Testing Asymmetric-Information Asset Pricing Models

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We provide evidence for the importance of information asymmetry in asset pricing by using three natural experiments. Consistent with rational expectations models with multiple assets and multiple signals, we find that prices and uninformed demand fall as asymmetry increases. These falls are larger when more investors are uninformed, turnover is larger and more variable, payoffs are more uncertain, and the lost signal is more precise. Prices fall partly because expected returns become more sensitive to liquidity risk. Our results confirm that information asymmetry is priced and imply that a primary channel that links asymmetry to prices is liquidity. (*JEL* G12, G14, G17, G24)

Theoretical asset pricing models routinely assume that investors have heterogeneous information. The goal of this article is to establish the empirical relevance of this assumption for equilibrium asset prices and investor demands. To do so, we exploit a novel identification strategy that allows us to infer changes in the distribution of information among investors and hence to quantify the effect of information asymmetry on prices and demands. Our results suggest that information asymmetry has a substantial effect on prices and demands and affects assets through a liquidity channel.

Asymmetric-information asset pricing models typically rely on a noisy rational expectations equilibrium (REE) in which prices, due to randomness in the risky asset's supply, only partially reveal the better-informed investors' information. Random supply might reflect the presence of "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).

For helpful suggestions, we thank Anat Admati, Yakov Amihud, Brad Barber, Xavier Gabaix, Charles Jones, Marcin Kacperczyk, Ambrus Kecskés, Bryan Routledge, Paolo Fulghieri (the Editor), and two anonymous referees, as well as audiences at Columbia University, University of California–Berkeley, Australian National University, and the 2009 Western Finance Association meeting. Send correspondence to Alexander Ljungqvist, New York University, Stern School of Business, 44 West Fourth Street, New York, NY 10012-1126; telephone (212) 998-0304; fax: (212) 995-4220. E-mail: al75@nyu.edu.

doi:10.1093/rfs/hhr134 Advance Access publication January 5, 2012

To derive empirical predictions that are representative of these models, Section 1 adapts the multiple-asset and multiple-signal REE model of Easley and O'Hara (2004) in order to show that increases in information asymmetry lead to a fall in share prices and a reduction in uninformed investors' demand for risky assets. An important channel that links information asymmetry to price is liquidity risk: prices fall because greater information asymmetry exposes uninformed investors to more liquidity risk. The model's comparative statics relate the changes in prices and demand to the fraction of better-informed investors, the supply of the asset, uncertainty about the asset's payoff, and the noisiness of investors' signals.

Testing if and how information asymmetry affects asset pricing poses a tricky identification challenge, so it is perhaps not surprising that, despite the fundamental nature of our research question, the existing empirical evidence is indirect. To appreciate the nature of the 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 firm's cash flows) that simultaneously affect bid-ask spreads and demand. Another problem is reverse causality. Spreads may increase precisely because demand is expected to fall. To overcome such problems of simultaneity, we need a source of exogenous variation in the degree of information asymmetry in the asset market. Exogeneity, in this setting, means that the change in information asymmetry must only affect prices and demands via its effect on the distribution of information among investors. In other words, the change needs to be independent of all other underlying determinants of asset prices and demands—particularly the asset's expected future payoff.

We identify three natural experiments that affect information asymmetry through their effect on the extent of research coverage by sell-side equity analysts. Analysts are among the most influential information producers in financial markets.[1] To see how their presence, or absence, affects the extent of information asymmetry, suppose that investors are heterogeneous in their ability to access asset payoff signals. Retail investors, for instance, tend to rely on publicly available signals (such as those published by analysts), while institutional investors, such as hedge funds and mutual funds, often have access to private signals, perhaps from in-house research departments. Now consider the extreme case of a market in which each stock is covered by exactly one analyst. The ideal experiment would be to ban analysts from researching some stocks at random. If institutions can produce the lost signals in-house more

[1] A large body of literature provides evidence that analyst research is an important channel through which information is impounded in stock prices; see, among others, Womack (1996), Barber et al. (2001), Gleason and Lee (2003), Jegadeesh et al. (2004), and Kelly and Ljungqvist (2008). Some scholars take the opposite view, regarding analysts mainly as cheerleaders for companies who produce biased research in the hope of currying favor with executives (see, for instance, James and Karceski 2006). While ultimately an empirical question, the skeptics' viewpoint should bias us against finding a relation between the presence of analysts and equilibrium prices and demands.

easily than can retail investors, information asymmetry will increase as a result. An econometrician could then easily measure the effect of a change in information asymmetry on equilibrium asset prices and investor demands.

Our first natural experiment exploits reductions in the number of sell-side analysts who cover a stock. The reductions were the result of forty-three brokerage firms in the United States closing their research departments between Q1, 2000, and Q1, 2008, resulting in a total of 4,429 coverage terminations. These closures were well publicized in the media, and investors could learn which stocks were affected by a closure, and why, from so-called termination notices or through various financial Web sites, such as the Dow Jones marketwatch.com service.

Brokerage closures are a plausibly exogenous source of variation in the extent of analyst coverage, as long as two conditions are satisfied. First, the resulting coverage terminations must correlate with an increase in information asymmetry. Section 2 shows that standard proxies for information asymmetry do indeed change as required. Second, the terminations must *only* affect price and demand through their effect on information asymmetry. In particular, they must be uninformative about the affected stocks' future prospects. This will be true, unless a brokerage firm quit research because its analysts had foreknowledge of imminent falls in some companies' share prices (which seems unlikely). Section 2 discusses why so many brokerage firms have quit research since 2000. We also show empirically that our sample terminations are indeed uninformative about the future prospects of the affected stocks, as required for identification.[2]

Section 3 tests the predictions of the Easley and O'Hara (2004) model. As predicted, both price and retail demand fall following coverage terminations, even though, as we show in Section 2, these terminations convey no information about a stock's future prospects. The estimated price effects are economically large, and they are estimated quite precisely. Using the market model, cumulative abnormal returns average −112 basis points on the day of an exogenous termination and increase to −261 basis points on average by day 5, with similar results for alternate benchmarks. We find no evidence of reversal over the following month. Institutional investors increase their holdings of affected stocks by around 1.4%, while retail investors sell. We confirm these findings using a second, independent natural experiment, which results in exogenous variation in analyst coverage that is not due to brokerage closures but to the terrorist attacks of September 11, 2001.

[2] Focusing on *exogenous* coverage terminations is essential if we want to causally link price changes around coverage terminations to changes in information asymmetry and estimate economic magnitudes. In contrast to our sample of closure-related terminations, the vast majority of coverage terminations instead reflect the analyst's private information and so are selective. As McNichols and O'Brien (1997) note, analysts usually terminate coverage to avoid having to issue a sell recommendation when they expect future performance declines. Reasons to avoid issuing a sell include not wanting to upset an investment banking client or jeopardize access to the company's management in the future.

A variety of cross-sectional tests support the comparative statics of the model. Prices and, to a noisier extent, retail demand experience larger declines, when more investors are uninformed, turnover is larger and more variable, and the asset's payoff is more uncertain.

Finally, using the Acharya–Pedersen (2005) liquidity pricing model, we show that affected stocks become more exposed to liquidity risk following exogenous coverage terminations and, as a result, their expected returns increase. We interpret this finding as support for the notion that prices fall in anticipation of greater information asymmetry after the announcement of a brokerage closure. This, in turn, increases uninformed investors' liquidity-risk exposure and therefore their required returns.

While our main empirical focus is on exogenous coverage terminations, we also identify a third natural experiment that yields exogenous *increases* in coverage. These lead to price changes that are the mirror image of those that follow terminations. On the relevant announcement day, prices increase, on average, by 101 basis points net of market returns.

Our tests contribute to our understanding of asset pricing by providing direct evidence about the role of asymmetric information, the channel through which it operates, and the magnitude of its effects on prices and demands. While the fact that our findings support the key mechanism of asymmetric-information asset pricing models is perhaps not unexpected, such direct evidence is in fact quite rare, due to the identification problems we noted earlier. Thus, our second contribution is to provide a clean identification strategy with which to capture exogenous changes in asymmetric information. The extant literature instead relies on a variety of proxies for information asymmetry that are employed either in the cross-section or as within-firm changes. Both are potentially problematic: in the cross-section, problems are due to omitted variables; in differences, problems may arise because firm-level changes are unlikely to be exogenous.

Arguably, the most notable proxy for information asymmetry in the empirical literature is Easley and O'Hara's (1992) PIN measure. PIN is based on the idea that the presence of privately informed traders can be inferred noisily from order flow imbalances. Easley, Hvidkjær, and O'Hara (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. While we offer no opinion on the matter, PIN has proven controversial.[3] The advantage of our approach, relative to this literature, is that it exploits exogenous variation in the distribution of information and thus sidesteps the need for an information-asymmetry proxy whose potential correlation with unobserved variables is unknown and contentious.

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3 Mohanram and Rajgopal (2009) find that PIN is not priced if one extends Easley, Hvidkjær, and O'Hara's (2002) sample period. Duarte and Young (2009) show that PIN is only priced to the extent that it proxies for illiquidity rather than asymmetry.

The bulk of the empirical asset pricing literature has bypassed information asymmetry and focuses instead on the role of liquidity—perhaps because liquidity is much easier to measure than is information asymmetry.[4] Following theoretical models, such as Amihud and Mendelson (1986), Acharya and Pedersen (2005), and Huang (2003), empirical work treats liquidity as exogenous. Our findings suggest that liquidity varies with information asymmetry, which is consistent with the microstructure models of Kyle (1985), Glosten and Milgrom (1985), and Diamond and Verrecchia (1991). Thus, one fundamental driver of asset prices appears to be information asymmetry, which is consistent with the models of Grossman and Stiglitz (1980), Hellwig (1980), Admati (1985), Wang (1993), and Easley and O'Hara (2004).

Our article is also loosely related to the analyst literature. While we are interested in coverage terminations purely as a source of exogenous variation with which to identify changes in the supply of information, there is active interest in the causes and consequences of coverage terminations. For the most part, this literature studies selective (as opposed to exogenous) terminations (see Khorana, Mola, and Rau 2007; Scherbina 2008; Kecskes and Womack 2009; Ellul and Panayides 2009; Madureira and Underwood 2008). The exception is a contemporaneous article by Hong and Kacperczyk (2010), who exploit exogenous reductions in analyst coverage that are due to mergers among brokerage firms whose coverage universes overlap. This is similar to our focus on brokerage closures. However, while we are interested in estimating the pricing and demand effects of information asymmetry among investors, their focus is on the effect of competition among analysts on the extent of bias in earnings forecasts. Their results show that competition keeps analysts on their toes (in the sense that forecast bias increases when one of the two analysts who cover a stock is fired in the wake of a merger). Combined with our results, this suggests that terminations lower not only the level of information production about a stock but also its quality. Either would increase information asymmetry. Our article sheds light on the questions of whether this is priced, whether it affects investor demands, and what channel links information asymmetry to prices.

1. The Model

The purpose of formalizing the effect of information asymmetry on asset prices and investor demands is to guide our empirical tests. A convenient starting point is the rich family of noisy rational expectations equilibrium (REE) models of asset pricing with asymmetric information. REE models neatly embed a variety of features that have natural interpretations in our setting, including payoff signals (used to model the information that is produced by

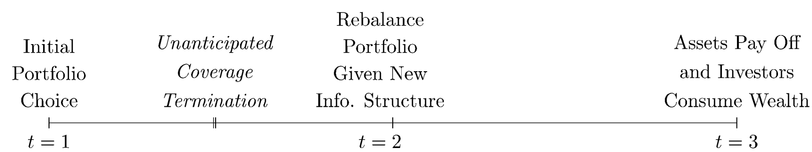
[4] Prominent examples include Pàstor and Stambaugh (2003), Amihud and Mendelson (1986), Amihud (2002), Hasbrouck and Seppi (2001), Bekaert, Harvey, and Lundblad (2007), Jones (2002), and Eleswarapu (1997).

analysts), privately informed investors (i.e., institutional investors with internal research capabilities), and uninformed investors (i.e., retail investors who rely on public information in order to make investment choices).

To emphasize the nondiversifiable nature of information risk, we work with a slightly modified version of Easley and O'Hara's (2004) multiple-asset REE framework. These authors show that information risk is systematic in the sense that risk premia induced by information asymmetry persist even in economies with an arbitrarily large number of assets. The intuition is that uninformed investors' portfolios always include a larger share of stocks with bad news and a smaller share of stocks with good news, relative to those of the informed. Holding more stocks does not eliminate this information risk. Thus, the uninformed require compensation for bearing information risk. The Easley–O'Hara model focuses on how the distribution of information in the market (i.e., the fraction of signals that are private rather than public) affects the cost of capital. It is therefore a natural departure point for our empirical strategy.

Our formulation closely follows Easley and O'Hara (2004), with the exception that we extend the model to a three-period setting and consider different comparative statics. Interested readers can refer to their article for additional modeling details.

There is a unit mass of investors who have identical initial wealth, W_1, and are risk-averse with constant absolute risk aversion (CARA) preferences $U(C) = E[-\exp(-\delta C)]$. The parameter δ summarizes agents' disutility of risk. Investors trade securities in periods 1 and 2 and consume in periods 2 and 3. As discussed below, we allow for the unanticipated loss of a public signal between periods 1 and 2. The following timeline summarizes the sequence of events.



Investors trade a one-period risk-free asset with price and payoff equal to one, as well as N assets that generate risky payoffs one period ahead. Risky asset m has aggregate supply $X_m \sim N(\bar{X}_m, \frac{1}{\eta_m})$ each period and per-unit payoff $v_m \sim N(\bar{v}_m, \frac{1}{\rho_m})$ in the subsequent period. The realizations of all assets' payoffs and of their supplies are independently and identically distributed each period. The parameters of the payoff and supply distributions are common knowledge.

Each trading period, investors receive the following signals about asset m's next-period payoff:

$$(\underbrace{s_{m,1}, \ldots, s_{m,I_m-H_m}}_{private}, \underbrace{s_{m,I_m-H_m+1}, \ldots, s_{m,I}}_{public}).$$

Each signal has a mean equal to the true payoff υ_m and variance γ_m^{-1}. A fraction μ_m of investors, which we will refer to as institutions, observe all I_m signals. The complementary fraction $1-\mu_m$ of "retail" investors observe only public signals.

The set of H_m public signals includes information disseminated by analysts working for brokerage firms. This information may, for example, take the form of earnings forecasts, research reports, trading recommendations, or industry analyses, and it is transmitted to investors for free. This mirrors Wall Street practice. Investors are not charged per analyst report received, so at the margin, the cost of observing an 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. [5]

All investors observe prices. Because institutions observe all signals, they treat payoff information contained in equilibrium prices, P_m $(m=1,\ldots,N)$, as redundant. However, retail investors observe only public signals, so for them, P_m contains useful conditioning information for payoff υ_m.

As in Easley and O'Hara (2004), we assume that the fraction of institutions, μ_m, is exogenously fixed. This assumption is not restrictive. In practice, adjustment costs prohibit retail investors from becoming institutional investors in the short term.[6] Our identification strategy considers the short-term price and demand effects of a coverage termination over periods of up to three months. This timescale is much shorter than the long horizons over which endogenous separation between institutions and retail investors might take place. Following Easley and O'Hara (2004), we abstract from these long-run implications and focus on the immediate portfolio implications of a change in information asymmetry, holding the distribution of publicly and privately informed traders fixed.

1.1 Equilibrium effects

The assumptions of CARA utility and normally distributed random variables imply that each investor $i \in \{institution, retail\}$ solves

$$\max_{C_{i,2},C_{i,3},\{\tilde{D}_{i,m,1},\tilde{D}_{i,m,2},m=1,\ldots,N\}} E(C_{i,2}|\mathcal{F}_{i,1}) - \frac{\delta}{2}Var(C_{i,2}|\mathcal{F}_{i,1}) + \beta\left(E(C_{i,3}|\mathcal{F}_{i,2}) - \frac{\delta}{2}Var(C_{i,3}|\mathcal{F}_{i,2})\right),$$

subject to wealth evolution $W_{i,t+1} = W_{i,t} + \sum_{m=1}^{N} \tilde{D}_{i,m,t}(\upsilon_{m,t+1} - P_{m,t})$, $t=1,2$, with $W_{i,1}=W_1$. Investors consume all of their wealth at the end of

[5] 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 (2006a).

[6] μ_m can be endogenized by positing a finite cost for establishing the institutional technology needed to extract private signals, in immediate analogy with Grossman and Stiglitz (1980).

period 3. $\tilde{D}_{i,m,t}$ denotes investor i's demand for asset m in period t, and $\mathcal{F}_{i,t}$ denotes the information set available to investor i at time t. $P_{m,t}$ is the price of risky asset m at time t.

CARA preferences also imply that equilibrium demands are independent of wealth. In addition, all random variables (payoffs, asset supplies, and signals) are independently and identically distributed across periods. As a result, equilibrium demands and prices in each period can be found as solutions to the myopic one-period problem. The following result, shown by Easley and O'Hara (2004), describes equilibrium in periods $t = 1, 2$.[7]

Proposition 1. (Easley and O'Hara 2004) The equilibrium price of risky asset m in period $t = 1, 2$, given parameters (I_m, H_m, γ_m, ρ_m, η_m, $\bar{\upsilon}_m$, $\bar{X}_m$, μ_m, and δ), is

$$P_{m,t} = a\bar{\upsilon}_m + b\sum_{i=1}^{I_m - H_m} s_{m,i,t} + c\sum_{i=I_m-H_m+1}^{I} s_{m,i,t} - dX_{m,t} + e\bar{X}_m.$$

The equilibrium demand of an institutional investor is

$$D_{institution,m,t} = \frac{1}{\delta}\left(\rho_m\bar{\upsilon}_m + \gamma_m\sum_{i=1}^{I_m} s_{m,i,t} - P_{m,t}(\rho_m + \gamma_m I_m)\right),$$

while retail demand is $D_{retail,m,t} = (X_m - \mu_m D_{institution,m,t})/(1-\mu_m)$.

The equilibrium effects of an exogenous change in coverage between periods 1 and 2 can be analyzed by comparing equilibrium prices and demands from Proposition 1 subject to the prevailing information distribution before and after the event. A coverage termination is modeled as a public signal becoming private. Thus, the only structural difference in asset markets between periods 1 and 2 is the number of public signals. Let H_m denote the number of public signals in period 1 so that $H_m - 1$ public signals remain following a coverage termination. The next proposition summarizes the equilibrium changes that result when an analyst ceases to disseminate research on asset m. To lighten notation, we suppress asset-specific subscripts when the reference asset is unambiguous.

Proposition 2. Holding the overall number of signals constant, the expected change in equilibrium price of risky asset m resulting from an exogenous coverage termination is

$$\Delta EP \equiv E[P_{m,2} - P_{m,1}] = \frac{-\bar{X}\,(1-\mu)}{\Omega} < 0,$$

[7] The expressions for coefficients a, b, c, d, and e are provided in their article.

where $\Omega = \gamma \left(\eta\mu^2(I\gamma + \rho)(I-H) + (I-H)\mu + H + \frac{\rho}{\gamma}\right)\Big((I-H+1)$ $(\eta\gamma\,\mu^2(I + \frac{\rho}{\gamma}) + \mu) + \frac{\rho}{\gamma} + H - 1\Big)$. The expected change in institutional investor demand is

$$\Delta EID \equiv E[D_{institution,m,2} - D_{institution,m,1}] = \frac{\bar{X}(1-\mu)(\rho+\gamma I)}{\delta\Omega} > 0.$$

By analogy, an exogenous coverage initiation gives retail investors access to a previously unavailable signal and thereby reduces the degree of information asymmetry between retail and institutional investors. This, in turn, increases the asset's price, increases retail demand, and decreases institutional demand.

Proposition 3. Holding the overall number of signals constant, an exogenous coverage initiation increases the firm's expected equilibrium price and decreases expected institutional investor demand: $\Delta EP > 0$ and $\Delta EID < 0$.

Note that both Propositions 2 and 3 hold the overall number of signals constant. This isolates changes in the distribution of information from changes in the quantity of information. A necessary condition for this to hold is that institutions can reproduce lost public signals, while retail investors cannot.

1.2 Discussion

Investors optimize their demands by trading off mean and variance conditional on their information sets. A change in coverage affects only the information set of retail investors, who are dependent on public signals. From their perspective, a coverage termination increases the conditional payoff variance, while it has a mean zero effect on the expected payoff. This lowers retail demand and thus the equilibrium price. At a lower price, privately informed institutions, 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 retail investors? Following a coverage termination, there are fewer public signals so that retail investors have a smaller information set. Observing the price, P, cannot fully reveal the finer information set of institutions. Investors do not observe aggregate supply, X, so retail investors cannot simply back out institutions' information set from observed prices; they cannot tell if a price change reflects a change in aggregate supply or a change in private signals. Instead, they noisily infer private signals by forming an expectation of payoff v conditional on observed price P and the remaining public signals. In this way, a coverage termination increases the conditional volatility of the asset's payoff from the perspective of retail investors. This is utility-decreasing, since investors are risk-averse.

A coverage termination also increases investors' exposure to aggregate supply risk. Proposition 1 demonstrates that equilibrium prices are linear in the signals and in aggregate supply. The exposure of price to supply shocks is given by $-d$, which is strictly negative. Following Brennan and Subrahmanyam (1996) and Amihud (2002), we can interpret asset supply X as trade volume and the coefficient on X as the price impact of trade. As the following corollary shows, a coverage termination increases the price impact of trade by increasing d.

Corollary 1. Let the price impact of trade be defined as $d = -\partial P/\partial X$. Then, following a coverage termination, the price impact of trade increases by the amount

$$-\Delta \frac{\partial P}{\partial X} = \frac{\gamma\,(1-\mu)\,(\eta\mu\rho + I\eta\gamma\mu + 1)}{\Omega} > 0.$$

Corollary 1 says that price falls as information asymmetry increases because price becomes more sensitive to liquidity (aggregate supply) risk. Expected payoffs remain unchanged, so we have

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

1.3 Robustness

Our setup makes two simplifying assumptions common in the REE literature. First, Easley and O'Hara (2004) assume that asset payoffs are independent. Independence is sufficient to derive the theoretical expressions that guide our empirical analysis given our focus on how changes in information asymmetry, as induced by exogenous coverage changes, affect a stock's *own* price and demand. Admati (1985) and Veldkamp (2006b) show how allowing for correlated payoffs gives rise to cross-stock price and demand effects. Such cross-stock effects would not alter the predictions for the own-stock effects we focus on. In the empirical section, we will report evidence that is consistent with coverage terminations also inducing cross-stock price changes. Second, we note that it is straightforward to recast the analysis as a market-maker–based liquidity model in the spirit of Kyle (1985) and Diamond and Verrecchia (1991). Assuming a risk-averse market-maker, as in Diamond and Verrecchia (1991), would leave our propositions, corollaries, and comparative statics qualitatively unchanged. What we call the change in the price impact of trade in Corollary 1 would be analogous to the change in Kyle's λ, and the prediction of Corollary 1 holds even when the market-maker is risk-neutral. We prefer the Easley and O'Hara (2004) setting because it embeds multiple public and private signals and many risky assets. These features produce additional testable implications, which are summarized in the next section.

1.4 Further testable implications

1.4.1 Comparative statics. When institutions comprise a larger fraction of the investor base in the beginning, fewer investors are adversely affected by the loss of an analyst signal. Thus, we have

Comparative Static 1. The larger the fraction of institutional investors among the firm's shareholders, the smaller the negative price impact of a coverage termination and the smaller the resulting increase in institutional demand for the company's stock: $\partial \Delta EP/\partial \mu > 0$ and $\partial \Delta EID/\partial \mu < 0$.

Corollary 1 states that a termination increases the (negative) sensitivity of price to aggregate supply. Thus, stocks with larger aggregate supply will experience larger price and demand changes. Thus, we have

Comparative Static 2. The greater the mean aggregate supply, the larger the negative price impact of a coverage termination and the greater the resulting increase in institutional demand for the stock: $\partial \Delta EP/\partial \bar{X} < 0$ and $\partial \Delta EID/\partial \bar{X} > 0$.

Because institutional investors possess private information, retail investors try to infer private signals from the observed price. This inference problem is harder the more variable the aggregate supply and payoff, which in turn increases the value of analyst research to retail investors. This intuition yields the following comparative statics regarding the variance parameters η^{-1} and ρ^{-1}:

Comparative Static 3a. The more variable the aggregate supply, the larger the negative price impact of a coverage termination and the greater the resulting increase in institutional demand for the stock: $\partial \Delta EP/\partial(\eta^{-1}) < 0$ and $\partial \Delta EID/\partial(\eta^{-1}) > 0$.

Comparative Static 3b. The more variable the asset's payoff, the larger the negative price impact of a coverage termination and the greater the resulting increase in institutional demand for the stock: $\partial \Delta EP/\partial(\rho^{-1}) < 0$ and $\partial \Delta EID/\partial(\rho^{-1}) > 0$.

Signal noise has a more complicated and, in general, ambiguous effect.

Comparative Static 4. Changes in signal noise have an ambiguous effect on equilibrium price and demand following an exogenous termination.

Consider the two extremes. If signals are highly precise, retail investors can filter private signals from the observed price very effectively, so losing a signal has a negligible impact on prices and demand. If signals are so noisy as to be essentially uninformative, institutions have little informational advantage over retail investors. As a result, a coverage termination results in a negligible increase in information asymmetry among investors and so also has little impact on prices and demand. Small movements away from these extremes amplify the effects of a termination: $\lim_{\gamma^{-1}\to 0} \partial \Delta EP/\partial \gamma^{-1} < 0$ and $\lim_{\gamma^{-1}\to \infty} \partial \Delta EP/\partial \gamma^{-1} > 0$. (Opposite inequalities hold for the derivatives of ΔEID.) At intermediate values, the derivatives of ΔEP and ΔEID with respect to signal noise cannot be unambiguously signed. However, these limiting derivatives imply that, to a second-order approximation, the effect of signal noise on price change is U-shaped, $\partial^2 \Delta EP/\partial(\gamma^{-1})^2 > 0$, while the effect on the change in institutional demand is inverse U-shaped, $\partial^2 \Delta EID/\partial(\gamma^{-1})^2 < 0$.

1.4.2 Retail versus institutional brokerage closures. Some brokerage firms exclusively cater to institutional investors, while others serve retail investors or both. Their closures should affect information asymmetry and hence prices and demands differently. If institutional broker research is largely unavailable to retail investors (i.e., if it is private), we expect smaller price and demand effects following closures of institution-only brokers. How much prices will fall depends on the fraction of an institutional broker's clients that can produce the signal in-house. In the limit, if all clients produce the signal in-house, then the information asymmetry retail investors face is unchanged, so prices and demands should remain unchanged following institutional brokerage closures.

Implication 1. Holding the overall number of signals constant, closures of brokers that exclusively serve institutional investors will have smaller negative price impacts and weaker increases in institutional demand for terminated stocks than closures of brokers serving retail investors.

2. Identification and Terminations Sample

2.1 Identification strategy

Our identification strategy is straightforward. To examine the effects of an increase in information asymmetry on prices and demands, we need a source of exogenous variation in information asymmetry. Rather than relying on observed changes in various proxies for information asymmetry—which may vary for endogenous reasons—we identify exogenous shocks to the information production about a stock. Most of our analysis focuses on terminations of analyst coverage that are the result of brokerage firms closing their research departments. Identification requires that such terminations correlate with an increase in information asymmetry but do not otherwise correlate with price or demand.

As McNichols and O'Brien (1997) observe, coverage changes are usually endogenous. 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 effects of fewer public signals. Similarly, an analyst may drop a stock because institutions have lost interest in it (Xu 2006). If institutional interest correlates with price, price may fall following a termination for reasons that are unrelated to changes in information asymmetry.

We avoid these biases by focusing on closures of research operations, rather than selective terminations of individual stocks' coverage. The last decade has seen substantial exit from research in the wake of adverse changes to the economics of producing research.[8] The fundamental challenge for equity research is a public goods problem: clients are reluctant to pay for research because it is hard to keep private, and hence it is provided for free. In the words of one observer, "Equity research is a commodity and it is difficult for firms to remain profitable because research is a cost center."[9]

Traditionally, brokerage firms have subsidized research with revenue from trading ("soft dollar commissions"), market-making, and investment banking. Each of these revenue streams has diminished since the early 2000s. The prolonged decline in trading volumes that accompanied the bear market of 2000–2003, along with increased competition for order flow, has reduced trading commissions and income from market-making activities. Soft dollar commissions have come under attack from both the U.S. Securities and Exchange Commission (SEC)[10] and institutional clients.[11] Finally, concerns that analysts publish biased research to please investment banking clients (Dugar and Nathan 1995) have led to new regulations, such as the 2003 Global Settlement, which have made it harder for brokers to use investment banking revenue to cross-subsidize research.

Brokerage firms have responded to these adverse changes by downsizing or closing their research operations. We ignore those that downsized (because it is hard to convincingly show the absence of selection bias in firing decisions) and focus instead on a comprehensive set of forty-three closures that were announced between 2000 and the first quarter of 2008.[12] These include twenty-two retail brokers and twenty-one institutional brokerage houses.[13] We identify the affected stocks from the coverage table of Reuters Estimates (which lists, for each stock, the precise dates during which each broker and analyst in the Reuters database actively cover a stock), the I/B/E/S stop file (which

8 See "The Flight of the Sell-side Analyst." *CFO Magazine*, July 8, 2004.

9 David Easthope, analyst with Celent, a strategy consultancy focused on financial services, quoted in Papini (2005).

10 The SEC issued new interpretative guidance of the safe-harbor rule under Section 28(e) of the 1934 Securities Exchange Act, which governs soft dollar commissions, in 2006. See http://www.sec.gov/rules/interp/2006/34-54165.pdf.

11 According to the consultancy TABB Group, nearly 90% of all larger institutional investors stopped or decreased use of soft dollars between 2004 and 2005 (*BusinessWire* 2005).

12 Our sample excludes Bear Stearns and Lehman Brothers, both of which failed in 2008 and were integrated into J. P. Morgan and Barclays Capital, respectively. Lehman is not a suitable shock for identification purposes, because Barclays, which had no U.S. equities business of its own, took over Lehman's entire U.S. research department.

13 To identify broker type, we rely to a large extent on the brokers' historical homepages accessed through the Wayback Machine (http://web.archive.org). We supplement this information, where needed, with news coverage surrounding closure announcements as well as self-descriptions from the Securities Industry Association's Yearbook.

has similar, albeit less reliably dated, information), and so-called termination notices that were sent to brokerage clients and can be retrieved from Investext.

Of the forty-three brokerage closures, twenty-two are implemented by the closing broker on a stand-alone basis. The remaining twenty-one closures occurred in the wake of brokerage–firm mergers. Mergers can result in two economically distinct types of reductions in coverage, only one of which is exogenous. The potentially endogenous type involves a stock that is covered by one or both brokers before the merger and that ceases to be covered by the surviving broker after the merger. Such a termination is potentially endogenous because the surviving broker made a choice to no longer cover the stock. We exclude this type of termination and instead focus on the other type: a stock that was covered by analysts from *both* brokers before the merger and by only one analyst after the merger. Such a stock loses one public signal, as one of the two analysts ceases to provide coverage.

The use of stand-alone brokerage closures—which represent over 60% of our observations—to identify exogenous coverage changes is new to the literature. The identification strategy behind the merger-related observations is similar in spirit to that of Hong and Kacperczyk (2010). However, we believe our implementation more cleanly identifies truly exogenous reductions in coverage. Hong and Kacperczyk (2010) require that a stock be covered by both brokers at some point in the year before the merger and by the surviving broker at some point in the year after the merger. However, unlike us, they do not condition on stop dates or reinitiation dates. As a result, their algorithm will, in part, capture two types of endogenous coverage changes:

1) Endogenous coverage reductions—specifically, when a broker terminated coverage sometime before the merger. In these cases, the decision to drop coverage preceded the merger and so should be treated as likely endogenous. To illustrate, we compare our treatment of the 2000 acquisition of DLJ by CSFB to Hong and Kacperczyk's (2010). This is the largest merger event in their sample, accounting for 352 of their 1,656 observations. We find that their sample contains thirty-nine stocks that, according to the I/B/E/S stop file, were in fact terminated months before the merger. (An additional 23 cases in their sample are endogenous based on the more precise coverage information available to us courtesy of Reuters Estimates.)
2) Endogenous coverage reinstatements—specifically, when the surviving broker in fact terminated coverage at the time of the merger and then subsequently reinitiated coverage a few months later (and before the first anniversary of the merger). In these cases, the merger led to an endogenous decision to no longer cover the stock and a subsequent endogenous decision to reinstate coverage. In the DLJ-CSFB merger, we find that their sample includes twenty such endogenous coverage reinstatements.

These eighty-two endogenous coverage changes—accounting for 23% of their observations in this particular merger—are screened out by our filters to ensure that we identify only plausibly exogenous coverage terminations.[14]

In principle, a brokerage closure could either increase information asymmetry or reduce the quantity of information. It depends on whether the previously public signal is lost or becomes private. For the number of signals to decline, the analyst would have to leave the industry and institutional investors would have to refrain from replacing the lost signal in-house. While this is possible, anecdotal evidence suggests it is unlikely. The following quote illustrates why:

> Few in the industry think [laid-off] analysts will have trouble getting new work. Demand for analysts is strong, but the landscape has shifted. More research dollars are flowing away from . . . so-called "sell side" firms that sell their research to others. Instead, "buy side" firms such as hedge funds and other money managers are hiring in-house research staffs, paying top dollar to keep those investing insights all to themselves. (*Businessweek* 2007)

To the extent that signals are nonetheless completely lost, the price reactions we find in the wake of brokerage closures are upper bounds on the effect of information asymmetry on asset prices.

2.2 Sample

The forty-three brokerage closures are associated with 4,429 coverage terminations after filtering out securities with CRSP share codes >12 (REITs, ADRs, noncommon stocks, closed-end funds, etc.), companies without share price data in CRSP, and fifty-one companies due to be delisted in the next sixty days. Appendix A lists the forty-three brokerage closure events. It also states if an event is a stand-alone or merger-related closure, if the broker's clientele primarily consists of retail or institutional investors, the number of coverage terminations associated with each closure, and the number of industries covered by the broker at the time of closure.[15]

The average brokerage closure involves 103 stocks; the maximum is 480. There are 2,180 unique stocks in the sample, so the average company is hit by two terminations over our sample period (maximum: 12). The forty-three brokerage firms employed 557 analysts (not counting junior analysts without coverage responsibilities), so the average closure involved thirteen analysts, and the average analyst covered eight stocks. Sample terminations span all thirty Fama and French (1997) industries and do not cluster. The average

[14] In addition, Hong and Kacperczyk's (2010) algorithm misses a large number of eligible coverage terminations, ostensibly due to an error in the I/B/E/S file the authors worked with. For example, of the 391 terminations in our DLJ-CSFB merger sample, 134 are not in their sample.

[15] The twenty-two stand-alone closures account for 2,731 of the 4,429 coverage terminations. Merger-related closures account for the remaining 1,698 terminations.

(median) broker covered 10.7 (10) Fama–French industries, which rises to 18.2 when weighted by the number of stocks covered. Few brokers were sector specialists.

The top graph in Figure 1 illustrates the time distribution of the brokerage closures. The figure shows that the closures are spread out fairly equally over time. At least one brokerage firm quit research in twenty-four of the thirty-three quarters between Q1, 2000, and Q1, 2008. On two occasions, two brokerage firms quit research on the same day. Overall, there is no obvious evidence of the closures clustering in time. The daily number of affected stocks is shown in the bottom graph in Figure 1. Here, there is somewhat more evidence of clustering. Roughly half the terminations in the sample occurred in the first 2.5 years of the sample period. (The largest number of terminations occurred in Q4, 2000, when four brokerage closures resulted in a total of 968 terminations.) There were virtually no terminations in the two years from Q4, 2002, a period during which many brokerage firms downsized their research operations instead of completely closing them (see Kelly and Ljungqvist 2008).

Table 1 compares summary statistics for sample stocks, the CRSP universe of publicly traded U.S. stocks, and the universe of U.S. stocks covered by at least one analyst according to I/B/E/S. Sample stocks—like I/B/E/S stocks more generally—are on average significantly larger than CRSP stocks. They are also more liquid and more volatile than the average CRSP stock. On average, 10.6 analysts covered a sample stock before a coverage termination, compared to 6.4 in the I/B/E/S database, which suggests that brokerage firms that exited research over our sample period disproportionately covered stocks with above-average analyst following. To the extent that less-covered stocks are proportionately more affected by a termination, this biases our tests against finding any effects.[16]

2.3 Announcement dates

The relevant announcement date is the date investors learn that a brokerage firm has closed its research department. Closures are widely reported, and we obtain announcement dates from Factiva. It is also important that investors can infer if a stock was terminated for endogenous or exogenous reasons. For the broker's clients, this is easy. The New York Stock Exchange's Rule 472(f)(5) and NASD Rule 2711(f)(5) require that clients are sent a termination notice every time a brokerage firm terminates coverage (including when it completely exits research). Termination notices must include the rationale for the termination, which removes any doubt, in our setting, that investors might misinterpret why coverage was terminated. The most common statement in our sample is simply that research has been closed "based on our decision

16 Presumably, the increase in information asymmetry is greatest when a stock loses coverage entirely. Unfortunately, our sample contains only seven stocks that were "orphaned" as a result of a closure-induced coverage termination.

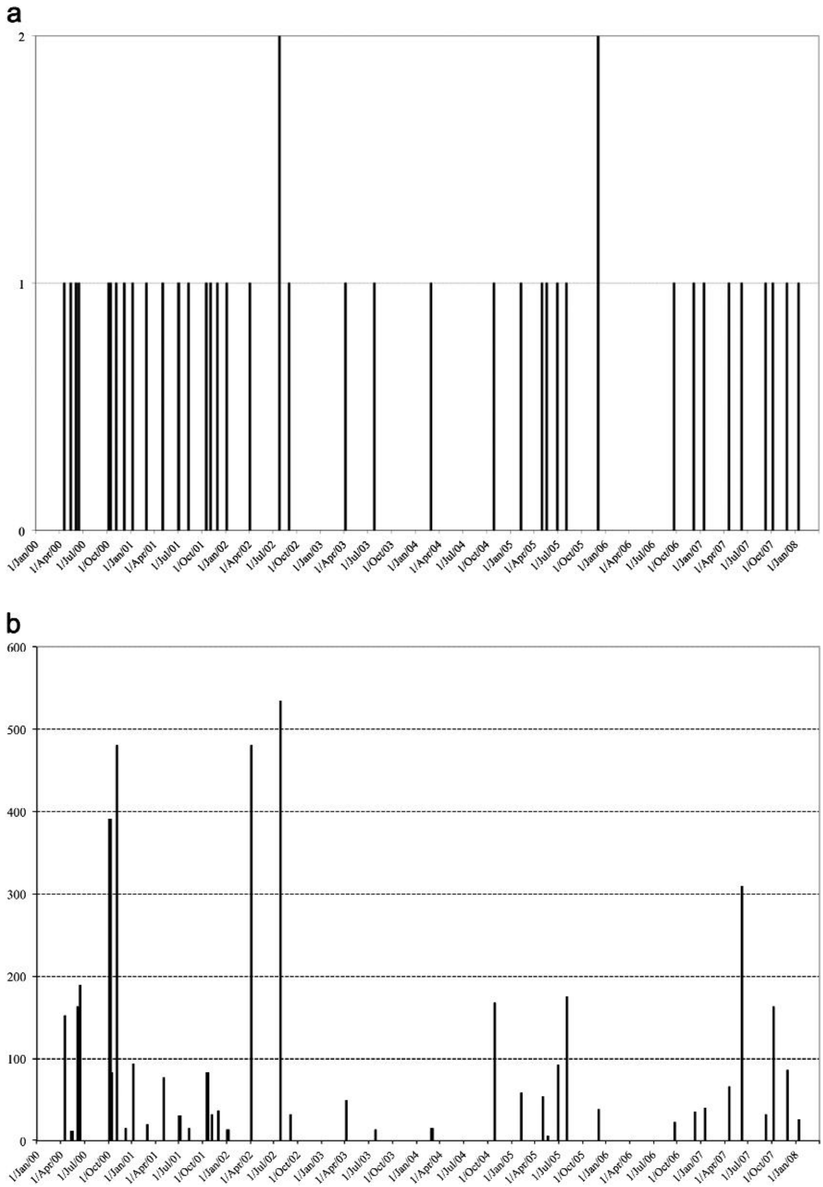


Figure 1
Breakdown of brokerage closures and coverage terminations
The sample of coverage terminations is derived from forty-three brokerage closures over the period Q1, 2000, through Q1, 2008. The top graph shows the daily number of brokerage closures, while the bottom graph shows the associated number of coverage terminations at the stock level.

Table 1
Closure-induced coverage terminations: Summary statistics

	Terminations sample	CRSP universe in 2004	Universe of covered stocks in 2004
Equity market value ($ m)			
Mean	7,146.7	2,943.3	6,211.0
Median	1,017.8	319.6	397.1
Range	3.5–512,833	0.7–385,883	2.9– 385,883
Monthly stock turnover			
Mean	0.20	0.18	0.17
Median	0.12	0.09	0.11
Range	0–3.0	0–17.9	0–9.4
Daily return volatility (annualized %)			
Mean	52.1	45.1	37.0
Median	60.0	39.1	41.6
Range	11.6–214.3	8.1–277.4	8.1–175.5
Number of brokers covering the stock			
Mean	10.6		6.4
Median	9		4
Range	1–40		1–42
Number of unique firms	2,180	5,298	4,305

The sample consists of 4,429 coverage terminations for 2,180 unique firms between Q1, 2000, and Q1, 2008. 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 U.S. stocks with analyst coverage in the I/B/E/S database. For each firm in the terminations sample, we calculate equity value and turnover for the month prior to the first termination date. For the universes of CRSP stocks and covered stocks, these variables are computed for December 2004 (the midpoint of our sample period). Annualized volatility for terminations is the standard deviation of daily log returns in the twelve-month period that ends one month prior to a termination times $\sqrt{252}$. For the universes of CRSP stocks and covered stocks, volatilities are the annualized daily standard deviations for firms in these samples during calendar year 2004. (In each column, a firm is omitted from this calculation if it has fewer than 200 days of nonmissing returns.) The number of brokers who cover a stock in the terminations sample for the month prior to the termination is taken from the I/B/E/S forecast summary file. (Each month, I/B/E/S reports the number of outstanding earnings-per-share forecasts for the coming fiscal year.) The broker count for the universe of covered stocks represents the I/B/E/S forecast count in December 2004.

to exit the research business.” Other investors may learn the reason for a termination through third-party news aggregators, such as the Dow Jones marketwatch.com service, which provides a synopsis of a broker’s reason for terminating coverage.

2.4 Are sample terminations exogenous?

Closure-related terminations constitute a suitably exogenous shock to the information environment, *unless* brokerage firms quit research because their analysts possessed negative private information about the stocks they covered. Press reports around the time of the closure announcements make it clear that the closures are unlikely to have been motivated by negative private information about individual stocks and so are plausibly exogenous. The following three examples illustrate.

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 in the last thirty 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 2005)

Dutch bank ABN Amro closed its loss-making U.S. equities business in March 2002. Among the 950 redundancies were twenty-eight analysts who covered nearly 500 U.S. stocks. Board member Sergia Rial made it clear that the decision reflected competitive pressure and strategic considerations: “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* 2002)

George K. Baum & Co. closed its research and investment banking operations in October 2000, blaming a 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* 2000)

These quotes suggest that brokerage firms quit research for strategic reasons, rather than to conceal negative private information about stocks their analysts covered. Still, it is useful to test formally whether sample terminations are indeed uninformative about the affected stocks’ future prospects.

2.4.1 Exogeneity test. Suppose a broker terminates coverage at time t. If this conveys negative private information about the stock, we should be able to predict its future performance from the fact that coverage was terminated. Testing this 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 and result in a negative earnings surprise. If, on the other hand, the termination is exogenous, earnings will not systematically differ from consensus.

This test is more powerful than simply checking whether terminations forecast changes in operating performance (such as return on assets (ROA)). By conditioning on consensus, the test exploits differences in the analyst’s and the market’s information sets under the null that the analyst has negative private information. Earnings surprises are ideal in this context because they are based on an observable forecast: $surprise_{t+1} = EPS_{t+1} - E[EPS_{t+1}|\mathit{consensus}\ \mathit{info}_{t-1}]$. For other measures of operating performance, such as ROA, the market’s forecast is unobservable and must be estimated by the econometrician, which introduces noise and decreases the power of the test.

We implement the test in a large panel of CRSP companies over the period from 2000 to 2008. Mirroring the construction of the terminations sample, we filter out companies with CRSP share codes greater than twelve, which leaves 5,823 CRSP firms and 96,455 firm-quarters. The dependent variable is the scaled earnings surprise, which is 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, and book value data come from Compustat.

The main variables of interest are two indicators. The first identifies the 4,429 terminations caused by the forty-three brokerage closures. Its coefficient should be statistically zero if closure-induced terminations are uninformative about future earnings and negative if our identifying assumption is invalid. The second identifies 10,349 firm-quarters in which the I/B/E/S stop file records that one or more brokers terminated coverage for reasons unrelated to a brokerage closure. We will refer to these as "endogenous" terminations and expect a significantly negative coefficient. We control for lagged returns and return volatility measured over the prior 12 months, the log number of brokers that cover the stock, and year and fiscal-quarter effects. We also add firm fixed effects to control for omitted firm characteristics and allow for autocorrelation in quarterly earnings surprises by using Baltagi and Wu's (1999) AR(1)-estimator for unbalanced panels. This yields the following estimates:

$$\begin{aligned} earnings\ surprise = &\underset{.051}{.607} lagged\ return - \underset{.053}{.276} return\ volatility \\ &- \underset{.120}{.220} log\ \#\ brokers + \underset{.172}{.008} sample\ termination \\ &- \underset{.117}{.267} endogenous\ termination. \end{aligned}$$

Standard errors are shown below the coefficients. The firm fixed effects are jointly significant with an F-statistic of 4.2, and earnings surprises are significantly autocorrelated with $\rho = 0.37$.

As expected, earnings surprises are significantly more negative following what we have labeled endogenous terminations (p-value $=$ .022). The effect is economically large. On average, an endogenous termination is followed by a quarterly earnings surprise that is 16.5% more negative than the sample average over this period. Closure-induced terminations are, by contrast, neither economically nor statistically related to subsequent earnings surprises (p-value $=$ .965). This is consistent with closure-induced terminations being uninformative about future performance and thus plausibly exogenous.

2.5 Do coverage terminations increase information asymmetry?

Identification also requires that coverage terminations increase information asymmetry among investors. To test whether they do, we examine changes around coverage terminations in three popular empirical proxies for information asymmetry: bid-ask spreads, Amihud's (2002) illiquidity measure,

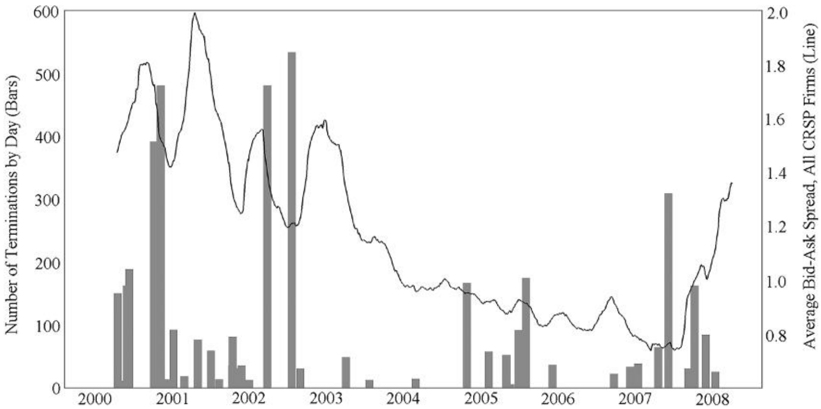


Figure 2
Average daily bid-ask spread, all CRSP firms, 2000-Q1 2008
The figure illustrates the extent to which proxies for information asymmetry have been subject to secular trends and swings over our sample period in the market as a whole. The figure shows trailing three-month average daily bid-ask spreads computed as 100 * (ask–bid) / [(ask+bid) / 2], averaged across the universe of firms in CRSP, for each day in our sample period. Similar secular trends and swings (available upon request) are observed in other proxies for information asymmetry, such as the log Amihud illiquidity measure and the number of days with zero or missing returns in CRSP. The quarterly number of closure-induced coverage terminations over the sample period is indicated by the gray vertical bars.

and Lesmond, Ogden, and Trzcinka's (1999) stale-returns measure. We also examine changes in earnings surprises and return volatilities at future earnings releases, as these news events resolve a greater deal of uncertainty when information asymmetry is larger.

Throughout, we use difference-in-differences (diff-in-diff) tests to help remove secular trends that affect similar companies at the same time (Ashenfelter and Card 1985). Our proxies for information asymmetry have been subject to secular trends and swings over our sample period. For example, Figure 2 shows that average bid-ask spreads (across all stocks in CRSP) drifted downward between 2000 and 2007 and then markedly increased during the financial crisis. One consequence of these market-wide secular trends and swings is that the average stock in the economy will experience nonzero changes in popular measures of information asymmetry over time. The purpose of our diff-in-diff tests is to test whether stocks that suffer exogenous coverage terminations experience changes in information asymmetry that exceed those expected in the absence of such terminations; i.e., whether they suffer increases in information asymmetry over and above contemporaneous trends.

To remove common influences, diff-in-diff 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 match firms on the Fama and French (1993) pricing factors by using the Daniel et al. (1997) algorithm. Specifically, we choose as controls for terminated stock i five firms in the same size and book-to-market quintile in the preceding month of June, subject to the condition that control firms did not themselves experience a termination in the quarter before or after. In view of the evidence in Table 1 that firms with coverage terminations are larger and more liquid than CRSP firms in general, we also require that control firms be covered by one or more analysts in the quarter before the event.

2.5.1 Results. Table 2, Panel A, reports changes in bid-ask spreads around coverage terminations. If exogenous terminations increase information asymmetry, spreads should widen. For each stock, we compute the average bid-ask spread (normalized by the mid-quote) from daily data over three- or six-month estimation windows that end ten days before and start ten days after a termination. Net of the change among control firms, spreads increase on average by 1.8% to 2.1%, which is consistent with an increase in information asymmetry. These changes are quite precisely estimated with bootstrapped *p*-values of .011 and .010.[17]

Wang (1994) predicts that the correlation between absolute return and dollar volume increases in information asymmetry. This makes Amihud's (2002) illiquidity measure (AIM)—the log of one plus the ratio of absolute return and dollar volume—a natural proxy for information asymmetry. Panel B accordingly reports changes in AIM. Relative to control stocks, AIM increases after a termination, which implies that the correlation between absolute return and volume increases as predicted. The increases are again economically meaningful. They average 14.6% to 18.0% relative to the pretermination means. They are also highly statistically significant.

An alternative measure of illiquidity is due to Lesmond, Ogden, and Trzcinka (1999). A large number of zero- or missing-return days may indicate that a stock is illiquid. Panel C shows that net of the control-firm change, this number increases following a coverage termination, by an average of 2.9% to 3.6%.[18]

Panel D looks at earnings announcements. After a termination, returns should be more volatile around earnings announcements, as more uncertainty is left unresolved (West 1988). We also expect greater absolute earnings surprises—to the extent that posttermination consensus forecasts reflect coarser information sets. Consistent with these predictions, log daily return

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17 By construction, terminations 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 that are based on a block bootstrap with 10,000 replications and a block length of 100, the approximate average number of terminations per brokerage closure event.

18 Note that for the six-month estimation window, the number of days with zero or missing returns actually declines for both treatment and control firms (presumably because markets are generally becoming more liquid in the wake of decimalization and competition from ECNs). However, the number declines relatively faster for control firms, so that the difference-in-difference statistic shows a relative increase in illiquidity for stocks that lose coverage.

Table 2
The effect of coverage terminations on information asymmetry

	Terminations		Matched controls				*Economic magnitude*
	Before	After	Before	After	Mean DiD	$p-$value DiD = 0	*(percentage change)*
Panel A: Bid-ask spreads							
3-mo. window	1.126	1.219	1.089	1.162	0.020	.011	1.8
6-mo. window	1.120	1.201	1.086	1.143	0.024	.010	2.1
Panel B: Amihud illiquidity measure (AIM)							
3-mo. window	0.104	0.147	0.099	0.127	0.015	<.001	14.6
6-mo. window	0.094	0.145	0.089	0.124	0.017	.001	18.0
Panel C: Missing and zero-return days (in % of days in the relevant window)							
3-mo. window	3.092	3.129	2.962	2.887	0.112	.080	3.6
6-mo. window	3.039	2.879	2.925	2.678	0.087	.123	2.9
Panel D: Earnings announcements							
Volatility (% p.a.)	37.82	38.80	33.24	28.81	5.418	<.001	14.3
Earnings surprise	0.637	0.744	0.647	0.667	0.088	<.001	13.8

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 selecting stocks with the same Daniel et al. (1997) size and book-to-market quintile assignment in the month of June prior to a termination, subject to the conditions that control firms 1) were covered by one or more analysts in the three months before the event; and 2) were not themselves subject to a coverage termination in the quarter before and after the event. When more than five matches exist, we choose the five stocks that are closest to the sample stock in terms of the relevant pre-event information asymmetry measure. (We lose 158 observations that involve sample stocks without viable controls and varying numbers of observations due to missing data necessary in calculating a particular measure. The number of observations ranges from about 4,100 to 4,300.) For each sample stock i, we construct a difference-in-difference test as DiD = $(post_i - pre_i) - (\overline{post - pre}_{Control\ Group\ i})$. The table reports the cross-sectional mean of the DiD statistic along with the mean percentage change (= DiD / mean before – 1). The latter illustrates the economic magnitude of the change in information asymmetry. We test the null hypothesis that information asymmetry is unchanged around a coverage termination using bootstrapped p-values. These adjust for potential cross-sectional dependence that is due to overlapping estimation windows caused by time clustering as multiple stocks are terminated in each brokerage firm closure. They are calculated by using a block bootstrap with a block length of 100, the approximate average number of terminations per brokerage closure event. The first three proxies for information asymmetry are computed over three- and six-month windows, which end ten days prior to the termination announcement or start ten days after the announcement date. Panel A reports changes in bid-ask spreads. Spreads are computed as 100 * (ask–bid) / [(ask+bid) / 2] using daily closing bid and ask data from CRSP. Panel B reports changes in the log Amihud illiquidity measure. This is defined as the natural log of one plus the ratio of the absolute stock return to the dollar trading volume and scaled by 10^6 (see Amihud 2002, p. 43). Panel C reports the changes in the number of days with zero or missing returns in CRSP (using missing return codes -66, -77, -88, and -99). In Panel D, we report the effects of terminations on quarterly earnings announcements. The first measure is the annualized daily return volatility in a three-day window around earnings announcements for all earnings announcements that occur 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.

volatility in the three days around earnings announcements increases by 14.3% net of the change among control firms (p-value < .001), while the average magnitude of absolute earnings surprises increases by 13.8% (p-value < .001).

The evidence in Table 2 is consistent with the interpretation that information asymmetry increases following a termination. Thus, sample terminations appear to be both uninformative about firms' future prospects and correlated with changes in information asymmetry, as required for identification.

3. The Effect of Information Asymmetry on Price and Demand

3.1 Changes in price following coverage terminations

3.1.1 Visual evidence. Figure 3 plots daily abnormal returns for trading days –22 through +22 relative to the day a brokerage firm closure was announced. Abnormal returns are computed by using either the market model or the Fama–French three-factor model, with factor loadings estimated in a one-year pre-event window. The figure shows that average daily abnormal returns oscillate around zero in the month before a coverage termination, so there is no evidence of abnormal returns before the termination announcements. This is consistent with the notion that the brokerage closures were unanticipated.[19] On announcement, prices fall dramatically and continue to do so (though by smaller amounts) for the next few days. After day +5, abnormal returns again oscillate around zero for the remainder of the posttermination month, which suggests that the negative announcement returns are permanent, at least over this window.

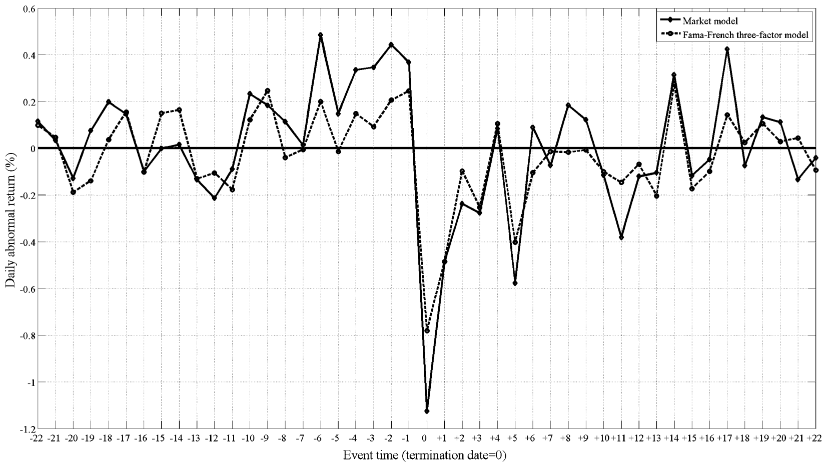

Figure 3
Daily abnormal returns around closure-induced coverage terminations
The terminations sample consists of 4,429 coverage terminations for 2,180 unique firms between Q1, 2000, and Q1, 2008. We compute abnormal returns for the forty-five trading days beginning on trading day -22 and ending on trading day +22, relative to the day a brokerage firm closure was announced (day 0). We use two alternate benchmarks: the market model and the Fama–French three-factor model. Factor loadings for these benchmarks are estimated in a one-year pre-event window, which ends twenty-three trading days before the termination date, and abnormal returns during the event window are calculated by using the estimated model as a benchmark. (Results are nearly identical if we include a Carhart (1997) momentum factor in the Fama–French model.)

[19] News reports confirm this. For example, when Wells Fargo quit research, one of the fired analysts commented, “We were told in a firm-wide announcement. I had no idea [it was coming].” (Wall Street Letter 2005)

3.1.2 Announcement returns. To test Proposition 2 more formally, we test if the price changes around termination announcements shown in Figure 3 are economically and statistically significant. We focus on cumulative abnormal returns (CARs) from the close on the day before the announcement to the announcement-day close $[-1, 0]$, one $[-1, +1]$, three $[-1, +3]$, and five days later $[-1, +5]$. Table 3, Panel A, reports the results. Consistent with Proposition 2, prices fall significantly following announcement. On the announcement day, CARs average −112 and −78 basis points for the market model and the Fama–French model, respectively (not annualized). There is continued drift over the next few days, with CARs over the $[-1, +5]$ window averaging −261 and −191 basis points, respectively. For the remainder of the first trading month after a closure (i.e., the window $[+5, +22]$), average CARs are small and not statistically different from zero, which indicates that the announcement returns are not subsequently reversed. For the median sample firm, the price falls over the $[-1, +5]$ window imply a fall in market value of \$19.4 million to \$26.6 million, depending on the benchmark used. This suggests that coverage terminations have economically meaningful price effects.[20]

The estimated price falls are also highly statistically significant, using a standard Patell event-study test. Of course, each of the forty-three brokerage closures results in multiple terminations on the same day, which could cause cross-sectional dependence in the event returns. A popular way to adjust for potential cross-sectional dependence is to estimate standard errors by using a block bootstrap. The bootstrapped p-values, shown in Table 3, confirm that the announcement CARs are highly statistically significant. Another way to account for potential cross-sectional dependence is to form portfolios of stocks that are terminated on the same day, which essentially treats the average CAR of each of the forty-three brokerage closures as a single observation. These portfolio CARs, also reported in Table 3, average between −60 and −185 basis points, depending on benchmark and window. They too are highly statistically significant, with p-values of 1% or better.

3.1.3 Alternative identification strategy: 9/11. As a reality check on our identification strategy, we briefly explore a second natural experiment. Two brokerage firms, Keefe, Bruyette & Woods and Sandler O'Neill & Partners, suffered horrendous casualties in the September 11, 2001 terrorist attacks. Over the next three weeks, they announced a total of 356 coverage terminations as the names of nearly thirty analysts killed in the World Trade Center were confirmed. As Table 3, Panel B, shows, the CARs for this sample are similar to those in the closures sample. On the announcement day, e.g., CARs average

20 The average announcement-day CARs for the twenty-two stand-alone closure events are −132 and −92 basis points for the market model and the Fama–French model, respectively. For the twenty-one merger-related closures, these averages are −80 and −54 basis points. A bootstrap test for difference in means finds no statistical difference in the two groups' average CARs for either the market model or the Fama–French model.

Table 3
Changes in price around coverage terminations

CAR calculated over the period . . .		No. of obs.	Market model		Fama-French 3-factor model	
Panel A: Closure-induced terminations						
Standard diff-in-diff approach:						
Close on day before termination to close on termination day	[−1, 0]	4,414	−1.12	***/***	−0.78	***/***
Close on day before termination to close on day +1	[−1, +1]	4,414	−1.61	***/***	−1.26	***/***
Close on day before termination to close on day +3	[−1, +3]	4,414	−2.12	***/***	−1.61	***/***
Close on day before termination to close on day +5	[−1, +5]	4,414	−2.61	***/***	−1.91	***/***
Close on day +5 after termination to close on day +22	[+5, +22]	4,414	0.19		−0.39	
Portfolio strategy correcting for time clustering:						
Close on day before termination to close on termination day	[−1, 0]	43	−1.03	***	−0.60	***
Close on day before termination to close on day +1	[−1, +1]	43	−1.33	***	−0.86	***
Close on day before termination to close on day +3	[−1, +3]	43	−1.57	***	−1.08	***
Close on day before termination to close on day +5	[−1, +5]	43	−1.85	***	−1.26	**
Close on day +5 after termination to close on day +22	[+5, +22]	43	0.06		−0.78	
Panel B: Terminations due to 9/11						
Standard diff-in-diff approach:						
Close on day before termination to close on termination day	[−1, 0]	356	−0.87	***/***	−0.77	***/***
Close on day before termination to close on day +1	[−1, +1]	356	−0.90	***/***	−0.65	***/***
Close on day before termination to close on day +3	[−1, +3]	356	−0.76	***/***	−0.44	/*
Close on day before termination to close on day +5	[−1, +5]	356	−1.08	***/***	−0.62	
Panel C: Imminent delistings						
Standard diff-in-diff approach:						
Close on day before termination to close on termination day	[−1, 0]	51	0.53		0.76	
Close on day before termination to close on day +1	[−1, +1]	51	−0.32		−0.04	
Close on day before termination to close on day +3	[−1, +3]	51	0.57		0.71	
Close on day before termination to close on day +5	[−1, +5]	51	0.59		0.56	

We compute cumulative abnormal returns over various windows around coverage terminations using two benchmarks: the market model and the Fama–French three-factor model. Factor loadings are estimated in a one-year pre-event window ending eleven days before the termination date, and cumulative abnormal returns during the event window are calculated using this estimated model as a benchmark. (Results are nearly identical if we include a Carhart (1997) momentum factor in the Fama–French model.) We report these cumulative abnormal return metrics for the closures sample (Panel A); a separate sample of terminations at two brokerage firms located in the World Trade Center that were devastated in the 9/11 attacks (Panel B); and a separate sample of closure-induced terminations of stocks facing imminent delisting, for which a termination should be of little consequence (Panel C). To correct for after-hours announcements, we use time stamps to determine the first trading day when investors could react to a closure-induced termination. Abnormal returns are reported in percents. In our main specifications, they are averaged across all 4,414 sample stocks for which returns can be computed. (The sample falls short of 4,429 because we require a minimum of fifty trading days of pre-event stock prices to estimate model parameters.) We report significance levels using a block bootstrap to control for dependence among events; we also report a standard event-study Patell t-test. These two test statistics are separated by "/." In Panel A, we use a block length of 100 for the bootstrap, corresponding to the approximate number of terminations per brokerage-closure event. In Panels B and C, given the much smaller sample sizes, we use block lengths of 10 and 5, respectively. In addition, to account for potential cross-sectional dependence caused by time clustering as multiple stocks are terminated in each of the 43 brokerage firm closures, Panel A also reports a portfolio strategy that buys all stocks that are terminated on a given closure day. The estimate of the average cumulative abnormal portfolio return is the time-series average of the forty-three brokers' average cumulative abnormal returns, in analogy with a Fama–MacBeth regression; significance levels are computed from the standard deviation of the forty-three averages. The number of brokers in Panels B and C is too small to compute portfolio returns. We use ***, **, and * to denote statistical significance at the 0.1%, 1%, and 5% levels (two-sided), respectively.

−87 and −77 basis points (p-value $< .001$), using the market model and the Fama–French model, respectively.

The fact that we find price falls following coverage terminations using two independent sources of variation in information asymmetry, the forty-three brokerage closures and 9/11, supports the identification strategy behind our first natural experiment. While it is impossible to rule out that brokerage firms may have timed their closure announcements in a way that would spuriously lead us to find price falls, there can be no question that the 9/11 terminations were involuntary and, at the level of each analyst or affected stock, entirely random.

3.1.4 Hold-out sample. As a second check on our identification strategy, we return to the forty-three brokerage closures but now focus on the fifty-one stocks—excluded from the main sample—that face imminent delisting within sixty days of a closure. For these stocks, a termination should be of little consequence because the increase in information asymmetry over their remaining publicly traded lives is minimal in present value terms. Table 3, Panel C, reports the CARs for this hold-out sample. In contrast to the results shown in Panels A and B, CARs are generally small and positive, rather than large and negative. The lack of statistical significance suggests that investors are unconcerned about losing public signals if the stock is known to soon delist. This provides indirect support for our identification strategy.

3.1.5 Interpretation. The announcement returns reported in Panels A and B of Table 3 are upper bounds. They will overstate the price effect of increasing information asymmetry to the extent that a brokerage closure reduces the overall number of signals, i.e., if institutional investors choose not to replace a lost public signal in-house. Since analysts who were laid off in our brokerage closures frequently joined buy-side investors, any overstatement is likely minor, but it cannot be quantified in this particular setting. Section 3.5 will introduce an alternative experiment that will allow us to cleanly isolate the price effect of changes in information asymmetry. As we will see, it is nearly as large as the point estimates shown in Table 3.

3.1.6 Spillover effects. Coverage terminations likely have cross-stock spillover effects on stocks that do not themselves suffer terminations but whose payoffs are correlated with those of a terminated stock. Of course, Easley and O'Hara's (2004) model, on which ours builds, abstracts from correlated payoffs to keep the analysis tractable. But it is reasonable to expect that coverage terminations affect the prices of other stocks to the extent that they, in fact, share valuation factors (see, e.g., Admati 1985; Veldkamp 2006b). Which stocks have common valuation factors is not directly observable, but prior evidence suggests that information spillovers and return comovement are more common within industry than across industries (see, e.g., King 1966; Foster 1981; Ramnath 2002).

A simple way to test for cross-stock spillovers is thus to examine the return behavior of *other* stocks with *unchanged* analyst coverage in the *same* industry. We proceed as follows. For each termination, we form a value-weighted industry portfolio consisting of all stocks in the same Fama–French thirty-industry grouping *except* those terminated that day.[21] We then calculate abnormal returns for these industry portfolios around coverage terminations, benchmarked (as before) using either the market model or the Fama–French three-factor model.[22]

The average of the forty-three brokerage closures involves 102.3 terminations in 10.1 industries for a total of $43 \cdot 10.1 = 434$ industry shocks available for this test.[23] Since analysts specialize by industry, each industry usually suffers multiple coverage terminations in a given brokerage closure event. To capture the resulting cross-industry variation in the number of coverage shocks an industry receives, we weight the industry portfolio returns by the number of stocks in the industry whose coverage was terminated in the event. This number averages 10.1 in our sample (i.e., 102.3 terminated stocks on average per closure event divided by 10.1 affected industries on average).

Under the null of no spillovers, industry abnormal returns will be zero around coverage terminations. The announcement-period CARs for the industry portfolios are shown in Table 4 and illustrated in Figure 4. On the announcement day, stocks with unchanged coverage in affected industries experience statistically significant price falls, averaging 34 and 20 basis points relative to the market model and the Fama–French three-factor model, respectively. The magnitude of these abnormal returns is similar when we widen the window to day +1, +3, or +5. Industry returns do not subsequently recover: cumulated over the [+5,+22] event window, abnormal returns average 30 and −12 basis points in total (not per day) for the two benchmarks. Neither estimate is significantly different from zero.

These patterns are consistent with cross-stock spillover effects: coverage terminations appear to affect not just the prices of the terminated stocks but also those of unaffected stocks in the same Fama–French industry.[24] The magnitude

[21] Industry portfolio assignments are based on SIC codes; industry definitions and SIC mappings are taken from Kenneth French's Web site.

[22] Following Fama and French (1997), each industry portfolio is formed one year prior to the event. This keeps the composition of the portfolio the same throughout both the estimation period and the event period. Delistings are handled using CRSP delisting returns.

[23] We exclude sixteen industry shocks involving twenty-eight terminations in Fama–French industry 30 ("Other"). Stocks in this industry portfolio are unlikely to share a valuation factor. Results are not sensitive to including these shocks in the test.

[24] An important methodological implication of these findings is that abnormal returns for coverage terminations must be computed using standard event-study measures, such as the market model or the Fama–French three-factor model used in Table 3, instead of using industry returns as a benchmark.

Table 4
Cross-stock spillover effects

CAR calculated over the period . . .		Unaffected stocks in affected industries: No. of obs.	Market model		Fama-French 3-factor model	
Close on day before termination to close on termination day	[−1, 0]	434	−0.34	***/***	−0.20	***/***
Close on day before termination to close on day +1	[−1, +1]	434	−0.42	***/***	−0.31	***/***
Close on day before termination to close on day +3	[−1, +3]	434	−0.55	***/***	−0.44	***/***
Close on day before termination to close on day +5	[−1, +5]	434	−0.48	***/***	−0.36	***/***
Close on day +5 after termination to close on day +22	[+5, +22]	434	0.30		−0.12	

To test whether coverage terminations induce cross-stock spillover effects, we examine the return behavior of *other* stocks with *unchanged* analyst coverage in the *same* industry around each termination announcement. The forty-three brokerage closures involve terminations of stocks from 10.1 Fama–French thirty-industry groupings on average, giving a total sample size of 434 industry-events that are subject to coverage terminations. For each of these 434 industry-events, we calculate value-weighted industry portfolio returns by using all stocks in the relevant Fama–French industry, *except* those terminated that day. (Industry portfolio assignments are based on SIC codes; industry definitions and SIC mappings are taken from Kenneth French's Web site.) We then calculate abnormal returns for each of the 434 industry portfolios around coverage terminations by using either the market model or the Fama–French three-factor model as a benchmark, as in Table 3. To capture cross-event variation in the number of coverage shocks an industry receives in a given brokerage closure, we weight the abnormal industry returns by the number of stocks in the industry whose coverage was terminated in the event. The resulting average CARs are shown under the heading "Unaffected stocks in affected industries." Abnormal returns are reported in percents. We report significance levels by using a block bootstrap of length 100 to control for dependence among events; we also report a standard event-study Patell t-test. These two test statistics are separated by "/." We use ***, **, and * to denote statistical significance at the 0.1%, 1%, and 5% levels (two-sided), respectively.

of the price falls surrounding terminations is more than three times larger for the terminated stocks (in Table 3) than for other stocks in their industry (in Table 4), which is economically reasonable.

3.1.7 Broker type: Retail versus institutional brokers. Implication 1 predicts that announcement-week CARs should be more negative for brokerage firms that serve retail investors than for those that exclusively cater to institutions. The intuition is that a signal produced by an institutional broker is largely unavailable to retail investors, so whether institutions obtain the signal from the broker or produce it themselves should have no effect on the information asymmetry retail investors face in the market.

Table 5, Panel A, tests this prediction by splitting the sample by broker type. Of the forty-three brokerage closures, twenty-one involve institutional brokers and twenty-two involve brokers with retail clients. These account for 1,911 and 2,518 of the 4,429 coverage terminations, respectively. Consistent with Implication 1, we find significantly greater price falls following exogenous coverage terminations at retail brokers than at institutional brokers. For example, on the announcement day, price falls average −159 basis points for retail brokers and −49 basis points for institutional brokers, relative to the market model. (Results are similar when we use the Fama–French three-factor model and when we widen the window around the closure announcements.)

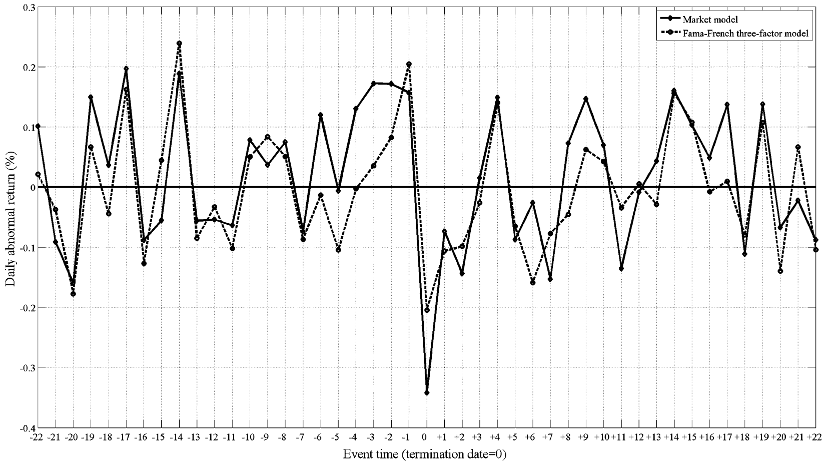

Figure 4
Daily industry spillover returns around closure-induced coverage terminations
To test whether coverage terminations induce cross-stock spillover effects, we examine the return behavior of *other* stocks with *unchanged* analyst coverage in the *same* industry around each termination announcement. The figure shows daily abnormal returns for unaffected stocks in the same Fama–French thirty-industry grouping. The average brokerage closure affects 10.1 Fama–French industries or 434 industry-events in total. Daily abnormal returns are constructed as follows. First, for each of the 434 industry-events, we compute the value-weighted average return for all stocks in the relevant Fama–French industry, *except* those terminated that day. Then, we subtract either the market-model return or the Fama–French three-factor model return in order to obtain abnormal industry returns. Factor loadings for these benchmarks are estimated in a one-year pre-event window, which end twenty-three trading days before the termination date. Finally, we compute the event-weighted average abnormal industry return by weighting the 434 abnormal industry returns by the numbers of stocks in the industry whose coverage was terminated in the events. As in Figure 3, we show abnormal returns for the forty-five trading days, which begin on trading day −22 and end on trading day +22, relative to the day a brokerage firm closure was announced (day 0).

The fact that the price changes are not zero after institutional-broker closures suggests a widening of information asymmetry among institutional investors, as some produce the lost signal in-house and others choose not to.

3.1.8 Signal precision. Some signals are likely more precise, and therefore potentially more valuable, than others.[25] For example, Malloy (2005) reports evidence that analysts provide more accurate research the closer they are located to a company they cover. A likely explanation for this pattern is that geographically proximate analysts possess an informational advantage over more distant analysts. Another measure of signal quality is the analyst's reputation. Stickel (1992) finds that analysts who are voted "all-stars" in the annual survey published by *Institutional Investor* magazine produce more accurate research than do unrated analysts. Against this background, we explore whether terminations that involve local analysts or highly rated analysts are associated with larger price falls.

25 We say "potentially" because we cannot sign the relation between signal noise and price changes unambiguously; see Comparative Static 4.

Table 5
Broker type and signal precision

	Number of obs.		Market model						Fama-French 3-factor model					
Panel A: Broker type	Retail	Inst.	Retail		Institutional		Difference		Retail		Institutional		Difference	
[−1, 0]	2,509	1,905	−1.59	***/***	−0.49	**/***	−1.10	*/***	−1.18	***/***	−0.25	*/***	−0.93	*/***
[−1, +1]	2,509	1,905	−2.30	***/***	−0.69	***/***	−1.61	**/***	−1.84	***/***	−0.49	**/***	−1.35	**/***
[−1, +3]	2,509	1,905	−2.84	***/***	−1.17	***/***	−1.67	*/***	−2.27	***/***	−0.75	***/***	−1.52	**/***
[−1, +5]	2,509	1,905	−3.53	***/***	−1.40	***/***	−2.14	**/***	−2.64	***/***	−0.96	***/***	−1.68	*/***
	Number of obs.		Market model						Fama-French 3-factor model					
Panel B: Distance	Local	Distant	Local		Distant		Difference		Local		Distant		Difference	
50-mi. radius														
[−1, 0]	724	3,690	−1.63	***/***	−1.02	***/***	−0.62	*/***	−1.10	**/***	−0.71	***/***	−0.39	/*
[−1, +1]	724	3,690	−2.37	***/***	−1.46	***/***	−0.91	*/***	−1.74	*/***	−1.16	***/***	−0.58	/*
[−1, +3]	724	3,690	−3.37	*/***	−1.87	***/***	−1.50	**/***	−2.46	†/***	−1.44	***/***	−1.01	*/**
[−1, +5]	724	3,690	−4.00	*/***	−2.34	***/***	−1.67	**/***	−2.57	*/***	−1.79	***/***	−0.78	/*
100-mi. radius														
[−1, 0]	851	3,563	−1.58	***/***	−1.01	***/***	−0.58	†/***	−1.09	**/***	−0.70	***/***	−0.39	/*
[−1, +1]	851	3,563	−2.26	***/***	−1.45	***/***	−0.81	*/***	−1.67	**/***	−1.16	***/***	−0.51	/*
[−1, +3]	851	3,563	−3.16	***/***	−1.87	***/***	−1.30	*/***	−2.31	*/***	−1.44	***/***	−0.87	†/**
[−1, +5]	851	3,563	−3.75	**/***	−2.34	***/***	−1.41	*/***	−2.44	*/***	−1.79	***/***	−0.72	/*
200-mi. radius														
[−1, 0]	1,102	3,312	−1.55	***/***	−0.97	***/***	−0.57	†/***	−1.14	***/***	−0.66	***/***	−0.48	/***
[−1, +1]	1,102	3,312	−2.20	***/***	−1.41	***/***	−0.78	*/***	−1.69	***/***	−1.11	***/***	−0.58	/**
[−1, +3]	1,102	3,312	−3.16	***/***	−1.77	***/***	−1.39	*/***	−2.40	**/***	−1.35	***/***	−1.05	†/***
[−1, +5]	1,102	3,312	−3.79	***/***	−2.22	***/***	−1.57	*/***	−2.62	**/***	−1.68	***/***	−0.94	/**

(*continued*)

Table 5
Continued

	Number of obs.		Market model						Fama-French 3-factor model					
Panel C: Analyst reputation	Rated	Unrated	Rated		Unrated		Difference		Rated		Unrated		Difference	
[−1, 0]	723	3,691	−1.46	***/***	−1.05	***/***	−0.41	/*	−1.16	**/***	−0.70	***/***	−0.46	/**
[−1, +1]	723	3,691	−2.08	***/***	−1.52	***/***	−0.56	*/*	−1.96	***/***	−1.12	***/***	−0.84	**/***
[−1, +3]	723	3,691	−1.84	†/***	−2.17	***/***	0.33	/	−2.04	**/***	−1.53	***/***	−0.51	/†
[−1, +5]	723	3,691	−3.17	***/***	−2.50	***/***	−0.67	/†	−3.38	***/***	−1.63	***/***	−1.76	*/***

This table examines how the impact of an exogenous coverage termination varies with the type of broker (Panel A) and the precision of the lost analyst signal (Panels B and C). Panel A splits the sample by broker type into terminations by brokerage firms that serve retail investors or exclusively cater to institutions. We expect terminations by institutional brokers to lead to smaller price falls. We use two proxies for signal precision: 1) the geographic distance between the analyst and the company losing coverage (Panel B); and 2) the analyst's ranking in accordance with the annual *Institutional Investor* survey or the "Best on the Street" rankings produced by the *Wall Street Journal* (Panel C). Cumulative abnormal returns are computed as in Table 3. Abnormal returns are reported in percents. Note that the sample falls short of 4,429 because we require a minimum of fifty trading days of pre-event stock prices in order to estimate model parameters. We report significance levels by using a block bootstrap with a block length of 100 in order to control for dependence among events; we also report a standard event-study Patell t-test. These two test statistics are separated by "/." We use ***, **, *, and † to denote statistical significance at the 0.1%, 1%, 5%, and 10% levels, respectively.

To measure proximity, we first identify each analyst's location as of the brokerage closure date from Nelson's annual *Directory of Investment Research* or, when unavailable, from the analyst's telephone number listed in his or her research reports in the Investext archive.[26] Similarly, for each stock the analyst covers, we find the location of the company's headquarters in 10-Q and 10-K filings that we retrieve from the SEC's Edgar service.[27] We then retrieve the longitude and latitude of each analyst and company location from the *Census 2000 U.S. Gazeteer*.[28] Finally, we compute the geographic distance between each analyst–company pair by using the Haversine formula for the distance between two points on the globe.

To measure analyst reputation, we check if the analyst was rated an "all-star" in the most recent *Institutional Investor* survey, which is published annually in October, or if he was rated a "master stock picker" in the most recent "Best on the Street" rankings produced by the *Wall Street Journal* preceding the brokerage-firm closure date.

Table 5 tests for differences in announcement CARs between geographically proximate and distant analysts (Panel B) and between rated and unrated analysts (Panel C). We consider three cutoffs for proximity, namely, analysts who are located within fifty, 100, or 200 miles of the company in question. We find that CARs are substantially more negative when a local analyst terminates coverage, regardless of the cutoff used. For example, for the fifty-mile cutoff, local terminations are associated with average announcement-day CARs of −163 basis points using the market model, while distant terminations are associated with abnormal price falls that average −102 basis points. This 62-basis-point difference is significantly different from zero at the 0.047 level with bootstrapped standard errors and at the 0.001 level using a Patell event-study test. Widening the radius to 100 or 200 miles has little effect on the estimated difference. When we widen the window around the closure announcements, the differences in market-model CARs between local and distant analysts increase to −141 to −167 basis points, depending on radius used. These differences are statistically significant at the 5% level or better, except in two cases, which are significant only at the 10% level. When we use the Fama–French three-factor model to compute abnormal returns, the pattern remains the same though the differences in CARs are generally more noisily estimated.

The results for rated versus unrated analysts, shown in Panel C, look similar. Stocks terminated by rated analysts tend to experience significantly more negative announcement returns than do stocks terminated by unrated analysts. The differences in average CARs range from −41 basis points

26 We cannot simply use the location of the brokerage firm's headquarters as many brokers have branch offices in multiple cities.

27 Compustat also has location information, but it is not historic. Thus, when a firm moves its headquarters, Compustat overwrites the address fields for each premove fiscal year.

28 Available at http://www.census.gov/tiger/tms/gazetteer/zcta5.txt.

on the announcement day to −176 basis points for the [–1,+5] window.[29] Overall, these patterns are consistent with the intuition that investors react more strongly to the loss of more precise signals.

3.2 Changes in demand following coverage terminations

According to Proposition 2, institutions increase their holdings of the stock following a coverage termination, while retail investors sell. Unfortunately, we have no high-frequency trading data with which to estimate changes in institutional and retail demand.[30] Instead, we use the quarterly CDA/Spectrum data to compute the change in the fraction of a firm's outstanding stock held by institutions required to file 13f reports.[31] As in Section 2.5, we report diff-in-diff tests. Clearly, use of quarterly data will generate less timely and noisier results than the pricing results discussed in the previous section.

Table 6, Panel A shows that 13f filers as a group increase their holdings from 61.2% to 62.1% of shares outstanding following the average termination with no contemporaneous change among control stocks. The average difference-in-differences is 0.9 percentage points, or 1.4% relative to the pretermination average, with a bootstrapped *p*-value of .043. Results are virtually identical when we account for clustering by forming portfolios of stocks that experience terminations at the same time. These results suggest that 13f institutions increase their holdings while others—chiefly retail investors—sell following terminations, which is consistent with Proposition 2.[32]

Panel B focuses on the 9/11 sample. Here, institutional holdings of affected stocks increase by 3.6% following a coverage termination, although this point estimate is not statistically significant (possibly due to the small sample). Panel C shows that institutional holdings of stocks that face imminent delisting do *not* increase—in fact, they fall significantly—when coverage is terminated. As in the previous section, this provides indirect support for our identification strategy.

3.3 Testing the comparative statics

To test the comparative statics, we regress market-model announcement-day CARs[33] (ΔEP) and the diff-in-diff changes in institutional holdings (ΔEID) on proxies for key model parameters. The proxy for the fraction of institutions among the firm's shareholders (μ) is simply the fraction of the company's stock held by 13f filers pretermination. We use the first two moments of

29 For one window, [–1,+3], the difference is positive when using the market model, albeit not significantly so.

30 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.

31 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.

32 By contrast, Xu (2006) finds that institutions *reduce* their ownership following *endogenous* terminations. Xu's finding is consistent with the view that, unlike our exogenous terminations, his endogenous ones are implicit sells.

33 Results are robust to instead using Fama–French three-factor CARs and alternative estimation windows.

Table 6
Changes in institutional holdings around coverage terminations

	No. of obs.	Terminations: Before drop	Terminations: After drop	Matched controls: Before drop	Matched controls: After Drop	Mean DiD	$p-$value DiD = 0	*Economic magnitude (percentage change)*
Panel A: Closure-induced terminations (in %)								
Standard diff-in-diff approach:	4,264	61.2	62.1	62.0	62.0	0.9	.043	*1.4*
Portfolio strategy correcting for time clustering:	43	59.4	60.2	62.0	62.0	0.8	.034	*1.4*
Panel B: Terminations due to 9/11 (in %)								
Standard diff-in-diff approach:	351	37.8	39.5	33.1	33.5	1.3	.294	*3.6*
Panel C: Imminent delistings (in %)								
Standard diff-in-diff approach:	17	45.4	15.0	51.6	51.9	−30.7	.007	*−67.6*

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 = $(post_i - pre_i) - (\overline{post - pre}_{Control\ Group\ i})$, i.e., the difference between the pre- and posttermination 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 2. We report these statistics for the closures sample (Panel A); a separate sample of terminations at two brokerage firms located in the World Trade Center that were devastated in the 9/11 attacks (Panel B); and a separate sample of closure-induced terminations of stocks facing imminent delisting, for which a termination should be of little consequence (Panel C). The 13f data are taken from Thomson Reuters' CDA/Spectrum database. In Panel A, we lose 158 observations that involve sample stocks that have no viable controls and seven observations with missing 13f data. In Panel B, we similarly lose four observations without valid matches and one observation without 13f data. In Panel C, we lose five observations without valid matches. In addition, because these are stocks that are about to be delisted, we lose thirty observations for which no 13f data are available in the quarter after the termination. We report both standard diff-in-diff estimates, treating each observation as independent and using a block bootstrap to control for dependence among events; and (in Panel A) a portfolio strategy that buys all stocks that are terminated on a given closure day. In the latter case, the estimate of the average change in institutional holdings in the portfolio strategy is the time-series average of the forty-three brokers' average change in institutional holdings, in analogy with a Fama–MacBeth regression; significance levels are computed from the standard deviation of the forty-three averages. In Panel A, we use a block length of 100 for the bootstrap, corresponding to the approximate number of terminations per brokerage closure event. In Panel B, given the much smaller sample sizes, we use a block length of 10. In Panel C, where we have only seventeen observations due to the fact that most companies in this sample are delisted before the next 13f filing date, we compute a simple t-test.

the distribution of log daily share turnover to proxy for the mean ($\bar{X}$) and uncertainty (η^{-1}) of aggregate supply. Payoff uncertainty (ρ^{-1}) is proxied for with the standard deviation of quarterly earnings per share. The main proxy for signal noise (γ^{-1}) is the dispersion of analyst earnings forecasts. In addition, we parameterize signal noise with the two measures analyzed in subsection 3.1.8—the analyst's geographic proximity to the company and her reputation—as well as the analyst's experience. We also control for whether a termination coincides with a negative earnings surprise,[34] for the number of other analysts that cover the stock at the time of the termination, and for unobserved brokerage-firm–specific effects by using fixed effects.

The results generally support the comparative statics of the model. Table 7, column 1, focuses on price changes in the whole sample, while columns 2 and 3 split the sample into retail–broker and institutional–broker terminations, respectively.[35] The adjusted R^2 of 13.3% in column 1 is quite large for a cross-sectional return regression. All coefficients have the predicted sign and are, unless otherwise noted, significant at the 5% level or better (using bootstrapped standard errors to reduce the impact of clustering). We find that price falls are significantly larger in retail stocks (i.e., in stocks with smaller institutional holdings), which is consistent with Comparative Static 1. They are larger when turnover is larger and more variable, which is consistent with Comparative Statics 2 and 3a, and earnings are more volatile (p-value $=$ .065), which is consistent with Comparative Static 3b. To capture the potentially U-shaped relation between price changes and signal noise, which is discussed in relation to Comparative Static 4, we include both the level and square of forecast dispersion. The coefficient for the level is negative, while for the squared term it is positive, which supports a U-shaped relation. Price falls are also larger when the analyst is local, as in subsection 3.1.8, and the longer the individual has worked as an analyst. (Analyst reputation, on the other hand, is not significant.) Finally, the extent of coverage by other analysts has no effect on the magnitude of price falls in our data.[36]

The results are very similar in the retail–broker sample in column 2, except that each coefficient is greater in magnitude than in the sample as a whole, as is the adjusted R^2. Column 3, by contrast, has much less explanatory power, and

[34] This is designed to capture negative firm-level news that might be causing the price falls we observe. This turns out not to be an issue. Brokerage closures coincide with negative earnings news about as often as chance alone would predict. In the I/B/E/S universe, 39.72% of all earnings announcements are negative surprises over our sample period. With four quarterly earnings announcements per year, 252 trading days, and a two-day window, there is a $4 \cdot 2/252 \cdot 0.3972 = 1.26\%$ chance that a coverage termination randomly coincides with a negative earnings announcement or the day before—about the same as the observed frequency of 1.09% in our terminations sample. Excluding these rare cases has no material effect on the estimated CARs.

[35] Since the regressions include brokerage-firm fixed effects, we cannot include a dummy for broker type in column 1.

[36] While we might intuitively expect a negative coefficient, the derivative of ΔEP with respect to H, the number of public signals, depends on a complex interaction of the other model parameters and so cannot be signed unambiguously.

Table 7
Cross-sectional determinants of changes in price and in institutional demand

		Dependent variable:					
		CAR [−1,0] in %			DiD change in 13f holdings		
		(1)	(2)	(3)	(4)	(5)	(6)
μ	13f Holdings in quarter before drop	1.250***	2.072***	−0.076	−0.060***	−0.061***	−0.062***
		0.394	*0.554*	*0.492*	*0.009*	*0.011*	*0.013*
$\bar{X}$	Mean daily log turnover	−0.635***	−1.047***	−0.112	0.004*	0.007*	0.001
		0.108	*0.165*	*0.141*	*0.002*	*0.003*	*0.003*
η^{-1}	SD of log turnover	−1.235***	−1.443***	−1.045**	−0.007	0.004	−0.019
		0.275	*0.423*	*0.343*	*0.005*	*0.007*	*0.019*
ρ^{-1}	SD of earnings per share	−0.327†	−0.519†	−0.081	0.035***	0.041***	0.029*
		0.177	*0.289*	*0.183*	*0.009*	*0.011*	*0.013*
γ^{-1}	Analyst forecast dispersion	−2.237*	−3.769**	−0.029	−0.009	0.007	−0.010
		0.882	*1.372*	*0.906*	*0.039*	*0.054*	*0.059*
γ^{-1}	Analyst forecast dispersion^2	0.869*	1.417*	−0.023	−0.088	−0.181†	−0.020
		0.396	*0.577*	*0.473*	*0.076*	*0.110*	*0.113*
γ^{-1}	= 1 If analyst is local	−0.349*	−0.551*	−0.008	−0.003	−0.005	0.001
		0.160	*0.258*	*0.168*	*0.004*	*0.005*	*0.005*
γ^{-1}	= 1 If analyst is rated	−0.205	0.063	−0.743*	0.006	0.002	0.027
		0.235	*0.267*	*0.308*	*0.005*	*0.006*	*0.017*
γ^{-1}	Log experience of covering analyst	−0.375***	−0.616***	−0.059	0.000	0.003	−0.006
		0.103	*0.156*	*0.106*	*0.002*	*0.002*	*0.004*
	= 1 If coincides w/ neg. earnings surprise	−1.605	−3.916*	1.110	0.006	−0.012	0.028
		1.081	*1.582*	*1.153*	*0.016*	*0.022*	*0.024*
	Log no. other brokers covering the stock	−0.010	−0.052	0.055	0.000	0.000	−0.001
		0.116	*0.174*	*0.138*	*0.002*	*0.003*	*0.004*
	Adjusted R^2	13.3%	16.0%	5.7%	4.0%	4.6%	4.0%
	Wald test: all coeff. = 0	75.5***	83.3***	21.9*	75.7***	62.4***	36.6***
	No. of observations	4,321	2,446	1,875	4,162	2,342	1,820

We test the model's comparative statics by regressing proxies for changes in price and institutional demand around coverage terminations on proxies for μ (the fraction of informed investors); $\bar{X}$ (mean aggregate supply); η^{-1} (aggregate supply uncertainty); ρ^{-1} (payoff uncertainty); and γ^{-1} (signal noise). The proxy for price changes is the market-adjusted CAR from the day before the announcement of a brokerage-firm closure to the end of the announcement day. The proxy for informed-demand changes 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 termination, net of the mean contemporaneous change in institutional holdings in matched control firms (see Table 6 for further details). The independent covariates are defined as follows. The proxy for μ is the fraction of the company's stock that is held by institutional investors as of the quarter-end prior to the termination. Mean and variance of aggregate supply are based on the first two moments of the distribution of log daily turnover, which is estimated over the six months that end one month prior to the termination. Our proxy for payoff variance is the standard deviation of quarterly earnings per share, using up to twenty quarters of data prior to the termination. Our proxies for signal noise include the level and square of analyst forecast dispersion (defined as the time-series mean of the standard deviation of analyst earnings per share forecasts in the year prior to the termination); whether the analyst is local to the company suffering a termination (defined as being located within a 200-mile radius of its headquarters); whether the analyst is an "all-star" or a *Wall Street Journal* "master stock picker"; and for the analyst's experience (the log number of quarters since the analyst first appeared in the I/B/E/S databases or, if missing, when he or she obtained their first license according to the Financial Industry Regulatory Authority's "Broker Check" service). Where necessary, variables are winsorized to reduce noise and avoid outliers driving the results. We control for whether the termination coincides with a negative earnings surprise and for the number of remaining analysts that cover the stock (estimated from the I/B/E/S forecast summary file). All specifications include broker fixed effects. These remove observed and unobserved time-invariant broker-specific effects so that we test the comparative statics within-broker. We cannot also include time-specific effects, since each broker closes only once. We also cannot include any broker characteristics, such as its size or retail-focus, as such variables are perfectly collinear with the broker fixed effects. Instead, we split the sample into retail brokers (columns 2 and 5) and institutional brokers (columns 3 and 6). We report significance levels by using a bootstrap with 1,000 replications in order to control for cross-sectional dependence among stocks within each closure event. These 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.

only two coefficients (those for aggregate supply uncertainty, η^{-1}, and analyst reputation) are significant. The contrast between columns 2 and 3 supports Implication 1, since the CARs following institutional–broker terminations should be much smaller (less negative) compared to retail–broker terminations.

Columns 4–6 repeat these analyses for changes in institutional demand. The results are much noisier than those for price changes—not surprisingly, given the quarterly nature of the 13f data. The signs for $\partial \Delta EID/\partial \mu$, $\partial \Delta EID/\partial \bar{X}$, and $\partial \Delta EID/\partial \rho^{-1}$ are all as predicted and statistically significant in the whole and retail–broker samples. We view these results as encouraging, though, given the obvious data limitations, they should be interpreted with caution.

3.4 Testing Corollary 2: Changes in expected returns

Price falls 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 how a stock's exposure to systematic liquidity risk changes following a coverage termination and relative to matched controls. Our empirical specification follows Acharya and Pedersen (2005), who propose an equilibrium model that describes how exposure to aggregate liquidity risk affects a firm's expected return. Their pricing equation is

$$E\left(r_{i,t} - r_{f,t}\right) = E(c_{i,t}) + \lambda(\beta_{1i} + \beta_{2i} - \beta_{3i} - \beta_{4i}),$$

where $r_{i,t}$ is stock i's month-t return; $r_{f,t}$ is the risk-free rate; $c_{i,t}$ is a measure of stock i's illiquidity; λ is the price of risk; and the four betas measure exposure to aggregate risks, as embodied in the comovement between stock returns and market returns (β_1); stock illiquidity and aggregate illiquidity (β_2); stock returns and aggregate illiquidity (β_3); and stock illiquidity and market returns (β_4).

The Acharya–Pedersen model captures the intuition that stocks should have lower prices (i.e., higher expected returns) when the stock return covaries more with the market return and less with aggregate illiquidity and when the stock's illiquidity covaries more with aggregate illiquidity and less with the market return. The first point is the CAPM rationale; the other three capture that an investor prefers stocks that are liquid when their portfolio is illiquid, all else equal.

We separately estimate the Acharya–Pedersen regression for each stock i using weekly data over twelve-, eighteen-, and twenty-four-month windows that end two weeks prior to the termination announcement or start two weeks after the announcement date. We focus on the weekly frequency to alleviate potential nonsynchronicity concerns, while providing enough data points in reasonably short windows to obtain reliable beta estimates.[37] Aggregating over

37 A monthly frequency provides too few data points if we want to keep the estimation windows reasonably short to avoid confounding events. A daily frequency introduces a nonsynchronicity problem. A common method to

sample firms, we obtain mean betas before and after a coverage termination. We repeat this procedure for control firms, selected as described previously, and compute differences-in-differences for each of the four betas. The regression equation demonstrates that all betas are multiplied by the same price of risk. As a result, the effect of a termination on expected returns depends on the change in the total risk loading, $(\beta_1 + \beta_2 - \beta_3 - \beta_4)$. We provide tests of the mean diff-in-diff in the total risk loading and in the individual betas.

The results, shown in Table 8, are consistent with Corollary 2. Regardless of estimation window, terminated stocks experience an increase in total risk loading of between 0.164 and 0.191, which is equivalent to an increase of between 11.9% and 14.3% over their pretermination means of 1.34 to 1.38. These point estimates are significantly different from zero using bootstrap tests. The overall effect of these increased loadings is to increase the expected returns of stocks experiencing coverage terminations.

The increase in the total risk loading is dominated by a significant drop in β_4, which captures a greater tendency for stock illiquidity to be high when the aggregate market is falling in value. The comovement between market illiquidity and the illiquidity of sample stocks (β_2) nearly doubles, with statistical significance. While unrelated to liquidity, we also find that posttermination returns load less strongly on the market factor (i.e., $\Delta\beta_1 < 0$). Contrary to the model's predictions, $\Delta\beta_3$ has a sign that indicates decreased liquidity risk, in some cases significantly so. This effect is, however, economically small (averaging between 0.001 and 0.003) and is swamped by the liquidity risk increases captured by $\Delta\beta_2$ and $\Delta\beta_4$.

3.5 Coverage (re-)initiations

A tenable reading of our evidence is that retail investors value analyst coverage and react to exogenous coverage terminations by reducing their valuation of the affected stocks. In principle, exogenous coverage initiations should have the opposite effect. As Proposition 3 notes, an initiation makes a new public signal available to retail investors, which reduces information asymmetry and thereby increases retail investors' valuation of the stock. Testing this implication is, however, tricky for two reasons. First, initiations usually expand the set of signals, which, in and of itself, would increase prices. Second, surely most initiations are endogenous: analysts choose which stocks to initiate on and usually do so with a buy recommendation. Most initiations thus likely carry information about the stock's future prospects. Disentangling the effects of more information and of the information content itself from the effect of reduced information asymmetry requires a subtle experiment.

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adjust for nonsynchronous data is to include lags of independent variables and sum the loadings over all lags (see Scholes and Williams 1977). However, construction of the Acharya–Pedersen illiquidity measure involves an adjustment for lagged aggregate market value. Including lagged independent variables therefore produces mechanical covariation between stock illiquidity and lagged market returns, used in calculating β_4, so this nonsynchronicity adjustment cannot be used. Using weekly data is therefore preferable.

Table 8
The effect of coverage terminations on liquidity risk exposure

	Terminations		Matched controls			
	Before drop	After drop	Before drop	After drop	Mean DiD	p−value DiD = 0
Panel A: 12-mo. window						
Net DiD: $\Delta(\beta_1+\beta_2-\beta_3-\beta_4)$					0.168	.005
β_1 (stock return vs. market return)	1.177	1.143	1.122	1.090	−0.003	.701
β_2 (stock illiquidity vs. mkt illiquidity)	0.016	0.026	0.046	0.039	0.017	<.001
β_3 (stock return vs. market illiquidity)	−0.032	−0.023	−0.026	−0.020	0.003	.015
β_4 (stock illiquidity vs. market return)	−0.112	−0.174	−0.233	−0.139	−0.156	.008
Panel B: 18-mo. window						
Net DiD: $\Delta(\beta_1+\beta_2-\beta_3-\beta_4)$					0.191	<.001
β_1 (stock return vs. market return)	1.176	1.162	1.114	1.128	−0.028	.024
β_2 (stock illiquidity vs. mkt illiquidity)	0.016	0.031	0.042	0.048	0.009	.006
β_3 (stock return vs. market illiquidity)	−0.027	−0.024	−0.024	−0.022	0.001	.082
β_4 (stock illiquidity vs. market return)	−0.146	−0.330	−0.219	−0.192	−0.211	<.001
Panel C: 24-mo. window						
Net DiD: $\Delta(\beta_1+\beta_2-\beta_3-\beta_4)$					0.164	<.001
β_1 (stock return vs. market return)	1.167	1.174	1.101	1.139	−0.031	.009
β_2 (stock illiquidity vs. mkt illiquidity)	0.019	0.035	0.045	0.049	0.013	<.001
β_3 (stock return vs. market illiquidity)	−0.027	−0.024	−0.023	−0.022	0.002	.006
β_4 (stock illiquidity vs. market return)	−0.165	−0.337	−0.267	−0.254	−0.184	<.001

The table reports cross-sectional means of exposure to systematic liquidity risk before and after a termination for sample stocks and their matched controls. Changes in liquidity risk are assessed within the framework of Acharya and Pedersen (2005), who propose an equilibrium model that describes how individual firms' exposure to aggregate liquidity risk affects expected returns. The resulting pricing equation, $E(r_{i,t}^{-}r_{f,t}) = E(c_{i,t})+\lambda(\beta_{1i}+\beta_{2i}-\beta_{3i}-\beta_{4i})$, is described in Section III.D. The four betas capture exposures to aggregate risks embodied by the comovement between stock returns and the market return (β_1); stock illiquidity and aggregate illiquidity (β_2); stock returns and aggregate illiquidity (β_3); and stock illiquidity and the market return (β_4). Increases in expected returns that are due to liquidity risk are associated with an increase in β_2 and decreases in β_3 and β_4. Our empirical methodology closely follows Acharya and Pedersen (2005), adapted to a weekly frequency (Wednesday close to Wednesday close). For each stock, illiquidity is calculated as the adjusted Amihud measure, which is defined as $\min(0.25 + 0.30 Illiq_t^i P_{t-1}^M,\ 30.00)$, where $Illiq_t^i$ is the ratio of absolute stock return to the dollar trading volume (scaled by 10^6) for stock i averaged over the days in week t; and P_{t-1}^M is the ratio of the capitalizations of NYSE and AMEX stocks at the end of week t– 1 to the value at the end of the first week of 1998. Aggregate illiquidity is the value-weighted average illiquidity of NYSE and AMEX stocks each week. Betas are calculated by using innovations in stock illiquidity and aggregate illiquidity defined as residuals from AR(2) models because of the persistence in illiquidity levels. As in previous tables, for each sample termination, a control group is formed by selecting stocks with the same Daniel et al. (1997) size and book-to-market benchmark assignment in the month of June prior to a termination, subject to the conditions that control firms 1) were covered by one or more analysts in the three months before the event; and 2) did not themselves experience a coverage termination in the quarter before and after the event. When more than five matches exist, we choose the five stocks closest to the sample stock in terms of market beta. (We lose 158 observations that involve stocks without any viable controls.) Betas are winsorized to 2.5% in each tail in order to mitigate the effect of extreme outliers and reduce noise. We then calculate a difference-in-difference test for each sample stock i, DiD = $(post_i - pre_i) - (\overline{post\text{-}pre}_{Control\ Group\ i})$, and report the cross-sectional mean. Betas are calculated by using weekly data over twelve-, eighteen-, and twenty-four-month windows that end two weeks prior to the termination announcement or start two weeks after the announcement date. We test the null hypothesis that exposure to systematic liquidity risk is unchanged around a coverage termination using bootstrapped p-values. These adjust for potential cross-sectional dependence that is due to overlapping estimation windows caused by time clustering as multiple stocks are terminated in each brokerage-firm closure event. The bootstraps use a block length of 100, corresponding to the approximate number of terminations per closure event.

Happily, our third natural experiment plausibly corresponds to the exogenous transformation of a private into a public signal, while holding the overall number of signals constant. This natural experiment arises when a brokerage firm that serves retail clients acquires a brokerage firm that exclusively caters to institutions. Before such a merger, the acquirer's retail clients had no access to the target's institutional research; in the terminology of our model, such research constituted a set of private signals. The effect of the merger is then to give retail clients access to the research output of the acquired (institutional) research department. In other words, previously private signals now become public signals without changing the overall number of signals.[38] As a result, information asymmetry is reduced and investors should respond by rebalancing their portfolios in such a way that prices rise.[39] Unlike in the brokerage-closure setting, we can thus cleanly isolate the price effect of changes in information asymmetry in this test.

Note that these are *re*initiations, rather than initiations. Importantly, reinitiations of this type are not news. The following quote, taken from the reinitiation announcement of retail broker Oppenheimer & Co., illustrates our identification strategy.

> We are initiating coverage of the following companies [list omitted]. This initiation of coverage is the result of the completion of the sale of certain U.S. businesses of CIBC World Markets Corp. to Oppenheimer & Co. Inc., including U.S. Equity Research. *Our investment thesis on these stocks remains unchanged.* (Emphasis added.)

Thus, these coverage reinitiations are uninformative about the future prospects of the stocks, yet the model predicts a price reaction as previously private signals become public.

We build a database of merger-induced coverage reinitiations as follows. First, we identify all brokerage-firm mergers between 1990 and 2010 from the Securities Industry Association Yearbook. Eight of these involve a merger between a retail broker and an institutional broker. Second, we check if the institutional broker covered any stocks in the six months before the merger that the retail broker did not. This step eliminates two of the eight mergers.[40]

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38 In practice, once the research departments have been merged, the brokerage firm announces the reinitiation of the target's coverage list—typically, hundreds of stocks—in a single day and from then on, its retail clients have access to research on an expanded set of stocks.

39 Note that this natural experiment is quite distinct from Hong and Kacperczyk's (2010). They focus on cases where *both* brokers covered a stock before the merger, as they are interested in the loss of competition among analysts as one of the premerger analysts is made redundant. In other words, in their experiment, the number of public signals falls. By contrast, our natural experiment keeps the number of analysts covering the stock (and hence the total number of signals) constant, by focusing on cases where only the institutional broker covered the stock before the merger. Thus, our experiment captures instances of previously private signals becoming public.

40 Merrill Lynch's acquisition of Petrie Parkman, an oil and gas boutique with limited coverage, and the merger of Dillon Read, a retail broker, with SBC Warburg, an institutional broker with hardly any U.S. coverage.

Table 9
Changes in price around coverage reinitiations

Cumulative abnormal return (CAR) calculated over the period . . .		No. of obs.	Market model		Fama-French 3-factor model	
Close on day before reinitiation to close on reinitiation day	[−1, 0]	1,010	1.01	***/***	0.61	***/***
Close on day before reinitiation to close on day +1	[−1, +1]	1,010	1.34	***/***	0.70	***/***
Close on day before reinitiation to close on day +3	[−1, +3]	1,010	1.39	***/***	0.73	***/***
Close on day before reinitiation to close on day +5	[−1, +5]	1,010	1.30	***/***	0.95	***/***
Close on day +5 after reinitiation to close on day +22	[+5, +22]	1,010	−0.55		−0.03	

We compute cumulative abnormal returns over various windows around coverage reinitiations by using two benchmarks: the market model and the Fama–French three-factor model. Factor loadings are estimated in a one-year pre-event window that ends eleven days before the reinitiation date, and cumulative abnormal returns during the event window are calculated by using the estimated model as a benchmark. (Results are nearly identical if we include a Carhart (1997) momentum factor in the Fama–French model.) Abnormal returns are reported in percents. The reinitiations sample consists of 1,019 coverage reinitiations for 902 unique firms between Q4, 1997, and Q1, 2008. We lose nine observations because we require a minimum of fifty trading days of pre-event stock prices in order to estimate model parameters. We report significance levels by using a block bootstrap of length 100 in order to control for dependence among events; we also report a standard event-study Patell t-test. These two test statistics are separated by "/." We use ***, **, and * to denote statistical significance at the 0.1%, 1%, and 5% levels (two-sided), respectively.

The remaining six mergers involve 1,220 stocks covered by the institutional broker but not by the retail broker ahead of the merger. We identify reinitation dates from Reuters Estimates and I/B/E/S; these are the announcement dates used in our tests (adjusted for after-hours announcements as needed). Finally, we screen out 201 of the 1,220 reinitiations that likely conveyed new information.[41] This leaves a final sample of 1,019 coverage reinitiations for 902 unique firms between Q4, 1997, and Q1, 2008, for which institutional research becomes available to retail clients by way of plausibly uninformative reinitiations.

The associated announcement-period CARs for the coverage reinitiations are shown in Table 9 and illustrated in Figure 5. In the figure, we observe a sharp spike in abnormal returns on the announcement day when prices rise on average by 101 basis points, relative to the market model, and by sixty-one basis points, relative to the Fama–French three-factor model. Table 9 reports that both price increases are statistically significant in block bootstraps and using a standard Patell event-study test. There is a moderate amount of drift over the next day and no evidence of subsequent reversal over the remainder of the postannouncement month. Thus, at least over this horizon, the price increases following plausibly exogenous coverage reinitiations appear to be permanent. These patterns are the mirror image of the price movements surrounding exogenous coverage terminations.

41 Namely, 152 reinitiations accompanied by forecast revisions and forty-three reinitiations accompanied by earnings forecasts for fiscal periods for which the broker had not yet made forecasts before the merger.

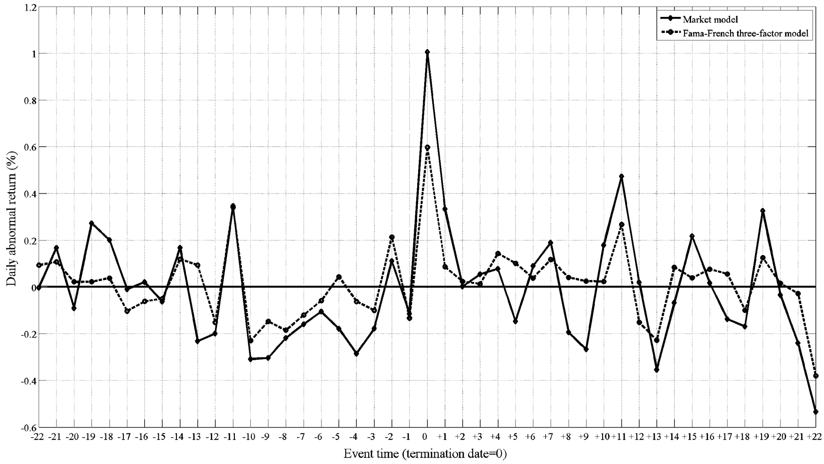

Figure 5
Daily abnormal returns around merger-induced coverage reinitiations
The reinitiations sample consists of 1,019 coverage reinitiations for 902 unique firms between Q4, 1997, and Q1, 2008. We compute abnormal returns for the forty-five trading days, which begin on trading day –22 and end on trading day +22, relative to the day reinitiations were announced in the wake of a merger of a retail broker with an institutional broker (day 0). We use two alternate benchmarks: the market model and the Fama–French three-factor model. Factor loadings for these benchmarks are estimated in a one-year pre-event window, which ends twenty-three trading days before the reinitiation date, and abnormal returns during the event window are calculated by using the estimated model as a benchmark. (Results are nearly identical if we include a Carhart (1997) momentum factor in the Fama–French model.)

For the median firm in our reinitiations sample (with a market value of $1,226.5 million), the CARs for the [–1,+5] window imply that market value increases by between $11.7 million and $12.4 million in the week following a reinitiation. These magnitudes are in line with those from the terminations sample. They suggest that reductions in information asymmetry have economically meaningful price effects. Overall, our results suggest that prices fall when information asymmetry increases and rise when information asymmetry falls.

4. Conclusion

Asset pricing models routinely assume that investors have heterogeneous information, but it is an open empirical question how important information asymmetry really is for asset prices. This article provides direct evidence of the effect of information asymmetry on asset prices and investor demands by using three plausibly exogenous sources of variation in the supply of information.

The first variation we exploit is caused by the closure of forty-three brokerage firms' research operations between 2000 and the first quarter of 2008. These closures led to analyst coverage of thousands of stocks being terminated for reasons that we argue were unrelated to the stocks' future prospects. The second source of variation comes from the 9/11 tragedy, which also led to coverage terminations. The third captures analyst research that used to be available only to institutional investors being made available to retail investors in the wake of mergers between retail and institutional brokers.

Following exogenous coverage terminations, information asymmetry increases (proxied by a range of standard measures such as bid-ask spreads), while share prices and uninformed (i.e., retail) investors' demands fall. Consistent with the comparative statics of a standard rational expectations equilibrium model with multiple assets and multiple signals, we find that the falls in price and retail demand are larger, the more retail investors hold the stock; when turnover is larger and more variable; and when the asset's payoff is more uncertain. We show theoretically that prices fall because expected returns become more sensitive to liquidity risk and provide empirical support for this prediction. In sum, our results imply that information asymmetry has a substantial effect on asset prices and that a primary channel that links asymmetry to prices is liquidity.

We provide a new approach to statistically evaluating the determinants of risk premia which stands in contrast to traditional methods. Our tests take advantage of natural experiments, an identification strategy common to corporate finance but rarely found in the asset pricing literature. Asset pricing tests usually specify a linear beta-pricing model for returns and use a regression-based method (e.g., Fama and MacBeth 1973) to test whether a candidate risk factor is priced. This method, while ubiquitous, is fraught with specification sensitivity. Jagannathan and Wang (1998) and Kan and Zhang (1999) show that when a linear beta-pricing model is misspecified (as it almost certainly is in any application), regression-based estimators often find a significant risk premium associated with a risk factor or firm characteristic, even when the true risk premium is zero. Because changes in information asymmetry in our setting are plausibly exogenous, tests for their impact on asset prices are robust to variations in model specification, as evident in the consistency of our findings across different benchmark models in Table 3. The benefit of a quasi-randomized experiment is that the treatment effect can be measured simply as the average difference between treatment and control groups, dispelling specification concerns in unrelated parts of the pricing model. We believe that our natural experiments can serve as useful sources of exogenous variation for future empirical studies of the effects of information asymmetry in financial markets.

Appendix A. List of Brokerage Closures

Broker	Date	Reason	Broker type	No. of terminated stocks	No. of Fama-French 30 sectors covered
Schroders	Apr. 2000	Acquired	Retail or both	151	22
Wit Capital, Ltd.	May 2000	Acquired	Retail or both	12	3
Brown Brothers Harriman & Co.	June 2000	Closure	Institutional	163	25
J.C. Bradford & Co.	June 2000	Closure	Retail or both	189	13
George K. Baum & Co.	Oct. 2000	Closure	Retail or both	83	14
Donaldson, Lufkin & Jenrette Securities	Oct. 2000	Acquired	Retail or both	391	26
Paine Webber & Co.	Nov. 2000	Acquired	Retail or both	480	26
R.J. Steichen & Co.	Dec. 2000	Acquired	Retail or both	14	8
Hambrecht & Quist	Jan. 2001	Acquired	Retail or both	93	15
Wasserstein Perella & Co.	Feb. 2001	Acquired	Institutional	19	7
ING Financial Markets	May 2001	Acquired	Institutional	77	13
Epoch Partners, Inc.	June 2001	Acquired	Retail or both	30	5
Emerald Research	July 2001	Closure	Retail or both	30	11
SunTrust Equitable Securities	Aug. 2001	Acquired	Retail or both	14	5
Wachovia Securities	Oct. 2001	Acquired	Retail or both	36	9
Conning & Co.	Oct. 2001	Closure	Institutional	82	3
Tucker Anthony Sutro Capital Markets	Nov. 2001	Acquired	Retail or both	32	7
Hoak, Breedlove, Wesneski, & Co.	Nov. 2001	Closure	Institutional	36	10
Sutro & Co.	Jan. 2002	Acquired	Retail or both	13	2
ABN AMRO	Apr. 2002	Closure	Institutional	480	26
Frost Securities, Inc.	July 2002	Closure	Institutional	77	10
Robertson Stephens	July 2002	Closure	Retail or both	456	18
Vestigo-Fidelity Capital Markets	Aug. 2002	Closure	Institutional	31	6
Commerce Capital Markets, Inc.	Apr. 2003	Closure	Institutional	49	10
The Chapman Company	July 2003	Closure	Retail or both	13	4
Montauk Capital Markets Group	Feb. 2004	Closure	Institutional	15	2
Schwab Soundview Capital Markets	Oct. 2004	Closure	Institutional	167	14
J.B. Hanauer & Co.	Feb. 2005	Closure	Institutional	58	14
Tradition Asiel Securities, Inc.	Apr. 2005	Closure	Institutional	53	13
Parker/Hunter Inc.	May 2005	Acquired	Retail or both	6	4
IRG Research	June 2005	Closure	Institutional	92	16
Wells Fargo Securities	Aug. 2005	Closure	Retail or both	175	14
Advest, Inc.	Dec. 2005	Acquired	Retail or both	13	1
Legg Mason Wood Walker, Inc.	Dec. 2005	Acquired	Institutional	24	6
Moors & Cabot, Inc.	Sept. 2006	Closure	Institutional	23	2
Petrie Parkman & Co.	Dec. 2006	Acquired	Institutional	34	2
Ryan Beck & Co.	Jan. 2007	Acquired	Retail or both	39	7
Cohen Bros. & Co.	Apr. 2007	Closure	Institutional	65	2
Prudential Equity Group, Inc.	June 2007	Closure	Institutional	309	23
Cochran, Caronia Securities, LLC	Sept. 2007	Acquired	Institutional	31	2
A.G. Edwards & Sons	Oct. 2007	Acquired	Retail or both	163	21
Nollenberger Capital Partners	Nov. 2007	Closure	Retail or both	85	10
CIBC World Markets	Jan. 2008	Acquired	Institutional	26	11

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