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SHAPING LIQUIDITY:
ON THE CAUSAL EFFECTS OF VOLUNTARY DISCLOSURE

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Shaping Liquidity: On the Causal Effects of Voluntary Disclosure
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ABSTRACT

Can managers influence the liquidity of their firms' shares? We use plausibly exogenous variation in the supply of public information to show that firms seek to actively shape their information environments by voluntarily disclosing more information than is mandated by market regulations and that such efforts have a sizeable and beneficial effect on liquidity. Firms respond to an exogenous loss of public information by providing more timely and informative earnings guidance. Responses appear motivated by a desire to reduce information asymmetries between retail and institutional investors. Liquidity improves as a result of voluntary disclosure and in turn increases firm value. This suggests that managers can causally influence their cost of capital via voluntary disclosure.

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Improved liquidity raises a firm's market value by lowering its discount rate (see Amihud and Mendelson (1986, 1989), Brennan and Subrahmanyam (1996), and Amihud (2002)). While liquidity is often viewed as resulting from market makers' and investors' actions in an exogenously specified information environment, we examine if corporate managers can actively influence the liquidity of their firms' shares. An important channel through which they might do so is voluntary disclosure. Theoretical models such as Diamond (1985) and Diamond and Verrecchia (1991) show that managers may commit to disclose more information than is mandated by market regulations in order to reduce information asymmetry among their investors. Consistent with this, recent survey evidence suggests that managers provide voluntary disclosure to "reduce the information risk that investors assign to our stock" (Graham, Harvey, and Rajgopal (2005)).

Whether managers can indeed affect their information environments, and thereby their liquidity and cost of capital, remains an open question. The main empirical challenge is that voluntary disclosure is *voluntary*: managers *choose* to disclose more information for reasons that could well affect liquidity directly. For example, evidence indicates that firms disclose more when earnings are easier to predict (Chen, Matsumoto, and Rajgopal (2011)); but lower earnings uncertainty would reduce information asymmetry, and so increase liquidity, independently of disclosure. Thus, showing that disclosure affects liquidity causally, and by how much, has proved challenging. Healy and Palepu (2001) highlight this endogeneity problem in their survey, noting that "disclosure changes are unlikely to be random events" and concluding that "it is difficult to draw strong conclusions about the direction of causality."

We use plausibly exogenous variation in the supply of public information to show that firms seek to shape their information environments through voluntary disclosure and that such efforts improve their liquidity. The former result confirms the central assumption made in theoretical models of disclosure. The latter result contributes to our understanding of liquidity in financial markets, by showing that managers can actively influence the liquidity of their shares and, ultimately, firm value.

Our tests exploit a natural experiment first explored in Kelly and Ljungqvist (2012, henceforth KL). In 2000-2008, 43 brokers closed their research operations as a result of adverse changes in the economics of sell-side research. The closures led to over 4,000 coverage terminations for U.S. firms.[1] KL demonstrate that the closures are unrelated to individual firms' future prospects and so are plausibly exogenous at the level of the affected stocks. They then show that when a stock loses (some) analyst coverage in the wake of a closure, information asymmetry among investors increases, the firm's share price falls by 1.12% to 2.61%, and retail investors sell the stock. These patterns are most pronounced when the firm loses coverage by an analyst serving retail investors.

We use this experiment not only to investigate how managers respond to exogenous shocks to their information environments but also to establish the causal effects of their responses. To do so, we identify firms in KL's sample with a history of providing voluntary disclosure in the form of guidance regarding their quarterly EPS numbers, the most prominent performance measure that a firm supplies to investors.[2] We then match each 'treated' firm to an observably similar 'control' firm that did not experience an exogenous coverage shock at the same time and estimate difference-in-difference tests and treatment regressions.

After replicating KL's finding that coverage shocks have a first-order adverse effect on liquidity (as measured by Amihud's (2002) illiquidity measure, *AIM*), we investigate the dynamics of liquidity. Extending KL's analysis, we show that the contemporaneous liquidity hit partially reverses one quarter later. To establish whether this reversal is the result of an increase in voluntary disclosure, we need to rule out other channels through which liquidity might recover.

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[1] A small subset of 564 of these stocks simultaneously lost a market-maker. As we will show, our results are not driven by changes in market-making. An even smaller subset of 63 stocks also lost an underwriting relationship.

[2] Alternative measures of disclosure are the external AIMR ratings of disclosure policy (Lang and Lundholm (1993, 1996), Botosan and Plumlee (2002)); the frequency of 8-K filings (Leuz and Schrand (2009)); or the length of 10-K filings (Leuz and Schrand (2009)). These measures would likely have lower power in our setting because it is unclear how much immediate discretion managers have over their financial statements. We focus on earnings guidance because it allows us to examine a discretionary disclosure action that is immediately available to managers (assuming that their firm has a guiding history) and that is both observable to the econometrician and easily measurable. This, in turn, enables us to identify the causal effects of voluntary disclosure on liquidity.

The most prominent alternative channel is changes in market-making. If research closures tend to coincide with closure of the broker's market-making operation, then liquidity might fall not because of a change in the information setting but due to a change in trading mechanics, and liquidity might subsequently recover due to entry of new market makers. We use detailed data of brokers' trading operations to show that liquidity reversals are not driven by an initial loss and subsequent recovery of market-making.

We then turn to a falsification (or placebo) test to rule out other potential confounding effects. The results of the placebo test are most consistent with the conjecture that the post-shock recovery in liquidity reflects firms' deliberate efforts to fill the information gap created by the loss of coverage. Specifically, we show that the observed liquidity recovery only occurs among firms with a history of providing guidance ("guiders"): one quarter after the shock, liquidity returns to near its pre-shock level on average. Firms without a guidance history ("non-guiders"), on the other hand, suffer a permanent reduction in liquidity. Moreover, guiders – but not non-guiders – respond to a coverage shock by increasing disclosure in a sustained and informative way.

The contrast between guiders and non-guiders is striking: firms that can, react to a coverage shock by increasing disclosure and their liquidity then recovers, while firms without a history of guiding almost never begin to guide and suffer a lasting hit to liquidity.[3] These patterns suggest that disclosure can cause liquidity to improve. Any alternative interpretation of the observed liquidity reversals must explain why liquidity improves only among guiders and not among non-guiders. Thus, these tests bolster our interpretation that disclosure causally improves liquidity.

Further evidence in favor of an asymmetric-information channel comes from the fact that firms respond only to the loss of a retail analyst, not when they lose coverage by an analyst catering exclusively to institutional investors. This suggests that managers supply guidance primarily with the aim to reduce information asymmetry (and thereby increase share price) by communicating

[3] By revealed preference, non-guiders view the costs of committing to sustained disclosure as outweighing the benefits. We review potential reasons why non-guiders rarely become guiders after a coverage shock in Section 3.

with retail investors. This is consistent with asymmetric-information asset pricing models in which retail investors are less well-informed than institutional investors (Kyle (1985)).

To quantify the effect of voluntary disclosure on liquidity, we estimate treatment regressions that exploit the observed liquidity dynamics to construct an instrument for disclosure. Clearly, contemporaneous coverage shocks cannot be used as an instrument as they affect liquidity directly and so violate the exclusion restriction. But *lagged* coverage shocks could be a valid instrument. Identification requires that lagged coverage shocks a) lead to more disclosure, b) do not affect liquidity directly, and c) do not correlate with some omitted variable that in turn affects liquidity.

Our findings show that firms respond strongly to coverage shocks by increasing guidance, suggesting that firms view disclosure as a substitute for public information, at least to some extent. Thus, condition a) is satisfied. While condition b) cannot be tested directly, there is no evidence of drift in the response of liquidity to coverage shocks beyond the quarter of the shock (quite the contrary, given the observed reversals). Thus, investors appear to react immediately and coverage shocks have no obvious *direct* effect on liquidity one quarter later. And the absence of reversals among non-guiders puts constraints on possible violations of condition c), as argued previously.

When we use lagged coverage shocks to instrument a firm's disclosure choices, we find that increased disclosure has a beneficial effect on liquidity. This suggests that managers can indeed affect their information environments through disclosure, consistent with Graham, Harvey, and Rajgopal's (2005) survey evidence. The economic effects of disclosure estimated in our IV models are between 7 and 12 times greater than when we naïvely ignore endogeneity. This suggests that naïve estimates are substantially downward biased and confirms Leuz and Wysocki's (2008) conjecture that the small economic effects found in prior studies linking disclosure to quantities such as liquidity or the cost of capital likely reflect endogeneity biases. Finally, we document that "talking up liquidity" pays off for firms in the sense of increasing their market value.

Our study makes three contributions. First, prior literature has been unable to identify an economically meaningful benefit to voluntary disclosure. Once we correct for endogeneity, we find that firms can affect the liquidity of their shares substantially through voluntary disclosure. We show that greater liquidity leads to increased firm value, which implies that voluntary disclosure reduces a firm's cost of capital.[4] While not unexpected in light of the evidence reported in Graham, Harvey, and Rajgopal (2005), quantifying the liquidity effect is nonetheless important given companies' legitimate concerns that voluntary disclosure could result in shareholder lawsuits (Skinner (1994)). We focus on voluntary disclosure to isolate a feature of firms' information environments that is under managers' direct control. Related work on the liquidity effects of transparency (Lang and Maffett (2011a,b) and Lang, Lins, and Maffett (2012)) or information quality (Ng (2011)) considers various measures that can impact investors' uncertainty about a firm but does not isolate managerial discretion or address the question of causality.[5]

Second, we contribute to an active debate among practitioners and academics as to whether guidance is desirable. Consultants such as McKinsey[6] and Deloitte[7] and influential institutions such as the Business Roundtable and the CFA Institute[8] advise against the practice, citing legal costs, 'punishment' by investors for missed earnings, and lack of evidence that disclosure raises stock prices or mitigates volatility. Early economic models viewed disclosure negatively. Hirshleifer (1971), Trueman (1973), and Hakansson, Kunkel, and Ohlsen (1982) show that disclosure can reduce investors' ability to share risk. Fama and Laffer (1971) and Hakansson (1977), on the other hand, argue it can raise firm value by lowering investors' information acquisition costs. Later work by Diamond (1985) shows that disclosure can in fact improve risk-sharing in a general-equilibrium setting, while Fishman and Hagerty (1989) argue that disclosure

[4] This complements the findings of Leuz and Verrecchia (2000), who link a reduction in information asymmetry to a firm's commitment to increased disclosure via voluntary adoption of an international reporting regime, and Daske et al. (2008), who link improvements in liquidity to the introduction of international reporting standards.
[5] As Lang and Maffett (2011b) note, "A problem with most of the transparency literature is that transparency is clearly a choice variable, likely determined jointly with a variety of other variables, making assessments of causality difficult. For this reason, the literature to date has generally been careful about inferring causality."
[6] See Hsieh, Koller, and Rajan (2006).
[7] See http://www.corpgov.deloitte.com/site/caneng/financial-reporting/transparency/earnings-guidance.
[8] Quoted in Houston, Lev, and Tucker (2010).

improves stock price efficiency and thereby leads to more efficient managerial investment decisions. Our analysis speaks directly to the beneficial effect of disclosure on liquidity and value.

Third, we present novel evidence suggesting that voluntary disclosure is primarily aimed at reducing information asymmetries between retail and institutional investors. This new stylized fact is consistent with asset pricing models that stress the importance of information asymmetries for investor demands and hence asset prices.

The paper proceeds as follows. Sections 1 and 2 describe our sample and empirical strategy. Section 3 prepares the grounds for our identification strategy by showing that exogenous coverage shocks have a large adverse effect on liquidity, which subsequently reverses. Section 4 investigates whether the reversals reflect management efforts to fill information gaps, left by coverage shocks, through increased voluntary disclosure. Section 5 reports instrumental-variables regressions that model the causal effect of disclosure on liquidity and value. Section 6 concludes.

1. Sample and Data

Our sample combines data from CRSP, Compustat, and I/B/E/S with data from First Call's Company Issued Guidelines (CIG) database. We begin by constructing an unbalanced panel of all firms in the CRSP-Compustat merged file for the period 1999 through 2009. The unit of observation in all our tests is a firm-fiscal quarter. For every firm-fiscal quarter, we retrieve quarterly guidance from First Call; analyst coverage and forecast information from I/B/E/S; returns, price, and volume data from CRSP; and quarterly financial data from Compustat. We also compute, for every firm-fiscal quarter, Amihud's (2002) illiquidity measure (*AIM*), which measures the price impact of trades. Section 5.3 reports robustness tests using six other popular liquidity proxies. For full details of the construction of all our variables, see Appendix A.

From our panel, we extract a treatment sample of firms that suffer exogenous coverage terminations and thus shocks to their information environments. We compare the guidance behavior and liquidity dynamics of these treated firms to a control sample composed of matched firms that do not suffer exogenous shocks to their analyst coverage. This approach allows us to difference away secular trends and swings in liquidity. Figure 1 shows that market-wide illiquidity, as measured by *AIM*, generally trended down beginning in late 2002 and rose sharply in the wake of the collapse of Lehman Brothers on September 15, 2008.

KL document that the coverage shocks we use are plausibly exogenous, so we do not need to worry about unobserved heterogeneity, selection, or endogeneity contaminating our tests. All that remains is to make sure that treated and control firms are observably similar and so comparable.

1.1 Treatment Sample

KL identify a sample of 2,180 unique firms suffering 4,429 exogenous coverage terminations as a result of 43 brokerage closures over the period Q2, 2000 through Q1, 2008.[9] KL's unit of analysis is a firm-day. As mentioned, our panel setup focuses instead on firm-fiscal quarters. Some of KL's firms are hit with multiple coverage shocks in a given fiscal quarter, leaving 4,185 firm-fiscal quarters with one or more termination events. To assemble our treatment sample, we impose three filters, as set out in Table 1. First, we remove 1,122 firm-fiscal quarters involving 737 firms that had no history of guidance as of the termination quarter. Dropping such firms means we focus on within-firm changes in guidance policy (i.e., the intensive margin).[10] Second, we require that a treated firm has suffered no exogenous coverage terminations in the previous four quarters. This requirement eliminates 794 firm-fiscal quarters involving 447 'serially shocked' firms and ensures that we observe a clean treatment effect. Third, since our tests are in the spirit of diff-in-diffs, we remove four instances of firms that did not trade in the quarter before a termination and two firms that lost coverage in their last fiscal quarter of listing on the NYSE, Amex, or Nasdaq. These

[9] Note that our sample period begins at the time Regulation Fair Disclosure came into effect. Reg FD prohibits firms from selectively disclosing information to investors or analysts. Most prior work on disclosure uses pre-Reg FD data (see Botosan (1997), Coller and Yohn (1997), Botosan and Plumlee (2002), and Brown and Hillegeist (2007)).

[10] The extensive margin (i.e., initiation of a guidance program) is also of potential interest. However, only 57 firms provide guidance for the first time in the first fiscal quarter after an exogenous coverage termination, so firms do not appear to respond to loss of coverage by initiating guidance for the first time. We will return to this point below in the context of a 'placebo' test focusing on non-guiders.

filters yield 2,263 termination quarters for which we next seek to identify control firms.

1.2 Control Firms

KL document that firms suffering exogenous coverage terminations have significantly larger market capitalizations, are covered by significantly more analysts, and have significantly more volatile stock returns than the average firm in CRSP and I/B/E/S. These characteristics are known to correlate with liquidity (Breen, Hodrick, and Korajczyk (2002), Irvine (2003), and Chordia, Roll, and Subrahmanyam (2000)). Thus, to ensure that our tests are not confounded by these systematic differences, we match firms in terms of log market value of equity, the number of analysts covering the stock, and return volatility, all measured in the fiscal quarter before the treated firm's coverage termination. In addition, given our focus on liquidity, we also match on *AIM*.[11] As in the case of the treatment sample, we also require that a potential match has not itself suffered an exogenous termination in the previous four quarters.[12]

Of the 2,263 treated firm-fiscal quarters, 168 cannot be matched to any eligible control firm within standard tolerances (specifically, a 0.005 caliper). The final treatment sample therefore consists of 2,095 firm-fiscal quarters with one or more exogenous coverage termination events affecting 1,468 unique firms and a corresponding sample of 2,095 matched controls.

1.3 Panel-Regression Sample

For the purposes of our tests, we retain (up to) four quarters before and (up to) four quarters after each of the 2,095 termination quarters for both treated firms and their matched controls.[13] In total, the estimation sample used in our panel-regression tests consists of 17,017 firm-fiscal quarters for treated firms and 17,239 firm-fiscal quarters for their controls.

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[11] Our results are robust to allowing liquidity to have an industry or market-wide component.

[12] Mirroring the other filters we impose on treated firms, we require that control firms are already guiders and trade in both the quarter before and the quarter after a treated firm's coverage termination. The match is implemented using a nearest-neighbor propensity score match without replacement. As in KL, both treated and control firms are also required to be operating companies (CRSP share codes ≤ 12).

[13] We refer to these nine-quarter spans as termination episodes. Recall that we require firms not to have suffered an exogenous coverage termination in the four fiscal quarters before entering our sample. Thus, none of the 2,095 termination episodes overlaps in time for the same firm and so each treated firm-fiscal quarter is present at most once.

To avoid look-ahead bias, our filters allow firms to suffer further exogenous shocks to their analyst coverage in the four quarters after the initial termination quarter. Such further shocks occur in 592 post-termination quarters, bringing the total number of affected quarters to 2,687 (= 2,095 + 592). Bearing in mind that some firms suffer multiple shocks in a given fiscal quarter, the overall number of coverage shocks captured in our panel-regression sample is 2,821, or 63.7% of the 4,429 coverage shocks in KL's sample.[14]

1.4 Descriptive Statistics

Table 2 shows how tightly matched treated and control firms are. Panel A reports summary statistics for the four variables we match on, along with differences in means and medians between the two groups of firms. Market capitalization averages $7,861 million for treated firms and $7,919 million for controls. The average treated firm is covered by 6.9 analysts pre-termination, compared to an average of 7 analysts among the controls. Average monthly return volatility is 3.2% for treated firms and 3.3% for controls. And finally, log *AIM* averages 0.049 for both groups. None of these differences in means is statistically significant at even the 10% level. The same is true of the medians, which are similarly close.

Panel B reports eight measures of voluntary disclosure. At the broadest level, 33.4% of treated firms and 31.9% of the controls provide some kind of guidance in the quarter preceding a termination. Forecasts (defined as guidance issued before the end of the fiscal quarter) are more common than pre-announcements (defined as guidance issued after the end of the fiscal quarter but before actual earnings are announced): 27.5% (26.7%) of treated (control) firms provide forecasts while 9.4% (8.3%) of treated (control) firms pre-announce. None of the differences between treated and control firms is significant in the quarter before the match.

In terms of *form*, guidance can be quantitative (providing a numerical earnings forecast or forecast range); or it can be qualitative (to the effect that earnings are forecast to be above, below,

[14] Non-guiders account for 1,174 of the 1,608 KL coverage shocks that do not make it into our sample. The remainder comes mostly from serially shocked firms.

or in line with expectations without providing a numerical estimate). Consistent with many prior studies, we find that quantitative guidance is more common: it is provided by around 30% of firms, compared to around 5% of firms that provide qualitative guidance. At the time of matching, there are no significant differences between treated and control firms in these proportions.

We distinguish three types of *content*: negative, positive, and 'hot air.' We code guidance as negative (positive) if management supplies an earnings estimate that falls below (exceeds) the prevailing consensus (i.e., the median analyst forecast) one day before the guidance date. We code the remaining guidance as 'hot air,' capturing cases where management supplies guidance that does not differ from the prevailing consensus. In an average quarter, negative guidance is around twice as likely as positive guidance (about 18% versus 9%). Around 7% of firms provide guidance that amounts to hot air. As is the case for the other variables reported in Table 2, treated and control firms do not differ significantly in the content of their guidance before the coverage shock.

To provide a broader context for these patterns, Figure 2 shows trends in quarterly guidance in First Call's CIG database going back to 1998, the year First Call began to systematically collect guidance. The top left chart neatly illustrates the large impact of the SEC's Regulation Fair Disclosure, which came into effect on October 23, 2000. Quarterly instances of guidance increased from 735 in Q3, 2000 to 1,132 in Q4, 2000 and 1,566 in Q1, 2001, consistent with firms shifting from a private to a public channel of communication. Since then guidance has trended downwards. In Q3, 2010, the last quarter for which guidance data is available, firms issued 588 pieces of guidance according to First Call. The remaining three graphs in Figure 2 provide breakdowns of voluntary guidance by horizon, form, and content. They show that forecasts are more numerous than pre-announcements, that guidance is increasingly (and now virtually exclusively) quantitative in nature, and that negative guidance outnumbers positive guidance around two-to-one. These patterns mirror those we see in our sample.

2. Empirical Strategy

Our aim is two-fold: to test if firms respond to a change in their information environments by changing the amount of information they disclose voluntarily; and to test if voluntary disclosure has a causal effect on quantities such as liquidity and firm value. To establish the former, we estimate a diff-in-diff. This requires an exogenous shock to a firm's information environment, which we borrow from KL. The identification assumption is that KL's coverage terminations do not coincide with other shocks that would make it hard to isolate changes in the information environment from other confounding changes. The chief identification concern is that the brokerage-house closures that cause coverage terminations could also upset trading liquidity if market-making operations were shut down at the same time. Though this turns out to rarely be the case in practice, we show that our results hold when we exclude the small number of firms that experience simultaneous shocks to analyst coverage and to market-making.

To establish whether firms' disclosure behavior has a causal effect on liquidity and firm value, we estimate instrumental-variables regressions using KL's coverage terminations as plausibly random shocks to the perceived benefits of voluntary disclosure. The identification assumption that is central to the causal interpretation of our findings is that coverage shocks are uncorrelated with unobservable factors that might impact liquidity and firm value. In addition to ruling out changes in market-making as an alternative channel, we use a falsification (or placebo) test to put restrictions on the nature of any remaining unobservable factor. Specifically, the placebo test implies that any alternative channel must result in a recovery in liquidity and firm value *only* among firms with a history of disclosing more than is mandated by law and regulation and not among firms without a guidance history. While unobserved channels can never be ruled out, the most straightforward dimension on which these two groups of firms differ is voluntary disclosure.

To map the following discussion of our empirical results to our empirical strategy, it is useful to summarize the timeline of events as revealed by the data. In quarter t = 0, a firm suffers an exogenous shock to its information environment in the form of a loss of analyst coverage. Investors react instantly to the increased information asymmetry by reducing the firm's liquidity and its value in the same quarter. Guiders – but not non-guiders – respond by increasing voluntary disclosure for a period of four quarters beginning in quarter $t = 0$. In quarter $t = +1$, investors react to the increased disclosure and liquidity and firm value recover.

3. The Effect of Coverage Shocks on Liquidity

3.1 Contemporaneous Effects

We begin by replicating, in our sample, KL's finding that exogenous coverage terminations are associated with an immediate, sizeable, and significant reduction in liquidity. KL perform a diff-in-diff test comparing changes in *AIM* from the 3 months before a coverage termination day to the 3 months after for treated firms and a set of control firms matched on pre-event size, book-to-market, and liquidity. Our matching criteria are somewhat different from theirs,[15] as is our unit of time (fiscal quarters rather than event days), and as a result of filtering out non-guiders and serially shocked firms, our treatment sample is a subset of theirs.

As Table 3 shows, these sampling differences do not affect the results. Like KL, we observe sizeable and significant increases in log *AIM* following coverage terminations.[16] In the sample of 2,095 treated firm-quarters, illiquidity increases by an average of 0.024 from the prior-quarter average of 0.049. The 2,095 control firm-quarters also see their illiquidity increase, though by less: their change averages 0.014.[17] The difference-in-differences of 0.010 is both economically large and statistically significant (at the 0.033 level) based on bootstrapped standard errors

[15] KL focus on the asset pricing implications of shocks to information asymmetry and thus match on size, book-to-market, and liquidity, three of the most common asset pricing characteristics. Our focus instead is on liquidity per se. As mentioned earlier, liquidity has been linked to size, volatility, and analyst coverage, so our matching criteria are designed to hold these characteristics constant between the treatment and control samples.
[16] Irvine (2003) finds that liquidity improves following coverage initiations and Ellul and Panayides (2009) find that liquidity suffers following coverage terminations. In contrast to KL and to our research design, these studies employ endogenous coverage changes, making it hard to rule out that the analysts in their samples react to an omitted variable that correlates with changes in liquidity.
[17] The fact that control firms experience non-zero changes in *AIM* reflects secular trends and swings in liquidity and illustrates the need to perform diff-in-diff tests. See KL for further discussion.

stratified by fiscal quarter.[18,19]

These results are unchanged if we condition on other contemporaneous firm-level changes that could affect liquidity. Table 4, column 1 reports a firm-level least-squares panel regression of log *AIM* on an indicator for firms suffering a coverage shock and three lagged covariates that prior literature has linked to liquidity: log market cap, the log number of analysts covering the stock, and return volatility. We also include fiscal-quarter fixed effects to control for possible seasonalities in a firm's information environment over the course of its fiscal year; an indicator for firms that are net equity issuers;[20] and year effects. The diff-in-diff estimate declines marginally, from 0.010 to 0.008. It remains statistically significant, regardless of the assumptions we make about the variance-covariance matrix: clustering the standard errors by firm yields a *p*-value of 0.002 (column 1) while bootstrapping them stratified by quarter yields a marginally higher *p*-value of 0.004 (column 2). To validate these *p*-values, we follow Bertrand, Duflo, and Mullainathan (2004). We randomly generate 1,000 sets of 4,185 'pseudo coverage shocks', filter the observations as in Table 1, create matched control samples, and re-estimate the regression. This yields 1,000 estimates of the effect of made-up (rather than actual) 'shocks' on liquidity. The empirical probability of observing a coefficient as large as 0.008 in these random data is 0.044 (44 out of the 1,000 trials). This suggests that the clustered standard errors in columns 1 and 2 are downward biased, but not by enough to falsely reject the null of no (real) effect at the 5% level.[21]

3.2 Dynamics

Having confirmed KL's finding that coverage terminations have a first-order adverse effect on liquidity, we next investigate the dynamics of liquidity. Column 3 adds three lags of the coverage-

[18] Bootstrapping adjusts for potential cross-sectional dependence due to time clustering as multiple stocks are terminated in each of the 43 brokerage-firm closures.

[19] The diff-in-diff estimate is a third smaller than the 0.015 diff-in-diff reported in KL. This is due to the fact that we exclude non-guiders. Non-guiders' liquidity is more sensitive to coverage shocks, in part because they are covered by significantly fewer analysts to begin with (4.8 versus 6.9, on average).

[20] While equity issuance is not an obvious determinant of liquidity, Lang and Lundholm (1993) show that net equity issuers have reason to provide more voluntary disclosure.

[21] The other coefficients confirm that larger firms and firms covered by more analysts are significantly more liquid, consistent with prior work, as are net equity issuers.

shock indicator. Shocks dated t = –1 have a statistically significant effect on illiquidity (p=0.004) but, perhaps surprisingly, the effect is *negative*.[22] This suggests that the contemporaneous liquidity hit due to coverage shocks at t = 0 is partially reversed one quarter later. This novel finding, which extends KL's analysis, suggests that the effect of coverage terminations on liquidity is, to some extent, transitory. Lags dated t = –2 and t = –3 are neither economically nor statistically significant, so the adjustment process appears to be completed within one fiscal quarter.

What could explain these liquidity reversals? They could of course simply be random, but using our 1,000 trials of randomly generated 'pseudo coverage shocks', we *never* see a significant increase in *AIM* being followed by a significant decrease (or vice versa).

The two principal non-random explanations for the recovery in liquidity are either that firms seek to fill the information gap created by the loss of analyst coverage or that some force external to the firm causes liquidity to improve after a while. A leading external force that could be at work is changes in market-making.[23] If the closure of a brokerage firm's research operation tends to coincide with the closure of its market-making operation, liquidity might initially fall not because of a change in the information environment but due to a change in the mechanics of trading. After a while, liquidity might recover not because the firm takes curative action but due to other brokerage houses stepping up their market-making activities in the firm's stock.

This channel can be tested directly. We first identify which of the 43 brokerage firms in the KL sample had market-making operations at the relevant time, based on data from Nastraq and Thomson-Reuters. Twelve did not. For the 31 that did, we then identify which closed down both their market-making and research operations at the same time. Fifteen of the 31 brokerage firms

[22] In our 1,000 trials using randomly generated 'pseudo coverage shocks', the empirical probability of observing a coefficient as large (in absolute terms) as the one estimated for t = –1 is 0.02 (20 out of 1,000 trials).

[23] Another potential external channel is beneficial changes in other analysts' behavior. This can easily be ruled out. We know from Hong and Kacperczyk (2010) that the remaining analysts covering the stock produce more biased research after a coverage termination, causing the information environment to deteriorate rather than to improve. We replicate this finding in our setting. Nor does a coverage termination induce other analysts to initiate coverage: we find no significant increase in analyst coverage one quarter after the initial shock (p=0.968).

kept their market-making operations going while 16 shut them down. However, these 16 brokers did not necessarily make markets in all of the 2,752 stocks that their analysts covered. In fact, the overlap is small: only 564 stocks suffered both a loss of market-making and a loss of research. That represents 12.7% of the 4,429 treated stocks in KL. After applying the filters to assemble our matched treatment and control panels, there are 318 (out of 2,095) treated firm-quarters involving a simultaneous shock to market-making and research coverage.

Column 4 allows the treatment effect and subsequent reversal to depend on whether a firm receives only a research shock or both a research and a market-making shock. For the former, we see a decline in liquidity in the shock quarter and a subsequent reversal. The point estimates are virtually identical to those obtained for the sample as a whole (see column 3). Given that these firms suffer 'pure' research shocks, reversals cannot be driven by an initial loss and subsequent recovery of market-making.[24] Interestingly, the liquidity dynamics of firms suffering both types of shocks are statistically indistinguishable from those of firms suffering only a loss of research coverage. Thus, the additional market-making shock does not exacerbate the liquidity decline.

In column 5, we use a different sample, composed of firms that suffer *only* an exogenous reduction in the number of market makers – without at the same time suffering a reduction in analyst coverage – and their matched controls.[25] Not surprisingly, liquidity declines substantially when a firm loses a market maker (p<0.001). But unlike in the case of a loss of analyst, there is no subsequent recovery. In fact, liquidity continues to decline significantly, compared to matched control firms, for two more quarters. The stark difference in average liquidity dynamics between firms losing an analyst and firms losing a market-maker is interesting. While both result in an

[24] Indeed, when we model the dynamics of the number of market makers around coverage shocks (CRSP variable *mmcnt*), we find no evidence of a fall in the number of market makers coincident with the analyst loss (p=0.878), nor any evidence of a subsequent rebound in the number of market makers (p=0.726).

[25] These exogenous market-maker shocks come from a sample of 50 closures of market-making operations in 2000-2008. Where a broker-dealer closes both market-making and research, we exclude stocks that are affected by both shocks. We apply the same filters to this sample as to the main sample (see Section 1.2) and identify control firms using the same matching criteria as before, except that we also match on the pre-shock number of market-makers.

immediate reduction in liquidity, the mechanism is presumably different: unlike a loss of market-maker, a loss of analyst coverage changes the firm's information environment. We will later test whether firms respond differently to the two types of shock and whether such differences in responses can explain the lack of liquidity reversal following market-making shocks.

Overall, these results go along way towards ruling out a confounding market-making channel. We next turn to a falsification (or placebo) test to investigate the potential for other omitted variables to be confounding our results.

3.3 Placebo Test

As Angrist and Krueger (1999) discuss, a placebo test uses a different sub-population in which the treatment effect is expected *not* to be observed (called the placebo group) because the sub-population is thought to be immune to the treatment or does not have access to the treatment. In our setting, the treatment in question is an increase in voluntary disclosure and the treatment effect is the liquidity reversal one quarter after the coverage shock. If we observed the same treatment effect in the placebo group as in the sub-population of treated firms, we would infer that the treatment (more disclosure) is unlikely to cause the observed treatment effect (liquidity reversal). In this case, some omitted variable would confound the treatment effect, and so we could not conclude that more disclosure caused the liquidity reversal.

We use firms with no history of providing guidance as the placebo group.[26] (As we have already seen, non-guiders rarely become guiders following a coverage shock.[27] By revealed preference, for the majority of non-guiders suffering a shock, the marginal cost of initiating a commitment to guidance exceeds the expected benefit.) Loss of research coverage depresses liquidity for both guiders and non-guiders, but only guiders can react to the shock by increasing voluntary disclosure. Thus, if the liquidity reversals we observe among guiders are caused by management's curative efforts, and not by some other channel that is available to both guiders and

[26] Recall that the treatment and control samples used in our tests so far screen out such non-guiding firms.
[27] See footnote 13.

non-guiders, we expect to find no significant reversals among non-guiders. On the other hand, if the reversal pattern is confounded by some omitted variable, liquidity should subsequently recover regardless of disclosure behavior.

To implement the placebo test, we create a matched sample of non-guiders using the same algorithm as for the guiders. We start with the 1,122 shocks suffered by non-guiders (see Table 1), remove serially shocked firms, and find eligible controls matched on size, analyst coverage, volatility, and *AIM*. This yields 769 panels of treated firms and 769 panels of matched controls. Only 27 of the 769 treated non-guiders (3.5%) become guiders in the quarter after their coverage shock, confirming that losing an analyst is not a sufficiently large shock to induce non-guiders to commit to becoming guiders.

Column 6 of Table 4 estimates the liquidity regression in the non-guider sample, controlling for observed determinants of liquidity. As in the guider sample, we find a strong and significant contemporaneous effect of coverage shocks on the illiquidity of non-guiders. The point estimate of 0.021 is three times larger than the equivalent point estimate of 0.007 for guiders in column 3. This reflects the fact that non-guiders are smaller (mean: $4 billion) and covered by fewer analysts (mean: 4.8), and so are more sensitive to shocks to their information environments. More importantly, for our purposes, we find that the contemporaneous increase in illiquidity following coverage shocks is *not* subsequently reversed. The effect of lagged shocks on *AIM* is -0.006, which is not only statistically insignificant (p=0.417) but also economically small relative to the estimate of the initial liquidity drop of 0.021. Further lags are similarly small and insignificant. Thus, coverage shocks appear to have a persistent effect on the liquidity of non-guiders.

This absence of reversals among treated non-guiders contrasts with the strong evidence of reversals among treated guiders. It is hard to reconcile with an omitted variable: if some other mechanism caused the liquidity reversals, why would this process only happen among guiders and not among non-guiders? For example, it is not obviously consistent with an overreaction story: if the reversals reflected the correction of an initial overreaction among investors, there is no obvious reason why non-guiders should not also see reversals.

Instead, a plausible interpretation of this falsification test is that guiders respond to coverage shocks by disclosing more information, which leads to a subsequent improvement in liquidity, while non-guiders choose not to become guiders and so their liquidity remains depressed. While we do not claim that this is the only possible interpretation of the observed liquidity reversals in our treatment sample, we note that any alternative interpretation needs to be able to explain why liquidity later recovers only among guiders and not also among non-guiders.

3.4 The Extensive Margin

While incidental to the focus of our paper, it is intriguing that so few non-guiders start guiding when hit with a coverage shock: in the overall sample, only 57 non-guiders become guiders after losing coverage; in the placebo sample, only 27 do. By revealed preference, non-guiders view the cost of voluntary disclosure as greater than the benefit. It is an empirical question whether the coverage shocks in our sample are sufficiently large to change this cost-benefit analysis for a substantial fraction of the non-guiders. The fact that few become guiders suggests that the costs of disclosure for these firms must be quite large relative to the size of the liquidity shocks they suffer.

Prior literature discusses what these costs might be. They include the risk that voluntary disclosure could benefit competitors (Campbell (1979), Verrecchia (1983), Bhattacharya and Ritter (1983)) or result in shareholder lawsuits (Skinner (1994), Francis, Philbrick, and Schipper (1994)). Moreover, the literature emphasizes that it is a *sustained* commitment to disclosure that improves a firm's information environment, not a one-off piece of guidance (see Diamond and Verrecchia (1991), Leuz and Verrecchia (2000), or Clinch and Verrecchia (2011)). Once begun, guidance is costly to discontinue: Chen, Matsumoto, and Rajgopal (2011) show that discontinuation announcements lead to significant share price falls and that analysts interpret them as implicit admissions that future earnings will be lower than expected. Firms are thus unlikely to start guiding unless they believe they will want to continue guiding in future.

3.5 Identifying Assumptions

The results in Table 4 suggest that contemporaneous coverage shocks are not a suitable candidate for instrumenting voluntary disclosure for the purpose of estimating the effect of disclosure on liquidity or firm value. The reason is that they directly affect a firm's liquidity and so violate the exclusion restriction. But lagged coverage shocks have the potential to be a valid instrument. Their reduced-form effect on illiquidity in Table 4 is negative, which is consistent with the proposed disclosure channel. This is reassuring: as Angrist and Krueger (2001) note, if we do not see the proposed causal relation of interest in the reduced form, it is probably not there.

To be a valid instrument, lagged coverage shocks must satisfy three conditions. As argued earlier, they must not correlate with either 1) liquidity directly (other than through their effect on disclosure) or 2) any omitted variable that in turn determines liquidity. Our tests using market-making shocks and the placebo group of non-guiders suggest that these conditions are plausibly satisfied: it is hard to think of an alternative channel, besides disclosure, that would cause the increase in illiquidity to subsequently reverse only among guiders and not among non-guiders.

The third condition is that coverage shocks must correlate with voluntary disclosure, and to avoid weak-instrument problems, must do so 'strongly.' We next test directly if firms (specifically, guiders) indeed disclose significantly more information when their information environment has been hit with an exogenous coverage shock.

4. Guidance Response to Exogenous Shocks to Information Environment

4.1 Baseline Models

Table 5, Panel A relates one particular form of voluntary disclosure – management forecasts of quarterly earnings – to coverage shocks dated from t = –4 to t = +4. Following Angrist (2001), column 1 is estimated as a linear probability model with firm fixed effects. An exogenous coverage shock has a positive and significant contemporaneous effect on the probability that a firm issues a forecast at $t = 0$ (p<0.001 when standard errors are clustered by firm). The economic magnitude is sizeable: firms suffering coverage shocks are 4.9 percentage points more likely to issue a forecast than are observably similar control firms. Relative to the pre-shock probability of 27.5% reported in Table 2, this represents an increase of 17.8% (0.049/0.275 – 1). This result is consistent with our conjecture that guiders respond to coverage shocks by disclosing more information and supports the validity of the proposed instrument.

The first three lags of the shock indicator are also positive and statistically significant (at the 0.013 level or better), suggesting that firms increase their voluntary disclosure for a period of one year following a coverage shock. The lagged coefficients are somewhat smaller than the contemporaneous effect of 0.049 but remain sizeable at 0.033 to 0.042. We find no evidence that firms anticipate future shocks, in view of the small and insignificant coefficients estimated for the leads. This supports KL's conclusion that the coverage shocks are exogenous. Among the firm characteristics, only size is significant; it has a positive effect on disclosure. Overall, the regression has good fit in light of the relatively high R^2 of 49.7%.

Column 2 shows what happens if instead of clustering the standard errors by firm, we cluster by calendar quarter. As in the diff-in-diffs shown in Table 3, where we also cluster by quarter, this helps to adjust for potential cross-sectional dependence due to time clustering of the 43 brokerage closures. The results are virtually identical: the p-value for contemporaneous shocks increases marginally from <0.001 to 0.004 while those for the first three lags range from 0.019 to 0.028.

Linear probability models are simple to estimate but sometimes generate coefficients that imply probabilities outside the unit interval. While the results in columns 1 and 2 suggest that this potential drawback does not apply here, column 3 checks if our results are robust to estimating a logit model with firm fixed effects instead. None of our conclusions is affected, so the remainder of the paper reports linear probability models whenever the dependent variable is binary.

As a further robustness check, column 4 models the *number* of forecasts management issues in a quarter, instead of the probability of a forecast being issued. Given the count nature of this dependent variable, we estimate a Poisson model with firm fixed effects. This too leaves our conclusions unaffected: firms respond to coverage shocks by increasing their disclosure, and they sustain increased disclosure for a period of four quarters.

To understand why firms step up disclosure when they lose analyst coverage, we briefly return to our alternative sample of firms that suffer an exogenous reduction in the number of market makers but no simultaneous reduction in analyst coverage. Both types of shock hurt liquidity, but we see a subsequent recovery only after coverage shocks. Column 5 of Table 5, Panel A shows that firms do *not* change their guidance behavior when losing a market-maker, despite the hit to liquidity. This is interesting for two reasons. First, the fact that liquidity does not subsequently recover hints at a causal effect of voluntary disclosure on liquidity. Second, the stark difference in firms' response to the two types of liquidity shock suggests that managers view them as distinct: liquidity shocks that result from an increase in information asymmetries among investors, in the wake of coverage terminations, can be cured (or at least mitigated) by voluntarily disclosing more information; liquidity shocks that result from changes in the mechanics of trading cannot.

These patterns are consistent with the view that managers consider guidance to be a substitute for externally produced information, such as analyst research, and suggest that the mechanism that brings about the liquidity reversals we observe in the data is management's deliberate attempts to mitigate the consequences of an exogenous increase in information asymmetry.

4.2 What Kind of Information Do Firms Disclose?

As we saw in Table 2, guidance can take a number of forms. Panel B of Table 5 relates the guidance measures introduced in Table 2 to coverage shocks and firm characteristics. Given the results of Panel A, we focus on shocks dated $t = -3$ to $t = 0$ and estimate linear probability models with firm, year, and fiscal-quarter fixed effects. Standard errors are clustered by firm as before. The first three columns focus on the guidance horizon and show that firms respond to coverage shocks by issuing forecasts of future earnings rather than pre-announcing imminent earnings. They are thus more likely to issue guidance earlier in the fiscal quarter, consistent with the view that pre-announcements are largely nondiscretionary (reflecting, for example, profit warnings).

The next two columns distinguish between quantitative and qualitative guidance. With the exception of a small and marginally significant coefficient for shocks at $t = -2$, we find no evidence that firms become more likely to issue qualitative guidance following coverage shocks. They do, however, become significantly more likely to issue quantitative guidance, and the effect is again economically large and sustained for a period of four quarters.

The final three columns consider the content of the guidance. In response to losing coverage, firms become 15.8% more likely to disclose negative news (p=0.002),[28] but they do not increase their disclosure of positive news, nor do they become more likely to release information that amounts to 'hot air'. That managers are more likely to disclose negative news is consistent with the findings of Hong, Lim, and Stein (2000), who conclude on p. 279 that "analyst coverage is especially important in propagating bad news." Along these lines, if managers wish to alleviate information asymmetry arising from coverage terminations, being forthcoming with negative news is likely to be a good substitute for lost coverage and also signals a strong commitment to continued disclosure, consistent with Clinch and Verrecchia (2011).

4.3 Which Firms Respond More Strongly?

In this section, we explore cross-sectional variation in firms' responses to coverage terminations. We first consider variation in the severity of the coverage shock by adding two indicator variables set equal to 1 if a firm suffered a "particularly severe" shock in quarters $t = 0$ or $t = -1$. We measure severity using the size of the diff-in-diff change in Amihud's (2002) illiquidity measure, ΔAIM, and consider different cut-offs for severity. In column 1 of Table 5, Panel C, we classify shocks resulting in above-median increases in illiquidity as more severe.

[28] This compares the coefficient of 0.029 in column 6 to the pre-shock likelihood of a treated firm releasing negative news (0.183 in Table 2).

The test shows that responses are concentrated among firms experiencing severe declines in liquidity. Firms suffering below-median shocks are nearly as likely as their controls to issue a forecast: the coefficient estimate for contemporaneous shocks is 0.008 with a *p*-value of 0.426. In contrast, firms suffering severe shocks are 5.1 percentage points more likely to issue a forecast than firms suffering below-median shocks and 5.9 percentage points more likely to do so than their controls (= 0.051 + 0.008). Both differences are highly statistically significant ($p<0.001$).

Column 2 repeats this exercise with a higher threshold, namely firms with ΔAIM in the top quartile of the distribution. The results are similar to those in column 1, except that the effect of non-severe shocks (those in the first three quartiles) becomes larger and statistically significant (presumably due to the experience of third-quartile firms, which are coded as suffering severe shocks in column 2 but not in column 3). In column 3, we code severe shocks as those leading to a ΔAIM that is significantly different from zero at the 5% level (in a two-tailed test). The estimates resemble those in the other specifications. Thus, regardless of how we measure severity, our results suggest that firms respond more strongly to more severe shocks to their liquidity.

Columns 4 through 7 consider firm characteristics that may influence the likelihood of a disclosure response. Prior work argues that firms that manage earnings are reluctant to provide voluntary disclosure (Dye (1988), Trueman and Titman (1988), Schipper (1989), and Jo and Kim (2007)). If so, we expect that earnings managers welcome the reduced attention that accompanies a coverage shock and, therefore, eschew a curative disclosure action. Consistent with this prediction, we find a lower likelihood of a disclosure response among firms that habitually meet or beat analyst expectations (column 4) or have particularly high discretionary accruals (column 5).

Column 6 reports a significantly smaller guidance response among stocks predominantly owned by institutional investors, while column 7 shows that managers respond more strongly to a coverage termination if their stock is covered by relatively few analysts to begin with.[29]

Panel D of Table 5 relates firms' disclosure responses to analyst characteristics. KL show that exogenous terminations involving 'local' analysts (those that are located close to a firm's headquarters) are associated with significantly larger price falls. This is consistent with prior evidence (e.g., Malloy (2005)) that local analysts provide more accurate earnings forecasts and thus more pricing-relevant signals. In columns 1 through 3, we investigate whether firms respond more strongly to the loss of a local analyst. Column 1 codes a local analyst as one located within a 50-mile radius of the firm's headquarters. Upon losing a distant analyst, firms are 3.1 percentage points more likely to issue a forecast than are their controls ($p<0.001$). Upon losing a local analyst, this likelihood increases by an additional 5.9 percentage points ($p=0.001$) for a total increase of 9 percentage points (=0.031+0.059) relative to the controls ($p<0.001$). Columns 2 and 3 expand the radius to 100 miles and 200 miles, respectively, which reduces the differential effect somewhat, to 5.2 and 3.1 percentage points, respectively (both significant at the 5% level or better). These results indicate that firms step up their disclosure by more after losing a more local analyst.

KL predict that closures of institutions-only brokers should have a smaller (or no) effect on share prices than closures of retail brokers and find this to be true. For our purposes, this should translate into a smaller (or no) guidance response following loss of an institutional analyst, as many institutions can either obtain the lost analyst signal from another broker or substitute for it in-house. Column 4 adds an indicator set equal to 1 if a firm loses an institutional analyst to capture the incremental effect relative to losing a retail analyst. The results confirm our prediction. Upon losing a retail analyst, firms are 6.1 percentage points more likely to issue guidance ($p<0.001$). In contrast, firms do not respond to the loss of an institutional analyst: the incremental effect is –4.9 percentage points, meaning they are only 1.2 percentage points more likely than their controls to issue guidance that quarter (=0.061–0.049). This is neither large economically nor

[29] We also find a negative correlations with firm size and forecast dispersion, though these are noisily estimated.

significant statistically (p=0.193).

These patterns suggest the possibility that firms target retail investors with their voluntary disclosures. They echo our earlier finding of stronger disclosure responses among firms with a retail-heavy shareholder base. Since the categorization into “retail” and “institutional” investors plausibly proxies for the “uninformed” and “informed” investors in an asymmetric-information model, this evidence also supports the interpretation that information asymmetry is an important channel through which managers influence liquidity.[30]

Column 5 reports how disclosure responds to terminations involving highly-rated analysts (those ranked all-stars by *Institutional Investor* magazine or master stock pickers by the Wall Street Journal). When losing an unrated analyst, firms are 3.1 percentage points more likely to issue guidance than their controls (p<0.001). When losing a highly-rated analyst, this likelihood increases by an additional 10.2 percentage points (p<0.001). The total response to an all-star’s termination of 13.3 percentage points is more than four times larger than the response to losing an unrated analyst.

Column 6 tests whether firms respond more to losing an analyst who provides a high degree of firm-specific information. To quantify this, we measure how an analyst contributes to a firm’s non-synchronicity with the market and its industry (see Roll (1988) and Roulstone and Piotroski (2004); Appendix A provides details on how we construct this measure). When non-synchronicity is high, stock returns may be interpreted as incorporating a relatively high degree of firm-specific (rather than aggregate or industry-wide) information. We find that disclosure responds nearly twice as strongly when losing an analyst who produces more firm-specific information (up by 5.6 percentage points) relative to losing an analyst who produces relatively more industry-specific information (up by 2.5 percentage points).

[30] KL find that exogenous coverage terminations induce retail investors to sell the stock and institutional investors to buy it. Stock purchases by informed investors, despite the loss of a public signal, is a unique prediction of asymmetric-information models and provides further support that voluntary disclosure responds to changes in information asymmetry.

The final characteristic we consider is the analyst's forecast accuracy relative to her peers (see Hong and Kubik (2003)). Column 7 shows that relative forecast accuracy makes no difference to firms' responses to coverage shocks.

4.4 Summary

The results in Table 5 confirm our conjecture that firms seek to mitigate shocks to their information environments by changing the amount of information they disclose voluntarily: consistent with the interpretation that they respond to changes in the distribution of information among investors, we find that firms increase voluntary disclosure in response to coverage shocks but not when their liquidity declines following the exogenous loss of a market-maker. These increases are persistent rather than short-lived and take the form of informative guidance.

From an identification point, these patterns are encouraging: essentially, the specifications in Table 5 represent the first stage in a two-stage model that instruments disclosure with coverage shocks to identify the causal effect of disclosure on liquidity. Identification requires a strong correlation between coverage shocks and disclosure, and Table 5 shows such a strong correlation. Before we can estimate the second stage, however, we need to verify that the data support the timeline that our identification strategy assumes. Table 5 implies that contemporaneous shocks affect disclosure, while Table 4 implies that contemporaneous shocks violate the exclusion restriction. Thus, we have no instrument for the contemporaneous effect of disclosure on liquidity. But we can potentially exploit lagged coverage shocks as an instrument for similarly lagged disclosure choices, as lagged coverage shocks have no apparent direct effect on liquidity. This approach will work as long as disclosure affects liquidity with a lag. We now test whether it does.

5. The Effect of Voluntary Disclosure on Liquidity and Firm Value

5.1 "Naïve" Relation Between Voluntary Disclosure and Liquidity

We begin by estimating "naïve" regressions of log *AIM* on disclosure. They are naïve in the sense of not attempting to correct for the endogeneity of disclosure. Column 1 of Table 6 includes indicator variables for contemporaneous and once-lagged management forecasts alongside firm characteristics and firm, year, and fiscal-quarter fixed effects. The coefficient estimate for contemporaneous guidance is negative but neither large, at –0.004, nor statistically different from zero (p=0.227). The estimate for lagged guidance of –0.010 (p=0.006) is nearly three times larger. This supports the timeline underpinning our identification strategy. Economically, the naïve effect is modest in size. A one standard deviation increase in the lagged propensity to guide is associated with only a 7.6% reduction in log *AIM*, holding all other covariates at their sample means.

The remaining eight columns of Table 6 regress log *AIM* on the first lag of the eight guidance measures introduced in Table 2. We find significant correlations between liquidity and each of the lagged forecasts, quantitative and qualitative guidance, and negative news, but not between liquidity and pre-announcements, positive news, or guidance that amounts to hot air.

Table 6 also reports tests for first-order serial correlation in the dependent variable using the modified Durbin-Watson test developed for panel-data models by Bhargava, Franzini, and Narendranathan (1982). The test fails to reject the null of no first-order serial correlation in *AIM* and thus supports our chosen regression specification.[31]

5.2 Causal Effect of Voluntary Disclosure on Liquidity

We can now test whether voluntary disclosure has a causal effect on liquidity. We do so by regressing liquidity in quarter t = +1 on the firm's disclosure choices dated t = 0, instrumented using coverage shocks dated t = –3 to t = 0. (See Table 5, Panel B for the corresponding first-stage estimates.) To recap, the key identifying assumption is that coverage shocks dated –3 to 0 do not affect liquidity in quarter t = +1 other than through their effect on the firm's guidance choice. (See Table 4 for the corresponding reduced-form estimates.)

Since identification requires that the instrument must correlate strongly with the endogenous variable, we focus on the four disclosure choices that, according to the first-stage estimates in

[31] Accordingly, our findings are robust to allowing for an AR(1) process in the dependent variable.

Table 5, Panel B, respond significantly to the instrument: management forecasts, forecasts or pre-announcements, quantitative guidance, and negative news. Staiger-Stock (1997) tests show that coverage shocks are strong instruments for each of these four disclosure choices.

Table 7 reports the results. Across the four specifications, we find that voluntary disclosure has a large and negative causal effect on a firm's subsequent illiquidity; that is, disclosure improves liquidity. The sign of this relation is the same as in the naïve regressions in Table 6, but the magnitude of the effect is between 7 and 12 times greater: the coefficient estimates vary from –0.075 for management forecasts to –0.148 for negative news. (Each coefficient is statistically significant, with *p*-values ranging from 0.034 for management forecasts to 0.001 for negative news.) The large difference between the IV coefficients in Table 7 and their naïve counterparts in Table 6 suggests that failure to control for the endogeneity of voluntary disclosure seriously biases estimates of the beneficial effect of disclosure on liquidity downwards.

5.3 Alternative Liquidity Measures

Goyenko, Holden, and Trzcinka (2009) review various liquidity proxies and find that Amihud's (2002) illiquidity measure is the best empirical measure of price impact. Table 8 explores robustness to using six other popular proxies to capture the causal effect of voluntary disclosure on liquidity, namely bid-ask spreads (Panel A), Goyenko, Holden, and Trzcinka's (2009) effective tick size (Panel B), Madhavan, Richardson, and Rooman's (1997) lambda, which measures the adverse selection component of the spread (Panel C), the fraction of zero-return days (Lesmond, Ogden, and Trzcinka (1999) (Panel D), Easley et al.'s (1996) probability of informed trading ("PIN") (Panel E), and Pastor and Stambaugh's (2003) gamma (Panel F). (For variable definitions, see Appendix A.) As before, we focus on the four guidance choices that respond to coverage shocks. The estimates in Table 8 show that our conclusions hold for each of these alternative liquidity measures.

In addition to establishing robustness, these tests shed light on the channel linking voluntary disclosure and liquidity. One of the measures, lambda, is designed to capture the permanent component of price impact, which is interpreted as the price response resulting from private information being impounded into prices. Panel C shows large and highly statistically significant effects of voluntary disclosure on liquidity as measured by lambda. This suggests that disclosure has an informational effect on liquidity, rather than a purely transaction-cost effect, and so supports an information-asymmetry interpretation of our results.

5.4 The Effect of Voluntary Disclosure on Cash Flow Uncertainty

To provide further evidence that disclosure affects liquidity through an information channel, we briefly examine earnings surprises. These are revealing about the information channel because they capture investors' uncertainty about cash flows without being affected by alternative drivers of liquidity, such as the number of market makers, tick size, or other microstructure features.

KL show that coverage shocks cause the remaining analysts to forecast earnings with greater error, consistent with the stock's information environment having deteriorated. We ask whether firms can mitigate this deterioration through increased voluntary disclosure. Using our IV strategy, we find that voluntary disclosure causes a significant decrease in future absolute earnings surprises, which supports our interpretation that increased disclosure enhances liquidity by improving a stock's information environment.[32]

5.5 The Effect of Voluntary Disclosure on Firm Value

While our primary focus is on liquidity, it is worth asking whether "talking up liquidity" pays off for firms in the sense of increasing their market value. KL show that firms suffer a hit to their market value when losing an analyst. Using a reduced-form regression analogous to those shown in Table 4, we confirm this result in our data (not tabulated). Specifically, the average firm's book-to-market ratio increases by 0.051 in the quarter of the shock (p<0.001). In the next quarter, however, the average firm recovers around half of the initial value reduction (p<0.001). These

[32] Results are available on request.

dynamics mirror the liquidity reversals shown in Table 4. To see if they are caused by a disclosure response, we estimate a version of our Table 7 two-stage model with book-to-market as the dependent variable. Specifically, we estimate the causal effect of voluntary disclosure at $t = 0$ on firm value at $t = +1$ using exogenous shocks to analyst coverage as an instrument for disclosure.

Using the same four measures of disclosure as before, Table 9 shows that increased disclosure results in a significant subsequent recovery. The IV coefficients imply that a one standard deviation increase in the probability of voluntary disclosure in response to a coverage shock is associated with a subsequent reduction in the book-to-market ratio of between –0.017 and –0.044. The average book-to-market ratio in our sample is 0.540, so disclosure responses produce percentage changes in book-to-market ratios ranging from –2.7% to –8.2%, all else equal.

6. Conclusions

Our tests exploit a natural experiment, first explored in Kelly and Ljungqvist (2012), that amounts to a plausibly exogenous shock to a stock's information environment. This allows us not only to investigate how firms respond to such shocks but also to establish how firms' responses in turn affect liquidity or value. The exogenous nature of the shocks allows us to instrument firms' responses and thus to establish a causal link between disclosure and liquidity or value.

We have three main results. First, we show that firms respond to shocks to their information environments by stepping up disclosure. Second, firms respond only to the loss of coverage by a retail analyst and not when they lose coverage by an analyst serving institutional investors. This novel empirical finding is consistent with the notion that voluntary disclosure is motivated by a desire to communicate with retail investors. Since retail investors are often thought of as being at an informational disadvantage relative to institutions, firms appear to view disclosure as a way to reduce information asymmetries among investors. Theoretical asset pricing models show that reducing information asymmetries leads to a lower cost of capital and higher share prices. The recovery in firm value that we see in the data is consistent with this channel.

Third, we show that voluntary disclosure has a large and beneficial effect on both liquidity and firm value and that these effects are plausibly causal. This result provides a justification to firms for voluntarily disclosing more information than is mandated by law and regulations, despite the potential for shareholder lawsuits claiming ‘inaccurate’ or ‘misleading’ disclosure. It also suggests that managers can, at least to some extent, shape the liquidity of their firms’ stock.

The dynamic pattern in liquidity that we exploit for identification is consistent with the prediction of a recent model of managers’ incentives to commit to voluntary disclosure. Clinch and Verrecchia (2011) show that increases in information asymmetry can have two countervailing effects, namely the application of a higher discount rate by investors and an increase in disclosure in response. Clinch and Verrecchia predict a negative contemporaneous relation between disclosure and liquidity that subsequently reverses if and when investors believe that managers are committed to continuing the increased disclosure. Our results support this prediction.

Our results also speak to the question whether management disclosure and research analyst coverage are substitutes or complements. Reduced-form regressions come to the conclusion that they are complements, since firms with a large analyst following appear to engage more intensely in voluntary disclosure (e.g., Lang and Lundholm (1996)). If, as is likely, both management and analysts produce more information as a response to uninformed investor demand for information, a positive reduced-form correlation between disclosure and coverage would arise even if these signals were substitutes—a bias resulting from failure to account for endogeneity. Our identification strategy, which in effect captures exogenous shocks to uninformed investor demand for information, disentangles supply and demand to reveal that management disclosure and analyst coverage are in fact substitutes: when an analyst randomly drops coverage, management steps in to replace the lost signal. This, in turn, improves the liquidity of their firms’ stock.

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Appendix A. Variable Definitions.

Liquidity measures

log AIM is the quarterly average of the natural log of 1 plus Amihud's (2002) illiquidity measure (*AIM*). It is constructed as follows. Following Amihud, we use daily CRSP data (CRSP variables *ret*, *prc*, and *vol*) to calculate the ratio of absolute stock return to dollar volume [1,000,000∗|*ret*|÷(|*prc*|∗*vol*)] for each day in a fiscal quarter. We then average these daily *AIM* over the fiscal quarter and take logs.

Bid-ask spread is the quarterly average of a firm's daily bid-ask spread,. We use daily closing bid and ask data from CRSP (variables *ask* and *bid*) to calculate 100*(*ask*–*bid*)/[(*ask*+*bid*)/2]. We then average these daily bid-ask spreads over the fiscal quarter. Observations with crossed quotes (negative spreads) are excluded.

Effective tick is the quarterly average of Goyenko, Holden, and Trzcinka's (2009) effective tick measure, constructed using code supplied by Ruslan Goyenko. Using daily CRSP data (CRSP variables *prc* and *vol*) and based on end-of-day price clustering, we calculate an average effective spread over the quarter as the probability-weighted average of each effective spread size deflated by price.

Lambda (λ) is the quarterly average of the probability that a trade occurs inside the bid-ask quotes. It is constructed using intra-day data following Madhavan, Richardson, and Roomans (1997) and Armstrong et al. (2011). Specifically, we gather trade-by-trade and quote data from the Institute for the Study of Security Markets (ISSM) and the Trades and Automated Quotes (TAQ) database provided by the NYSE. We use the Lee and Ready (1991) algorithm with a five-second lag to determine the buy/sell direction of a trade. We then estimate λ using the following firm-specific regression: $\Delta p_t/p_{t-1} = \psi\,\Delta D_t + \lambda\,(D_t - \rho D_{t-1}) + u_t$, where p_t is the transaction price, D_t is the sign of trade (+1 if buy, –1 if sell), and ρ is the AR(1) coefficient for D_t. As discussed in Armstrong et al. (p. 38), deflating Δp_t by lagged price p_{t-1} yields an estimate of λ as a percentage of price and so ensures cross-sectional comparability.

Fraction zero-return days is the fraction of trading days with zero returns in a quarter. Following Lesmond, Ogden, and Trzcinka (1999) and Goyenko, Holden, and Trzcinka (2009), we use daily CRSP data (variables *ret* and *vol*) to calculate the fraction of trading days with *vol* > 0 and *ret* = 0 during the fiscal quarter.

PIN is the stock-by-stock quarterly average of Venter and De Jongh's (2006) PIN measure. As discussed in Brown and Hillegeist (2007), the Venter-De Jongh model allows for a strong positive correlation between the daily number of buys and sells typically observed in the data. We obtain quarterly values of this measure from (http://janssenbrown.net/StephenBrownresearch/index.html). These data are available only from 2003 onwards.

Gamma is the quarterly average of Pastor and Stambaugh's (2003) price impact measure. Using daily CRSP data, we estimate the following regression each month: $r^e_{t+1} = \theta + \phi r_t + (Gamma)sign(r^e_t)(Volume_t) + \varepsilon_t$, where r^e_t is the stock's excess return above the CRSP value-weighted market return on day t and $Volume_t$ is the dollar volume on day t. We then average the value of gamma obtained from this monthly regression over the fiscal quarter.

Cash flow uncertainty measure

Earnings surprise is the quarterly difference between actual earnings per share and the prevailing median analyst estimate (taken from the unadjusted detail files maintained by I/B/E/S), deflated by book value per share (i.e., Compustat item *ceqq* divided by Compustat item *cshoq*).

Disclosure measures

Forecast/pre-announcement is an indicator variable set equal to 1 if the firm provides earnings guidance (in the form of a forecast or a pre-announcement) in the fiscal quarter. We obtain guidance data from the Company Issued Guidelines (CIG) files of the First Call Historical Database (FCHD) maintained by Thomson Reuters. We limit analysis to quarterly forecasts and pre-announcements of earnings per share (*periodicity* = 'Q' and *data_type* = 'EPS') for the firm's common stock (*security_type* = 'COM'). Following Anilowski, Feng, and Skinner (2007), we remove observations with missing earnings announcement dates (*actdate*) and those with guidance dates (*anndate*) occurring on or after the actual earnings announcement date.

Forecast is an indicator variable set equal to 1 if management supplies earnings guidance prior to the end of the fiscal period (*anndate* < *fpe*).

Pre-announcement is an indicator variable set equal to 1 if management supplies earnings guidance after the end of the fiscal period but before the formal announcement of earnings (*fpe* < *anndate*).

Quantitative guidance is an indicator variable set equal to 1 if the earnings guidance contains a numerical estimate.

Qualitative guidance is an indicator variable set equal to 1 if the earnings guidance contains no numerical estimate.

For quantitative guidance, following Anilowski, Feng, and Skinner (2007), we code three additional variables relating to its content. To do so, we obtain analyst forecast data from the Summary Statistics files of the FCHD. This dataset supplies the prevailing consensus estimate on any given day. To ensure that the guidance does not influence our measure of the prevailing consensus, we measure consensus as the median analyst forecast of earnings per share as of the day before the guidance date (*anndate* – 1).

Negative news is an indicator variable set equal to 1 if management supplies an earnings estimate that falls below the prevailing consensus (i.e., the median analyst forecast) as of the day before the guidance date (*anndate* – 1).

Positive news is an indicator variable set equal to 1 if management supplies an earnings estimate that exceeds the prevailing consensus (i.e., the median analyst forecast) as of the day before the guidance date (*anndate* – 1).

Hot air is an indicator variable set equal to 1 if management supplies an earnings estimate that equals the prevailing consensus (i.e., the median analyst forecast) as of the day before the guidance date (*anndate* – 1).

Severity of shock measures

Above-median shock is an indicator variable set equal to 1 if the firm suffers an above-median shock to their liquidity, as measured by diff-in-diff changes in Amihud's (2002) illiquidity measure, ΔAIM.

Top-quartile shock is an indicator variable set equal to 1 if the firm suffers a shock to their liquidity (as measured by diff-in-diff changes in Amihud's (2002) illiquidity measure, ΔAIM) that places them in the top quartile of the distribution.

Significant shock is an indicator variable set equal to 1 if the firm suffers a shock to their liquidity (as measured by diff-in-diff changes in Amihud's (2002) illiquidity measure, ΔAIM) that is significantly different from zero at the 5% level (in a two-tailed test).

Firm characteristics

Market capitalization is defined as the fiscal quarter-end share price (CRSP variable *prc*) times the number of shares outstanding (CRSP variable *shrout*).

Book-to-Market is measured as the ratio of a firm's book value of equity (Compustat item *ceqq*) to its market value (Compustat item *prccq* multiplied by Compustat item *cshoq*).

 # analysts providing coverage is the maximum number of different analysts providing a forecast of earnings per share in the 90 days prior to the earnings announcement date (I/B/E/S variable *anndats*) in a given fiscal quarter, taken from the unadjusted detail files maintained by I/B/E/S.

Volatility is the standard deviation of daily returns (obtained from CRSP).

Net equity issuance is an indicator variable set equal to 1 if the firm's net equity issues in the fiscal year are positive. Following Frank and Goyal (2003), we calculate net equity issues as sales of common and preferred stock (Compustat item *sstk*) minus purchases of common and preferred stock (Compustat item *prstkc*).

Habitually meets-or-beats is an indicator variable set equal to 1 if the firm habitually meets-or-beats analyst expectations, which prior work indicates signals the presence of earnings management (Bartov, Givoly and Hayn (2002)). Specifically, we set this indicator equal to 1 if the firm has met or exceeded analyst expectations in at least 15 out of the prior 20 quarters.

High discretionary accruals is an indicator variable set equal to 1 if the firm's quarterly discretionary accruals exceed the median. Following Collins, Pungaliya, and Vijh (2012), we estimate quarterly discretionary accruals (as a percentage of lagged assets) using a cross-sectional version of the modified Jones model that controls for performance and firm growth.

High institutional ownership is an indicator variable set equal to 1 if the fraction of the firm's outstanding shares that are held by institutional investors filing 13f reports (according to Thomson Reuters) exceeds the median.

Low analyst coverage is an indicator variable set equal to 1 if the firm is covered by three or fewer analysts.

Analyst characteristics

Distance is the geographic distance between the locations of each analyst-company pair using the Haversine formula. Locations are coded by Zip codes that are translated into longitudes and latitudes using the *Census 2000 U.S. Gazeteer* (http://www.census.gov/tiger/tms/gazetteer/zcta5.txt). Analyst locations as of the brokerage closure date come from Nelson's *Directory of Investment Research* or, where unavailable, the analyst's telephone number as per his or her research reports in the Investext archive. Headquarter locations come from the firm's most recent 10-Q and 10-K filing before the termination date (not from Compustat, as Compustat's location information reflects a firm's location as of the download date).

Local analyst is an indicator variable set equal to 1 if the coverage shock involves the loss of an analyst located within a 50-mile, 100-mile, or 200-mile radius of the firm's headquarters.

Institutions-only broker is an indicator variable set equal to 1 if the coverage shock stems from the closure of an institutions-only brokerage house. To identify broker type, we rely to a large extent on the broker's 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.

Highly ranked analyst is an indicator variable set equal to 1 if the analyst is rated either as an 'all-star' in the most recent *Institutional Investor* survey before the termination or a 'master stock picker' in the most recent '*Best on the Street*' rankings produced by the *Wall Street Journal*.

Analyst firm-specific information production is constructed as follows. Using all stocks covered in the I/B/E/S recommendations database, we construct a three-way panel that tracks each stock's monthly non-synchronicity as well as the identity of all analysts covering each stock in each month. Non-synchronicity is defined as $\ln(R^2/(1-R^2))$, where R^2 comes from the following regression of stock *i*'s return on the market return and the return on the industry portfolio to which *i* belongs:

$$R_{i,t} = a_i + b_i R_{m,t} + c_i R_{ind,i,t} + e_{i,t}.$$

(The panel regression includes three further control variables: firms' market capitalization, book-to-market ratio, and lagged monthly non-synchronicity.) Since analysts add and delete coverage of stocks over time, we use both cross-sectional and time variation in analysts' coverage sets to infer an analyst's contribution of firm-specific information to a stock's estimated non-synchronicity. We include analyst fixed effects to capture the average impact each analyst has on the information environment of the stocks that she covers, and use the estimated fixed effect to proxy for an analyst's firm-specific information provision in the column 6 of Table 5D.

Relative forecast accuracy is an indicator variable set equal to 1 for analysts with above-median measures of relative forecast accuracy (as compared to the accuracy of their peers). We measure relative forecast accuracy following Hong and Kubik (2003). We begin by ranking the forecast errors made by each analyst for a given firm within the firm-year. Next, we convert the rank into a score that corrects for the number of analysts covering the firm that year. Finally, we average this score over the last three years for a given firm and analyst.

Figure 1. Average Daily Amihud Illiquidity Measure, All CRSP Firms, 1999-2010.
The figure shows the value-weighted average daily log Amihud illiquidity measure for the universe of firms in CRSP for the period from 1999 to 2010.

Figure 2. Overview of First Call's Guidance Data.
The top left graph shows the quarterly number of pieces of quarterly guidance issued voluntarily by management according to the 'Company Issued Guidelines' files of the First Call Historical Database. The other three graphs break down the guidance by horizon (top right), form (bottom left), and content (bottom right).

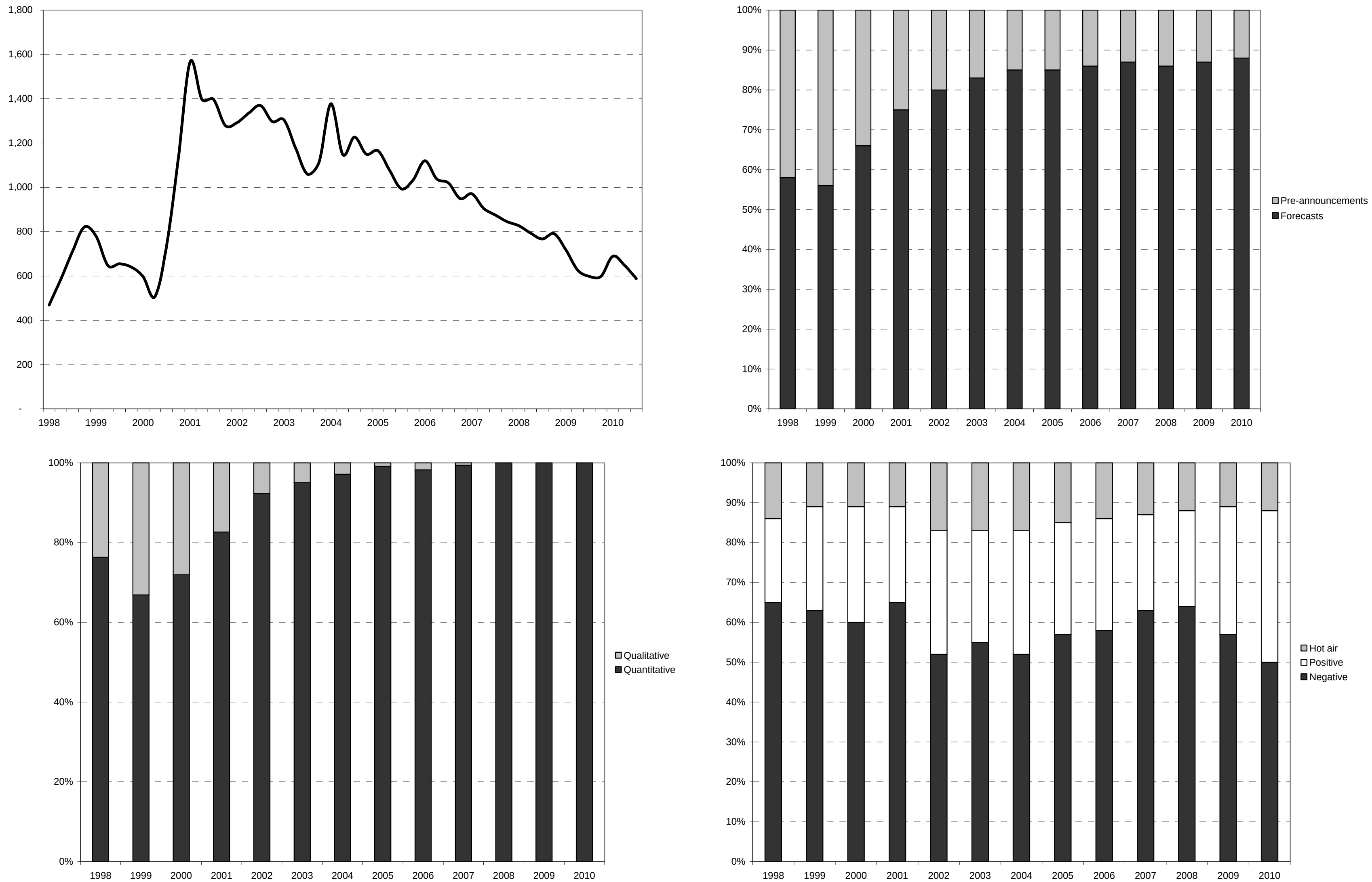

Table 1. Construction of Treatment Sample.

KL identify a sample of 4,429 exogenous coverage terminations affecting 2,180 unique firms as a result of 43 brokerage closures over the period Q2, 2000 through Q1, 2008. Their unit of analysis is a firm-day. We focus instead on firm-fiscal quarters. Some firms are hit with multiple exogenous coverage terminations in a given fiscal quarter, leaving 4,185 firm-fiscal quarters with one or more termination events. We filter out 1,922 firm-fiscal quarters as set out below, and lose 168 firm-fiscal quarters for which no valid match can be found in a nearest-neighbor propensity score match using a 0.005 caliper. This yields a final treatment sample consisting of 2,095 firm-fiscal quarters with one or more exogenous coverage termination events for 1,468 unique firms.

		Total	*Number of terminations per firm-fiscal quarter*		
			1	*2*	*3*
Kelly and Ljungqvist (2012):					
# unique treated firms (by Compustat *gvkey*)		2,180			
# coverage termination events		4,429	*3,948*	*460*	*21*
# firm-fiscal quarters w/ one or more termination events		4,185			
less: not yet guiders as of termination quarter	-1,122				
less: serially shocked firms	-794				
less: not traded in fiscal quarter before termination	-4				
less: lose coverage in firm's last fiscal quarter of listing	-2				
Treated firm-fiscal quarters eligible for matching		2,263			
less: no valid match with 0.005 caliper	-168				
Final treatment sample:					
# firm-fiscal quarters w/ one or more termination events		2,095			
# coverage termination events		2,821	*2,561*	*236*	*24*
# unique treated firms (by Compustat *gvkey*)		1,468			

Table 2. Descriptive Statistics.

The sample consists of 2,095 firm-quarters during which a firm suffers an exogenous coverage termination ('treated firms') and 2,095 firm-quarters for 'control firms.' Treated and control firms are matched on market cap, volatility, the number of analysts providing coverage, and the Amihud illiquidity measure (*AIM*), all measured as of the fiscal quarter before the coverage termination. The match is performed using a nearest-neighbor propensity score match with a 0.005 caliper. The table reports the firm characteristics we match on and the resulting propensity scores (Panel A) as well as a variety of voluntary management guidance measures (Panel B). All variables are measured as of the time of the match, i.e., the fiscal quarter before the coverage termination. We tabulate means, medians, and standard deviations for continuous variables and fractions for indicator variables. We also report pairwise differences in means, fractions, and medians between treated and control firms. To estimate statistical significance, we use *t*-tests (for means and fractions) and Pearson χ^2 tests (for medians). None of the differences between treated and control firms is significant at the 10% level or better. For variable definitions and details of their construction, see Appendix A.

	Treated firms			Control firms			Treated – controls	
	Mean or fraction	Std. dev.	Median	Mean or fraction	Std. dev.	Median	Difference in means/ fractions	Difference in medians
Panel A: Firm characteristics								
Matching variables:								
Market capitalization ($m)	**7,860.8**	22,100.0	1,486.9	**7,919.1**	22,700.0	1,564.1	-58.3	-77.2
# analysts providing coverage	**6.9**	5.8	5.0	**7.0**	5.9	6.0	-0.1	-1.0
Volatility	**0.032**	0.022	0.026	**0.033**	0.029	0.026	-0.001	0
log *AIM*	**0.049**	0.256	0.002	**0.049**	0.299	0.002	0	0
Propensity score	**0.164**	0.156	0.110	**0.164**	0.157	0.110	0	0
Panel B: Management guidance								
Guidance horizon:								
Forecast or pre-announcement	**0.334**			**0.319**			0.015	
Forecast	**0.275**			**0.267**			0.008	
Pre-announcement	**0.094**			**0.083**			0.011	
Form of guidance:								
Quantitative guidance	**0.304**			**0.299**			0.004	
Qualitative guidance	**0.048**			**0.038**			0.010	
Content of guidance:								
Negative news	**0.183**			**0.176**			0.008	
Positive news	**0.092**			**0.096**			-0.004	
'Hot air'	**0.069**			**0.068**			0.001	

Table 3. The Effect of Coverage Shocks on Liquidity: Diff-in-Diff Test.

The table measures the impact of an exogenous coverage termination on a stock's liquidity using a difference-in-differences test. The exogenous coverage terminations occurred as a result of 43 brokerage closures over the period Q2, 2000 through Q1, 2008. The sample consists of 2,095 treated firm-quarters and 2,095 control firm-quarters. Treated and control firms are matched on market capitalization, volatility, the number of analysts providing coverage, and *AIM*, all measured as of the fiscal quarter before the coverage termination. Liquidity is measured using the log Amihud illiquidity measure (*AIM*). We report average levels of log *AIM* in the fiscal quarter before a coverage termination (labeled 'before') and in the following quarter (labeled 'after'). We also report within-firm changes (labeled 'difference') and between-firm differences (labeled 'treated – controls'). The change in the between-firm differences is the difference-in-differences. Standard errors, reported in italics underneath the averages, are obtained from a bootstrap stratified by quarter with 1,000 replications. The bootstrap adjusts for potential cross-sectional dependence due to overlapping estimation windows caused by time clustering as multiple stocks are terminated in each of the 43 brokerage-firm closures. *** and ** denote significance at the 1% and 5% level (two-sided), respectively. In the last row, we report within-firm differences in % of the pre-shock level of log *AIM* as a measure of the economic significance of coverage shocks. Note that this statistic is estimated per firm and then averaged across firms; it is not the relative change in the reported averages.

	Treated firms	Control firms	Treated – Controls
Before	0.049	0.049	0.000
	0.006	*0.006*	*0.008*
After	0.073	0.063	0.010
	0.007	*0.007*	*0.010*
Difference	0.024***	0.014***	0.010**
	0.003	*0.003*	*0.004*
in % of before	*43.3%*	*34.8%*	

Table 4. The Effect of Coverage Shocks on Liquidity: Regression Results.

This table reports results from firm-level panel regressions of the log of one plus Amihud's (2002) illiquidity measure on indicators capturing exogenous analyst coverage terminations (columns 1, 2, 3, and 6), coverage terminations that coincide with exogenous reductions in market making (column 4), or exogenous reductions in market making unaccompanied by changes in analyst coverage (column 5). Coverage shocks are coded as indicators set equal to 1 if a firm suffers one or more exogenous coverage terminations during that fiscal quarter. In column 4, the coverage shocks are interacted with an indicator identifying a subset of 318 instances in which a firm simultaneously loses analyst coverage and market-making in the wake of a brokerage closure. For variable definitions and details of their construction, see Appendix A. The unit of observation is a firm-fiscal-quarter. All specifications are estimated using OLS with firm fixed effects, year effects, and a set of fiscal-quarter fixed effects. Standard errors are shown in italics underneath the coefficient estimates. In all columns except column 2, they are clustered at the firm level. In column 2, standard errors are obtained from a bootstrap stratified by quarter with 1,000 replications. *** and ** denote significance at the 1% and 5% level (two-sided), respectively. The number of observations in columns 1 and 2 is 34,256 firm-fiscal quarters for 2,095 treated and 2,095 matched control firm-quarters. Due to the presence of lags, the number of observations in columns 3 and 4 is 31,312 firm-fiscal quarters. Columns 5 and 6 use different samples. Column 5 uses a sample composed of 26,406 firm-fiscal quarters for 1,738 firms with a guidance history that suffer an exogenous reduction in the number of market makers without also suffering a reduction in analyst coverage and 1,738 matched control firms. In column 6, we use a sample 9,840 firm-fiscal quarters for 769 'non-guiders' losing analyst coverage and 769 matched control firms.

	Dependent variable: log Amihud Illiquidity Measure					
	Estimation sample: Guiders					Non-guiders
	(1)	(2)	(3)	(4)	(5)	(6)
Exogenous coverage shock						
at $t = 0$	0.008***	0.008***	0.007***	0.008***		0.021**
	0.002	*0.003*	*0.003*	*0.003*		*0.009*
at $t = -1$			-0.009***	-0.010***		-0.006
			0.003	*0.003*		*0.008*
at $t = -2$			-0.002	-0.002		0.003
			0.003	*0.003*		*0.009*
at $t = -3$			0.004	0.001		-0.008
			0.003	*0.003*		*0.007*
… and market-making shock						
at $t = 0$				-0.003	0.038***	
				0.007	*0.000*	
at $t = -1$				0.005	0.026***	
				0.007	*0.000*	
at $t = -2$				0.001	0.017**	
				0.010	*0.024*	
at $t = -3$				0.026*	0.009	
				0.014	*0.278*	
Firm characteristics						
log market cap at $t = -1$	-0.103***	-0.103***	-0.103***	-0.103***	-0.172***	-0.153***
	0.008	*0.005*	*0.008*	*0.008*	*0.000*	*0.020*
log coverage at $t = -1$	-0.008***	-0.008***	-0.007***	-0.007***	-0.003	-0.010**
	0.002	*0.001*	*0.002*	*0.002*	*0.342*	*0.004*
volatility at $t = -1$	0.188	0.188	0.095	0.095	-0.592***	0.130
	0.152	*0.151*	*0.145*	*0.147*	*0.000*	*0.338*
=1 if firm is net eq. issuer at t=–1	-0.019***	-0.019***	-0.019***	-0.019***	-0.032***	-0.036*
	0.005	*0.003*	*0.005*	*0.005*	*0.004*	*0.019*
Diagnostics						
Within-firm R^2	9.3%	9.3%	8.9%	8.9%	5.7%	9.8%
Wald test: all coeff. = 0	13.5***	29.0***	11.9***	9.4***	34.0***	5.0***
Number of episodes	4,190	4,190	4,190	4,190	3,476	1,538
Number of observations	34,256	34,256	31,312	31,312	26,406	9,840

Table 5, Panel A: The Effect of Coverage Shocks on Voluntary Disclosure.

This table reports results from firm-level panel regressions of one particular form of guidance, management forecasts, on an indicator capturing exogenous analyst coverage terminations (in columns 1 to 4) or exogenous reductions in market making unaccompanied by changes in analyst coverage (column 5). We include four leads (to test for the exogeneity of the shocks) and four lags (to allow for persistence in firms' guidance responses to the shocks). For definitions of the control variables and details of their construction, see Appendix A. Columns 1, 2, and 5 are estimated as linear probability models; column 3 is estimated as a conditional (fixed-effect) logit; and column 4 is estimated as a Poisson count model. The unit of observation is a firm-fiscal-quarter. All specifications include firm fixed effects, year effects, and a set of fiscal-quarter fixed effects. The fixed effects are not shown for brevity. Heteroskedasticity-consistent standard errors are shown in italics underneath the coefficient estimates. They are clustered by firm in columns 1, 3, and 5, and by calendar quarter in column 2. Fixed-effects Poisson models do not permit clustering and so the standard errors in column 4 are White. *** and ** denote significance at the 1% and 5% level (two-sided), respectively. The number of observations in columns 1 to 4 is 34,010 firm-fiscal quarters for 2,095 treated and 2,095 matched control firm-quarters. Column 5 uses a different sample, composed of 26,406 firm-fiscal quarters for 1,738 firms with a guidance history that suffer an exogenous reduction in the number of market makers without at the same time suffering a reduction in analyst coverage and 1,738 matched control firms.

		Dependent variable:				
		=1 if management issues forecast			Number of forecasts	=1 if mgt issues forecast
		(1)	(2)	(3)	(4)	(5)
Exogenous coverage shock						
Leads:	shock at t = +4	0.013	0.013	0.081	0.031	0.001
		0.014	*0.017*	*0.112*	*0.034*	*0.959*
	shock at t = +3	0.005	0.005	0.022	-0.002	-0.009
		0.014	*0.019*	*0.112*	*0.035*	*0.330*
	shock at t = +2	-0.008	-0.008	-0.126	-0.049	-0.002
		0.013	*0.015*	*0.108*	*0.035*	*0.800*
	shock at t = +1	0.019	0.019	0.150	0.035	0.013
		0.013	*0.020*	*0.104*	*0.033*	*0.169*
Contemporaneous:	shock at t = 0	0.049***	0.049***	0.432***	0.132***	0.013
		0.013	*0.016*	*0.100*	*0.032*	*0.166*
Lags:	shock at t = –1	0.033**	0.033**	0.278***	0.081***	0.012
		0.013	*0.013*	*0.103*	*0.031*	*0.163*
	shock at t = –2	0.042***	0.042**	0.321***	0.107***	0.008
		0.013	*0.018*	*0.097*	*0.030*	*0.360*
	shock at t = –3	0.036***	0.036**	0.252***	0.076***	-0.001
		0.013	*0.016*	*0.097*	*0.029*	*0.900*
	shock at t = –4	0.016	0.016	0.097	0.029	-0.004
		0.013	*0.016*	*0.100*	*0.031*	*0.651*
Firm characteristics						
log market cap at t = –1		0.064***	0.064***	0.625***	0.291***	0.054***
		0.007	*0.009*	*0.063*	*0.025*	*0.000*
log coverage at t = –1		0.001	0.001	0.012	0.001	0.001
		0.004	*0.005*	*0.035*	*0.011*	*0.905*
volatility at t = –1		-0.195	-0.195	-0.024	-0.016**	-0.002
		0.158	*0.183*	*0.015*	*0.007*	*0.002*
=1 if firm is net eq. issuer at t = –1		0.000	0.000	-0.006	-0.004	-0.019**
		0.009	*0.007*	*0.082*	*0.026*	*0.035*
Diagnostics						
R^2		49.7%	49.7%	4.4%	n.a.	59.9%
Wald test: all coefficients = 0		10.3***	51.5***	12.4***	18.3***	4.3***
Estimation:		OLS/FE	OLS/FE	Logit/FE	Poisson/FE	OLS/FE
Standard errors clustered on:		gvkey	quarter	gvkey	robust	gvkey

Table 5, Panel B: The Effect of Coverage Shocks on Voluntary Disclosure by Type of Guidance.

This table relates eight separate measures of voluntary guidance to an indicator capturing contemporaneous exogenous coverage terminations along with three lags. For variable definitions and details of their construction, see Appendix A. All specifications are estimated as linear probability models with firm fixed effects, year effects, and a set of fiscal-quarter fixed effects. The fixed effects are not shown for brevity. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. *** and ** denote significance at the 1% and 5% level (two-sided), respectively. The number of observations in each specification is 31,312 firm-fiscal quarters for 2,095 treated and 2,095 matched control firm-quarters.

	Guidance horizon			Form of guidance		Content of guidance		
Dependent variable = 1 if management issues …	Forecast	Pre-announce-ment	Both	Quanti-tative guidance	Quali-tative guidance	Negative guidance	Positive guidance	Hot air guidance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exogenous coverage shock								
at $t = 0$	0.041***	0.002	0.036***	0.034***	0.011	0.029***	0.013	0.007
	0.009	*0.007*	*0.009*	*0.010*	*0.006*	*0.009*	*0.007*	*0.006*
at $t = –1$	0.024***	-0.001	0.020**	0.020**	0.010	0.014	0.008	-0.003
	0.009	*0.006*	*0.009*	*0.009*	*0.006*	*0.009*	*0.007*	*0.007*
at $t = –2$	0.036***	-0.003	0.026***	0.019**	0.010**	0.023**	-0.001	0.016**
	0.009	*0.007*	*0.009*	*0.009*	*0.005*	*0.009*	*0.007*	*0.007*
at $t = –3$	0.033***	0.005	0.032***	0.025***	0.008	0.008	0.017**	0.009
	0.009	*0.008*	*0.010*	*0.010*	*0.006*	*0.009*	*0.008*	*0.007*
Firm characteristics								
log market cap at $t = –1$	0.059***	-0.001	0.057***	0.048***	0.011***	0.038***	0.006	0.013***
	0.007	*0.006*	*0.008*	*0.007*	*0.004*	*0.006*	*0.005*	*0.004*
log coverage at $t = –1$	0.000	0.000	0.002	0.009	-0.007**	0.004	0.003	0.004
	0.004	*0.004*	*0.005*	*0.005*	*0.003*	*0.005*	*0.004*	*0.003*
volatility at $t = –1$	-0.252	-0.236	-0.359**	-0.382**	-0.053	-0.352**	-0.110	-0.091
	0.150	*0.130*	*0.177*	*0.165*	*0.100*	*0.138*	*0.118*	*0.086*
=1 if firm is net equity issuer at $t = –1$	0.000	0.003	-0.001	-0.005	0.002	-0.029***	0.018***	0.013**
	0.009	*0.006*	*0.009*	*0.009*	*0.004*	*0.009*	*0.007*	*0.006*
Diagnostics								
R^2	49.9%	20.1%	45.5%	46.7%	12.1%	28.7%	16.8%	21.6%
Wald test: all coeff. = 0	14.2***	3.3***	12.3***	10.9***	4.8***	10.3***	2.5***	4.6***

Table 5, Panel C: The Effect of Severe Coverage Shocks and Firm Characteristics on Disclosure.

This table explores cross-sectional variation in firms' guidance responses to exogenous coverage shocks. Coverage shocks are coded as indicators set equal to 1 if a firm suffers one or more exogenous coverage termination that fiscal quarter and additional indicators are included that capture firms that suffer particularly 'severe' shocks (columns 1 through 3), manage earnings (columns 4 and 5), have above-median institutional holdings (column 6), or are covered by three or fewer analysts (column 7). Severity is measured using ΔAIM, the change in a firm's Amihud illiquidity measure around the coverage shock relative to the contemporaneous change experienced by its matched control. For variable definitions and details of their construction, see Appendix A. All specifications are estimated as linear probability models with firm and year effects and a set of fiscal-quarter fixed effects (not shown for brevity). Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively. The number of observations in each specification is 31,312 firm-fiscal quarters for 2,095 treated and 2,095 matched control firm-quarters.

	Dependent variable = 1 if management issues forecast						
	Severity of shock			Earnings management			
	above median	top quartile	significant at 5% level	habitually meets-or-beats	high discretionary accruals	High institutional holdings	Low analyst coverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Exogenous coverage shock							
at $t = 0$	0.008	0.026***	0.028***	0.049***	0.051***	0.058***	0.032***
	0.010	*0.008*	*0.008*	*0.009*	*0.012*	*0.011*	*0.009*
at $t = -1$	-0.002	0.013	0.019**	0.028***	0.030***	0.026**	0.020**
	0.011	*0.009*	*0.008*	*0.008*	*0.011*	*0.010*	*0.008*
at $t = -2$	0.027***	0.027***	0.027***	0.036***	0.029***	0.035***	0.036***
	0.008	*0.008*	*0.008*	*0.007*	*0.009*	*0.008*	*0.007*
at $t = -3$	0.032***	0.032***	0.032***	0.033***	0.028***	0.033***	0.033***
	0.008	*0.008*	*0.008*	*0.008*	*0.009*	*0.008*	*0.008*
=1 if severe shocks, earnings managers, predominantly owned by institutions, or covered by few analysts							
at $t = 0$	0.051***	0.037**	0.038**	-0.030**	-0.037**	-0.034**	0.032**
	0.014	*0.017*	*0.019*	*0.015*	*0.015*	*0.015*	*0.015*
at $t = -1$	0.042***	0.027	0.006	-0.012	-0.016	-0.008	0.015
	0.014	*0.017*	*0.019*	*0.015*	*0.015*	*0.014*	*0.015*
Firm characteristics							
log market cap at $t = -1$	0.058***	0.058***	0.057***	0.059***	0.065***	0.058***	0.059***
	0.007	*0.007*	*0.007*	*0.006*	*0.008*	*0.007*	*0.006*
log coverage at $t = -1$	0.001	0.001	0.001	0.000	0.002	-0.002	0.001
	0.004	*0.004*	*0.004*	*0.004*	*0.004*	*0.004*	*0.004*
volatility at $t = -1$	-0.372**	-0.364**	-0.361**	-0.251*	-0.329*	-0.259*	-0.247*
	0.157	*0.157*	*0.157*	*0.133*	*0.190*	*0.135*	*0.133*
=1 if firm is net eq. issuer at $t = -1$	0.000	0.000	-0.001	0.000	-0.011	0.004	0.000
	0.008	*0.008*	*0.008*	*0.008*	*0.009*	*0.009*	*0.008*
Diagnostics							
R^2	45.6%	45.6%	45.5%	49.9%	47.0%	49.3%	49.9%
Wald test: all coeff. = 0	15.8***	15.3***	15.3***	18.0***	15.5***	17.0***	18.1***

Table 5, Panel D: The Effect of Analyst Characteristics on Disclosure.

This table explores cross-sectional variation in firms' guidance responses to exogenous coverage shocks. Coverage shocks are coded as indicators set equal to 1 if a firm suffers one or more exogenous coverage termination that fiscal quarter and additional indicators are included that capture firms that lose a local analyst (columns 1 through 3), an institutions-only broker (column 4), a highly ranked analyst (column 5), an analyst who produces a high degree of firm-specific information (column 6), or an analyst whose forecasts are relatively more accurate than those of his peers (column 7). For variable definitions and details of their construction, see Appendix A. All specifications are estimated as linear probability models with firm and year effects and a set of fiscal-quarter fixed effects (not shown for brevity). Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively. The number of observations in each specification is 31,312 firm-fiscal quarters for 2,095 treated and 2,095 matched control firm-quarters.

	Dependent variable = 1 if management issues forecast						
	Loss of local analyst				Analyst		
	50 mile radius	100 mile radius	200 mile radius	Institutions only broker	ranking	firm-specific info production	relative forecast accuracy
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Exogenous coverage shock							
at $t = 0$	0.031***	0.031***	0.033***	0.061***	0.031***	0.025**	0.041***
	0.008	*0.008*	*0.008*	*0.010*	*0.008*	*0.010*	*0.008*
at $t = -1$	0.021***	0.018**	0.016**	0.027***	0.020***	0.020**	0.019**
	0.008	*0.008*	*0.008*	*0.010*	*0.007*	*0.009*	*0.008*
at $t = -2$	0.036***	0.036***	0.036***	0.034***	0.034***	0.035***	0.036***
	0.007	*0.007*	*0.007*	*0.007*	*0.007*	*0.007*	*0.007*
at $t = -3$	0.033***	0.033***	0.033***	0.032***	0.032***	0.033***	0.033***
	0.008	*0.008*	*0.008*	*0.008*	*0.008*	*0.008*	*0.008*
=1 if local analyst, institutions-only broker, ranked analyst, high firm-specific information production, or high forecast accuracy							
at $t = 0$	0.059***	0.052***	0.031**	-0.049***	0.102***	0.031**	0.000
	0.018	*0.017*	*0.015*	*0.013*	*0.025*	*0.013*	*0.015*
at $t = -1$	0.019	0.030*	0.032**	-0.010	0.033	0.008	0.019
	0.017	*0.016*	*0.015*	*0.013*	*0.024*	*0.013*	*0.015*
Firm characteristics							
log market cap at $t = -1$	0.059***	0.059***	0.059***	0.059***	0.059***	0.059***	0.059***
	0.006	*0.006*	*0.006*	*0.006*	*0.006*	*0.006*	*0.006*
log coverage at $t = -1$	0.000	0.000	0.000	0.000	0.001	0.000	0.000
	0.004	*0.004*	*0.004*	*0.004*	*0.004*	*0.004*	*0.004*
volatility at $t = -1$	-0.250*	-0.250*	-0.251*	-0.252*	-0.240*	-0.258*	-0.250*
	0.133	*0.133*	*0.133*	*0.133*	*0.133*	*0.133*	*0.133*
=1 if firm is net eq. issuer at $t = -1$	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	0.008	*0.008*	*0.008*	*0.008*	*0.008*	*0.008*	*0.008*
Diagnostics							
R^2	49.9%	49.9%	49.9%	49.9%	50.0%	49.9%	49.9%
Wald test: all coeff. = 0	18.4***	18.3***	18.2***	18.3***	18.5***	18.1***	18.0***

Table 6. Naïve Regressions of Liquidity on Voluntary Guidance.

We relate the log of one plus Amihud's (2002) illiquidity measure to eight separate measures of voluntary guidance and to a set of control variables. For variable definitions and details of their construction, see Appendix A. All specifications are estimated using OLS with firm fixed effects, year effects, and a set of fiscal-quarter fixed effects. The fixed effects are not shown for brevity. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We test for first-order serial correlation in the dependent variable using the modified Durbin-Watson test developed for panel-data models by Bhargava, Franzini, and Narendranathan (1982). The critical value at the 5% level for panels of our size is 1.96. *** and ** denote significance at the 1% and 5% level (two-sided), respectively. The number of observations is 34,256 firm-fiscal quarters for 2,095 treated observations and 2,095 matched controls.

	Dependent variable: log Amihud Illiquidity Measure								
	Guidance horizon				Form of guidance		Content of guidance		
	Forecast	Forecast	Pre-announcement	Both	Quantitative	Qualitative	Negative	Positive	Hot air
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Voluntary guidance									
=1 if firm issues … guidance at $t = 0$	-0.004								
	0.003								
=1 if firm issues … guidance at $t = -1$	-0.010***	-0.010***	-0.007	-0.011***	-0.010***	-0.012**	-0.012***	-0.001	-0.001
	0.003	*0.003*	*0.004*	*0.003*	*0.002*	*0.006*	*0.002*	*0.003*	*0.002*
Firm characteristics									
log market cap at $t = -1$	-0.102***	-0.102***	-0.103***	-0.103***	-0.103***	-0.103***	-0.103***	-0.103***	-0.103***
	0.009	*0.008*	*0.008*	*0.008*	*0.008*	*0.008*	*0.008*	*0.008*	*0.008*
log coverage at $t = -1$	-0.007**	-0.007***	-0.007***	-0.007***	-0.007***	-0.007***	-0.007***	-0.008***	-0.008***
	0.003	*0.002*	*0.002*	*0.001*	*0.002*	*0.002*	*0.002*	*0.002*	*0.002*
volatility at $t = -1$	0.199	0.200	0.188	0.200	0.196	0.193	0.196	0.189	0.189
	0.221	*0.153*	*0.152*	*0.153*	*0.152*	*0.153*	*0.152*	*0.152*	*0.152*
=1 if firm is net equity issuer at $t = -1$	-0.019***	-0.020***	-0.019***	-0.019***	-0.020***	-0.019***	-0.020***	-0.019***	-0.019***
	0.004	*0.005*	*0.005*	*0.005*	*0.005*	*0.005*	*0.005*	*0.005*	*0.005*
Diagnostics									
Within-firm R^2	9.3%	9.3%	9.3%	9.3%	9.3%	9.3%	9.4%	9.3%	9.3%
Wald test: all coeff. = 0	12.9***	13.4***	13.4***	13.4***	13.5***	13.4***	13.4***	13.5***	13.9***
Durbin-Watson test for serial corr.	0.9	0.9	0.9	0.9	0.9	0.9	0.9	0.9	0.9

Table 7. Causal Effect of Voluntary Disclosure on Liquidity.

We estimate the causal effect of voluntary disclosure at $t = 0$ on liquidity at $t = +1$ for four guidance choices using exogenous shocks to a firm's analyst coverage as an instrument. Specifically, we instrument a firm's guidance choice in quarter $t = 0$ with a set of indicator variables capturing exogenous coverage terminations in quarters $t = -3$ to $t = 0$. (See Table 5, Panel B for the corresponding first-stage estimates.) The identifying assumption is that exogenous coverage terminations dated –3 to 0 do not affect liquidity in quarter $t = +1$ other than through their effect on the firm's guidance choice. (See Table 4 for the corresponding reduced-form estimates.) We focus on the four guidance choices that, according to the first-stage estimates in Table 5, Panel B, respond significantly to the instrument. For variable definitions and details of their construction, see Appendix A. All specifications are estimated using OLS with firm fixed effects, year effects, and a set of fiscal-quarter fixed effects. The fixed effects are not shown for brevity. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. *** and ** denote significance at the 1% and 5% level (two-sided), respectively. The Staiger-Stock (1997) test is a Wald test of weak instruments, i.e., of the extent of correlation between the guidance choice and the instrument. It has a critical value of 10 in an F-test. The number of observations in each specification is 27,870 firm-fiscal quarters for 2,095 treated observations and 2,095 matched controls. (The lower number of observations compared to Table 5, Panel B reflects the fact that the first stage is lagged relative to the second stage.)

	Dependent variable: log Amihud Illiquidity Measure at $t = +1$			
	Forecast	Forecast or pre-announce-ment	Quanti-tative	Negative
	(1)	(2)	(3)	(4)
Instrumented voluntary guidance				
guidance choice at $t = 0$	-0.075**	-0.081**	-0.085**	-0.148***
	0.035	*0.037*	*0.039*	*0.044*
Firm characteristics				
log market cap at $t = 0$	-0.101***	-0.101***	-0.101***	-0.100***
	0.008	*0.008*	*0.008*	*0.008*
log coverage at $t = 0$	-0.005***	-0.005***	-0.005***	-0.005***
	0.002	*0.002*	*0.002*	*0.002*
volatility at $t = 0$	0.089	0.090	0.088	0.078
	0.158	*0.158*	*0.158*	*0.158*
=1 if firm is net equity issuer at $t = 0$	-0.018***	-0.018***	-0.018***	-0.020***
	0.005	*0.005*	*0.005*	*0.006*
Diagnostics				
Within-firm R^2	8.5%	8.5%	8.5%	8.5%
Wald test: all coeff. = 0	12.2***	12.2***	12.3***	12.3***
Staiger-Stock test (F)	30.8***	20.8***	15.9***	10.1***

Table 8. Causal Effect of Voluntary Disclosure on Alternative Liquidity Measures.

This table repeats the analysis of Table 7 using six alternative proxies for liquidity: Bid-ask spreads, effective tick size, lambda, the fraction of zero-return days in a quarter, PIN, and Pastor and Stambaugh's (2003) gamma. As in Table 7, we estimate the causal effect of voluntary disclosure at t = 0 on liquidity at t = +1 for four guidance choices using exogenous shocks to a firm's analyst coverage as an instrument. Specifically, we instrument a firm's guidance choice in quarter t = 0 with a set of indicator variables capturing exogenous coverage terminations in quarters t = –3 to t = 0. (See Table 5, Panel B for the corresponding first-stage estimates.) The identifying assumption is that exogenous coverage terminations dated –3 to 0 do not affect liquidity in quarter t = +1 other than through their effect on the firm's guidance choice. We focus on the four guidance choices that, according to the first-stage estimates in Table 5, Panel B, respond significantly to the instrument. For variable definitions and details of their construction, see Appendix A. We include the same controls as in Table 7, but to conserve space, we only report the coefficients for the guidance variables. All specifications are estimated using OLS with firm fixed effects, year effects, and a set of fiscal-quarter fixed effects. The fixed effects are not shown for brevity. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. *** and ** denote significance at the 1% and 5% level (two-sided), respectively. Due to missing observations on bid-ask spreads and lambda, the number of observations is 27,441 firm-fiscal quarters in Panel A, 26,741 firm-fiscal quarters in Panel B, and 25,223 firm-fiscal quarters in Panel C. PIN data in Panel E is only available from 2003 onwards, resulting in a smaller sample size of 12,829 firm-fiscal quarters. Panels D and F use the full sample of 27,870 firm-fiscal quarters.

	Dependent variable: Liquidity at t = +1			
	Forecast (1)	Forecast or pre-announce-ment (2)	Quanti-tative (3)	Negative (4)
Panel A. Dependent variable: Bid-ask spreads at t = +1				
Instrumented guidance choice at t = 0	-0.033***	-0.032***	-0.037***	-0.035***
	0.003	*0.003*	*0.003*	*0.003*
Within-firm R^2	13.0%	12.9%	13.2%	12.7%
Panel B. Dependent variable: Effective tick size at t = +1				
Instrumented guidance choice at t = 0	-0.144***	-0.146***	-0.158***	-0.166***
	0.006	*0.006*	*0.006*	*0.007*
Within-firm R^2	28.4%	28.5%	28.7%	28.3%
Panel C. Dependent variable: Lambda at t = +1				
Instrumented guidance choice at t = 0	-0.059**	-0.052**	-0.050*	-0.079**
	0.024	*0.027*	*0.030*	*0.032*
Within-firm R^2	4.6%	4.6%	4.6%	4.6%
Panel D. Dependent variable: Fraction zero-return days in quarter t = +1				
Instrumented guidance choice at t = 0	-0.073***	-0.075***	-0.082***	-0.080***
	0.007	*0.007*	*0.007*	*0.008*
Within-firm R^2	11.8%	11.8%	11.9%	11.7%
Panel E. Dependent variable: PIN at t = +1				
Instrumented guidance choice at t = 0	-0.045**	-0.048**	-0.046**	-0.036
	0.019	*0.019*	*0.020*	*0.026*
Within-firm R^2	5.8%	5.8%	5.8%	5.7%
Panel F. Dependent variable: Pastor-Stambaugh's gamma at t = +1				
Instrumented guidance choice at t = 0	-0.055***	-0.054***	-0.054***	-0.072***
	0.018	*0.018*	*0.020*	*0.022*
Within-firm R^2	3.3%	3.3%	3.3%	3.3%

Table 9. Causal Effect of Voluntary Disclosure on Firm Value.

We estimate the causal effect of voluntary disclosure at $t = 0$ on firm value at $t = +1$ (as measured by the ratio of the firm's book value of equity to its market value) using exogenous shocks to a firm's analyst coverage as an instrument for disclosure. Compared to Table 7, to avoid mechanical correlations with the dependent variable, we exclude the firm's log market capitalization from the regressors in the first-stage regressions (not shown) and in the second-stage regressions reported here. For variable definitions and details of their construction, see Appendix A. All specifications are estimated using OLS with firm fixed effects, year effects, and a set of fiscal-quarter fixed effects. The fixed effects are not shown for brevity. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. *** and ** denote significance at the 1% and 5% level (two-sided), respectively. The Staiger-Stock (1997) test is a Wald test of weak instruments, i.e., of the extent of correlation between the guidance choice and the instrument. It has a critical value of 10 in an F-test. The number of observations in each specification is 27,870 firm-fiscal quarters for 2,095 treated observations and 2,095 matched controls.

	Dependent variable: Book-to-market ratio			
	Forecast	Forecast or pre-announcement	Quantitative	Negative
	(1)	(2)	(3)	(4)
Instrumented voluntary guidance				
guidance choice at $t = -1$	-0.334***	-0.453***	-0.282**	-0.981***
	0.117	*0.120*	*0.121*	*0.149*
Firm characteristics				
log coverage at $t = -1$	2.088***	2.197***	2.018***	2.508***
	0.007	*0.007*	*0.007*	*0.007*
volatility at $t = -1$	1.769***	1.754***	1.781***	1.640***
	0.542	*0.540*	*0.541*	*0.536*
=1 if firm is net equity issuer at $t = -1$	-0.017	-0.017	-0.018	-0.031*
	0.017	*0.017*	*0.016*	*0.016*
Diagnostics				
Within-firm R^2	2.3%	2.4%	2.3%	2.7%
Wald test: all coeff. = 0	14.5***	14.8***	14.4***	16.7***
Staiger-Stock test (F)	25.4***	16.7***	12.9***	8.1***