 + \sigma_v^2\,\delta\ \sigma_u^2\ \sigma_x^2 + \sigma_u^2\,\delta^2} > 0.$$

In the absence of an analyst, the uninformed infer the signal from the observed price. This inference problem is harder the more variable are the aggregate supply and payoff, which in turn increases the value of analyst research to the uninformed. Thus, we have the following implications:

Implication 3a: *The more variable is aggregate supply, the larger is the negative price impact of a coverage termination and the greater is the resulting increase in informed demand for the stock:*

$$\frac{\partial \Delta EP}{\partial \sigma_x^2} = \frac{-\sigma_u^4\ \sigma_v^4\ \bar{X}\,\delta^2\,(1-\delta)}{\left(\ \sigma_x^2\ \ \sigma_v^4 +\ \sigma_v^2\,\delta^2 +\ \sigma_v^2\,\delta\ \ \sigma_u^2\ \ \sigma_x^2 +\ \sigma_u^2\,\delta^2\right)^2 R} < 0$$

$$\frac{\partial \Delta EID}{\partial \sigma_x^2} = \frac{(1-\delta)\ \ \sigma_u^2\ \ \sigma_v^2\ \ \bar{X}\,\delta^2\left(\ \sigma_u^2 +\ \sigma_v^2\right)}{\left(\ \sigma_x^2\ \ \sigma_v^4 +\ \sigma_v^2\,\delta^2 +\ \sigma_v^2\,\delta\ \ \sigma_u^2\ \ \sigma_x^2 +\ \sigma_u^2\,\delta^2\right)^2} > 0.$$

Implication 3b: *The more variable is the asset's payoff, the larger is the negative price impact of a coverage termination and the greater is the resulting increase in informed demand for the stock:*

$$\frac{\partial \Delta EP}{\partial \sigma_u^2} = \frac{-\sigma_u^2\ \ \sigma_v^6\ \bar{X}\ \ \sigma_x^2\ (1-\delta)\left(\ \sigma_v^2\ \ \sigma_u^2\ \ \sigma_x^2 + 2\ \ \sigma_u^2\,\delta^2 + 2\ \ \sigma_x^2\ \ \sigma_v^4 + 2\ \ \sigma_v^2\,\delta^2 +\ \sigma_v^2\,\delta\ \ \sigma_u^2\ \ \sigma_x^2\right)}{R\ \left(\ \sigma_u^2 +\ \sigma_v^2\right)^2\left(\ \sigma_x^2\ \ \sigma_v^4 +\ \sigma_v^2\,\delta^2 +\ \sigma_v^2\,\delta\ \ \sigma_u^2\ \ \sigma_x^2 +\ \sigma_u^2\,\delta^2\right)^2} < 0$$

$$\frac{\partial \Delta EID}{\partial \sigma_u^2} = \frac{(1-\delta)\ \ \sigma_v^4\ \bar{X}\ \ \sigma_x^2\ \left(\ \sigma_v^2\ \ \sigma_x^2 + \delta^2\right)}{\left(\ \sigma_x^2\ \ \sigma_v^4 +\ \sigma_v^2\,\delta^2 +\ \sigma_v^2\,\delta\ \ \sigma_u^2\ \ \sigma_x^2 +\ \sigma_u^2\,\delta^2\right)^2} > 0.$$

The effect of signal noise is more complicated. Consider the two extreme cases. If the signal is very precise, the uninformed can filter the signal from the observed price very effectively, so losing the signal has a negligible impact on demand and prices. If the signal is so noisy as to be essentially uninformative, it is of little use to the informed who therefore have little informational advantage over the uninformed. As a result, a coverage termination results in negligible information asymmetry among investors and so has little impact on demand and prices. In between these two extremes, losing a noisy but informative analyst signal increases the uninformed investors' inference problem and so leads to a price fall and a decrease in uninformed demand. Thus, we have:

Implication 4: *The effect of signal noise on the price impact of a coverage termination is U-shaped, while its effect on the change in informed demand is inverse U-shaped:*

$$
\begin{aligned}
\frac{\partial \Delta EP}{\partial \sigma_v^2} &= \frac{-\sigma_u^4\ \sigma_v^2\ \bar{X}\ \sigma_x^2\ (1-\delta)\left(2\ \sigma_u^2\ \sigma_v^2\,\delta^2 +\ \sigma_v^2\,\delta\ \sigma_u^4\ \sigma_x^2 + 2\ \sigma_u^4\delta^2 -\ \sigma_x^2\ \sigma_v^6\right)}{R\ (\ \sigma_u^2 +\ \sigma_v^2)^2\left(\ \sigma_x^2\ \sigma_v^4 +\ \sigma_v^2\,\delta^2 +\ \sigma_v^2\,\delta\ \sigma_u^2\ \sigma_x^2 +\ \sigma_u^2\,\delta^2\right)^2} \\
&< 0 \text{ iff } \frac{2\ \sigma_u^2\ \sigma_v^2\,\delta^2 +\ \sigma_v^2\,\delta\ \sigma_u^4\ \sigma_x^2 + 2\ \sigma_u^4\delta^2}{\sigma_x^2\ \sigma_v^6} > 1 \\
\frac{\partial \Delta EID}{\partial \sigma_v^2} &= \frac{(1-\delta)\ \sigma_u^2\ \bar{X}\ \sigma_x^2\ \left(\ \sigma_u^2\,\delta^2 -\ \sigma_x^2\ \sigma_v^4\right)}{\left(\ \sigma_x^2\ \sigma_v^4 +\ \sigma_v^2\,\delta^2 +\ \sigma_v^2\,\delta\ \sigma_u^2\ \sigma_x^2 +\ \sigma_u^2\,\delta^2\right)^2} \\
&> 0 \text{ iff } \frac{\sigma_u^2\delta^2}{\sigma_x^2\sigma_v^4} > 1.
\end{aligned}
$$

II. Identification

Identification of the effects of an increase in information asymmetry on asset prices and investor demands requires an instrument. To satisfy the exclusion restriction, the instrument must correlate with an increase in information asymmetry but must not otherwise correlate with price or demands.

As McNichols and O'Brien (1997) observe, coverage changes are usually endogenous and so do not generally satisfy the exclusion restriction. Coverage terminations, in particular, are often viewed as implicit sell recommendations (Scherbina (2008)). The resulting share price fall may hence reflect the revelation of an analyst's negative view of a firm's prospects rather than the effect of reduced research coverage. Similarly, an analyst may drop a stock because institutional investors have lost interest (Xu (2006)). If institutional interest correlates with price, price may fall following coverage terminations for reasons unrelated to changes in information asymmetry.

A. Identification Strategy

To avoid these biases, we exploit what we argue is exogenous variation in research coverage induced by restructuring among brokerage firms over the period 2000 to 2005. Brokerage firms have traditionally subsidized their research departments with revenue from trading ("soft dollar commissions"), market-making, and investment banking. Each of these revenue streams diminished in the early 2000s. The prolonged decline in trading volumes that accompanied the bear market of 2000-2003 along with increased competition for order flow reduced brokerage revenue and income from market-making activities. More recently, soft dollar commissions have attracted renewed regulatory interest, culminating in tightened S.E.C. rules adopted in 2006.[7] At the same time, institutional clients are increasingly banning the use of soft dollars.[8] And concerns that analysts publish biased research to please investment banking clients (Michaely and Womack (1999), Dugar and Nathan (1995), Lin and McNichols (1998)) have led to new regulations, such as the 2003 Global Settlement, which have made it harder for the investment banking division to cross-subsidize research.

Brokerage firms have responded to these adverse changes in the economics of producing research by closing or downsizing their research operations. A subset of them have done so in a way likely to be uninformative about the future prospects of the companies whose coverage they terminated.

A.1. Closures

Using news sources accessed through Factiva, we identify 20 brokerage firms that closed their research departments between 2000 and 2005. These range from large firms (e.g., Wells Fargo,

[7] The SEC's new interpretative guidance of the safe-harbor rule under Section 28(e) of the 1934 Securities Exchange Act is available at http://www.sec.gov/rules/interp/2006/34-54165.pdf.

[8] According to the TABB Group, a consultancy, nearly 90% of all larger institutional investors stopped or decreased use of soft dollars between 2004 and 2005 (BusinessWire, June 1, 2005).

8/2005) to smaller outfits (e.g., Schwab's Soundview Capital Markets division, 10/2004) and encompass both retail-oriented firms (e.g., First Montauk, 2/2004) and institutional brokerage houses (e.g., Emerald Research, 7/2001) and brokers with either generalist (e.g., ABN Amro, 3/2002) or specialist coverage (e.g., Conning & Co., 10/2001).

Press reports make it clear that the closures are unlikely to have been motivated by negative information about individual stocks and so likely satisfy the exclusion restriction. For example, commenting on his decision to close down IRG Research, CEO Thomas Clarke blamed regulation:

> "With the brokerage industry facing some of the most far-reaching regulatory changes within the last 30 years, including S.E.C. rulings regarding the use of soft dollars and possibly the unbundling of research and trading costs, we could not see the economics working in our favor without substantial additional investment." (Dow Jones, June 28, 2005)

Dutch bank ABN Amro closed its loss-making U.S. equities and corporate finance business in March 2002. Among the 950 redundancies were 28 senior analysts who, along with junior team members, covered nearly 400 U.S. stocks. Board member Sergia Rial commented on the reasons:

> "We're withdrawing from businesses in which we're strategically ill-positioned and cannot create a sustainable profit stream, whether the market turns around or not." (Chicago Tribune, March 26, 2002)

Regional broker George K. Baum closed its capital markets division in October 2000, blaming lack of profitability even during the booming late 1990s:

> "Neither the retail brokerage nor equity capital markets divisions made money in the past two years." (Knight Ridder Tribune Business News, Oct. 13, 2000)

We identify the stocks affected by the 20 brokerage closures using the coverage table of Reuters Estimates, which lists, for each stock, the dates during which each broker and analyst in the Reuters database actively cover a stock. When dating the terminations, we use the time stamp of the first press release announcing the brokerage closure. This allows us to pinpoint the precise trading day on which investors can first react to the news.

A.2. Sector Terminations

While 20 brokers got out of research altogether, more than 150 brokers downsized their research departments between 2000 and 2005. In 2003, for instance, Goldman Sachs, Morgan Stanley, and J.P. Morgan reportedly cut research staffing by between 15 and 25 percent (Reuters, May 23, 2003).

In many cases, brokers fired entire sector teams as opposed to individual analysts covering part of a sector. We focus on such sector terminations to reduce the likelihood that brokers selectively fired analysts whose stocks had poor future prospects (which could violate the exclusion restriction). Later in this section, we report evidence suggesting that sector terminations do not reflect past or future sector performance in our data.

To illustrate, on May 23, 2003, Citigroup dropped coverage of eight of the 43 sectors its analysts had covered, namely metals/mining, life sciences, utilities, healthcare services, airlines, industrials, specialty chemicals, and telecom equipment/wireless. On Oct. 12, 2001, Wachovia Securities laid off the eight analysts who covered technology hardware, telecommunications, and consumer durables.

To identify sector terminations, we first search the Reuters coverage table for days on which a brokerage firm terminates coverage of at least 75% of the stocks it had been covering in a sector. As sector definitions vary by broker, we define sectors using S&P's six-digit GICS codes.[9] We do not impose a 100% termination rate within a GICS sector to allow for the possibility that GICS do not

[9]Bhojraj, Lee, and Oler (2003) compare four industry classifications, concluding that "GICS classifications are significantly better at explaining stock return co-movements, as well as cross-sectional variations in valuation multiples, forecasted and realized growth rates, R&D expenditures, and various key financial ratios." There are currently 68 GICS sectors at the six-digit level. See www2.standardandpoors.com/spf/xls/index/gics_map_aug2008.xls.

coincide precisely with a given broker's sector definition;[10] our results are not sensitive to this. In addition, we search in Factiva and Bloomberg for announcements of sector terminations not already identified in the Reuters data. Finally, we mine three text archives of analyst reports (Investext, Thomson One, and the client-intranet of a large brokerage house) for client notices announcing sector terminations.[11] Such termination notices are mandatory under NYSE Rule 472(f)(5) and NASD Rule 2711(f)(5) which require that:

> "[A] member provide notice to customers that it is terminating research coverage of an issuer that is the subject of a research report. ... [I]f the research analyst covering the subject company has left the member, or where the member has terminated coverage on an industry or sector ... the rationale for termination will be required." The broker must give notice "... using the means of dissemination equivalent to those it ordinarily uses to provide the customer with its research reports."[12]

The rules were brought in to prevent brokerage firms from terminating coverage secretly. Under the rules, the broker's clients learn of the coverage termination directly from the broker. In fact, the news is quickly disseminated to the wider market. As a proxy for wider dissemination, we search the Dow Jones marketwatch.com service. We find that this website typically reports news of terminations within one day of the termination notices. Moreover, marketwatch.com provides a synopsis of the broker's reason for termination, allowing non-clients to infer whether a stock was terminated for endogenous or exogenous reasons.

There is considerable overlap between the sector terminations identified from Reuters, the news reports, and the text archives. In the rare instances where the sources disagree on announcement

[10] Suppose some oil & gas stocks (GICS code 101020) are covered by the energy team. If the broker fires the oil & gas team but not the energy team, we will observe less than 100% of the oil & gas GICS coverage being terminated.

[11] Search terms used include: "Terminate/discontinue/withdraw/suspend/cease/drop/stop coverage", "Not rated/covered". The vast majority of termination notices identified this way relate to individual companies (not sectors) which are dropped for endogenous-sounding reasons, such as "The company no longer fits with our core investment objectives and coverage universe;" "owing to potential bankruptcy proceedings;" or "because of the company's relatively low institutional ownership, low retail interest, and low trading volume." We do not include such terminations in our sample as they likely violate the exclusion restriction.

[12] The first quote is from http://www.sec.gov/rules/sro/34-48252.htm. The second quote is from http://finra.complinet.com/en/display/display_main.html?rbid=2403&element_id=3675.

dates, we choose the earliest one. For news reports and for many of the termination notices from the text archives, we have time stamps and so can pinpoint announcements with great precision.[13]

A.3. Sample

Combining the broker-closure and sector terminations, we have 21,782 candidate events. We then apply the following filters. First, using CRSP delisting codes, we remove 434 stocks that were involuntarily delisted from an exchange within three months of a termination notice. Second, we remove 5,396 terminations that are followed by a reinitiation by the same analyst at a new employer, or by a different analyst at the original brokerage firm, within three months of the drop.[14] Finally, we exclude 528 REITs, 326 ADRs, 80 non-common stocks and closed-end funds (i.e., CRSP share codes > 12), and 79 companies without valid CRSP permno identifiers.

Our final sample consists of 14,939 coverage terminations, 1,793 of which are due to brokerage closures. The sample includes 4,022 unique stocks and spans all Fama-French industries, 172 brokerage houses, and 1,552 analysts.[15] The sample contains 1,816 terminations in 2000, 1,720 in 2001, 3,593 in 2002, 2,685 in 2003, 2,391 in 2004, and 2,734 in 2005. In 1,347 cases, the termination left a stock without any analyst coverage. We refer to these as orphaned stocks.

Table I compares summary statistics for the size, liquidity, and return volatility of the stocks in our sample to the CRSP universe of publicly traded U.S. stocks and to the universe of U.S. stocks covered by at least one brokerage firm according to Reuters Estimates or I/B/E/S. Sample stocks

[13]We deliberately do not use data from I/B/E/S or First Call to date terminations. Ellul and Panayides (2007), who do, comment: "The date that we use is the one when the last estimate of the EPS estimate is recorded by either I/B/E/S and/or FirstCall. In practice, this is the date when the analyst's report is *communicated* to I/B/E/S and FirstCall. It is not necessarily the date when the analyst's report is *published*." Kecskes and Womack (2007) face a similar problem, and resort to conducting their event study at an annual frequency. The news reports, termination notices, and the Reuters Estimates coverage table we use do not suffer from this shortcoming.

[14]If clients know, or believe, that coverage will shortly be re-initiated, the expected change in information asymmetry may be negligible, and the data bear this out: The average price fall for the 5,396 re-initiations we screened out is one-tenth the size of the average price fall in our final sample.

[15]We identify the analyst covering the stock from the name on the termination notice or the Reuters coverage table where available, or else from the I/B/E/S earnings forecast database.

are on average larger and more liquid than the average stock in the CRSP universe. The comparison to the Reuters and I/B/E/S universe shows that this reflects a general tendency among analysts to cover larger and more liquid stocks. In addition, sample stocks – like Reuters and I/B/E/S stocks more generally – are less volatile than CRSP stocks. On average, 6.4 analysts covered a sample stock before a coverage drop, in line with covered stocks more generally. Overall, the average sample firm looks similar to the average firm in the Reuters Estimates and I/B/E/S databases, in terms of size, liquidity, volatility, and analyst coverage.

B. Do Terminations Increase Information Asymmetry?

Identification requires that coverage terminations increase information asymmetries among investors. We use four popular empirical measures to proxy for information asymmetries: Easley et al.'s (1997) probability of informed trading (*PIN*), bid-ask spreads, Amihud's (2002) illiquidity measure, and Lesmond et al.'s (1999) measure of illiquidity based on the number of zero or missing return days. In addition, we examine changes in earnings surprises and return volatilities at earnings releases around coverage terminations.[16]

Throughout, we use difference-in-difference (DiD) tests to help remove biases due to omitted variables or secular trends affecting similar companies at the same time (Ashenfelter and Card (1985)). For instance, bid-ask spreads may fall over time due to decimalization or competition from ECNs, obscuring the effect of the termination treatment. To remove common influences, DiD tests compare the change in a variable of interest for treated firms to the contemporaneous change for a set of control firms matched to have similar characteristics but which are themselves unaffected by the treatment. Given our focus on asset pricing, we follow Hong and Kacperczyk (2007) and

[16]For brevity, the tests focus on the whole sample. In the subsample of closure-related terminations, we find results that are at least as strong economically, and in most instances considerably stronger. These are available on request.

match firms on the Fama-French (1993) and Carhart (1997) pricing factors using the Daniel et al. (1997) algorithm. Specifically, we randomly choose as controls for firm i five unique firms in the same size, book-to-market, and momentum quintile in the month of June prior to a termination, subject to the condition that control firms did not themselves experience a termination in the quarter before and after the event. In view of the evidence from Table I that firms with analyst coverage are larger and more liquid than CRSP firms in general, we also require that controls must be covered by one or more analysts in the three months before the event.

B.1. Results

Table II, Panel A reports results for *PIN*. If our coverage terminations increase information asymmetries, we expect *PIN* to increase. We obtain *PIN* data for 12,011 of our sample events from Stephen Brown (see Brown, Hillegeist, and Lo (2004)). Even though the data are only available quarterly, the results are quite precisely estimated. Net of the change among control firms, *PIN* increases on average by 1.6% between the quarter before a termination and the quarter after. This and the following statistics are each highly statistically significant.[17]

Panel B reports changes in bid-ask spreads around coverage terminations. (This and the next two statistics are computed from daily data over either six- or 12-month estimation windows ending 10 days before and starting 10 days after a termination announcement.) Spreads decline for both sample and control stocks, presumably reflecting the benign effects of decimalization etc., but they decline more slowly for sample stocks. As a result, the difference-in-difference tests show that bid-ask spreads increase relative to control stocks following coverage termination, by around 3.3%. This is consistent with an increase in information asymmetry.

[17] Both closure-related and sector terminations naturally cluster in time by broker. This poses a problem for standard cross-sectional tests, so we bootstrap the standard errors following Politis and Romano (1994). We report p-values based on the dependent block bootstrap with 10,000 replications and Politis and White (2004) block lengths.

Wang (1994) predicts that the correlation between absolute return and dollar volume increases in information asymmetry. Panel C tests this prediction using changes in Amihud's (2002) illiquidity measure (AIM). For the six-month window, AIM averages -3.892 before and -3.966 after a coverage termination, implying that the correlation between absolute return and volume *de*creases.[18] The difference-in-differences, on the other hand, averages 0.046, implying an *in*crease in information asymmetry following coverage terminations. The 12-month estimate is similarly positive, averaging 0.075. These estimates correspond to a 1.2% and 1.9% average increase in AIM, respectively.

An alternative measure of illiquidity is due to Lesmond et al. (1999). A large number of zero-return or missing-return days in a given estimation window may indicate an illiquid stock. Panel D shows that this number increases by 7.1% and 13.1% following a coverage termination, net of the control-firm change and measured over six- and 12-month windows, respectively.

Our final test focuses on earnings announcements. Following coverage terminations, we expect more volatile returns around earnings announcements (as more uncertainty is left unresolved; see West (1988)) and greater absolute earnings surprises (if post-termination consensus forecasts reflect reduced information sets). Panel E confirms these predictions. Log daily return volatility in the three days around earnings announcements increases by 2% net of the change among control firms, while the average magnitude of earnings surprises increases by 10.8%.

B.2. Summary

Each proxy in Table II yields evidence consistent with the interpretation that information asymmetry increases following coverage terminations. Thus, our terminations sample appears to satisfy the first of the two conditions for identification. Before we turn to the second, we briefly discuss

[18]This illustrates the importance of using a difference-in-differences estimator. The numerator of AIM is stationary (a return) whereas the denominator is non-stationary (price $\cdot$ volume). If returns are constant over time but dollar volumes increase, ΔAIM will generally be negative.

untabulated results for the subsample of closure-related terminations and for orphaned stocks. Closure-related terminations behave just like the sample overall, with similar-sized changes in our proxies for information asymmetry. In the case of orphans, we find considerably larger changes, indicating that these suffer the greatest increase in information asymmetry. To illustrate, AIM increases by between 17.1% and 24.7% when a stock becomes an orphan, while absolute earnings surprises increase by 36.8% on average.

C. Are Sample Terminations Exogenous?

Closure-related terminations satisfy the exclusion restriction unless brokerage firms closed their research departments because their analysts had negative private signals about the stocks they covered. In view of the quotes in Section A.1., it seems more likely that brokerage firms quit research for strategic and financial reasons (often in the context of withdrawal from investment banking more generally) than to mask negative news about companies their analysts covered.

Sector terminations are trickier. The exclusion restriction would be violated if brokers systematically chose to terminate sectors whose characteristics correlate with variables of interest, such as value or demand. Whether they do so is an empirical question we can test.

We model a brokerage firm's choice of which sectors to stop covering at time t using McFadden's (1974) choice model. For each broker, the choice set from which sector terminations are drawn includes all sectors the broker covers at time t. The unit of analysis is hence a broker-sector pair. The dependent variable equals one if a sector is chosen for termination and zero otherwise. To be included in the estimation sample, we require that the brokerage firm drop at least one sector at time t (so we model the choice of which sectors are terminated conditional on the firm's decision to restructure). We exclude brokerage firms that close down their research departments altogether (i.e., those that terminate every sector they cover).

Brokers are assumed to select those sectors whose termination will maximize their expected profit. We parameterize expected profit as a function of sector performance (namely, prior and future sector returns and prior and future sector earnings surprises); sector characteristics (return volatility, a proxy for competition from other brokers, and two proxies for the revenue a sector generates for that particular brokerage firm by way of trading commissions and investment banking fees); and analyst characteristics (an *Institutional Investor* 'all-star' indicator, the analyst's forecast accuracy relative to his sector-matched peers at other brokerage firms, his experience, and his tenure with the broker). The precise variable definitions can be found in Table III.

The model is estimated using probit without (column 1) or with random brokerage-firm effects (column 2). We find no evidence that brokers terminate sectors with poor past or poor anticipated stock performance. The same goes for earnings surprises: Brokers do not terminate sectors whose earnings have recently disappointed, nor do they terminate sectors whose subsequent earnings will fall short of consensus. The coefficient estimates are not only statistically insignificant, they are also economically small, as the marginal effects in Table III indicate. These results suggest that sector termination choices are not information-driven. Instead, brokers are significantly more likely to drop sectors that produce low commission or investment banking revenue as well as those covered by inexperienced, recently hired, unrated analysts with poor forecasting records.[19]

As a final test of the exclusion restriction, we test directly whether sample terminations are uninformative about the affected stocks' future prospects, as required for identification. Suppose at time t, a broker terminates coverage. If this signals private information about the stock, we should be able to predict its future performance from the fact that coverage was terminated. Testing this

[19]Though not shown, year effects are not statistically significant. Brokers thus appear not to herd in which sectors they terminate in a given year.

requires an assumption about the nature of the signal. Assume that it is negative information about $t+1$ earnings that is not yet reflected in the consensus earnings forecast dated $t-1$ (i.e., before other analysts knew of the coverage termination). When earnings are eventually announced, they will fall short of the $t-1$ consensus, resulting in a negative earnings surprise. If, on the other hand, the termination is exogenous, earnings will not differ systematically from consensus.

We implement this test in a panel of CRSP companies over the period 2000Q1 to 2006Q1. (We filter out companies with share codes greater than 12, leaving 4,148 firms and 65,629 firm-quarters.) The dependent variable is earnings surprise, defined as $(EPS_{t+1} - consensus\ forecast_{t-1})/(book\ value\ of\ assets\ per\ share_{t+1})$. Earnings and forecast data come from I/B/E/S. The variables of interest are two indicators. The first identifies the sample terminations. Its coefficient should be statistically zero if sample terminations are uninformative about future earnings. The second identifies 16,354 non-sector (and so likely endogenous) terminations available in the Reuters Estimates coverage table.[20] Here, we expect a negative coefficient. We also control for lagged earnings surprises, returns and return volatility measured over the prior 12 months, the log number of brokers covering the stock, and year effects.[21] This yields the following estimates:

$$\begin{aligned} \textit{earnings surprise} &= \underset{.017}{.364}\ \mathrm{L}(\textit{earnings surprise}) + \underset{.040}{.466}\textit{return} - \underset{.370}{4.814}\textit{volatility} + \underset{.019}{.228}\#\ \textit{brokers} \\ &\quad - \underset{.042}{.128}\textit{endogenous termination} + \underset{.040}{.016}\textit{sample termination} + \textit{year effects} \end{aligned}$$

The adjusted R^2 is 14.6%. We report heteroskedasticity-consistent standard errors (clustered on CRSP permno) below the coefficients. Earnings surprises are serially correlated, more positive for

[20] We identify 53,092 'endogenous' terminations from the Reuters coverage table. Endogenous terminations tend to cluster, as multiple brokers drop coverage of the same stock at or around the same time. Thus, there is an endogenous termination in only 16,354 of the 65,629 firm-quarters in the panel.

[21] Adding firm fixed effects to control for unobserved heterogeneity in firm characteristics halves the effect of one control (volatility) but otherwise does not alter our results.

high-return and low-volatility companies and among firms covered by many analysts. Controlling for these effects, earnings surprises are significantly more negative following 'endogenous' terminations ($p = 0.002$). On average, an endogenous termination is followed by a quarterly earnings surprise that is 21.6% more negative than the sample average over this period. The 14,939 sample terminations, by contrast, are neither statistically nor economically related to subsequent earnings surprises. This is consistent with sample terminations being uninformative and thus exogenous.

III. The Effect of Information Asymmetry on Price and Demands

A. Change in Price

To test the first part of Proposition 1, we compute cumulative abnormal returns (CARs) using two benchmarks (the market model and the Fama-French three-factor model) from the close on the day before the termination announcement to the close on the announcement day [–1,0], one day later [–1,+1], or three days later [–1,+3]. Table IV reports the results for the overall sample (Panel A) as well as the subsample of terminations that do not coincide with a negative earnings surprise (Panel B) and the subsample of closure-related terminations (Panel C), and broken down by the number of other brokers covering the stock (Panel D). In each case, CARs are highly statistically significant, regardless of how we compute standard errors.

Consistent with Proposition 1, price falls following the announcement of a coverage termination. For the sample as a whole, CARs range from -51 to -71 basis points (not annualized).[22] For the median sample firm, this amounts to a fall in market value of between \$1.8 and \$2.1 million. Could confounding events be driving this result? If our terminations are truly exogenous, we

[22]Despite the widespread view that endogenous coverage terminations are implicit sell recommendations, we are not aware of studies estimating announcement returns for such implicit sells. For explicit sell recommendations (i.e., downgrades to sell), Womack (1996) finds average three-day excess returns of -3.87%.

effectively have a randomized trial and controls for confounding events are superfluous. Analysis of one particular event, coincident negative earnings surprises, illustrates. There are about as few coincident negative earnings surprises as chance alone would predict. When these are excluded, CARs range from -47 to -60 basis points, barely changing the result.

Among the subsample of closure-related terminations, CARs are more negative than in the sample as a whole, averaging between -1.13% and -4.02% depending on benchmark. Stratified by coverage, CARs increase monotonically (become less negative) the more brokers continue to cover a stock. To illustrate, over the [-1,0] interval, orphaned stocks lose 99 basis points in the market model, while stocks covered by more than 15 other analysts lose only 35 basis points. This suggests that the degree of information asymmetry decreases in the extent of analyst coverage.

Though not shown, we find no evidence that these price falls are temporary. After one month, for instance, CARs still average -47 basis points.

B. Changes in Demand

According to Proposition 1, investors who choose to become informed following a coverage termination increase their holdings while the uninformed sell. Although we cannot identify directly who becomes informed, it seems plausible that retail investors are less likely to produce their own research than are institutional investors, which not only likely have a cost advantage in producing research themselves but often also maintain trading accounts with multiple brokerage firms and so face a relatively smaller loss of analyst information to begin with. We thus assume that a larger fraction of institutions than of retail investors becomes informed.[23]

[23] In an auxiliary test, we compare the price impact of sample terminations at two types of brokerage firms: Those that serve both retail and institutional investors, and those that serve only institutional investors. (Brokers are classified based on information from the annual Factbook of the Securities Industry and Financial Markets Association.) Announcement-day CARs are significantly lower for terminations at institution-only brokers, averaging -18 vs. -61 basis points using the three-factor model. This is consistent with our assumption that institutions are

Unfortunately, we have no high-frequency trading data with which to estimate changes in institutional and retail demand.[24] Instead, we use the quarterly CDA/Spectrum data to compute the change in the fraction of a sample company's outstanding stock held by institutions required to file 13f reports.[25] Panel A of Table V shows that 13f institutions as a group increase their holdings from 61.9% to 62.9% of shares outstanding following the average termination ($p < 0.001$). Net of the contemporaneous change in control firms, the average increase is 0.9 percentage points ($p < 0.001$). Thus, 13f institutions are unusually large net buyers following coverage terminations. If institutions are more likely to generate their own research than are retail investors, this result is consistent with Proposition 1.

For robustness, Panel B provides similar evidence for the subsample of closure-related terminations. Panel C shows a breakdown of the mean net changes in institutional holdings by the number of brokers covering the stock. While not monotonic, institutional holdings increase the most, and retail holdings decrease the most, when coverage drops to zero.[26]

C. Testing the Comparative Statics: Price

To test the price-related comparative statics in Implications 1 through 4, we regress Fama-French announcement-day CARs on proxies for the five parameters of the model: δ (the fraction of informed investors), $\bar{X}$ (mean aggregate supply), σ_x^2 (variance of aggregate supply), σ_u^2 (payoff variance), and σ_v^2 (signal noise).[27] We also control for whether the termination coincides with a negative earnings surprise as well as firm fixed effects (using CRSP permnos) and announcement-year effects.

less affected by terminations.

[24]Trade size is sometimes used to infer retail trades, but decimalization in January 2001 and the growth in algorithmic trading mean that small trades are no longer viewed as a good proxy for retail trades.

[25]Investment companies and professional money managers with over $100 million under management are required to file quarterly 13f reports. Reports may omit holdings of fewer than 10,000 shares or $200,000 in market value.

[26]Xu (2006) finds that institutions reduce their ownership of orphaned stocks in a sample that does not screen out endogenous terminations. Xu's finding is consistent with the view that *endogenous* terminations are implicit sells.

[27]Results are robust to using market-model CARs and alternative estimation windows.

Our main proxy for δ is the fraction of the company's stock held by institutional investors, call it φ. We do not claim that every institution will choose to become informed (i.e., that $\delta = \varphi$). For the proxy to work, we only require that δ correlate positively with φ. This will be the case if institutions are more likely to become informed than retail investors. We use the first two moments of the distribution of log daily share turnover to proxy for $\bar{X}$ and σ_x^2. The proxy for payoff variance σ_u^2 is the standard deviation of the year-on-year growth rate in quarterly earnings per share. We parameterize signal noise σ_v^2 as a function of the number of other analysts covering the stock and the quality of the analyst whose coverage is lost. Stocks covered by fewer analysts should have noisier signals, while high-quality analysts presumably produce more informative signals, so their terminations should lead to larger price falls.

Table VI reports the results. The adjusted R^2 of 37.2% in column 1 suggests this model has reasonable fit. With one exception, all coefficients are statistically significant at the 5% level or better, and all have the predicted sign. Price falls are larger, the smaller are institutional holdings, the larger and more variable is turnover, the more volatile is earnings growth ($p = 0.081$), the fewer analysts cover the stock, and for terminations involving all-stars or more experienced analysts or those with high relative forecast accuracy. Economically, the largest effect is $\partial \Delta EP/\partial \bar{X}$. A one standard deviation in our proxy for $\bar{X}$ is associated with a fall in price of 31 basis points, all else equal. The smallest effect is $\partial \Delta EP/\partial \sigma_u^2$ (-5.4 basis points).

Our findings are robust to restricting the sample to closure-related terminations (column 2). All signs are unchanged and the economic magnitude of each effect is stronger (with the exception of the extent of coverage and analyst experience, which become statistically insignificant).

Column 3 returns to the sample as a whole and adds the size of the brokerage firm's retail and institutional sales forces as further proxies for $(1 - \delta)$ and δ, respectively. Interestingly, only the size of the retail sales force appears to matter: The larger the retail sales force, the more negative the price change ($p = 0.005$); a one standard deviation increase (say, from Ladenburg, Thalman to Prudential Securities) is associated with a 10.3 basis point lower CAR. The fact that the size of the broker's institutional sales force has no effect is consistent with our assumption that retail investors are more sensitive to changes in research coverage while institutions are more likely to become informed some other way.

Recall that Implication 4 predicts a U-shaped relation between ΔEP and signal noise. In column 4, we add the level and square of analyst forecast dispersion as further proxies for σ_v^2. The data provide some support for a U-shaped relation: The coefficient for the level of forecast dispersion is negative ($p = 0.099$) while that for the squared term is positive ($p = 0.034$). However, the implied minimum is in the far right tail of the empirical distribution of forecast dispersion, so as a practical matter, in our data, ΔEP decreases in all our proxies for signal noise.

D. Testing the Comparative Statics: Demands

To test the demand-related comparative statics in Implications 1 through 4, we estimate the same four specifications as in the previous section but use the difference-in-difference change in institutional holdings as the dependent variable. This is defined as in Section III.B and is intended to proxy for the change in informed demand, ΔEID.

The results, shown in Table VII, generally support the implications. In column 1, the signs for $\partial \Delta EID/\partial \delta$, $\partial \Delta EID/\partial \bar{X}$, $\partial \Delta EID/\partial \sigma_x^2$, and $\partial \Delta EID/\partial \sigma_u^2$ are all as predicted, and each coefficient is highly statistically significant. (Recall that the expected signs of the demand effects are exactly opposite to those for the price effects.) Only one of our proxies for signal noise, the analyst's all-star status, is significant; it has the predicted positive sign. Restricting the sample to closure-related terminations (shown in column 2) gives less precisely estimated coefficients, and we lose support for $\partial \Delta EID/\partial \bar{X} > 0$ and $\partial \Delta EID/\partial \sigma_x^2 > 0$. Controlling for the size of the broker's sales force (in column 3) improves the significance of two signal noise proxies, all-star status and experience. Adding forecast dispersion and its square (in column 4) provides support for the predicted inverse U-shaped relation between changes in informed demand and signal noise.

Our results for changes in demand are generally noisier than those for changes in price. This is not surprising, given the quarterly nature of the 13f institutional ownership data. We view these results as encouraging, though, given the data limitations, they should be interpreted with caution.

E. Testing Corollary 2: Change in Expected Returns

Falling prices following an increase in information asymmetry suggest that investors' expected returns have increased. Corollary 2 states that expected returns increase because affected stocks become more sensitive to liquidity risk. To test this prediction, we estimate the following model:

$$r_{i,t}^e = \alpha_i + (\beta_i + \Delta\beta_i^{Post} I_{t\in Post} + \Delta\beta_i^{Post\&Term} I_{t\in Post} I_{i\in Term}) Factors_t + \epsilon_{i,t} \tag{7}$$

where $r_{i,t}^e$ is stock i's month-t return in excess of the riskfree rate; $I_{t\in Post}$ is an indicator for the post-event period; $I_{i\in Term}$ identifies firms in the terminations sample (as opposed to matched controls, selected as before); and $Factors_t$ is a vector which includes the three Fama-French factors (MKT, SMB, and HML) and a liquidity factor, LIQ.[28] We use LIQ to proxy for liquidity risk. Table VIII reports results for three alternative versions of LIQ: Sadka's (2006) liquidity factor (Panel A), Pastor and Stambaugh's (2003) nontraded liquidity factor (Panel B), and Pastor and Stambaugh's traded liquidity factor (Panel C).

[28] We do not include Carhart's (1997) momentum factor because several studies find that momentum is largely driven by liquidity, which is our focus. See Pastor and Stambaugh (2003) and Sadka (2006).

Regression (7) allows us to measure whether a stock's factor loadings change following a termination, compared to otherwise similar control firms which experience no termination. This effect is captured by the difference-in-differences term, $\Delta\beta_i^{Post\&Term}$. To estimate the model, we impose the restriction that $\Delta\beta_i^{Post}$ and $\Delta\beta_i^{Post\&Term}$ are common to all firms and use a two-step approach analogous to that of Pastor and Stambaugh (2003); the details of the estimation can be found in Table VIII.[29] The model is estimated with monthly data using 12-, 18-, or 24-month windows ending the month before a termination or beginning one month after.

The results are consistent with Corollary 2. With three alternative liquidity factors and three estimation windows, we have nine sets of results. In each, the estimate for $\Delta\beta_{LIQ}^{Post\&Term}$ is positive, significantly so in six of the nine. These results suggest that the returns of companies experiencing coverage terminations become more sensitive to liquidity risk, relative to matched firms with unchanged analyst coverage. Economically, the effects are largest using Sadka's (2006) liquidity factor. Relative to the pre-termination means, liquidity betas in Panel A increase by between 5.9% and 8.3%. (The corresponding increases in Panels B and C range from 1.6% to 6.4%.)

In each specification, post-termination returns load significantly less strongly on the market factor. Changes in loadings on the SMB and HML factors, on the other hand, are model-specific. Using the Sadka (2006) factor, the changes are statistically zero (see Panel A). Using the Pastor-Stambaugh (2003) factors, $\Delta\beta_{SMB}^{Post\&Term}$ is positive and significant for the traded liquidity factor with 18- or 24-month estimation windows (see Panel C), though note that data for this factor are available only through 2004. $\Delta\beta_{HML}^{Post\&Term}$ is positive and significant using either Pastor-Stambaugh factor with 18- or 24-month estimation windows (see Panels B and C).

[29]Like theirs, ours is a system of asset return equations for thousands of firms with time-varying factor loadings. The cross-equation parameter restrictions and two-step approach allow us to reduce the parameterization of the model and increase the precision of the estimates in a computationally efficient way. For further details, see Pastor and Stambaugh (2003, p. 665).

The overall effect of the changes in factor loadings is to increase the expected returns of stocks experiencing coverage terminations. For example, using mean factor returns for our sample period, the changes in coefficients in Panel A imply that expected returns increase by between 14 and 44 basis points a year.[30]

IV. Conclusion

We test models of asset pricing under asymmetric information using plausibly exogenous variation in the supply of information caused by the closure or restructuring of brokerage firms' research operations. Consistent with predictions we derive from a Grossman and Stiglitz-type model, we find that share prices and uninformed investors' demands fall in response to exogenous terminations of analyst coverage. In the cross-section, the magnitudes of these falls are consistent with the comparative statics of the model: They are larger, the more investors are uninformed, the larger and more variable is turnover, the more uncertain is the asset's payoff, and the noisier is the better-informed investors' signal. Finally, we show that prices fall because expected returns become more sensitive to a factor proxying for liquidity risk.

Beyond providing relatively direct empirical support for asymmetric-information models of asset prices, we hope that our quasi-experiment can serve as a useful instrument for empirical work in other applications that examines the effects of information asymmetry. In this spirit, we intend to make our set of exogenous coverage terminations available to fellow researchers.

[30] The increase in expected returns implies that realized post-termination returns will exceed a benchmark that fails to account for the factor loading changes. In other words, unless the benchmark is suitably adjusted, firms experiencing a termination will *appear* to eventually bounce back, generating positive abnormal returns (or alpha).

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Appendix A: Proof of Proposition 1

In all cases, investor i solves

$$\max_{\tilde{D}^i} E[-e^{C_i}|\mathcal{F}^i] \text{ s.t. } C_i = R(W_0 - \tilde{D}_i) + \tilde{D}_i(u - RP),\ i \in \{informed, uninformed\} \tag{8}$$

where $\tilde{D}_i$ is investor i's demand for the risky asset, and subscript i denotes whether or not the investor observes the signal. Optimal demand is therefore given by

$$D_i = \frac{E[u|\mathcal{F}^i] - RP}{V[u|\mathcal{F}^i]} \tag{9}$$

where $V[u|\mathcal{F}^i]$ denotes the variance of u conditional on information set $\mathcal{F}^i$.

Case 1: Symmetric Information

An informed investor's expectation for the risky asset payoff, making use of the multivariate Gaussian nature of the model, is

$$E[u|\mathcal{F}^i] = E[u|s] = \theta + \frac{\sigma_u^2}{\sigma_u^2 + \sigma_v^2} s \tag{10}$$

while the variance of u conditional on the signal is

$$V[u|s] = \sigma_u^2 \Big(1 - \frac{\sigma_u^2}{\sigma_u^2 + \sigma_v^2}\Big). \tag{11}$$

The current case assumes symmetry with all investors informed. The market-clearing condition equates total optimal demand under symmetric information with total supply, $D_{informed} = X$, implying

$$\begin{aligned} P_{symm} &= \frac{1}{R}(E[u|s] - XV[u|s]) && (12)\\ &= \frac{1}{R}\Big(\theta + \frac{\sigma_u^2}{\sigma_u^2 + \sigma_v^2} s - \sigma_u^2 (1 - \frac{\sigma_u^2}{\sigma_u^2 + \sigma_v^2}) X\Big) && (13) \end{aligned}$$

Case 2: Asymmetric Information

In the absence of an analyst, a fraction δ of the investors pay c to obtain the payoff signal s. Uninformed investors base their demand solely on the observed price. To do this, they must filter from the price a noisy inference of the informed investors' signal. Under rational expectations, the uninformed know the price is linear in the informed investors' signal and the asset's supply, that is,

$$P = c_1 + c_2 s + c_3 X. \tag{14}$$

For now, we use this price form as a conjecture to be verified in equilibrium, at which point we also solve for the constants c_1, c_2, and c_3.

The conditional moments of u given an uninformed investor's information set are:

$$E[u|\mathcal{F}^{UI}] = E[u|P] = \theta + \frac{c_2 \sigma_u^2}{c_2^2(\sigma_u^2 + \sigma_v^2) + c_3^2 \sigma_x^2}(c_2 s + c_3[X - \bar{X}]) \tag{15}$$

$$V[u|P] = \sigma_u^2\Big(1 - \frac{c_2^2\sigma_u^2}{c_2^2(\sigma_u^2+\sigma_v^2)+c_3^2\sigma_x^2}\Big). \tag{16}$$

When a fraction δ of investors are informed, the equilibrium price is found from the market-clearing condition $\delta D_{informed} + (1-\delta)D_{uninformed} = X$, giving

$$P_{asymm} = \frac{1}{R}\Big(\frac{\delta}{V[u|s]} + \frac{(1-\delta)}{V[u|P]}\Big)^{-1}\Big(\frac{\delta E[u|s]}{V[u|s]} + \frac{(1-\delta)E[u|P]}{V[u|P]} - X\Big) \tag{17}$$

We can use conditional moments (15) and (16) to show that P_{asymm} is indeed linear in the informed investors' signal and the asset's supply. Matching the coefficients on s and X with the conjectured form (14) and solving for c_2 and c_3 gives

$$c_2 = \frac{\delta\ \sigma_u^2\ \left(\delta + \ \sigma_v^2\ \sigma_x^2\right)}{R\ (\ \sigma_x^2\ \sigma_v^4 + \left(\delta^2 + \delta\ \sigma_u^2\ \sigma_x^2\right)\ \sigma_v^2 + \ \sigma_u^2\,\delta^2)} \tag{18}$$

$$c_3 = \frac{-\sigma_v^2\ \sigma_u^2\ \left(\delta + \ \sigma_v^2\ \sigma_x^2\right)}{R\ (\ \sigma_x^2\ \sigma_v^4 + \left(\delta^2 + \delta\ \sigma_u^2\ \sigma_x^2\right)\ \sigma_v^2 + \ \sigma_u^2\,\delta^2)}. \tag{19}$$

Finally, we can write c_1 as a function of c_2 and c_3:

$$\begin{aligned} c_1 \quad &= \quad \frac{\theta}{R} - \frac{1-\delta}{R}\ c_2\ c_3\ \bar{X}\ (\ {c_2}^2\ (\ \sigma_u^2 + \ \sigma_v^2) + \ {c_3}^2\ \sigma_x^2)^{-1}\left(1 - \frac{{c_2}^2\ \sigma_u^2}{{c_2}^2\ (\ \sigma_u^2 + \ \sigma_v^2) + \ {c_3}^2\ \sigma_x^2}\right)^{-1} \\ &\times \left(\frac{\delta}{\sigma_u^2}\left(1 - \frac{\sigma_u^2}{\sigma_u^2 + \ \sigma_v^2}\right)^{-1} + \frac{1-\delta}{\sigma_u^2}\left(1 - \frac{{c_2}^2\ \sigma_u^2}{{c_2}^2\ (\ \sigma_u^2 + \ \sigma_v^2) + \ {c_3}^2\ \sigma_x^2}\right)^{-1}\right)^{-1}. \end{aligned} \tag{20}$$

The expressions shown in Proposition 1 follow directly from using these solutions in the price and demand expressions, and noting that the unconditional expectations of the signal and supply are zero and $\bar{X}$, respectively.

Table I. Coverage Terminations: Summary Statistics.

The sample consists of 14,939 coverage terminations (though the sample size varies depending on the availability of variables of interest). This table reports summary statistics for the market value of equity, share turnover (monthly volume divided by shares outstanding), daily return volatility, and the extent of coverage for each stock in the terminations sample; the CRSP universe (share codes 10 and 11); and the universe of stocks with analyst coverage in the union of the Reuters and I/B/E/S databases. For each firm in the terminations sample, we calculate equity value and turnover in the month prior to the first termination date. For the universes of CRSP stocks and covered stocks, these are first computed as monthly averages for 2002 (the midpoint of our sample period) firm-by-firm and then averaged cross-sectionally. Annualized volatility for terminations is the standard deviation of daily continuously compounded returns in the six-month period ending one month prior to a termination, times √252 (a sample firm is omitted from this calculation if it has fewer than three months, or 66 trading days, of non-missing returns.) For the universes of CRSP stocks and covered stocks, volatilities are the annualized daily standard deviations for firms in these samples during calendar year 2002. We exclude firms with fewer than 200 nonmissing return observations in the CRSP database. The number of brokers covering a stock in our terminations sample over the three months prior to the drop is based on combining data from the Reuters and I/B/E/S datasets. The broker count for the universe of covered stocks represents the number of unique brokers that covered each stock in the Reuters or I/B/E/S databases during the three months prior to June 30, 2002. The broker counts are then averaged cross-sectionally.

	Terminations sample	CRSP universe in 2002	Universe of covered stocks in 2002
Equity market value ($m)			
Mean	3,782.4	2,197.2	3,254.5
Median	533.3	145.8	384.8
Range	1.5 – 367,265	3.0 – 369,002	0.9 – 369,002
Monthly turnover			
Mean	0.16	0.11	0.14
Median	0.10	0.06	0.09
Range	0 – 2.58	0 – 2.96	0 – 2.86
Daily return volatility (annualized %)			
Mean	65.6	71.8	65.2
Median	56.7	58.9	55.6
Range	11.5 – 332.2	5.4 – 606.0	5.4 – 519.3
Number of brokers covering stock			
Mean	6.4		5.9
Median	4.0		4.0
Range	1 – 43		1 – 46
Number of firms	4,022	5,796	3,827

Table II. The Effect of Coverage Terminations on Information Asymmetry.

The table reports cross-sectional means of proxies for information asymmetry before and after a termination for sample stocks and their matched controls. For each sample termination, a control group is formed by randomly selecting five stocks with the same Daniel et al. (1997) style-benchmark assignment in the month of June prior to a termination, subject to the conditions that control firms a) were covered by one or more analysts in the three months before the event and b) did not themselves experience a coverage termination in the quarter before and after the event. (We lose 1,395 events involving stocks that do not satisfy Daniel et al.'s data conditions. We also drop observations that have no viable controls for a given test. The number of observations ranges from around 12,000 to around 14,000.) We then calculate a difference-in-difference test for each sample stock *i*, DiD $= (post_i - pre_i) - (\overline{post - pre}_{Control\ Group\ i})$, and report the cross-sectional mean. We also report the mean percentage change (DiD/mean before – 1). Panel A reports the mean probability of informed trading for stocks in the termination sample in the quarters immediately preceding and following the quarter in which the coverage termination occurred. We use Stephen Brown's quarterly *PIN* estimates. Panel B reports changes in bid-ask spreads. Spreads are computed as (ask–bid)/(ask+bid)/2 using daily closing bid and ask data from CRSP. This and the next two statistics are averaged over six-month and 12-month windows ending 10 days prior to the termination announcement or starting 10 days after the announcement date. Panel C reports changes in the log Amihud illiquidity measure. This is defined as the natural log of the ratio of the stock return to the dollar trading volume and scaled by 10^6; see Amihud (2002, p. 43). Panel D reports the changes in the number of days with zero or missing returns in CRSP (using missing return codes -66, -77, -88, and -99). Panel E reports the effects of terminations on quarterly earnings announcements. The first measure is the log of daily return volatility in a three-day window around earnings announcements for all earnings announcements occurring in a one-year window before or after the drop. The second measure is the mean absolute value of quarterly earnings surprises in a one-year window before or after the drop. A surprise is defined as the absolute value of actual quarterly earnings minus the latest I/B/E/S consensus estimate before the earnings announcement, scaled by book value of equity per share, and multiplied by 100 for expositional purposes. Earnings surprise cannot be computed for orphaned stocks. To adjust for potential cross-sectional dependence due to overlapping estimation windows, *p*-values for the difference-in-difference tests are calculated using a block bootstrap (with optimal block length chosen according to the optimality criterion of Politis and White (2004)).

	Terminations		Control group		Mean	*p*-value	*Economic magnitude (percentage*
	Before	After	Before	After	DiD	DiD = 0	*change)*
Panel A: *PIN*							
PIN	0.142	0.144	0.152	0.152	0.002	0.002	*1.6%*
Panel B: Bid-ask spreads							
6-month window	0.206	0.183	0.232	0.201	0.007	<0.001	*3.4%*
12-month window	0.206	0.156	0.229	0.172	0.007	0.002	*3.3%*
Panel C: Amihud illiquidity measure (*AIM*)							
6-month window	-3.892	-3.966	-3.585	-3.706	0.046	<0.001	*1.2%*
12-month window	-3.889	-4.069	-3.562	-3.816	0.075	<0.001	*1.9%*
Panel D: Missing and zero-return days							
6-month window	3.505	3.675	3.927	3.848	0.249	<0.001	*7.1%*
12-month window	7.133	6.948	8.223	7.107	0.931	<0.001	*13.1%*
Panel E: Earnings announcements							
Volatility	1.360	1.293	0.857	0.763	0.028	<0.001	*2.0%*
Earnings surprise	0.760	0.890	0.816	0.864	0.082	<0.001	*10.8%*

Table III. Conditional Choice Model of Sector Terminations.

We test whether a brokerage firm terminates sector coverage selectively based on the sector's past or future performance, which would undermine our claim that sector terminations contain no information relevant to the valuation of the stocks that lose coverage. The test is a based on a McFadden (1974) choice model. We model a brokerage firm's choice of which sectors to stop covering at time *t*. To be included in the estimation sample, we require that the brokerage firm drop at least one sector at time *t* (so we model the choice of which sectors are terminated conditional on the firm's decision to downsize research). We exclude brokerage firms that close down their research departments altogether (i.e., those that drop every sector they cover). For each broker, the choice set from which sector terminations are drawn includes all sectors the broker covers at time *t*. The unit of analysis is hence a broker-sector pair. The dependent variable equals one if a sector is chosen for termination and zero otherwise. Brokers are assumed to select those sectors whose termination will maximize their expected profit. Sectors are defined using six-digit GICS codes and sector terminations are identified from the sources described in Section II. The explanatory variables are defined as follows. Prior and future *sector performance* is measured as average market-adjusted cumulative returns in the sector over the twelve months before and after the decision date, respectively, using the CRSP value-weight index to make the market adjustment. Prior and future sector earnings surprises are measured as realized quarterly earnings per share minus consensus (i.e., median) forecasts and scaled by book value of equity per share, averaged over the prior four or next four quarters, respectively, and across stocks in the same GICS code. *Sector characteristics* include return volatility (measured over the prior twelve months and averaged across stocks in the same GICS); a proxy for competition from other brokers (the log of one plus the number of other brokers covering the sector during the three months preceding the termination decision); and two proxies for the fee income a sector generates for the brokerage firm in question, in the form of trading commissions and investment banking fees. To proxy for trading commissions, we compute the log aggregate dollar trading volume of only those stocks in a sector that the brokerage firm covered in the month before the termination decision. Sectors in which the brokerage firm covers few stocks, or with covered stocks that are associated with low dollar trading volumes, generate lower trading commissions. To proxy for the investment banking fees a brokerage firm earns in a given sector, we compute the log of the aggregate equity proceeds raised in the 12 months prior to a termination decision by those companies in the sector whose stocks were covered by the brokerage firm's analyst and for which the brokerage firm acted as the lead underwriter. (We use data for the prior 12 months since equity issuance is a relatively rare event.) *Analyst characteristics* include a dummy set equal to one if the analyst covering the sector was ranked first, second, or third in his sector in the annual *Institutional Investor* (*II*) all-star rankings (as of the October issue preceding the sector termination); the analyst's forecast accuracy relative to his sector-matched peers at other brokerage firms (constructed as in Hong and Kubik (2003)); his experience (the log of one plus the number of years the analyst has contributed forecasts to I/B/E/S); and his tenure with the broker (the log of one plus the number of years the analyst has worked for this brokerage firm). Where more than one analyst covers a sector, we compute the maximum value across team members, though our results are not sensitive to this coding. The model is estimated using probit without (column 1) or with random brokerage-firm effects (column 2). In column (1), standard errors are clustered within brokerage firms to reduce the influence of overlapping observations. The random-effects probit in column (2) cannot accommodate clustered standard errors. We use ***, **, *, and † to denote statistical significance at the 0.1%, 1%, 5%, and 10% levels (two-sided), respectively. In addition to the probit coefficients, we report marginal effects as well as economic effects (i.e., marginal effects times a one-standard deviation change in continuous covariates).

Table III. Continued.

	Dependent variable =1 if broker terminates sector coverage, = 0 otherwise					
	Coefficient *s.e.*	Marginal effect (dF/dx)	Economic effect (dF/dx times s.d.)	Coefficient *s.e.*	Marginal effect (dF/dx)	Economic effect (dF/dx times s.d.)
Sector performance						
market-adjusted return in prior 12 months	-0.061	-0.006	-0.003	-0.066	-0.008	-0.003
	0.065			*0.064*		
market-adjusted return in next 12 months	-0.025	-0.002	-0.001	0.020	0.002	0.001
	0.088			*0.089*		
average earnings surprise in prior 4 quarters	-0.006	-0.001	0.000	-0.010	-0.001	-0.001
	0.039			*0.040*		
average earnings surprise in next 4 quarters	0.059	0.006	0.003	0.059	0.007	0.004
	0.044			*0.044*		
Sector characteristics						
return volatility in prior 12 months	-0.846†	-0.082	-0.006	-1.060*	-0.123	-0.009
	0.467			*0.442*		
no. of other brokers covering the sector	-0.007	-0.001	0.000	-0.072	-0.008	-0.005
	0.049			*0.049*		
log lagged monthly dollar trading volume	-0.058***	-0.006	-0.008	-0.037†	-0.004	-0.006
	0.018			*0.020*		
log equity proceeds raised through broker, LTM	-0.070***	-0.007	-0.016	-0.063***	-0.007	-0.018
	0.013			*0.014*		
Analyst characteristics						
=1 if one or more team members are *II* "all-stars"	-0.584***	-0.049	-0.049	-0.575***	-0.059	-0.059
	0.077			*0.075*		
analyst relative forecast accuracy	-1.682***	-0.164	-0.015	-1.613***	-0.188	-0.017
	0.258			*0.256*		
log analyst experience	-0.066	-0.006	-0.004	-0.081*	-0.009	-0.006
	0.043			*0.041*		
log analyst tenure with broker	-0.061*	-0.006	-0.005	-0.039	-0.005	-0.004
	0.026			*0.029*		
Diagnostics						
Brokerage-firm random effects	No			Yes		
Wald-test: random effects = 0 (χ^2)	n.a.			52.7***		
Wald-test: all coef. = 0 (χ^2)	267.5***			196.1***		
Wald-test: all year effects = 0 (χ^2)	4.2			3.5		
Observed probability	0.063			0.063		
Pseudo-R^2	8.4 %			9.6 %		
No. of observations	8,804			8,804		

Table IV. Changes in Price Around Coverage Terminations.

We compute abnormal returns over three different windows using two separate benchmarks: The market model and the Fama-French three-factor model. (Results are nearly identical if we include a Carhart (1997) momentum factor in the Fama-French model.) We use the CRSP value-weight index to proxy for the market return. We report these abnormal return metrics for the overall sample (Panel A) as well as the subsample of terminations that do not coincide with a negative earnings surprise within one week of the termination date (Panel B) and the subsample of closure-related terminations (Panel C), and broken down by the number of other brokers covering the stock in the three months preceding the coverage termination, based on pooling data from Reuters Estimates and I/B/E/S (Panel D). To correct for after-hours announcements, we use time stamps to determine the first trading day after the announcement where available. Abnormal returns are reported in percent. We report test statistics that control for event-induced variance changes. For the market model abnormal returns, we report both the parametric Boehmer, Musumeci, and Poulsen (1991) standardized cross-sectional test and Cowan's (1992) non-parametric generalized sign test statistic (separated by "/"). For the Fama-French model, we report Brown and Warner's (1980) "crude dependence adjustment" *t*-test and the generalized sign test (separated by "/"). We use ***, **, and * to denote statistical significance at the 0.1%, 1%, and 5% levels (two-sided), respectively. The sample falls short of 14,939 because we require 50 trading days of pre-event stock prices to estimate the model parameters.

Estimation window: Close on day before termination to …		# of other brokers covering the stock in the prior 3 months	No. of obs.	Market model		Fama-French three-factor model	
Panel A: All terminations							
… close on day of termination	[-1,0]		14,890	-0.56	***/***	-0.51	***/***
… close on day +1	[-1,+1]		14,890	-0.64	***/***	-0.60	***/***
… close on day +3	[-1,+3]		14,890	-0.71	***/***	-0.66	***/***
Panel B: Terminations excluding coincident negative earnings surprises							
… close on day of termination	[-1,0]		14,405	-0.52	***/***	-0.47	***/***
… close on day +1	[-1,+1]		14,405	-0.58	***/***	-0.55	***/***
… close on day +3	[-1,+3]		14,405	-0.60	***/***	-0.57	***/***
Panel C: Closure-related terminations							
… close on day of termination	[-1,0]		1,788	-1.66	***/***	-1.14	***/***
… close on day +1	[-1,+1]		1,788	-2.59	***/***	-1.97	***/***
… close on day +3	[-1,+3]		1,788	-4.02	***/***	-2.91	***/***
Panel D: Terminations by number of brokers covering the stock							
… close on day of termination	[-1,0]	0	1,330	-0.99	***/***	-0.94	***/***
		1-5	3,847	-0.72	***/***	-0.65	***/***
		6-10	3,353	-0.54	***/***	-0.47	***/***
		11-15	2,383	-0.43	***/***	-0.40	***/***
		>15	3,977	-0.35	***/***	-0.31	***/***
… close on day +1	[-1,+1]	0	1,330	-1.03	***/***	-1.03	***/***
		1-5	3,847	-0.74	***/***	-0.69	***/***
		6-10	3,353	-0.55	***/***	-0.50	***/***
		11-15	2,383	-0.58	***/***	-0.55	***/***
		>15	3,977	-0.52	***/***	-0.50	***/***
… close on day +3	[-1,+3]	0	1,330	-1.23	***/**	-1.29	***/**
		1-5	3,847	-0.72	***/***	-0.65	***/***
		6-10	3,353	-0.63	***/**	-0.56	***/*
		11-15	2,383	-0.76	***/***	-0.69	***/***
		>15	3,977	-0.56	***/***	-0.53	***/***

Table V. Changes in Institutional Holdings Around Coverage Terminations.

The table reports the quarterly change in institutional investors' holdings of stocks that experience coverage terminations. We report the mean fraction of total stock outstanding that is held in aggregate by institutional investors filing 13f reports in the quarter before and the quarter after a termination. We then calculate a difference-in-difference test, DiD $=(post_i - pre_i) - (\overline{post - pre}_{Control\ Group\ i})$, that is, the difference between the pre- and post-termination change for sample stock i less the average change for control stocks. We also report percentage changes (DiD/mean before – 1). Control groups are formed as described in Table II. Panel A reports these statistics for the entire sample of coverage terminations. Panel B restricts the sample to closure-related terminations. Panel C provides a breakdown by the number of other brokers covering the stock in the three months preceding the coverage termination, using the whole sample. The 13f data are taken from Thomson Financial's CDA/Spectrum database. We lose 2,342 events involving stocks that do not satisfy DGTW's data conditions and 29 events with missing 13f data for either the sample firm or the controls. To adjust for potential cross-sectional dependence due to overlapping estimation windows, p-values for the difference-in-difference tests are calculated using a block bootstrap (with optimal block length chosen according to the optimality criterion of Politis and White (2004)). Significance levels of "own"-difference test statistics ($post_i - pre_i$) are similar (not reported).

bin	# of other brokers covering the stock in the prior 6 months	No. of obs.	Terminations Before drop	Terminations After drop	Control group Before drop	Control group After drop	Mean DiD	p-value DiD = 0	*Economic magnitude (percentage change)*
Panel A: All terminations (in %)									
All		12,568	61.9	62.9	58.3	58.5	0.9	<0.001	*1.4%*
Panel B: Closure sample (in %)									
All		1,473	61.0	61.4	57.9	57.6	0.6	0.023	*1.0%*
Panel C: Terminations by number of brokers covering the stock (in %)									
0	0	1,078	36.6	37.3	39.5	38.9	1.4	<0.001	*3.7%*
1	1-5	3,081	55.4	56.9	50.6	51.1	1.0	<0.001	*1.7%*
2	6-10	2,863	67.0	67.8	60.8	61.1	0.6	0.009	*0.9%*
3	11-15	2,072	68.7	70.0	64.5	64.7	1.1	<0.001	*1.6%*
4	>15	3,474	67.1	68.1	66.4	66.5	0.8	<0.001	*1.2%*

Table VI. Cross-sectional Determinants of Changes in Price.

We test the cross-sectional predictions of the model presented in Section I by regressing Fama-French-adjusted cumulative abnormal returns around coverage terminations on proxies for δ (the fraction of informed investors); $\overline{X}$ (mean aggregate supply); σ_x^2 (variance of aggregate supply); σ_u^2 (payoff variance); and σ_v^2 (signal noise). The main proxy for δ is the fraction of the company's stock held by institutional investors as of the quarter-end prior to the termination, estimated from 13f data. In column (3), we add the size of the brokerage firm's sales force (i.e., the number of registered representatives), taken from the Factbook of the Securities Industry and Financial Markets Association and dated January 1 of the termination year. Mean and variance of aggregate supply are based on the first two moments of the distribution of log daily turnover, estimated over the six months ending one month prior to the termination. Our proxy for payoff variance is the standard deviation of the year-on-year growth rate in quarterly earnings per share, using up to 20 quarters of data prior to the termination. We parameterize signal noise as a function of the number of remaining analysts covering the stock; three proxies for the quality of the analyst whose coverage is lost (see Table III); and (in column 4) the level and square of analyst forecast dispersion, defined as the time series mean of the standard deviation of analyst EPS forecasts in the year prior to the termination, scaled by book value per share. We also control for whether the termination coincides with a negative earnings surprise as well as firm fixed effects (using CRSP permnos) and announcement-year effects. (To conserve space, the firm and year effects are not reported.) Column (2) restricts the sample to closure-related terminations. Heteroskedasticity-consistent standard errors, clustered on CRSP permnos, are reported in italics beneath the coefficient estimates. We use ***, **, *, and † to denote significance at the 0.1%, 1%, 5%, and 10% levels (two-sided), respectively.

		Dependent variable: Fama-French CAR [-1,0], in %			
		(1)	(2)	(3)	(4)
δ	13f holdings in quarter before drop	0.489**	1.356*	0.451*	0.551**
		0.191	*0.679*	*0.209*	*0.200*
$(1-\delta)$	log no. of retail registered representatives			-0.032**	
				0.011	
δ	log no. of institutional registered reps			0.005	
				0.019	
$\overline{X}$	mean daily log turnover	-0.301***	-0.785***	-0.291***	-0.304***
		0.045	*0.189*	*0.050*	*0.049*
σ_x^2	std. dev. of log turnover	-0.351**	-1.198**	-0.331*	-0.312*
		0.131	*0.460*	*0.142*	*0.147*
σ_u^2	std. dev. of earnings growth	-0.361†	-1.487†	-0.341	-0.370†
		0.207	*0.909*	*0.279*	*0.210*
σ_v^2	log no. other brokers covering the stock	0.186***	0.100	0.179***	0.161***
		0.046	*0.181*	*0.051*	*0.050*
σ_v^2	=1 if covering analyst was an "all-star"	-0.323*	-2.041**	-0.290*	-0.276*
		0.138	*0.727*	*0.140*	*0.140*
σ_v^2	log experience of covering analyst	-0.139**	-0.377	-0.156**	-0.142**
		0.049	*0.230*	*0.054*	*0.050*
σ_v^2	relative forecast accuracy of covering analyst	-0.858*	-8.824***	-1.018*	-1.035**
		0.353	*1.484*	*0.401*	*0.354*
σ_v^2	analyst forecast dispersion				-4.991†
					3.027
σ_v^2	analyst forecast dispersion^2				10.046*
					4.732
	=1 if coincides w/ neg. earnings surprise	-2.829***	-2.478†	-3.084***	-2.844***
		0.689	*1.277*	*0.763*	*0.693*
	Adjusted *R*-squared	37.2%	15.6%	39.3%	36.3%
	Wald test: all coef. = 0	8.3***	7.5***	7.6***	7.4***
	No. of observations	13,371	1,579	11,484	12,791

Table VII. Cross-sectional Determinants of Changes in Informed Demand.

We test the cross-sectional predictions of the model presented in Section I by regressing a proxy for changes in informed demand around coverage terminations on proxies for δ (the fraction of informed investors); $\overline{X}$ (mean aggregate supply); σ_x^2 (variance of aggregate supply); σ_u^2 (payoff variance); and σ_v^2 (signal noise). Our proxy for the change in informed demand is the difference-in-difference (DiD) change in the fraction of the company's stock held by institutional investors from the quarter before to the quarter after a coverage termination, net of the mean contemporaneous change in institutional holdings in matched control firms. See Table V for details of the construction. The proxies for the independent covariates are as defined in Table VI. We also control for whether the termination coincides with a negative earnings surprise as well as firm fixed effects (using CRSP permnos) and announcement-year effects. (To conserve space, the firm and year effects are not reported.) As in Table V, we lose 2,342 events involving stocks that do not satisfy DGTW's data conditions and 29 events with missing 13f data for either the sample firm or the controls. The number of observations used in each regression depends on data availability for the proxies. Column (2) restricts the sample to closure-related terminations. Heteroskedasticity-consistent standard errors, clustered at the CRSP permno level, are reported in italics beneath the coefficient estimates. We use ***, **, *, and † to denote statistical significance at the 0.1%, 1%, 5%, and 10% levels (two-sided), respectively.

		Dependent variable: DiD change in 13f holdings			
		(1)	(2)	(3)	(4)
δ	13f holdings in quarter before drop	-0.443***	-0.343***	-0.459***	-0.444***
		0.024	*0.069*	*0.027*	*0.020*
$(1-\delta)$	log no. of retail registered representatives			0.000	
				0.000	
δ	log no. of institutional registered reps			-0.001	
				0.001	
$\overline{X}$	mean daily log turnover	0.016***	0.019	0.015***	0.015***
		0.004	*0.014*	*0.004*	*0.003*
σ_x^2	std. dev. of log turnover	0.014***	-0.003	0.012**	0.014***
		0.004	*0.014*	*0.004*	*0.004*
σ_u^2	std. dev. of earnings growth	2.810***	6.459**	2.852***	2.869***
		0.654	*2.203*	*0.730*	*0.559*
σ_v^2	log no. other brokers covering the stock	0.001	-0.013	0.000	0.001
		0.003	*0.015*	*0.004*	*0.003*
σ_v^2	=1 if covering analyst was an "all-star"	0.007†	0.013	0.009*	0.007†
		0.004	*0.029*	*0.004*	*0.004*
σ_v^2	log experience of covering analyst	0.000	-0.001	0.003*	0.003*
		0.000	*0.001*	*0.001*	*0.001*
σ_v^2	relative forecast accuracy of covering analyst	0.013	-0.037	0.013	0.013
		0.009	*0.037*	*0.010*	*0.010*
σ_v^2	analyst forecast dispersion				0.013*
					0.006
σ_v^2	analyst forecast dispersion^2				-0.003*
					0.001
	=1 if coincides w/ neg. earnings surprise	-0.017*	0.035	-0.013†	-0.017***
		0.007	*0.038*	*0.008*	*0.007*
	Adjusted R-squared	45.3%	19.9%	48.1%	45.4%
	Wald test: all coef. = 0	30.7***	4.0***	24.1	26.9***
	No. of observations	11,591	1,355	9,974	11,364

Table VIII. Changes in Factor Loadings Following Coverage Terminations.

The table reports changes in equity return factor loadings around coverage terminations. We estimate four-factor models of firms' monthly stock returns in excess of the risk-free rate, denoted $r^e_{i,t}$. The four factors are *MKT* (the excess of the monthly market return over the risk-free rate); *SMB* (the difference between the monthly returns of a value-weighted portfolio of small stocks and one of large stocks); *HML* (the difference between the monthly returns of a value-weighted portfolio of high book-to-market stocks and one of low book-to-market stocks); and one of three liquidity-risk factors, *LIQ*: Sadka's (2006) liquidity factor (Panel A), Pastor and Stambaugh's (2003) nontraded liquidity factor (Panel B), and Pastor and Stambaugh's traded liquidity factor (Panel C). *The MKT*, *SMB*, and *HML* factor series come from Kenneth French's website. All three *LIQ* factor series come from WRDS. Note that data for Pastor and Stambaugh's traded liquidity factor in Panel C is only available through 2004. The regression model is estimated with monthly data using 12-, 18-, or 24-month windows ending the month before a termination and beginning one month after. (If the termination occurred after the 15th day of a month, the post-termination window starts in the second month following the event.) The model takes the form $r^e_{i,t} = \alpha_i + (\beta_i + \Delta\beta^{Post} I_{t\in Post} + \Delta\beta^{Post\&Term} I_{t\in Post} I_{i\in Term}) Factors_t + \varepsilon_{i,t}$, where $I_{t\in Post}$ is an indicator for the post-event period; $I_{i\in Term}$ identifies firms in the terminations sample (as opposed to matched control firms, identified as in Table II); and *Factors* is a vector which includes the aforementioned factors. The difference-in-difference coefficients $\Delta\beta^{Post\&Term}$ capture whether a stock's risk loadings change following a termination, compared to otherwise similar control firms that experience no termination. To estimate the model, we impose the restriction that $\Delta\beta^{Post}$ and $\Delta\beta^{Post\&Term}$ are common to all firms and use a two-step approach analogous to Pastor and Stambaugh's equations (11)-(13). Specifically, for each firm in the termination and control groups, we concatenate the pre- and post-event returns and stack all firms' data in a single cross-sectional time-series. In step 1, we estimate the above model firm-by-firm using OLS and construct residuals $\eta_{i,t} = r^e_{i,t} - \alpha_i - \hat{\beta}_i Factors_t$. We use these residuals as the dependent variable in a second-step, pooled regression, $\eta_{i,t} = c + (\Delta\beta^{Post} I_{t\in Post} + \Delta\beta^{Post\&Term} I_{t\in Post} I_{i\in Term}) Factors_t + \nu_{i,t}$. To deal with outliers, we trim 0.5% of the observations from each tail. (Results are robust to degree of trimming and to winsorizing the residuals instead.) We also exclude sample firms for which no control firms with sufficient data are available. To save space, we report only the coefficients of interest, $\Delta\beta^{Post\&Term}$. To adjust for potential dependence arising from the serial correlation of terminations, *p*-values, reported in square brackets below the coefficient estimates, are calculated using a block bootstrap (with block length of 20).

Row #	Estimation window	Change in factor loadings for terminations relative to control firms ($\Delta\beta^{Post\&Term}$) *MKT*	*SMB*	*HML*	*LIQ*
Panel A: Sadka liquidity factor					
(1)	12-month	-0.08	0.00	0.00	0.70
		[<.001]	*[0.982]*	*[0.750]*	*[<.001]*
(2)	18-month	-0.05	-0.01	0.01	0.68
		[<.001]	*[0.643]*	*[0.686]*	*[<.001]*
(3)	24-month	-0.07	-0.01	0.04	0.59
		[<.001]	*[0.821]*	*[0.052]*	*[<.001]*
Panel B: Pastor-Stambaugh nontraded liquidity factor					
(4)	12-month	-0.05	0.00	0.02	0.03
		[0.004]	*[0.618]*	*[0.170]*	*[0.085]*
(5)	18-month	-0.06	0.01	0.03	0.02
		[<.001]	*[0.325]*	*[0.068]*	*[0.004]*
(6)	24-month	-0.07	0.01	0.05	0.01
		[<.001]	*[0.766]*	*[0.006]*	*[0.323]*
Panel C: Pastor-Stambaugh traded liquidity factor					
(7)	12-month	-0.10	0.01	0.02	0.07
		[<.001]	*[0.350]*	*[0.138]*	*[0.002]*
(8)	18-month	-0.06	0.06	0.03	0.02
		[<.001]	*[<.001]*	*[0.046]*	*[0.147]*
(9)	24-month	-0.06	0.04	0.04	0.04
		[<.001]	*[0.018]*	*[<.001]*	*[<.001]*