Legal Shocks to Informed Trading by Outsiders and Voluntary Management Disclosures

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Abstract

Litigation risk for management is an important driver of voluntary disclosures. We show that litigation risk for outside investors also drives management disclosures. The Second Circuit decision in *US v. Newman* increased outside investors' ability to trade on material nonpublic information, which prior research shows reduced the liquidity of the affected firms' stocks. We find that firms subject to *Newman* increase their earnings guidance activities, with guidance frequency rising approximately 13%, compared to firms not subject to *Newman*. The disclosure response is strongest for affected firms where plausibly informed hedge fund investors coexist with non-hedge-fund institutional investors. The Supreme Court subsequently narrowed *Newman* in *Salman v. US*; our findings reverse accordingly. In contrast to management litigation risk that drives managers to preemptively disclose bad news to reduce the legal consequences of stock price drops, reduced outsider litigation risk drives managers to disclose all news to keep the stock liquid.

JL Contributions: Conceptualization, data analysis, writing.
VN Contributions: Writing.

1. Introduction

Accounting research has extensively shown that litigation risk for management is an important driver of voluntary disclosures. We show that litigation risk for outside investors also drives management disclosures. We study the *United States v. Newman* (2014) ruling, where the Second Circuit Court unexpectedly raised the evidentiary standard required to prosecute certain forms of tippee trading by informed outside investors.[1] The Court's decision increased informed investors' ability to trade on material nonpublic information within the Second Circuit, which comprises Connecticut, New York, and Vermont. Importantly, the decision did not directly alter managers' direct legal risk or firms' mandatory disclosure obligations. The Supreme Court later narrowed *Newman* in *Salman v. United States* (2016), causing insider trading enforcement incentives to move back toward their prior alignment.

Pierce (2024) finds that these legal changes altered informed trading and the resulting capital-market environment. Informed trading increased among treatment (i.e., Second Circuit) firms (relative to non-Second Circuit control firms) following *Newman*. Specifically, hedge fund trades in Second Circuit treatment firms were abnormally profitable during the *Newman*-to-*Salman* period, with a significant portion of these returns occurring around future earnings announcements, suggesting that these hedge funds possessed superior private information about future earnings. Accordingly, stock liquidity declined, consistent with relatively uninformed investors overall price-protecting themselves in the face of increased information asymmetry. Many of these adverse capital markets effects reversed when the Supreme Court narrowed *Newman* in *Salman.*

[1] Section 2.3 explains the tipper-tippee relationship as it pertains to our setting.

A natural follow-on question that Pierce (2024) notes but does not address is if managers tried to ameliorate the adverse capital markets effects with voluntary disclosure. We perform that analysis, and find that that treatment firms affected by *Newman* significantly increase both the frequency and likelihood of issuing earnings guidance following *Newman*, relative to control firms not affected by *Newman*. These effects amount to a 0.08 increase in earnings guidance frequency per quarter relative to control firms, or approximately a 13% increase relative to the overall average earnings guidance frequency per quarter. The increase reverses following the Supreme Court's decision in *Salman* that narrowed *Newman*. These results, illustrated in Figure 1, show no pre-trends in earnings guidance behavior, a clear increase following *Newman*, and a decline following *Salman*. The results are robust to alternative disclosure measures such as press releases, controls for firm performance and market conditions, and specifications that isolate the *Newman* and *Salman* legal changes separately.[2]

Pierce (2024, Section 3) shows that informed tippees are often hedge funds whose trades suggest superior foreknowledge of future earnings. However, these funds may prefer non-disclosure by the firm to protect their private information, and may use their ownership power over management to ensure silence. As a counterweight, we therefore consider informationally disadvantaged investors whose ownership stake and influence are large enough to push management to disclose (Bird and Karolyi 2016; Schoenfeld 2017). We proxy for such investors using non-hedge fund institutional investors. We find that the disclosure response during the

[2] We control for management litigation risk both through our difference-in-differences design and various firm-level performance regressors (Kim and Skinner 2012).

Newman-to-*Salman* period is significantly stronger in firms where both hedge funds and non-hedge fund institutional investors coexist and weaker when one or neither is present.[3]

Management itself may be reluctant to increase voluntary disclosure to protect its job and perks (Beyer et al. 2010). Another way investors may motivate management to disclose in order to ameliorate adverse capital market effects is by tying its compensation to stock price (Nagar et al 2003). Consistent with this channel, we find that the disclosure response is stronger when CEOs have higher equity incentives, as measured by Delta. We conduct several additional corroborating analyses (such as analyst following) that are described in Section 4.

Our results raise the question as to why disclosure effects do not completely nullify Pierce's (2024) capital market effects. Nagar et al (2019) argue that disclosure is a costly and inefficient mechanism that can only partly reduce adverse capital market effects. This could be one reason why Pierce's and our results coexist.[4] Pierce also views the adverse capital market effects as evidence of markets counteracting regulatory shocks. Our disclosure results suggest that this claim would likely be valid even if Pierce had found no capital market effects --- regulation could have induced management disclosures that could have completely nullified any adverse capital markets effects.

It is also worth contrasting our results with the management litigation risk literature (reviewed in Section 2.1). There, the management's primary motive is to preemptively disclose bad news to lessen the adverse legal consequences of any future stock price drops (a "first

[3] Another potential advantage of the *Newman*-to-*Salman* shock is that we don't have to explicitly model the endogenous matching process between large investors and firms and can instead take the investor base as given (unless this matching changes quickly in response to the shock).

[4] A closer investigation would necessitate estimating a simultaneous model of capital market effects and voluntary disclosure, which is difficult due to the lack of good instrumental variables. Instead, we show that firms that disclose earlier in the quarter have higher liquidity later in the quarter, and this effect is amplified during the treatment period for the treatment firms.

moment" price effect). In our setting, reduced outsider litigation risk reduces the efficiency and the liquidity of the stock price. Given that outsiders' private information could be either positive or negative, management discloses more news rather than less news --- both good and bad --- to keep the stock price more liquid (a "second moment" price effect).

Finally, a concurrent working paper by Dai et al (2026) shows that firms subject to the *Newman* ruling change their operating strategy. They increase their patenting rates and reduce their reliance on trade secrecy. Since patents are public and trade secrets are not, this operating shift increases transparency. It is unlikely that such operating shifts are driving our results because publicly disclosed patents likely do not need to be further supported with additional management disclosures. There is also no reason to expect that patent adoption should vary with our ownership-based cross-sectional tests.

The remainder of the paper proceeds as follows. Section 2 reviews the literature and develops the institutional background and hypotheses. Section 3 describes the data and sample construction. Section 4 presents the main empirical results and robustness analyses. Section 5 concludes.

2. Literature Review and Hypothesis Development

2.1 Management Litigation Literature

An extensive literature examines management litigation risk as a driver of costly voluntary disclosure (Beyer et al. 2010). Skinner (1994) documents that managers preempt large adverse earnings surprises with earnings-related voluntary disclosures. Skinner (1997) shows that such timely disclosure, while initially incurring disclosure costs. is beneficial in terms of reducing subsequent settlement costs. Field et al (2005) argue that it is difficult to distinguish

whether disclosure deters litigation or whether firms expecting litigation are simply more likely to warn, and therefore jointly model managers' decisions to issue earnings warnings and firms' lawsuit risk in a simultaneous-equations framework. They find that firms facing greater expected litigation risk are more likely to issue earnings warnings. After accounting for this preemptive response, they find no evidence that warnings trigger litigation and some evidence that warnings deter certain types of litigation. Subsequent studies similarly show that earlier revelation of adverse earnings information is associated with lower litigation risk and that earnings warnings provide litigation benefits, while silence combined with insider selling is associated with particularly adverse litigation consequences (Donelson et al. 2012; Billings and Cedergren 2015). Further emphasizing the role of news direction, Rogers et al (2011) find that unusually optimistic qualitative disclosure is associated with subsequent shareholder litigation, while Cazier et al (2020) show that safe harbor provisions provide some protection to management in this regard. Finally, Cheng and Lo (2006) show that managers time their insider buying trades around bad news forecasts but do not adjust their forecasting activities around insider sales due to litigation risk. They conclude that management exploits voluntary disclosure for trading gain but only when litigation risk is low.

We do not build a simultaneous model of outsider litigation risk and voluntary disclosure, but instead employ the litigation shock studied by Pierce (2024). Houston et al. (2019) exploit three exogenous legal changes and show that increases in management litigation risk increase the issuance of bad news earnings forecasts. Likewise, Huang et al. (2020) use staggered adoption of universal demand laws as an exogenous reduction in managerial litigation exposure and find that adoption reduces short-horizon earnings warnings but does not change the disclosure patterns of good earnings news. In sum, management litigation risk is a "first-moment" price effect risk that

is triggered by a stock price drop; management therefore tries to lessen its impact with early release of bad news. Our outsider litigation risk is a "second-moment" price effect risk that affects stock liquidity. Outside investors' private information that causes other investors to price protect themselves through wider bid-ask spreads could be either good news or bad news; management therefore tries to ameliorate this inefficiency by making more rather than fewer disclosures, for both good and bad news. We turn to our setting next.

2.2 Theory

Theory has extensively examined how capital market frictions drive managers' costly voluntary disclosure decisions (Beyer et al. 2010; Blankespoor et al. 2020). This section draws on Lambert et al. (2012), a comprehensive model of trading by investors with heterogeneous private information that incorporates insights from several key models. When investors are small and price-takers, they rationally infer investors' private information contained in price but do not expect their own orders (that are partly based on their own private information) to change price. As a result, prices reflect only the average information precision or quality of all investors and not on how unevenly this private information is distributed across investors (information asymmetry). In other words, it is not the spread but the average investor informedness that matters. If the average informedness of investors is low, they demand a lower price or, equivalently, a return premium to hold the stock which in turn increases management's cost of capital and decreases its investment opportunity set. Management is then motivated to provide disclosures to increase the average informedness of investors. Note that management does not respond to the spread (information asymmetry) in this setting.

Things change when investors have pricing power because they now know that the price will change if they place a large order. As a result, information asymmetry additionally determines

prices. To the extent management wants to issue voluntary disclosures to reduce the cost of capital, disclosures will respond to both average information precision and information asymmetry, i.e., both the average investor informedness and the spread of information across investors matter. In sum, management's disclosure response to information asymmetry should theoretically occur primarily when shareholders are large and can move prices, and not when they are atomistic price-takers.

Pierce (2024) also finds a muted price response to earnings surprises, suggesting that prices became more informative to all investors, thus increasing investors' average informedness. This indirect information supply effect through an informed price is an unavoidable consequence of informed trading. If prices are noiseless, then they will perfectly reveal all information and information asymmetry among investors will vanish, eliminating any need for additional management disclosures. But it is well known that an equilibrium with noiseless prices cannot exist when information is costly to acquire and process (Blankespoor et al. 2020). When prices are noisy, some residual information asymmetry will continue to exist. Management then trades off the cost of disclosure against the benefits of reducing this residual information asymmetry.

In sum, when outsiders can trade aggressively on insider information --- that could be either bad or news --- due to reduced litigation risk, the resulting information asymmetry will cause an adverse selection problem whereby other trades will price-protect themselves reducing stock liquidity. This is a "second-moment" price effect risk that management tries to ameliorate by increasing both good and bad news disclosures. However, theory also suggests that investors need not unanimously prefer additional disclosure. Cianciaruso, Qiu, and Xue (2025) show that sometimes public disclosure can benefit relatively uninformed investors while reducing the surplus of an informed investor. In our setting, this logic suggests that powerful informed

outsiders such as hedge funds could use their ownership control to prevent management from disclosing. Following Bird and Karolyi (2016) and Schoenfeld (2017) who argue that institutional owners often use their ownership stake and influence to push management to disclose, we use non-hedge fund institutional owners as a less informed counterweight to hedge funds and show that disclosure effects are strongest when both types of investors are present.[5] We discuss the institutional details in Section 2.3.

2.3 *Newman* and *Salman* Settings

Our discussion of the legal setting and treatment assignment follows Pierce (2024). In 1983, the Supreme Court's decision in the landmark *Dirks v. SEC* established the modern framework for tippee liability under the federal securities laws.[6] Under *Dirks*, a tippee may be held liable only when the insider discloses information in breach of a fiduciary duty and receives a personal benefit from doing so. Courts applying this framework recognized that this benefit could take various forms besides monetary gifts, including certain intangible advantages and gifts of information to relatives or friends. However, the specific details of what constitutes a benefit remained a contentious topic.

In 2014, the U.S. Court of Appeals for the Second Circuit revisited this framework in *United States v. Newman*.[7] In a surprise ruling, the court held that, for outside investors who received material, non-public information, the government must show both that the insider received a personal benefit of a monetary nature, even in cases of friendship, and that the tippee

[5] In Section 4, we also examine other management disclosure incentives such as stock-based compensation.
[6] A tippee is defined as an outside investor who receives material, non-public information from an insider. Legally, they may face insider-trading liability if the information is used for trading.
[7] The case specifically involved a hedge fund manager who received material, non-public information about Dell's quarterly earnings. (U.S. Attorney's Office, Southern District of New York, 2013; see also https://corpgov.law.harvard.edu/2017/01/18/insider-trading-law-after-*Salman*/).

knew of that benefit. This high evidentiary standard made it easier for outsiders to trade on insider information as long as they did not compensate the tipper. A key feature of the ruling is that while it complicated prosecution of information sharing within professional or business networks, it did not directly alter managers' legal risks or the firms' financial reporting obligations.[8]

In 2016, the Supreme Court effectively narrowed *Newman* in *Salman v. United States* in regards to information sharing with friends or family.[9] The court held that an insider's gift of confidential information to a trading relative or friend is sufficient to establish personal benefit under Dirks and rejected these additional restrictions in *Newman*. As a result, during the period between the decision of *Newman* and its reversal in *Salman*, the enforcement of insider trading cases was generally narrower within the Second Circuit than under prior national practice.[10]

2.4 Salience of *Newman* and *Salman* to managers, informed investors, and uninformed investors

United States v. Newman was a major decision that was plausibly salient for managers, informed investors, and uninformed investors. The decision was regarded as a major defeat for the SEC with the "fallout" of the decision being tracked by major publications like *The New York*

[8] While the ruling also recognized that friendship counts as a personal benefit, other language in the ruling suggests that quid pro quo evidence is still needed for cases of friendship, which made prosecution of tippees difficult. Ahern (2017) finds that top-level management is rarely the tipper.
[9] Ahern (2017) notes that friends and family are a pre-eminent feature in insider information chains.
[10] To be more precise, *Salman* legally is a partial reversal of *Newman*, and started a broader process for a more formal dismantling of *Newman* in 2017 with *US. v. Martoma* (*2nd Cir.*), which was anticipated upon *Salman* (Khan 2018; Pierce 2024). We follow Figure 2 of Pierce (2024) and define the treatment period as *Newman*-to-*Salman*.

Times as well as *The Wall Street Journal*.[11] The SEC described the *Newman* ruling as "one of the most significant developments in insider trading law in a generation".[12]

The decision was particularly relevant for hedge funds, who are more likely to acquire private information and can trade in sufficient size for their trading decisions to affect prices and market liquidity. Hedge funds are known to trade on private information received through political intelligence, syndicated lending relationships, and tipper-tippee networks (Ahern 2017; Gao and Huang 2016; Massoud et al 2011; Yan and Zhang 2007). Additionally, hedge funds are frequently defendants in tipper-tippee insider trading cases and are a major source of SEC convictions despite the difficulty in prosecuting such cases (Ahern 2017). Indeed, Pierce (2024, Section 3) finds that, following *Newman*, hedge funds made abnormal profits, the bulk of which happened around future earnings announcements, suggesting that these funds had superior private foreknowledge of future earnings. We therefore provisionally view hedge funds as informed traders large enough to move prices.[13]

By contrast, traditional institutional investors are generally less informed than hedge funds about short-horizon private information (Yan and Zhang 2007). These investors may therefore be disadvantaged by an increase in informed hedge-fund trading. At the same time, their ownership stakes and economic importance may allow them to successfully push management for more disclosure (Bird and Karolyi 2016; Schoenfeld 2017). We therefore provisionally view non-hedge-fund institutional investors as relatively uninformed investors who

[11] See Fallout for the S.E.C. and the Justice Dept. From the Insider Trading Ruling - The New York Times; Court Decision Foils Insider-Trading Cases - WSJ.
[12] Petition for Rehearing and Rehearing En Banc at 22–23, U.S. v. Newman, 773 F.3d 438 (2d Cir. 2014) (No. 13-1837(L)).
[13] SEC Commissioner Aguilar notes that insider trading and counterparty risk by hedge funds represent a particular threat to market integrity (Aguilar 2009).

may possess sufficient ownership power and influence to counteract the non-disclosure pressure on management arising from factors such as job security concerns and a push for silence from powerful and privately informed hedge funds. Additionally, to the extent management is compensated on stock price, it has incentives to make costly disclosures to support the price (Nagar et al 2003). Stock compensation is thus another mechanism through which investors can persuade management to make costly disclosures.

Turning to the nature of the insider information, the hedge fund analysis in Pierce (2024, Section 3) suggests that earnings constitute the most important piece of leaked information. This is not surprising given that earnings are by far the most important input into all valuation models. Ahern (2017) finds that earnings information is the most commonly internally leaked information seen in convicted insider trading cases with 26% of the SEC convictions arising from internal earnings leaks.[14] In fact, earnings was also the specific piece of insider information in the *Newman* trial itself. We therefore conjecture that the most important disclosures demanded by uninformed investors will be earnings and therefore focus on management earnings guidance, though we use additional disclosure measures such as press releases as well. By contrast, we do not expect to see any significant changes in mandatory disclosures such as 8-Ks that firms have to file whenever a material event happens.[15]

2.5 Hypothesis Development

To recap, *United States v. Newman* was decided in Q4 2014, and its reversal *United States v. Salman* was decided in Q4 2016. Our analysis therefore focuses on the period starting

[14] The specific table of the sources of leaked information can be found in the Internet Appendix Tables 4 and 5 of Ahern (2017).
[15] Management has some discretion on the level of detail in some of the 8-K items. We do not study this issue.

from Q1 2013 through Q4 2018. We drop the two quarters in which *Newman* (Q4 2014) and *Salman* (Q4 2016) were decided from the main analyses. Our treatment indicator thus turns on in Q4 2014 (this quarter is dropped from the main analyses) and stays on through Q4 2016 (this quarter is also dropped from the main analyses). Following Pierce (2024, Sections 4 and 5), we use headquarters location to assign firms to federal judicial circuits, where firms headquartered in the Second Circuit (Connecticut, New York and Vermont) are classified as treatment firms, and firms headquartered in other circuits are classified as the control group.

Pierce (2024) finds that insider trading increased among treatment firms following *Newman* and reversed after *Salman*. Specifically, hedge fund trades in Second Circuit treatment firms were abnormally profitable during the *Newman*-to-*Salman* period, with a significant portion of these returns occurring around future earnings announcements, suggesting that these hedge funds possessed superior private information about future earnings. Pierce (2024) also finds lower stock liquidity, consistent with relatively uninformed investors overall price-protecting themselves due to increased information asymmetry. This increases the cost of capital to management, who will then try to respond with increased voluntary disclosure:

H1: *Following the Second Circuit's decision in Newman, firms headquartered in the Second Circuit increase their voluntary public disclosure relative to control firms, and this increase reverses after the Supreme Court's partial reversal of Newman in Salman.*

Evidence supporting H1 raises the question as to why disclosure effects do not completely nullify Pierce's (2024) capital market effects. Nagar et al (2019) argue that

disclosure is a costly and inefficient mechanism that can only partly reduce adverse capital market effects. This could be one reason why Pierce's and our results coexist.[16]

While H1 characterizes the average disclosure response to *Newman*, theory suggests that the effect should be strongest in settings where large informed traders are present in sufficient scale for the imperfect-competition (i.e., non price-taking) mechanism to be economically consequential, as well as where relatively uninformed investors have sufficient influence to counteract any non-disclosure preferences of large informed traders. Specifically, as discussed in Section 2.3, we use hedge-fund ownership to capture large privately informed investors and non-hedge-fund institutional owners to capture relatively uninformed but influential investors who may demand additional disclosure.

A potential advantage of the *Newman*-to-*Salman* shock is that we don't have to explicitly model the endogenous matching process between large investors such as hedge funds and firms, but can instead take a firm's investor base as a given regressor. We acknowledge that the matching itself may change quickly and significantly in response to the shock, but quick entry and exit are relatively costlier to large investors due to adverse price impact, compared to small investors. We test the simple cross-sectional prediction:

H2: *H1 is more pronounced in firms with both hedge funds and institutions compared to firms with only one or neither.*

One concern is that to the extent to which firms without hedge funds or institutional investors still have some other unidentified large price-moving shareholders, it would weaken the

[16] A closer investigation would necessitate estimating a simultaneous model of capital market effects and voluntary disclosure, which is difficult due to the lack of good instrumental variables.

power of H2. But this would bias against finding significant results. Another potential concern is that if hedge funds and institutions choose their stake endogenously in firms systematically based on some specific firm features, any cross-sectional variation in hedge fund and institutional ownership could represent cross-sectional variation in these firm features. To provide some confidence that investor ownership is indeed measuring investor presence, we exploit the variation in investor types. We conjecture that pushing management for more disclosure is a more salient governance tactic of a non-hedge fund institutional investor if its other portfolio firms make many disclosures. We therefore predict:

H3: *H1* is stronger when the non-hedge fund institutional investors' portfolio contains other high-disclosure firms.

A related implication of this investor-demand channel is that disclosure responses should be stronger in settings where the investor base is more information-sensitive. Firms with greater analyst coverage tend to have a larger base of investors who are sensitive to information asymmetry and thus have a higher demand for information (Healy and Palepu 2001). However, because analyst following is endogenous, Healy and Palepu's (2001) demand-side interpretation is not universally accepted. Armstrong et al (2011) take a supply-side perspective, arguing that large analyst following implies that investors are more equally informed and as a result, information asymmetry is low. Nonetheless, we predict:

H4: *H1 is more pronounced in firms with greater analyst following.*

Managers themselves may be reluctant to increase voluntary disclosure to protect their jobs and perks (Beyer et al. 2010). However, if managers' incentive compensation is more sensitive to stock price, they have stronger incentives to respond to the adverse-selection, price,

and liquidity consequences of informed trading by increasing disclosure (Nagar et al. 2003). We measure compensation sensitivity using the standard Delta measure.

H5: *H1 is more pronounced in firms with CEOs with above average Delta.*

We again acknowledge that to the extent analyst following and CEO Delta are endogenously chosen systematically based on some specific firm features, the cross-sectional variation in analyst following and CEO Delta could be proxying for cross-sectional variation in these firm features. We do not attempt to address this issue in our tests.

Finally, if disclosure mitigates information asymmetry arising from informed trading, it should also have positive observable effects on market liquidity (a proxy for adverse selection among investors). We therefore focus on the sensitivity of liquidity to disclosure. Disclosures in the first month of a quarter should be associated with improved liquidity outcomes in the subsequent two months of the quarter, particularly in the *Newman*-to-*Salman* period when informed trading capability is elevated.[17]

H6: *Firms headquartered in the Second Circuit that disclose in the first month of the quarter experience lower illiquidity in the next two months of the same quarter relative to control firms, and this effect is stronger in the Newman-to-Salman period.*

3. Data

3.1 Data sources

[17] In his Section 4, Pierce (2024) notes that he cannot compare the level of liquidity post-*Salman*, because it is contaminated by a change in the SEC's tick-size pilot program. In contrast, we focus not on the level but the slope or the sensitivity of liquidity to disclosure. We therefore retain the post-*Salman* period.

Based on the discussion in Section 2.3, we focus on the frequency of management earnings guidance as the primary measure of voluntary disclosure. Specifically, we compute the sum of the frequency of quarterly and annual EPS guidance management issues in each firm-quarter (Bamber et al. 2010). We also use frequency of press releases management issues in each firm-quarter as a second voluntary disclosure metric (Hu and Stephan 2022). In addition to these frequency-based measures, we construct binary indicators equal to one if management issued at least one earnings guidance or press release during the firm-quarter. We do not use Form 8-K filings as a primary disclosure measure because most 8-Ks are triggered by regulatory requirements tied to specific events and therefore do not represent voluntary disclosure decisions.[18]

United States v. Newman was decided in Q4 2014, and its reversal *United States v. Salman* was decided in Q4 2016. Our analysis therefore focuses on the period starting from Q1 2013 through Q4 2018. We drop the two quarters in which *Newman* (Q4 2014) and *Salman* (Q4 2016) were decided from the main analyses. Therefore, the treatment indicator turns on in Q4 2014 (this quarter is dropped from the main analyses) and stays on through Q4 2016 (this quarter is also dropped from the main analyses). Following Pierce (2024, Sections 4 and 5), we use headquarters location to assign firms to federal judicial circuits, where firms headquartered in the Second Circuit (Connecticut, New York and Vermont) are classified as treatment firms, and firms headquartered in other circuits are classified as the control group.[19]

We construct a quarterly panel of U.S. public firms for which management earnings guidance, financial information, market activity, and firm-specific news data are available. We

[18] In untabulated tests, we do not find results supporting H1 for 8-K frequencies.
[19] Pierce (2024, Section 5) shows this classification satisfies the stable unit treatment value assumption (SUTVA).

obtain earnings guidance and analyst measures from IBES, and we obtain financial statement variables, headquarters location, and firm identifiers from Compustat Quarterly. Market data comes from CRSP Daily and is aggregated to the quarterly level. We combine the IBES, Compustat, and CRSP datasets using the standard CRSP- Compustat Merged (CCM) and IBES-CRSP links. We manually review observations with ambiguous or conflicting links, and we drop Compustat firms that never appear in IBES.[20]

We separately obtain firm-specific press releases from RavenPack, which supplies time-stamped newswire releases linked to individual firms, and merge RavenPack to the Compustat–CRSP panel using the WRDS RavenPack Company Names file. We repeat the sample-construction process using RavenPack coverage rather than IBES guidance availability and do not require firms to issue management guidance.

In order to construct the hedge fund and non-hedge fund institutional investor measures, we obtain institutional ownership from the LSEG 13F institutional holdings database, and classify institutional ownership as a hedge fund or a non-hedge fund institutional investor, by adapting the Form ADV-based hedge fund classification used in prior studies (Brunnermeier and Nagel, 2004; Griffin and Xu, 2009; Gao and Huang, 2016; Pierce, 2024).[21] The ownership data is then aggregated at the Permno-Quarter level to find the total number of shares held by hedge funds and non-hedge fund institutional investors in the pre-*Newman* period (Q1 2013 through Q3

[20] The main results are robust when all Compustat firms are included (untabulated).
[21] Specifically, we classify an adviser as a hedge fund if it either reports at least one private fund classified as a hedge fund in Form ADV Schedule D, Section 7.B.1, or, for registered investment advisers, charges performance-based fees and reports that at least 50% of its clients are high-net-worth individuals or pooled investment vehicles. To match Form ADV advisers to LSEG 13F managers, we standardize adviser and manager names by converting names to uppercase, removing punctuation and legal suffixes, and constructing raw, core, and compact name fields Ambiguous matches are resolved using exact-name, core-name, compact-name, country, legal-suffix, and name-similarity diagnostics.

2014). This data is then combined with the main quarterly panel used in the main analysis using the CCM links.

To examine managerial incentives, we obtain executive Delta and Vega from the Coles, Daniel, and Naveen (2006) dataset, which is yearly. Due to the annual nature of the data, we focus on a modified pre-*Newman* period (from the beginning of 2013 through to the end of 2014). We merge these data to ExecuComp to identify CEO-year observations and retain one CEO per firm-year, selecting the CEO with the highest compensation when multiple CEO records are available. We then merge these firm-level incentive measures to the main quarterly panel.

3.2 Descriptive statistics

Table 1 presents descriptive statistics for the final estimation sample. The main unit of observation is firm-quarter. The baseline specifications (Table 2) use 57,578 firm-quarters, while models requiring additional control variables that could be missing, such as analyst coverage or turnover, contain 44,147 firm-quarters. Appendix A contains detailed descriptions of all variables.

In the final sample, approximately 38% of firm-quarters include at least one management earnings guidance, and firms that guide often issue multiple earnings guidances within the quarter, which is somewhat comparable to Chuk et al (2013), who find that around 27% of Compustat firms offer some form of management earnings guidance. On average, 0.615 earnings

guidance are released for each firm each quarter (including non-guiders).[22] Overall, our sample characteristics are similar to Table 7 of Pierce (2024).

4. Results

4.1 Testing Hypothesis H1: The Main Result

Table 2 presents the baseline difference-in-differences estimates of the effect of *U.S. v. Newman* and its subsequent reversal in *U.S. v. Salman* on firms' quarterly disclosure behavior. All specifications include firm and year-quarter fixed effects, and standard errors are clustered by state.[23] To mitigate industry effects, we also test and find robust results with the alternative two-way fixed effect of State and Year-Qtr x 4-digit SIC (untabulated).

The coefficients in the pre-*Newman* period are near zero and display no discernible pattern, consistent with the parallel-trends assumption. Immediately after *Newman*, treated firms increase their frequency of annual and quarterly earnings guidance relative to control firms. In Table 2, column (1), the coefficient on *Newman Treat × Post (but pre-Salman)* is 0.074, suggesting an increase in the average frequency of management earnings guidance issued during the quarter relative to control firms not in the 2nd Circuit (about a 12% increase relative to the overall mean). Column (2) shows a similar pattern for the binary earnings guidance indicator outcome. For the frequency of annual and quarterly earnings guidance (Column 3) and the binary earnings guidance (Column 4), the inclusion of the full set of firm and market controls yields slightly larger coefficients (0.089 and 0.042), implying increases of approximately 14% relative to the mean earnings guidance frequency and 11% relative to the mean choice to guide (which is

[22] When including non-guiders, Chuk et al (2013) imply an average Number of Guidance Dates to be 0.867 based on 27% of firms disclosing.
[23] The main specifications are robust to two-way clustering by State and Year-Qtr.

38% of the sample), respectively. These magnitudes are comparable to Balakrishnan et al. (2014), who find that after a covering analyst shop closes, affected firms are about 5 percentage points more likely to issue earnings guidance in a given quarter, corresponding to roughly an 18% increase relative to their pre-shock baseline.[24]

Several control variables load in ways consistent with prior disclosure research. Analyst coverage is positively associated with firm disclosure with magnitudes similar to those reported by Houston et al (2019). We estimate coefficients of 0.020 and 0.004 for analyst coverage on the earnings guidance frequency disclosures and the choice to guide, respectively, compared with their corresponding estimates of 0.028 and 0.006.

Market cap is also consistent in being positively linked to quarterly earnings guidance as the firm is more visible; the magnitude of our estimated coefficient at 0.062 closely aligns with the corresponding coefficient of 0.064 reported by Balakrishnan et al. (2014). Additionally, quarterly returns load negatively, consistent with the literature documenting that firms disclose more following periods of weaker returns or heightened valuation uncertainty (Naughton et al 2019). Together, these patterns suggest that the well-established economic determinants of disclosure are operational in our setting, and the observed effects of *Newman* and *Salman* are distinct from these determinants. Finally, Kim and Skinner (2012) argue that industry fixed effects combined with firm factors such as size and growth are good predictors of management litigation risk; our D-in-D specification and controls account for this risk as well.

[24] Results for earnings guidance count (cols (1) and (3)) remain significant when log transformed (log(# of Annual + Quarterly Guidance)+1) (untabulated) and when using hyperbolic curve. The magnitude of the log transformed is smaller than in Balakrishnan et al. (2014). While their Poisson event-study specification implies a contemporaneous semi-elasticity of roughly 13% in earnings guidance frequency following coverage shocks, our log(count+1) difference-in-differences estimate implies an average post-shock increase of approximately 4.5%.

Following Pierce (2024, Figure 5), Figure 1 plots coefficients from an event-study style specification, providing a visual assessment of pre-treatment dynamics and the evolution of treated–control differences around the two legal shocks. Figure 1 visually illustrates all the above effects and shows that the increase in disclosure is tightly linked to the *Newman*-to-*Salman* regime.[25] The event-study coefficients start as insignificant but sharply rise beginning in early 2015 and remain elevated through Q3 2016, the period during which *Newman* governed insider-trading liability in the Second Circuit. Following the Supreme Court's decision in *Salman* in December 2016, the estimated effects attenuate and converge back toward zero. This pattern is consistent with disclosure responding to enforcement-driven shifts in information incentives rather than persistent firm-level trends. Hypothesis H1 thus appears to be supported by the data.

4.2 Separate *Newman* and *Salman* Tests

To separately identify the contribution of *Newman* and *Salman*, Table 3 estimates their separate treatment effects within narrower event windows.[26] Panel A restricts the sample to the pre-*Newman* (Q1 2013 through Q3 2014) and *Newman* (Q4 2014 through Q3 2016) periods. Consistent with the baseline specification, treated firms significantly increase earnings guidance activity following *Newman*. The coefficient on *Newman Treat* × *Post* in column (1) is 0.084 and becomes modestly larger when controls are included in column (3).

In contrast, Panel B focuses on the reversal from *Newman* to *Salman* and restricts the sample to the *Newman* (Q1 2015 through Q3 2016) and *Salman* (Q4 2016 through Q4 2018) periods. In this specification, the coefficient on *Salman Treat* × *Post* is negative and statistically

[25] In Figure 1, we keep the *Newman* decision quarter (Q4 2014) and the *Salman* decision quarter (Q4 2016) for visualization purposes.
[26] In contrast to the main test, we do not drop the implementation quarter of *Newman* (Q4 2014) and *Salman* (Q4 2016). Results are consistent when they are excluded.

significant across all columns (e.g., –0.070 in column (1)), indicating that firms reduce disclosure when the restrictive tippee-evidence standard is reinstated.

Overall, the window-specific regressions in Table 3 are consistent with the baseline difference-in-differences results in Table 2. By examining each legal change within its relevant period, these tests show that the estimated disclosure effects arise in the expected windows and are similar in magnitude to those in Table 2. This pattern provides additional support for the interpretation that the main findings reflect responses to the legal changes rather than artifacts of the broader sample construction.

4.3 Additional Tests of H1

Table 4 considers firm-initiated press releases as a distinct channel of voluntary disclosure. Although press releases are not the primary outcome of interest, they provide an additional test of whether firms adjust other communication channels in response to changes in insider-trading enforcement. The estimates indicate some evidence of an increase in press-release activity among treatment firms compared to control firms in the *Newman*-to-*Salman* period. However, the response appears to operate primarily along the intensive margin, with limited change in the coefficient on the binary choice of whether a firm issues press releases at all.[27]

In Table O.A of the Online Appendix, we examine the annual and quarterly earnings guidance separately. For the frequency of quarterly earnings guidance (Panel A), the coefficient on *Newman Treat × Post (but pre-Salman)* in the fixed-effects regression specification without additional controls is 0.027, indicating that treatment 2nd Circuit firms increase quarterly earnings

[27] The number of press releases are winsorized at the 5% and 95% level, but the results are consistent when all observations are included as well as when values are log (# of press releases + 1) transformed.

guidance in response to *Newman*. While the magnitudes are smaller than those in the combined earnings guidance measure, the direction of the effect is consistent. For annual earnings guidance (Panel B), the estimated effect is larger at 0.060. Similar results obtain when we add the additional controls. These results suggest that annual earnings guidance contributes meaningfully to the overall difference-in-differences effects in Table 2.

In sum, the alternative-measure analyses show that the difference-in-differences in Table 2 are not unique to a specific type of earnings guidance. Annual, quarterly, and press-release disclosures all move in the same direction across legal regimes, reinforcing empirical support for hypothesis H1.

4.4 Testing Hypotheses H2 and H3: Non-Hedge Fund Institutional Demand for Disclosure

Table 5 examines whether the disclosure response to *Newman* is concentrated in treatment firms with hedge funds and non-hedge fund owners. To test this prediction, we separately calculate the percentage of outstanding shares owned by hedge funds and the percentage owned by non-hedge-fund institutional investors for each quarter from Q1 2013 through Q3 2014, the pre-*Newman* period. We then average each measure across those quarters to obtain one pre-*Newman* hedge-fund ownership percentage and one pre-*Newman* non-hedge-fund institutional ownership percentage for each firm. Using these firm-level pre-period measures, we classify firms as having high or low hedge-fund ownership based on whether pre-*Newman* hedge-fund ownership is at or above the sample median across treatment and control firms. We separately classify firms as having high or low non-hedge-fund institutional ownership using the corresponding sample median. These classifications yield four ownership-mix groups: Low HF / Low Inst, Low HF / High Inst, High HF / Low Inst, and High HF / High Inst. If hedge-fund ownership captures the potential for informed trading by large non-price taking investors

and non-hedge-fund institutional ownership captures uninformed investors capable of inducing management disclosures, the disclosure response should be strongest when both ownership types are high.

The results support this prediction. In Table 5, Panel A, which uses the frequency of annual and quarterly earnings guidance as the dependent variable, the coefficient on *Newman Treat × Post (but pre-Salman)* is economically large and statistically significant in the High HF / High Inst subsample, where the estimate is 0.136. The corresponding coefficients in the other three subsamples are smaller and statistically insignificant. Panel B shows a similar pattern for the choice to disclose. The probability of issuing any earnings guidance rises significantly for the High HF / High Inst group, with a coefficient of 0.065. Taken together, the pattern in Table 5 is consistent with H2: the disclosure response is strongest where large price-moving informed traders and influential, relatively uninformed institutional investors coexist.

The four ownership groups in Table 5 are not balanced. Table 6 therefore uses a continuous measure capturing the joint presence of hedge funds and non-hedge-fund institutional investors in the firm's ownership structure during the pre-*Newman* period. Specifically, for each firm, we multiply its pre-*Newman* hedge-fund ownership percentage by its pre-*Newman* non-hedge-fund institutional ownership percentage and label the product *Overlap*. The coefficient on *Newman Treat × Post (but pre-Salman) × Overlap* is positive and statistically significant across specifications. In the earnings-guidance-count regressions, the coefficient ranges from 0.342 to 0.572, and in the earnings-guidance-indicator regressions, it ranges from 0.219 to 0.378. These estimates indicate that the voluntary disclosure response to *Newman* increases with the product

of hedge-fund and non-hedge-fund institutional ownership. The evidence therefore reinforces Table 5.[28]

Table 7 further examines the non-hedge-fund institutional channel by testing whether the disclosure response is stronger when a firm's institutional investors are more exposed to disclosure practices elsewhere in their portfolios, as predicted by H3. For each non-hedge-fund institutional investor, we identify the firms in its Q3 2014 portfolio and calculate the fraction of those firms that issued guidance during that quarter. We then assign this investor-level measure to each firm held by the investor and average it across the firm's non-hedge-fund institutional investors. The resulting measure is our attempt to capture the extent to which a firm is held by institutional investors whose other portfolio firms had established guidance practices before *Newman*.

Table 7 supports H3. In columns (1) and (2), the coefficient on *Newman Treat* × *Post (but pre-Salman)* × *Inst Disclosure* is 0.054 for earnings-guidance frequency and 0.026 for the earnings-guidance indicator. These estimates indicate that the disclosure response to *Newman* is stronger when non-hedge-fund institutional investors have greater exposure to disclosure practices elsewhere in their portfolios. This pattern is consistent with such investors placing greater salience on disclosure as a means for addressing informational disadvantages.

4.5 Testing Hypotheses H4 and H5: Analyst Following and CEO Delta

[28] We rerun all tests in Section 4.2 and Table O.A of Section 4.3 using both the four ownership-group splits and the continuous *Overlap* triple-interaction specification. The results (untabulated) are qualitatively similar to Sections 4.2 and 4.3 using the *Overlap* specification but weaker with the ownership group splits. As noted in Section 2.5, firms without hedge funds or institutional investors may still have some other unidentified non-price taking investors, which would reduce the power of our tests and bias against finding strong results.

Table 8 tests hypotheses H4 and H5 by examining two additional dimensions of heterogeneity in firms' disclosure responses to *Newman*. Columns (1) and (2) focus on analyst coverage as an additional proxy for investor demand for information, while columns (3) and (4) examine managerial incentives to supply disclosure. To examine cross-sectional variation in the treatment effect, we separately interact the 2nd Circuit treatment firm indicator times the *Newman*-to-*Salman* time period indicator with an indicator for pre-*Newman* period high analyst coverage as well as with an indicator for high CEO Delta. For analyst coverage, we first average analyst following for each firm over the *Newman* pre-period, defined as Q1 2013 through Q3 2014. We then classify firms as High Analyst if their pre-period average analyst coverage is at or above the sample median across all firms. Similarly, for CEO Delta, we average log CEO Delta for each firm over the pre-*Newman* period (Q1 2013 through Q4 2014) using available CEO-year observations, and classify firms as High Delta if their pre-period average CEO Delta is at or above the sample median across all firms.

Columns (1) and (2) focus on firms' average analyst coverage in the pre-period. The coefficient on *Newman Treat* × *Post (but pre-Salman)* × *High Analyst* is positive and significant for both outcomes, with estimates of 0.096 for earnings guidance frequency and 0.030 for the earnings guidance indicator. Columns (3) and (4) focus on average CEO Delta in the pre-period. The triple-interaction term *Newman* Treat × Post (but pre-*Salman*) × High Delta is positive and significant for both guidance frequency (0.156, $p < .10$) and guidance indicator (0.026, $p < .01$). These results suggest that the relaxation of insider-trading enforcement under *Newman* has a larger effect on the disclosure choices treatment firms compared to control firms over the *Newman*-to-*Salman* time period when 2nd Circuit firms have greater analyst following and when

their managers have greater compensation exposure to stock-price. In sum, we find support for hypotheses H4 and H5.

4.6 Testing Hypothesis H6: Early Disclosure and Subsequent Liquidity

Table 9 examines whether 2nd Circuit treatment firms that issue earnings guidance in the first month of the quarter experience lower Amihud illiquidity in months two and three compared to control firms during the *Newman*-to-*Salman* period. We obtain the frequency of annual and quarterly earnings guidance each firm releases in the first month of each quarter (we also use a binary indicator version of this measure). We additionally calculate the average daily Amihud illiquidity of each firm over the months 2 and 3 of each quarter (see Appendix A). The coefficient of the interaction term *Newman Treat* × *Post (but pre-Salman)* × *Early Disclosure* on this subsequent illiquidity measure is negative and statistically significant across specifications. For the count measure of earnings guidance, the triple interaction coefficient is –1.902. Since lower Amihud values indicate greater liquidity, these estimates imply that treated firms that disclose early in the quarter in the *Newman*-to-*Salman* period experience improved late-quarter liquidity relative to both treated firms that do not disclose early and control firms. This evidence suggests that liquidity is more sensitive to management disclosure when investors' concerns about being informationally disadvantaged are elevated, as predicted by Hypothesis H6.

5. Conclusion

A rich accounting literature shows that management litigation risk is an important driver of costly voluntary disclosure. However, this risk is a "first-moment" price effect that is triggered by a stock price drop; management therefore tries to lessen its impact with early release of bad news. By contrast we study an outsider litigation risk setting where an exogenous legal shock

enabled certain outsiders to aggressively trade on material private information --- which could be either good or bad news --- without triggering insider trading laws. Other investors then protected themselves by making the stock less liquid, a "second-moment" price effect. We show that management attempted to counter this deterioration in price efficiency by disclosing more news instead of less --- both good and bad.[29] Future studies can examine litigation risk on other stakeholders and the resulting effects on voluntary disclosures by management.

[29] One consequence of informed investor trading noted in Section 2.2 is that prices become more informative. If prices are noiseless, then they will perfectly reveal all information and information asymmetry among investors will vanish; no extra disclosure is necessary. But it is well known that an equilibrium with noiseless prices cannot exist when information is costly to acquire and process (Blankespoor et al. 2020). When prices are noisy, some residual information asymmetry will continue to exist. Management then trades off the cost of disclosure against the benefits of partly (or fully) reducing this residual information asymmetry.

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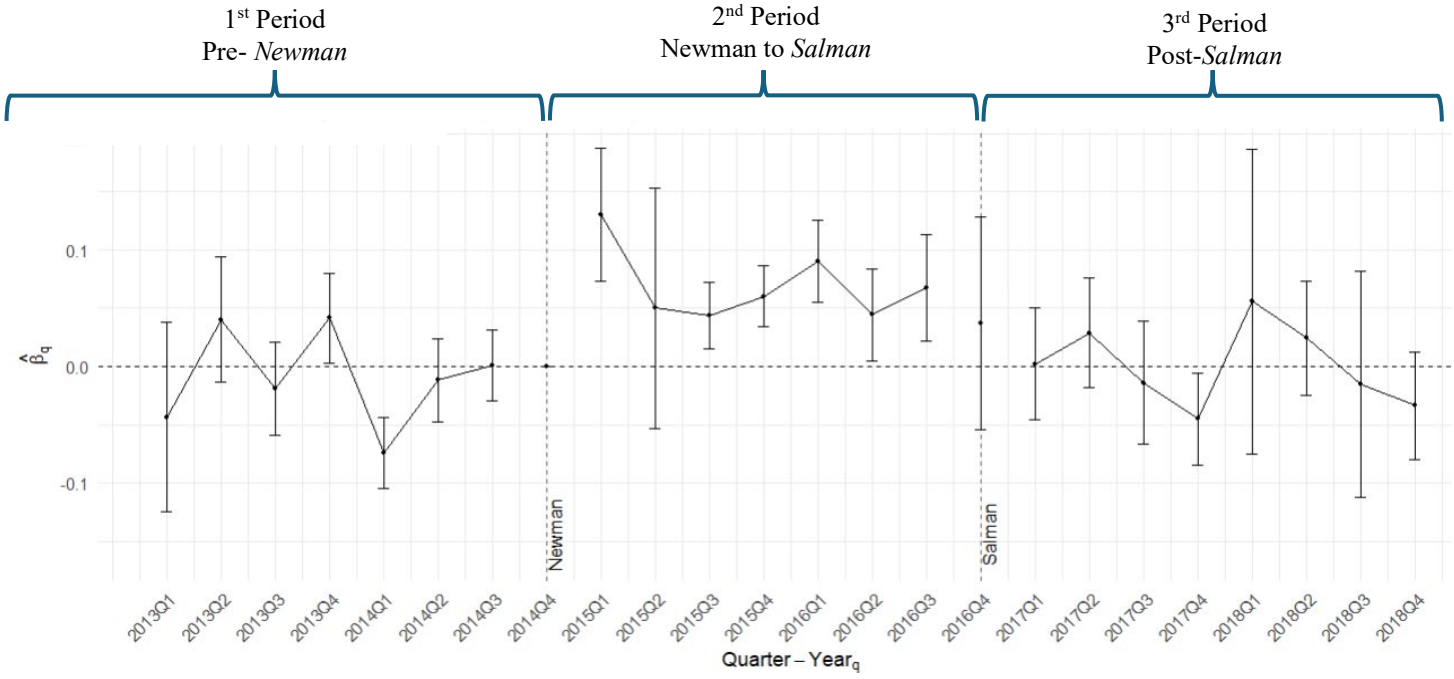
Figure 1: Pre-trend Visualization via Treatment x Quarter-Year Coefficients

We represent quarterly and annual earnings guidance with the coefficients $\hat{\beta}_q$ in a regression of the *# of annual and quarterly guidance* on interactions between indicators for each calendar-quarter and an indicator for firms headquartered in the Second Circuit, controlling for firm and quarter-year fixed effects and clustering standard errors by state.

$$\#\text{ of Annual and Quarterly Guidance}_{i,t} = \sum_{q=2013,Q1}^{2018,Q4} \hat{\beta}_q \Big(\mathbf{1}\{\text{datacqtr}_t = q\} \times \text{Newman_Treat}_i \Big) + \text{FirmFE}_i + \text{QuarterFE}_t + \varepsilon_{i,t}$$

The sample is divided into 3 separate time periods. The first period covers the period pre-*Newman* (Q1 2013 through Q3 2014). The second period covers the period from the enactment of *Newman* to its reversal by *Salman* (from Q1 2015 through Q3 2016, inclusive). The third period covers the period post-*Salman* (after Q1 2017).

The coefficients in the quarters preceding *Newman* show no systematic pre-trend, consistent with the parallel-trends assumption. In *Newman* to *Salman*, following the adoption of *Newman* on December 10th, 2014, the estimated effects rise, indicating an increase in earnings guidance activity among treated firms relative to control firms (i.e., firms not in the 2nd Circuit). This pattern appears to reverse with the Salman ruling on December 6th, 2016.



Notes: Points denote event-study coefficients relative to the omitted quarter (Q4 2014); bars indicate 95% confidence intervals based on standard errors clustered by state. Dashed vertical lines mark the *Newman* (Q4 2014) and *Salman* (Q4 2016) decisions.

Appendix A

Variable	Definition
Number of Annual and Quarterly Guidances$_{i,t}$	The sum of the frequency of quarterly and annual management earnings guidance firm *i*, issued during the quarter *t*. If there is no earnings guidance in the given quarter, the observation is denoted as 0. Firms with no earnings guidance observations during the sample period are excluded. (IBES Guidance Detail)
Choice to Guide$_{i,t}$	An indicator denoting 1 if firm *i* provided at least one management earnings guidance during the quarter *t*. The indicator is denoted 0 if the firm provided no management earnings guidance. Firms with no earnings guidance observations during the sample period are excluded. (IBES Guidance Detail)
Newman Treat$_i$ × *Post*$_t$ *but Pre Salman*	An indicator denoting 1 if firm i is headquartered during the quarter *t* in the *Newman* states (2nd Cir.) when the *Newman* insider-trading ruling is active (Q4 2014 through Q3 2016), before the ruling is reversed. The indicator is denoted as 0 when *Newman* is not active in the quarter *t* for firm *i*. *Newman* was ruled in Q4 2014 and *Salman* Reversed *Newman* in Q4 2016 (Constructed)
Salman Treat$_i$ × *Post*$_t$	An indicator denoting 1 if firm i is headquartered during the quarter *t* in the *Newman* states (2nd Cir.) following the *Salman* ruling (starting from Q4 2016) in the quarter *t* for firm *i*. *Salman* reversed *Newman* in Q4 2016, and the indicator is denoted as 0 when *Salman* is not active in the quarter *t* for firm *i*. (Constructed)
State$_{i,t}$	The U.S. state in which the firm is headquartered in the quarter *t* for firm *i*. (Compustat Fundamentals Quarterly)
Year-Qtr$_t$	The calendar year and quarter in which the quarterly financial statement was disclosed in. (Compustat Fundamentals Quarterly)
# of Press Releases$_{i,t}$	The sum of the frequency of press releases firm *i*, issued during the quarter *t*. If there is no press releases disclosed in the given quarter, the observation is denoted as 0. Values are winsorized at the 95th and 5th percentiles (RavenPack)
Choice to PR$_{i,t}$	An indicator denoting 1 if firm *i* provided at least one Press Release during the quarter *t*. The indicator is denoted 0 if the firm provided no press releases. (RavenPack)
Inst. And HF Overlap$_i$	Pre-*Newman* (Q1 2013 through Q3 2014) ownership overlap measure for firm *i* that captures the joint presence of hedge fund and non-hedge fund institutional ownership, where we classify institutional investors as hedge funds or non-hedge fund institutional investors using ADV disclosures (Explained in Section 4.4). Hedge fund ownership is calculated each quarter as the percentage of shares outstanding held by hedge fund investors, and non-hedge-fund institutional ownership is calculated each quarter as the percentage of shares outstanding held by non-hedge-fund institutional investors. These quarterly ownership measures are averaged separately over Q1 2013 through Q3 2014 to obtain firm *i*'s pre-*Newman* hedge fund ownership measure and pre-*Newman* non-hedge-fund institutional ownership measure. The overlap measure is calculated as the product of firm *i*'s pre-*Newman* hedge fund ownership and firm *i*'s pre-*Newman* non-hedge-fund institutional ownership. (Constructed from LSEG 13F institutional holdings)
Inst Disclosure$_i$	Investor disclosure exposure measure for firm i, which is calculated for each non-hedge-fund institutional investor as the fraction of firms in its Q3 2014 portfolio that issued guidance in Q3 2014. This investor-level exposure measure is assigned back to each firm held by that investor and averaged across firm *i*'s non-hedge-fund institutional investors to obtain one firm-level disclosure exposure measure. The firm-level exposure measure is set to zero if firm *i* has no matched non-hedge-fund institutional investors in Q3 2014. The resulting firm-level measure is z-scored for use in the regression analysis. Results are robust when not-standardized. (Constructed from LSEG 13F institutional holdings)

High_Analyst$_i$	An indicator denoting if the average pre-*Newman* period (Q1 2013 through Q3 2014) *Number of Analysts*$_{i,t}$ for firm *i* is above the median number of overall pre-*Newman* period *Number of Analysts*$_{i,t}$. For each firm, we first average analyst coverage over the pre-*Newman* period. We then compute the median of this firm-level pre-period average across all firms with available analyst coverage data. Firms with pre-*Newman* period average analyst coverage at or above this median are classified as High Analyst, while firms below the median are classified as Low Analyst. If no forecast is given at all during the pre-period, it is dropped (Constructed from IBES Detail Academic)
High Delta$_i$	An indicator denoting if the average pre-*Newman* period (beginning 2013 through the end of 2014) Delta$_i$ for firm *i* is above the median Delta across all firms in the pre-period, where Delta is sensitivity of the CEO's equity portfolio value to a 1% change in the firm's stock price for firm *i* in quarter *t*, which are then natural log-transformed. The indicator is denoted 0 if the firm is below the overall median pre-period Delta$_{i,t}$; if Delta is missing for all quarters during the pre-period, the firm is dropped. (Constructed by: Coles, Daniel, and Naveen 2006, Updated Version)
CEO_Vega$_{i,t}$	The sensitivity of the CEO's equity portfolio value to a 1% change in the volatility of the firm's stock return for firm *i* in quarter *t*, where Vega$_{i,t}$ is computed as the dollar change in the value of the CEO's option holdings for a 1% increase in stock return volatility. Option Vegas are calculated using Black–Scholes inputs and summed across all option holdings, which are then natural log-transformed. Results with missing values are dropped. (Constructed by: Coles, Daniel, and Naveen 2006, Updated Version).
Early Disclosure (Count)$_{i,t}$	Constructed from *Number of Annual and Quarterly Guidances*$_{i,t,}$ however, only counting earnings guidances in the first month of each quarter (IBES Guidance Detail)
Early Disclosure (Binary)$_{i,t}$	Constructed from *Choice to Guide*$_{i,t,}$ however, only including the choice to guide in the first month of each quarter (IBES Guidance Detail)
Late Amihud$_{i,t}$	Firm *i*'s average Amihud illiquidity over days 31 through the end of fiscal quarter *t*, calculated as the average daily ratio of absolute stock return to dollar trading volume using CRSP daily data, multiplied by 10^6. Higher values indicate lower liquidity. (Constructed using the CRSP Daily Stock File)
Log of Total Assets$_{i,t}$	The natural logarithm plus 1 of the firm's total assets at the end of quarter *t* for firm *i*. (Compustat Fundamentals Quarterly)
Book-to-Market$_{i,t}$	The ratio of the firm's book value of equity to its market value of equity at the end of quarter *t* for firm *i*. Book value is sourced from common equity and market value is calculated as the stock price multiplied by the number of shares. Observations with a book value of 0 or a market equity of less than 1 are dropped. (Compustat Fundamentals Quarterly)
Return on Assets$_{i,t}$	Net income divided by total assets for firm *i* in the quarter *t*. Total assets of 0 are dropped (Compustat Fundamentals Quarterly)
Leverage$_{i,t}$	The ratio of the firm's short term + long term debt to its total assets in the quarter t for firm *i*. If either short-term or long-term debt is missing, that specific value is labelled 0. If both short-term and long-term debt is missing, the entire observation is dropped. Total assets of 0 are dropped from the sample (Compustat Fundamentals Quarterly)
Log of Market Capitalization$_{i,t}$	The natural logarithm plus 1 of the market value of the firm's equity, calculated as share price multiplied by the number of shares outstanding in the quarter *t* for firm *i*. (Compustat Fundamentals Quarterly)
Number of Analysts$_{i,t}$	The total number of unique analysts making earnings forecasts during the quarter *t* for firm *i*. If there are no forecasts in the given quarter, the observation is denoted as 0, if no forecast is given at all in the entire sample, it is dropped. (IBES Detail Academic)

Qtr Stock Return$_{i,t}$	The cumulative stock return during the quarter *t* for firm *i*. The measure is calculated by compounding daily stock returns from the CRSP Daily Stock File over the firm-quarter. Specifically, daily returns are converted into a quarterly buy-and-hold return as the product of one plus each daily return during the quarter, minus one. Observations with missing stock prices are dropped. (CRSP Daily Stock File)
Stock Turnover in Prior Quarter$_{i,t-1}$	The cumulative daily stock volume over the quarter scaled by the number of shares outstanding, in the previous quarter (*t*-1) for firm *i*. (CRSP Daily Stock File)

Table 1: Descriptive Statistics for Quarters Q1 2013 Through Q4 2018

This table reports descriptive statistics for the firm-quarter variables used in the analysis. Each observation is at the firm-quarter level, denoted by *i* (firm) and *t* (quarter-year). The quarters of which *Newman* (Q4 2014) and *Salman* (Q4 2016) are adopted are not included in the sample. All control variables are defined in Appendix A. Median based variables may not have a median of 0.500 due to being calculated by firm and not firm-quarter.

Variable	*N*	*Mean*	*P25*	*Median*	*P75*
# of Annual and Quarterly Guidances$_{i,t}$	57,578	0.615	0.000	0.000	1.000
Choice to Guide$_{i,t}$	57,578	0.383	0.000	0.000	1.000
# of Press Releases$_{i,t}$	57,578	23.54	0.000	0.000	28.000
Choice to PR$_{i,t}$	57,578	0.404	0.000	0.000	1.000
Newman Treat$_i$	57,578	0.089	0.000	0.000	0.000
Newman Treat$_i$ x Post$_t$ but pre-Salman	57,578	0.029	0.000	0.000	0.000
Salman Treat$_i$ x Post$_t$	57,578	0.030	0.000	0.000	0.000
Inst. And HF Overlap$_i$	44,746	0.096	0.042	0.095	0.144
Inst Disclosure$_i$	57,578	0.000	-0.078	0.461	0.681
High Delta$_i$	30,739	0.526	0.000	1.000	1.000
High_Analyst$_i$	57,578	0.523	0.000	1.000	1.000
Log of Total Assets$_{i,t}$	55,803	7.403	6.001	7.473	8.809
Book to Market$_{i,t}$	51,341	0.590	0.252	0.444	0.722
Return on Assets$_{i,t}$	55,699	0.001	–0.001	0.007	0.018
Leverage$_{i,t}$	57,578	0.407	0.046	0.222	0.396
Log of Market Cap$_{i,t}$	54,391	7.257	5.962	7.378	8.669
Number of Analysts$_{i,t}$	57,578	4.423	1.000	3.000	6.000
Qtr Stock Return$_{i,t}$	46,534	0.026	-0.076	0.020	0.117
Stock Turnover in Prior Quarter$_{i,t-1}$	46,329	0.545	0.257	0.411	0.641
CEO_Vega$_{i,t}$	22,047	3.870	3.090	4.240	5.280
Late_Amihud$_{i,t}$	52,913	1.417	0.000	0.001	0.008

Table 2: Impact of *Newman* in Q4 2014 and Its Reversal in *Salman* Q4 2016 on Annual and Quarterly Management Earnings Guidance from Q1 2013 Through Q4 2018

This table reports regressions on the effect of *U.S. v. Newman* (Q4 2014) as well as its subsequent reversal (*U.S. v. Salman*; Q4 2016) on firms' annual and quarterly earnings guidance behavior. Index *i,t* denotes the firm-quarter observation from the sample, which covers the period: Q1 2013 through Q4 2018. The sample is divided into 3 separate time periods. The first period covers pre-*Newman* (before Q4 2014). The second period covers the period from the enactment of *Newman* to its reversal by *Salman* (from Q4 2014 through Q3 2016, inclusive). The third period covers the period post-*Salman* (after Q4 2016). The quarter adopting *Newman* (Q4 2014) and the quarter adopting *Salman* (Q4 2016) are dropped from the sample.

The dependent variable is either *# of Annual and Quarterly Guidances*$_{i,t}$, defined as the frequency of annual and quarterly management earnings guidances issued during the quarter, or *Choice to Guide*$_{i,t}$, an indicator equal to 1 if a firm issues at least one earnings guidance in the quarter and 0 otherwise. *Newman Treat*$_i$ *x Post*$_t$ *but Pre-Salman* equals one for treated firms in the post-*Newman* period before it is reversed with *Salman* (Specifically, the indicator is denoted as 1 if the headquarters of the company is located in the treated (2nd) Circuit from Q4 2014 through Q3 2016; if it is not in the Circuit or if it is outside of the time range it is denoted as 0). The main effect of *Newman Treat*$_i$ (by HQ location) and *Post*$_t$ are subsumed by the two-way fixed effects.

All control variables are defined in Appendix A. All specifications include *Gvkey* fixed effects and *Year-Quarter* fixed effects. Standard errors, reported in parentheses, are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Variable	*(1)* *# of Annual and Quarterly Guidances*$_{i,t}$	*(2)* *Choice to Guide*$_{i,t}$	*(3)* *# of Annual and Quarterly Guidances*$_{i,t}$	*(4)* *Choice to Guide*$_{i,t}$
Newman Treat$_i$ *x Post*$_t$ *but Pre-Salman*	0.074***	0.032***	0.089***	0.042***
	(0.010)	(0.007)	(0.011)	(0.008)
Log of Total Assets$_{i,t}$			−0.015	−0.011
			(0.027)	(0.014)
Book to Market$_{i,t}$			0.010**	0.005
			(0.005)	(0.003)
Return on Assets$_{i,t}$			0.066	0.029
			(0.050)	(0.032)
Leverage$_{i,t}$			0.107	0.061*
			(0.065)	(0.034)
Log of Market Cap$_{i,t}$			0.099***	0.062***
			(0.026)	(0.014)
Number of Analysts$_{i,t}$			0.020***	0.004***
			(0.003)	(0.001)
Qtr Stock Return$_{i,t}$			−0.075***	−0.038***
			(0.017)	(0.010)
Stock Turnover in Prior Quarter$_{i,t-1}$			−0.000	0.000
			(0.002)	(0.001)
Fixed Effects	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr
Observations	57,578	57,578	44,147	44,147
R^2	0.647	0.754	0.655	0.762
Adj. R^2	0.627	0.740	0.634	0.748
Period of Sample	Q1 2013-Q4 2018	Q1 2013-Q4 2018	Q1 2013-Q4 2018	Q1 2013-Q4 2018

Table 3, Panel A: Impact of Only *Newman* in Q4 2014 on Management Quarterly and Annual Earnings Guidance from Q1 2013 Through Q3 2016

This table reports regressions estimating the effect of only *U.S. v. Newman* on firms' annual and quarterly earnings guidance behavior. Index *i,t* denotes the firm-quarter observation from the sample period of Q1 2013 through Q3 2016. Unlike the main results, this table focuses on the first two periods: The first period is pre-*Newman* (before Q4 2014). The second period covers the enactment of *Newman* to its reversal by *Salman* (from Q4 2014 through Q3 2016, inclusive). These results include the quarter of the *Newman* transition (Q4 2014) for robustness, but not *Salman*. Results are robust when Q4 2014 is not included.

The dependent variable is either *# of Annual and Quarterly Guidances*$_{i,t}$, defined as the frequency of annual and quarterly management earnings guidance issued during the quarter, or *Choice to Guide*$_{i,t}$, an indicator equal to one if a firm issues at least one earnings guidance in the quarter and zero otherwise. *Newman Treat*$_i$ × *Post*$_t$ equals one for treated firms in the post-*Newman* period before it is reversed with *Salman* (Defined as 1 if the headquarters of the company is located in the treated (2nd) Circuit from Q4 2014 through Q3 2016; if it is not in the Circuit or if it is outside of the time range it is denoted as 0). The main effect of *Newman Treat*$_i$ (by HQ location) and *Post*$_t$ are subsumed by the two-way fixed effects.

All control variables are defined in Appendix A. All specifications include *Gvkey* fixed effects and *Year-Quarter* fixed effects. Standard errors, reported in parentheses, are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Variable	*(1)* *# of Annual and Quarterly Guidances*$_{i,t}$	*(2)* *Choice to Guide*$_{i,t}$	*(3)* *# of Annual and Quarterly Guidances*$_{i,t}$	*(4)* *Choice to Guide*$_{i,t}$
Newman Treat$_i$ *x Post*$_t$	0.084***	0.035***	0.103***	0.047***
	(0.014)	(0.013)	(0.015)	(0.012)
Log of Total Assets$_{i,t}$			0.029	−0.008
			(0.034)	(0.018)
Book to Market$_{i,t}$			0.003	0.001
			(0.007)	(0.006)
Return on Assets$_{i,t}$			0.063	0.061
			(0.087)	(0.043)
Leverage$_{i,t}$			0.068	0.054
			(0.119)	(0.067)
Log of Market Cap$_{i,t}$			0.068**	0.049***
			(0.031)	(0.018)
Number of Analysts$_{i,t}$			0.023***	0.004**
			(0.003)	(0.002)
Qtr Stock Return$_{i,t}$			−0.059***	−0.028**
			(0.019)	(0.013)
Stock Turnover in Prior Quarter$_{i,t-1}$			−0.001	−0.000
			(0.001)	(0.000)
Fixed Effects	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr
Observations	41,176	41,176	31,536	31,536
R^2	0.691	0.812	0.690	0.808
Adj. R^2	0.667	0.797	0.663	0.791
Period of Sample	Q1 2013-Q3 2016	Q1 2013-Q3 2016	Q1 2013-Q3 2016	Q1 2013-Q3 2016

Table 3, Panel B: Impact of Only *Salman* (Reversal of *Newman*) Q4 2016 on Annual and Quarterly Management Earnings Guidance from Q1 2015 Through Q4 2018

This table reports regressions estimating the effect of only *U.S. v. Salman* on firms' annual and quarterly earnings guidance behavior. Index *i,t* denotes the firm-quarter observation from the sample period of Q1 2015-Q4 2018. Unlike the main results, this table focuses on the latter two periods: periods 2 and 3. The second period covers the enactment of *Newman* to just before its reversal by *Salman* (from Q1 2015 through Q3 2016, inclusive), and the third period is post-Salman (after Q4 2016). As a robustness test, these results include the quarter of the *Salman* transition (Q4 2016), results are robust when it is excluded.

The dependent variable is either *# of Annual and Quarterly Earnings Guidances*$_{i,t}$, defined as the frequency of annual and quarterly management earnings guidances issued during the quarter, or *Choice to Guide*$_{i,t}$, an indicator equal to one if a firm issues at least one earnings guidance in the quarter and zero otherwise. *Salman Treat* × *Post*$_{i,t}$ equals one for treated firms in the post-*Salman* period (Defined as 1 if the headquarters of the company is located in the treated (2nd) Circuit from Q4 2016 through Q4 2018; if it is not in the Circuit or if it is outside of the time range, it is denoted as 0). The main effect of *Salman Treat*$_i$ (by HQ location) and *Post*$_t$ are subsumed by the two-way fixed effects.

All control variables are defined in Appendix A. All specifications include *Gvkey* fixed effects and *Year-Quarter* fixed effects. Standard errors, reported in parentheses, are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Variable	*(1)* *# of Annual and Quarterly Guidances*$_{i,t}$	*(2)* *Choice to Guide*$_{i,t}$	*(3)* *# of Annual and Quarterly Guidances*$_{i,t}$	*(4)* *Choice to Guide*$_{i,t}$
Salman Treat$_i$ *x Post*$_t$	−0.070***	−0.033***	−0.068***	−0.035***
	(0.014)	(0.005)	(0.012)	(0.006)
Log of Total Assets$_{i,t}$			−0.025	−0.004
			(0.036)	(0.017)
Book to Market$_{i,t}$			0.008*	0.004
			(0.005)	(0.003)
Return on Assets$_{i,t}$			0.072	0.033
			(0.050)	(0.031)
Leverage$_{i,t}$			0.204**	0.048
			(0.098)	(0.041)
Log of Market Cap$_{i,t}$			0.106***	0.057***
			(0.023)	(0.012)
Number of Analysts$_{i,t}$			0.020***	0.006***
			(0.003)	(0.001)
Qtr Stock Return$_{i,t}$			−0.083***	−0.039***
			(0.016)	(0.011)
Stock Turnover in Prior Quarter$_{i,t-1}$			−0.003	−0.001
			(0.003)	(0.001)
Fixed Effects	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr
Observations	40,041	40,041	30,887	30,887
R^2	0.685	0.789	0.685	0.791
Adj. R^2	0.661	0.773	0.660	0.775
Period of Sample	Q1 2015-Q4 2018	Q1 2015-Q4 2018	Q1 2015-Q4 2018	Q1 2015-Q4 2018

Table 4: Impact of *Newman* Q4 2014 and Its Reversal in *Salman* Q4 2016 on Press Releases from Q1 2013 Through Q4 2018

This table reports regressions on the effect of *U.S. v. Newman* (Q4 2014) as well as its subsequent reversal (*U.S. v. Salman*; Q4 2016) on firms' disclosure behavior via Press Releases. Index *i,t* denotes the firm-quarter observation from the sample, which covers the period: Q1 2013 through Q4 2018. The sample is divided into 3 separate time periods. The first period covers pre-*Newman* (before Q4 2014). The second period covers the period from the enactment of Newman to its reversal by *Salman* (from Q4 2014 through Q3 2016, inclusive). The third period covers the period post-Salman (after Q4 2016). The quarter adopting *Newman* (Q4 2014) and the quarter adopting *Salman* (Q4 2016) are dropped from the sample.

The dependent variable is either *# of Press Releases*$_{i,t}$, defined as the frequency of Press Releases issued during the quarter, or *Choice to PR*$_{i,t}$, an indicator equal to 1 if a firm issues at least one press release in the quarter and 0 otherwise. *Newman Treat*$_i$ *x Post*$_t$ *but Pre-Salman* equals one for treated firms in the post-*Newman* period before it is reversed with *Salman* (Specifically, this is denoted as 1 if the headquarters of the company is located in the treated (2nd) Circuit from Q4 2014 through Q3 2016; if it is not in the Circuit or if it is outside of the time range it is denoted as 0). The main effect of *Newman Treat*$_i$ (by HQ location) and *Post*$_t$ are subsumed by the two-way fixed effects.

All control variables are defined in Appendix A. All specifications include *Gvkey* fixed effects and *Year-Quarter* fixed effects. Standard errors, reported in parentheses, are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Variable	*(1)* *# of Press Releases*$_{i,t}$	*(2)* *Choice to PR*$_{i,t}$	*(3)* *# of Press Releases*$_{i,t}$	*(4)* *Choice to PR*$_{i,t}$
Newman Treat$_i$ *x Post*$_t$ *but Pre-Salman*	0.856** (0.393)	0.004*** (0.001)	0.434 (0.376)	0.001** (0.000)
Log of Total Assets$_{i,t}$			2.222*** (0.423)	−0.000* (0.000)
Book to Market$_{i,t}$			0.145** (0.065)	−0.000 (0.000)
Return on Assets$_{i,t}$			-2.862*** (1.009)	0.000 (0.000)
Leverage$_{i,t}$			1.374 (1.218)	0.001 (0.001)
Log of Market Cap$_{i,t}$			-0.101 (0.263)	0.000 (0.000)
Number of Analysts$_{i,t}$			0.021 (0.047)	0.000 (0.000)
Qtr Stock Return$_{i,t}$			1.391*** (0.212)	0.000 (0.001)
Stock Turnover in Prior Quarter$_{i,t-1}$			0.065* (0.037)	0.000 (0.000)
Fixed Effects	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr
Observations	57,578	57,578	44,147	44,147
R^2	0.948	0.990	0.951	0.999
Adjusted R^2	0.945	0.990	0.948	0.999
Period of Sample	Q1 2013-Q4 2018	Q1 2013-Q4 2018	Q1 2013-Q4 2018	Q1 2013-Q4 2018

Table 5, Panel A: Impact of *Newman* Q4 2014 and Its Reversal in *Salman* Q4 2016 on # of Annual and Quarterly Management Earnings Guidance from Q1 2013 Through Q4 2018 for Firms with Varying Levels of Institutional Investors and Hedge Funds

This table reports regressions on the effect of *U.S. v. Newman* (Q4 2014) as well as its subsequent reversal (*U.S. v. Salman*; Q4 2016) on firms' annual and quarterly earnings guidance behavior in a subsample analysis. The sample is divided into four subsamples based on pre-Newman ownership structure. For each firm, we average hedge fund ownership and non-hedge-fund institutional ownership over Q1 2013 through Q3 2014, measured as percentages of shares outstanding. Firms are classified as high or low on each measure using the corresponding pre-Newman sample median, yielding four groups: Low HF / Low Inst, Low HF / High Inst, High HF / Low Inst, and High HF / High Inst.

Index *i,t* denotes the firm-quarter observation from the sample, which covers the period: Q1 2013 through Q4 2018. The sample is divided into 3 separate time periods. The first period covers the period pre-*Newman* (before Q4 2014). The second period covers the period from the enactment of Newman to its reversal by *Salman* (from Q4 2014 through Q3 2016, inclusive). The third period covers the period post-Salman (after Q4 2016). The quarter adopting *Newman* (Q4 2014) and *Salman* (Q4 2016) are dropped from the sample.

The dependent variable is *# of Annual and Quarterly Guidances*$_{i,t}$, defined as the frequency of annual and quarterly management earnings guidances issued during the quarter. *Newman Treat*$_i$ *x Post*$_t$ *but Pre-Salman* equals one for treated firms in the post-*Newman* period before it is reversed with *Salman* (Specifically, this is denoted as 1 if the headquarters of the company is located in the treated (2nd) Circuit from Q4 2014 through Q3 2016; if it is not in the Circuit or if it is outside of the time range it is denoted as 0). The main effect of *Newman Treat*$_i$ (by HQ location) and *Post*$_t$ are subsumed by the two-way fixed effects.

All control variables are defined in Appendix A. All specifications include *Gvkey* fixed effects and *Year-Quarter* fixed effects. Standard errors, reported in parentheses, are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Subsample	*(1)* *Low HF, Low Inst. Investors*	*(2)* *Low HF, High Inst. Investors*	*(3)* *High HF, Low Inst. Investors*	*(4)* *High HF, High Inst. Investors*
Variable	*# of Ann. and Qtr. Guidances$_{i,t}$*	*# of Ann. and Qtr. Guidances$_{i,t}$*	*# of Ann. and Qtr. Guidances$_{i,t}$*	*# of Ann. and Qtr. Guidances$_{i,t}$*
Newman Treat$_i$ x Post$_t$ but Pre-Salman	0.031	0.010	0.105	0.136 ***
	(0.024)	(0.034)	(0.087)	(0.017)
Log of Total Assets$_{i,t}$	-0.017	0.045	-0.127	0.019
	(0.047)	(0.075)	(0.104)	(0.043)
Book to Market$_{i,t}$	-0.007	-0.137	0.068	0.016 **
	(0.016)	(0.156)	(0.070)	(0.006)
Return on Assets$_{i,t}$	0.005	0.112	-0.020	0.138
	(0.044)	(0.305)	(0.227)	(0.154)
Leverage$_{i,t}$	0.290 **	-0.425	0.177	0.103
	(0.138)	(0.286)	(0.191)	(0.125)
Log of Market Cap$_{i,t}$	0.095 ***	0.080	0.100	0.087 **
	(0.028)	(0.083)	(0.066)	(0.034)
Number of Analysts$_{i,t}$	0.021 ***	0.022 ***	0.013	0.022 ***
	(0.007)	(0.004)	(0.009)	(0.005)
Qtr Stock Return$_{i,t}$	-0.082 ***	-0.166 **	-0.040	-0.049 *
	(0.017)	(0.061)	(0.088)	(0.027)
Stock Turnover in Prior Quarter$_{i,t-1}$	0.006	-0.046	0.098 *	-0.037
	(0.005)	(0.077)	(0.049)	(0.024)
Fixed Effects	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr
Observations	8,026	5,735	2,847	19,710
R^2	0.635	0.631	0.677	0.638
Adj. R^2	0.611	0.608	0.652	0.616
Period of Sample	Q1 2013-Q4 2018	Q1 2013-Q4 2018	Q1 2013-Q4 2018	Q1 2013-Q4 2018

Table 5, Panel B: Impact of *Newman* Q4 2014 and Its Reversal in *Salman* Q4 2016 on Choice to Guide from Q1 2013 Through Q4 2018 for Firms with Varying Levels of Institutional Investors and Hedge Funds

This table reports regressions on the effect of *U.S. v. Newman* (Q4 2014) as well as its subsequent reversal (*U.S. v. Salman*; Q4 2016) on firms' annual and quarterly earnings guidance behavior in a subsample analysis. The sample is divided into four subsamples based on pre-Newman ownership structure. For each firm, we average hedge fund ownership and non-hedge-fund institutional ownership over Q1 2013 through Q3 2014, measured as percentages of shares outstanding. Firms are classified as high or low on each measure using the corresponding pre-Newman sample median, yielding four groups: Low HF / Low Inst, Low HF / High Inst, High HF / Low Inst, and High HF / High Inst.

Index *i,t* denotes the firm-quarter observation from the sample, which covers the period: Q1 2013 through Q4 2018. The sample is divided into 3 separate time periods. The first period covers the period pre-*Newman* (before Q4 2014). The second period covers the period from the enactment of Newman to its reversal by *Salman* (from Q4 2014 through Q3 2016, inclusive). The third period covers the period post-Salman (after Q4 2016). The quarter adopting *Newman* (Q4 2014) and *Salman* (Q4 2016) are dropped from the sample.

The dependent variable is *Choice to Guide*$_{i,t}$, an indicator equal to one if a firm issues at least one earnings guidance in the quarter and zero otherwise. *Newman Treat*$_i$ *x Post*$_t$ *but Pre-Salman* equals one for treated firms in the post-*Newman* period before it is reversed with *Salman* (Specifically, this is denoted as 1 if the headquarters of the company is located in the treated (2nd) Circuit from Q4 2014 through Q3 2016; if it is not in the Circuit or if it is outside of the time range it is denoted as 0). The main effect of *Newman Treat*$_i$ (by HQ location) and *Post*$_t$ are subsumed by the two-way fixed effects.

All control variables are defined in Appendix A. All specifications include *Gvkey* fixed effects and *Year-Quarter* fixed effects. Standard errors, reported in parentheses, are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Subsample	*(1)* *Low HF, Low Inst. Investors*	*(2)* *Low HF, High Inst. Investors*	*(3)* *High HF, Low Inst. Investors*	*(4)* *High HF, High Inst. Investors*
Variable	*Choice to Guide$_{i,t}$*	*Choice to Guide$_{i,t}$*	*Choice to Guide$_{i,t}$*	*Choice to Guide$_{i,t}$*
Newman Treat$_i$ x Post$_t$ but Pre-Salman	0.006	0.000	0.090	0.065 ***
	(0.017)	(0.014)	(0.100)	(0.020)
Log of Total Assets$_{i,t}$	0.003	-0.067 *	-0.058	0.016
	(0.017)	(0.035)	(0.069)	(0.022)
Book to Market$_{i,t}$	-0.009	0.002	0.029	0.008 **
	(0.010)	(0.074)	(0.044)	(0.004)
Return on Assets$_{i,t}$	-0.001	0.166	-0.044	0.029
	(0.036)	(0.159)	(0.120)	(0.103)
Leverage$_{i,t}$	0.148 *	-0.171	0.094	0.051
	(0.079)	(0.137)	(0.123)	(0.062)
Log of Market Cap$_{i,t}$	0.060 ***	0.063	0.075	0.056 ***
	(0.014)	(0.048)	(0.053)	(0.017)
Number of Analysts$_{i,t}$	0.008 *	0.005 *	0.007	0.004 *
	(0.004)	(0.003)	(0.005)	(0.002)
Qtr Stock Return$_{i,t}$	-0.051 ***	-0.038	0.009	-0.033 **
	(0.009)	(0.030)	(0.037)	(0.015)
Stock Turnover in Prior Quarter$_{i,t-1}$	0.003	0.010	0.048	-0.017
	(0.003)	(0.038)	(0.030)	(0.014)
Fixed Effects	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr
Observations	8,026	5,735	2,847	19,710
R^2	0.728	0.772	0.711	0.781
Adj. R^2	0.710	0.758	0.689	0.768
Period of Sample	Q1 2013-Q4 2018	Q1 2013-Q4 2018	Q1 2013-Q4 2018	Q1 2013-Q4 2018

Table 6, Impact of *Newman* Q4 2014 and Its Reversal in *Salman* Q4 2016 on Annual and Quarterly Management Earnings Guidance from Q1 2013 Through Q4 2018 for Firms with Overlapping Institutional Investors and Hedge Funds

This table reports regressions on the effect of *U.S. v. Newman* (Q4 2014) as well as its subsequent reversal (*U.S. v. Salman*; Q4 2016) on firms' annual and quarterly earnings guidance behavior, while allowing for cross-sectional heterogeneity based on the pre-*Newman* period (Q1 2013 through Q3 2014) existence of both hedge funds and institutional shareholders. Index *i,t* denotes the firm-quarter observation from the sample, which covers the period: Q1 2013 through Q4 2018. The sample is divided into 3 separate time periods. The first period covers the period pre-*Newman* (before Q4 2014). The second period covers the period from the enactment of Newman to its reversal by *Salman* (from Q4 2014 through Q3 2016, inclusive). The third period covers the period post-Salman (after Q4 2016). The quarter adopting *Newman* (Q4 2014) and the quarter adopting *Salman* (Q4 2016) are dropped from the sample.

The dependent variable is either $\#\ of\ Annual\ and\ Quarterly\ Guidances_{i,t}$, defined as the frequency of annual and quarterly management earnings guidance issued during the quarter, or $Choice\ to\ Guide_{i,t}$, an indicator equal to one if a firm issues at least one earnings guidance in the quarter and zero otherwise. $Newman\ Treat_i\ x\ Post_t\ but\ Pre\text{-}Salman$ equals one for treated firms in the post-*Newman* period before it is reversed with *Salman* (Specifically, this is denoted as 1 if the headquarters of the company is located in the treated (2^{nd}) Circuit from Q4 2014 through Q3 2016; if it is not in the Circuit or if it is outside of the time range it is denoted as 0). The main effect of $Newman\ Treat_i$ (by HQ location) and $Post_t$ are subsumed by the two-way fixed effects.

All control variables are defined in Appendix A. All specifications include *Gvkey* fixed effects and *Year-Quarter* fixed effects. Standard errors, reported in parentheses, are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Variable	*(1)* *# of Annual and Quarterly Guidances$_{i,t}$*	*(2)* *Choice to Guide$_{i,t}$*	*(3)* *# of Annual and Quarterly Guidances$_{i,t}$*	*(4)* *Choice to Guide$_{i,t}$*
New.Treat$_i$ x Post$_t$ but Pre-Salman x Inst. And HF Overlap$_i$	0.342 ** (0.158)	0.219 *** (0.048)	0.572 *** (0.137)	0.378 *** (0.108)
Newman Treat$_i$ x Post$_t$ but Pre-Salman	0.048 *** (0.011)	0.013 (0.009)	0.039 *** (0.013)	0.008 (0.009)
Log of Total Assets$_{i,t}$			-0.012 (0.030)	-0.002 (0.013)
Book to Market$_{i,t}$			0.006 (0.009)	0.001 (0.006)
Return on Assets$_{i,t}$			0.057 (0.055)	0.019 (0.039)
Leverage$_{i,t}$			0.085 (0.070)	0.047 (0.040)
Log of Market Cap$_{i,t}$			0.094 *** (0.026)	0.060 *** (0.014)
Number of Analysts$_{i,t}$			0.021 *** (0.003)	0.004 *** (0.001)
Qtr Stock Return$_{i,t}$			-0.076 *** (0.018)	-0.039 *** (0.010)
Stock Turnover in Prior Quarter$_{i,t-1}$			0.003 (0.004)	0.002 (0.002)
Fixed Effects	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr
Observations	44,746	44,746	36,318	36,318
R^2	0.643	0.770	0.652	0.778
Adj. R^2	0.623	0.757	0.632	0.765
Period of Sample	Q1 2013-Q4 2018	Q1 2013-Q4 2018	Q1 2013-Q4 2018	Q1 2013-Q4 2018

Table 7: Impact of *Newman* Q4 2014 and Its Reversal in *Salman* Q4 2016 on Annual and Quarterly Management Earnings Guidance from Q1 2013 Through Q4 2018 when Non-Hedge Fund Institutional Investors have Other Disclosing Firms

This table reports regressions on the effect of *U.S. v. Newman* (Q4 2014) as well as its subsequent reversal (*U.S. v. Salman*; Q4 2016) on firms' annual and quarterly earnings guidance behavior, while allowing for cross-sectional heterogeneity based on Q3 2014 ownership of institutional investors with other disclosing companies. Index *i,t* denotes the firm-quarter observation from the sample, which covers the period: Q1 2013 through Q4 2018. The sample is divided into 3 separate time periods. The first period covers the period pre-*Newman* (before Q4 2014). The second period covers the period from the enactment of Newman to its reversal by *Salman* (from Q4 2014 through Q3 2016, inclusive). The third period covers the period post-Salman (after Q4 2016). The quarter adopting *Newman* (Q4 2014) and the quarter adopting *Salman* (Q4 2016) are dropped from the sample.

The dependent variable is either # *of Annual and Quarterly Guidances*$_{i,t}$, defined as the frequency of annual and quarterly management earnings guidance issued during the quarter, or *Choice to Guide*$_{i,t}$, an indicator equal to one if a firm issues at least one earnings guidance in the quarter and zero otherwise. *Newman Treat*$_i$ *x Post*$_t$ *but Pre-Salman* equals one for treated firms in the post-*Newman* period before it is reversed with *Salman* (Specifically, this is denoted as 1 if the headquarters of the company is located in the treated (2nd) Circuit from Q4 2014 through Q3 2016; if it is not in the Circuit or if it is outside of the time range it is denoted as 0). The main effect of *Newman Treat*$_i$ (by HQ location) and *Post*$_t$ are subsumed by the two-way fixed effects.

All control variables are defined in Appendix A. All specifications include *Gvkey* fixed effects and *Year-Quarter* fixed effects. Standard errors, reported in parentheses, are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Variable	*(1)* *# of Annual and Quarterly Guidances$_{i,t}$*	*(2)* *Choice to Guide$_{i,t}$*	*(3)* *# of Annual and Quarterly Guidances$_{i,t}$*	*(4)* *Choice to Guide$_{i,t}$*
New.Treat$_i$ x Post$_t$ but Pre-Salman x Inst Disclosure$_i$	0.054 ***	0.026 ***	0.205 ***	0.095 ***
	(0.012)	(0.002)	(0.033)	(0.017)
Newman Treat$_i$ x Post$_t$ but Pre-Salman	0.070 ***	0.030 ***	-0.028 *	-0.015
	(0.008)	(0.007)	(0.015)	(0.019)
Log of Total Assets$_{i,t}$			0.008	0.007
			(0.032)	(0.016)
Book to Market$_{i,t}$			0.013 **	0.005
			(0.006)	(0.004)
Return on Assets$_{i,t}$			0.054	0.034
			(0.054)	(0.037)
Leverage$_{i,t}$			0.148 *	0.071 *
			(0.075)	(0.038)
Log of Market Cap$_{i,t}$			0.090 ***	0.055 ***
			(0.026)	(0.015)
Number of Analysts$_{i,t}$			0.022 ***	0.005 ***
			(0.003)	(0.001)
Qtr Stock Return$_{i,t}$			-0.083 ***	-0.040 ***
			(0.022)	(0.012)
Stock Turnover in Prior Quarter$_{i,t-1}$			-0.000	0.002
			(0.011)	(0.006)
Institutional Ownership in Prior Quarter $_{i,t-1}$			0.093	0.056
			(0.070)	(0.036)
Fixed Effects	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr
Observations	57,578	57,578	34,537	34,537
R^2	0.648	0.754	0.657	0.779
Adj. R^2	0.627	0.740	0.634	0.764
Period of Sample	Q1 2013-Q4 2018	Q1 2013-Q4 2018	Q1 2013-Q4 2018	Q1 2013-Q4 2018

Table 8: Impact of *Newman* in Q4 2014 and Its Reversal in *Salman* Q4 2016 on Annual and Quarterly Management Earnings Guidance from Q1 2013 Through Q4 2018, For Firms with High Number of Analysts and High Delta

This table reports regressions on the effect of *U.S. v. Newman* (Q4 2014) as well as its subsequent reversal (*U.S. v. Salman*; Q4 2016) on firms' annual and quarterly earnings guidance behavior, while allowing for cross-sectional heterogeneity based on managerial incentives and investor information demand. Index *i,t* denotes the firm-quarter observation from the sample, which covers the period: Q1 2013 through Q4 2018. The sample is divided into 3 separate time periods. The first period covers pre-*Newman* (before Q4 2014). The second period covers the period from the enactment of *Newman* to its reversal by *Salman* (from Q4 2014 through Q3 2016, inclusive). The third period covers the period post-*Salman* (after Q4 2016). The quarter adopting *Newman* (Q4 2014) and the quarter adopting *Salman* (Q4 2016) are dropped from the sample.

The dependent variable is either # *of Annual and Quarterly Guidances*$_{i,t}$, defined as the frequency of annual and quarterly management earnings guidances issued during the quarter, or *Choice to Guide*$_{i,t}$, an indicator equal to 1 if a firm issues at least one earnings guidance in the quarter and 0 otherwise. *Newman Treat*$_i$ *x Post*$_t$ *but Pre-Salman* equals one for treated firms in the post-*Newman* period before it is reversed with *Salman* (Specifically, the indicator is denoted as 1 if the headquarters of the company is located in the treated (2nd) Circuit from Q4 2014 through Q3 2016; if it is not in the Circuit or if it is outside of the time range it is denoted as 0). The main effect of *Newman Treat*$_i$ (by HQ location) and *Post*$_t$ are subsumed by the two-way fixed effects.

To examine cross-sectional variation, *Newman Treat*$_i$ *x Post*$_t$ *but Pre-Salman* is interacted with indicators for above-median analyst coverage (*High Analyst*$_i$) and above-median CEO Delta (*High Delta*$_i$), each constructed using firm-level averages over the pre-Newman period. High Delta is an indicator equal to one if a firm's average CEO Delta in the pre-period is above the sample median, and High Analyst is an indicator equal to one if a firm's average analyst coverage in the pre-period is above the sample median.

All control variables are defined in Appendix A. All specifications include *Gvkey* fixed effects and *Year-Quarter* fixed effects. Standard errors, reported in parentheses, are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Variable	*(1)* *# of Annual and Quarterly Guidances$_{i,t}$*	*(2)* *Choice to Guide$_{i,t}$*	*(3)* *# of Annual and Quarterly Guidances$_{i,t}$*	*(4)* *Choice to Guide$_{i,t}$*
Newman Treat$_i$ x Post$_t$ but Pre-Salman x High Analyst$_i$	0.096 ***	0.030 **		
	(0.009)	(0.012)		
Newman Treat$_i$ x Post$_t$ but Pre-Salman x High Delta$_i$			0.156 *	0.026 ***
			(0.086)	(0.005)
Newman Treat$_i$ x Post$_t$ but Pre-Salman	0.033 ***	0.025 ***	0.019	0.019 **
	(0.011)	(0.006)	(0.019)	(0.008)
Log of Total Assets$_{i,t}$	-0.001	-0.008	-0.053	-0.013
	(0.027)	(0.014)	(0.040)	(0.021)
Book-to-Market$_{i,t}$	0.009 *	0.004	0.001	-0.003
	(0.005)	(0.004)	(0.018)	(0.009)
Return on Assets$_{i,t}$	0.058	0.030	0.177	0.026
	(0.048)	(0.032)	(0.131)	(0.083)
Leverage$_{i,t}$	0.111 *	0.060 *	0.141	0.054
	(0.066)	(0.034)	(0.149)	(0.054)
Log of Market Cap$_{i,t}$	0.104 ***	0.063 ***	0.082 **	0.041 *
	(0.025)	(0.014)	(0.038)	(0.021)
Number of Analysts$_{i,t}$			0.022 ***	0.003
			(0.004)	(0.002)
Qtr Stock Return$_{i,t}$	-0.085 ***	-0.040 ***	-0.068 **	-0.029 **
	(0.017)	(0.010)	(0.031)	(0.012)
Stock Turnover in Prior Quarter$_{i,t-1}$	0.001	0.000	-0.004	0.012
	(0.002)	(0.001)	(0.028)	(0.018)
CEO Vega$_{i,t}$			-0.010	0.000
			(0.007)	(0.002)
Fixed Effects	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr
Observations	43,970	43,970	17,600	17,600
R^2	0.654	0.762	0.651	0.808
Adj. R^2	0.633	0.747	0.627	0.794
Period of Sample	Q1 2013-Q4 2018	Q1 2013-Q4 2018	Q1 2013-Q4 2018	Q1 2013-Q4 2018

Table 9: Impact of Newman in Q4 2014 and Its Reversal in Salman Q4 2016 on Late-Quarter Stock Illiquidity from Q1 2013 Through Q4 2018, For Disclosing Firms in Quarter

This table reports regressions on the effect of *U.S. v. Newman* (Q4 2014) as well as its subsequent reversal (*U.S. v. Salman*; Q4 2016) on firms' late-quarter stock illiquidity, while allowing for cross-sectional heterogeneity based on firms' choice to disclose in the first month of each quarter. Index *i,t* denotes the firm-quarter observation from the sample, which covers the period: Q1 2013 through Q4 2018. The sample is divided into 3 separate time periods. The first period covers pre-*Newman* (before Q4 2014). The second period covers the period from the enactment of *Newman* to its reversal by *Salman* (from Q4 2014 through Q3 2016, inclusive). The third period covers the period post-*Salman* (after Q4 2016). The quarter adopting *Newman* (Q4 2014) and the quarter adopting *Salman* (Q4 2016) are dropped from the sample.

The dependent variable is *Late Amihud*$_{i,t}$, defined as the the average Amihud illiquidity in months 2 and 3 of each quarter. *Newman Treat*$_i$ *x Post*$_t$ *but Pre-Salman* equals one for treated firms in the post-*Newman* period before it is reversed with *Salman* (Specifically, the indicator is denoted as 1 if the headquarters of the company is located in the treated (2nd) Circuit from Q4 2014 through Q3 2016; if it is not in the Circuit or if it is outside of the time range it is denoted as 0). The main effect of *Newman Treat*$_i$ (by HQ location) and *Post*$_t$ are subsumed by the two-way fixed effects.

To examine cross-sectional variation, *Newman Treat*$_i$ *x Post*$_t$ *but Pre-Salman* is interacted with *Early Disclosure (Count)*, defined as each firm's number of disclosures in the first month of each quarter, as well as *Early Disclosure (Binary)*, defined as an indicator that's 1 if the firm makes any disclosure in the first month of each quarter and 0 otherwise.

All control variables are defined in Appendix A. All specifications include *Gvkey* fixed effects and *Year-Quarter* fixed effects. Standard errors, reported in parentheses, are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Variable	*(1) Late Amihud$_{i,t}$*	*(2) Late Amihud$_{i,t}$*	*(3) Late Amihud$_{i,t}$*	*(4) Late Amihud$_{i,t}$*
Newman Treat$_i$ x Post$_t$ but Pre-Salman × Early Disclosure (Count)$_{i,t}$	-1.902***		-2.080***	
	(0.622)		(0.676)	
Newman Treat$_i$ x Post$_t$ but Pre-Salman × Early Disclosure (Binary)$_{i,t}$		-2.177***		-2.376***
		(0.643)		(0.691)
Early Disclosure (Count)$_{i,t}$	0.023		0.052	
	(0.105)		(0.131)	
Early Disclosure (Binary)$_{i,t}$		0.027		0.057
		(0.126)		(0.157)
Newman Treat$_i$ x Post$_t$ but Pre-Salman	1.574*	1.602*	2.000**	2.029**
	(0.858)	(0.848)	(0.978)	(0.966)
Early Amihud $_{i,t}$			0.201**	0.201**
			(0.088)	(0.088)
Log of Total Assets$_{i,t}$			-0.061	-0.061
			(0.518)	(0.518)
Book-to-Market$_{i,t}$			0.346	0.346
			(0.481)	(0.481)
Return on Assets$_{i,t}$			-16.225	-16.227
			(10.080)	(10.080)
Leverage$_{i,t}$			-1.861	-1.861
			(1.263)	(1.263)
Log of Market Cap$_{i,t}$			-0.717	-0.716
			(0.605)	(0.606)
Number of Analysts$_{i,t}$			-0.033*	-0.033*
			(0.019)	(0.019)
Qtr Stock Return$_{i,t}$			-1.536	-1.537
			(1.161)	(1.161)
Stock Turnover in Prior Quarter$_{i,t-1}$			-0.313**	-0.313**
			(0.153)	(0.153)
Fixed Effects	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr	Gvkey, Year-Qtr
Observations	52,913	52,913	44,134	44,134
R^2	0.213	0.213	0.223	0.223
Adj. R^2	0.167	0.167	0.175	0.175
Period of Sample	Q1 2013-Q4 2018	Q1 2013-Q4 2018	Q1 2013-Q4 2018	Q1 2013-Q4 2018