The Dark Side of Geographically Dispersed Information: Evidence from Lockdown of Subsidiaries[1]

August 28, 2025

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Abstract

We examine how geographically dispersed subsidiary information affects a firm's information environment using the pandemic-induced, localized lockdowns of subsidiaries that restricted analysts' access to local information. Using detailed geolocation data, we find that restricting subsidiary access reduces forecast errors and underreaction bias for analysts located near affected subsidiaries. This attention shift away from local subsidiary signals aggregates to more accurate consensus forecasts, which increases stock price informativeness, diminishes insiders' trading advantages, and improves stock liquidity, without affecting firm fundamentals. Our findings support the bias hypothesis: localized subsidiary information can distort attention and delay information incorporation, and restricting such access fosters more efficient and symmetric markets.

Keywords: Subsidiaries, Geographically-Dispersed Information, Analysts, Liquidity, Price Informativeness, Insiders, COVID-19.

JEL Codes: G12, G22, G23, G32

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[1] The authors thank Zhao Jin, Enrichetta Ravina, Honglin Ren, Wensi Xie, Zhen Zhou, and participants at MFA, CICF, RMBS Summer Forum, University of Edinburgh, University of Missouri, and University of Cincinnati for helpful comments and suggestions.

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Introduction

Information is fundamental to financial markets, yet the origins and aggregation of information remain imperfectly understood. A growing literature recognizes that firms operate through geographically dispersed units—subsidiaries, affiliates, plants—creating localized information flows that may differ sharply from those available at corporate headquarters (García and Norli, 2012; Bernile, Kumar, and Sulaeman, 2015; Addoum, Kumar, and Law, 2020).[1] Such localized information, obtained through interactions with local employees, customers, suppliers, and regulators, has the potential to improve market participants' understanding of firm fundamentals. However, it may also distort perceptions if attention to local signals crowds out broader firm-level insights. This tension raises a central open question: does access to geographically dispersed information sources enhance a firm's overall information environment, or does it bias attention and distort information processing?

Access to localized information from geographically dispersed operations can affect the firm's information environment through two competing mechanisms. The information advantage view holds that proximity to subsidiary information provides unique, complementary insights, gathered through direct interactions and observations, that when aggregated, improve analyst forecasts accuracy and stock price informativeness (e.g., Cheng, Du, Wang, and Wang, 2016)). This perspective is akin to a Christmas tree whose many subsidiary "lights" collectively illuminate the firm's overall picture. In contrast, the bias/attention allocation view predicts that investors and analysts with local access may overemphasize subsidiary-specific signals (e.g., Kang, Stice-Lawrence, and Wong, 2021) while neglecting broader firm-level information, distorting aggregation and delaying price adjustments (Sims, 2003, 2006; Veldkamp, 2006). Here, the Christmas tree analogy flips: focusing too intently on individual lights distracts from seeing the whole tree. Distinguishing between these views has important implications: the information hypothesis implies that greater geographic dispersion of signals enhances informational efficiency, while the bias hypothesis suggests the opposite, that efficiency is maximized when information is less geographically dispersed. This distinction has normative and policy relevance, particularly for regulations that determine the availability of disaggregated subsidiary- or plant-level data. This question is economically important because firms with subsidiaries dominate public markets: in our sample, over 80% of U.S. market capitalization belongs to firms with multiple, predominantly private subsidiaries operating across several states and industries. [2]

[1] Coval and Moskowitz (2001) and Bernstein, Giroud, and Townsend (2016) examine information advantages arising from geographic proximity to corporate headquarters, rather than to geographically dispersed subsidiaries.

[2] In our sample, the average firm operates ten subsidiaries across six states and ten ZIP codes. Nearly all subsidiaries are private, with firms holding subsidiaries in two to three industries on average. This widespread geographic and industry dispersion creates significant localized information, which may not be representative of the firm-level performance.

Studying the effects of geographically dispersed information poses significant challenges because such information is often private, idiosyncratic, and endogenous to firm characteristics.[3] To overcome these challenges, we exploit the COVID-19 pandemic's localized subsidiary lockdowns as a natural experiment. These lockdowns represent plausibly exogenous shocks that restrict physical access to subsidiary-level information for specific market participants, effectively "switching off" the local subsidiary lights on the Christmas tree of information, while leaving other sources of firm information unaffected. We measure lockdown intensity using reductions in foot traffic at subsidiary locations. Using analyst- and subsidiary-level geolocation data and a generalised difference-in-difference approach, we compare forecasts of the same firm by analysts located near affected subsidiaries with (i) analysts located near unaffected subsidiaries (ii) analysts located near the firm's headquarters, and (iii) analysts far from both headquarters and subsidiaries combined and separately. This detailed comparison isolates the effect of restricted access to local subsidiary information from broader pandemic-related shocks. We first examine the impact on individual analysts' forecast accuracy and bias, then aggregate to the firm level to study implications for consensus forecasts, stock price informativeness, insider trading profitability, and market liquidity. Because our analyses compare within-firm outcomes across heterogeneous geographic exposures, our identification strategy mitigates concerns that the results are driven by unobserved time-varying firm factors or general COVID-19 disruptions rather than the loss of subsidiary-specific access.

Our first set of results shows that restricting access to subsidiary-level information improves forecast performance for analysts located near the affected subsidiaries. In our difference-in-differences framework, these nearby analysts experience a 3.6% reduction in forecast errors relative to the sample mean, with no comparable effect for analysts located near headquarters or far from both headquarters and subsidiaries. When nearby subsidiaries are locked down, the magnitude of analysts' forecast revisions following earnings announcements decreases (a 0.63% reduction relative to the mean), reflecting less over-precision in pre-lockdown forecasts and greater reliance on broader firm-level signals.

Consistent with the bias/attention allocation hypothesis, we find that restricting subsidiary-level information via localized lockdowns improves the forecast accuracy of analysts located near affected subsidiaries. Across our difference-in-differences specifications, these analysts experience a 4%–19% reduction in forecast errors relative to analysts located far from headquarters but near unaffected subsidiaries, or far from both headquarters and subsidiaries. The effect is absent when headquarters are

[3] While subsidiaries operating in different industries introduce elements of conglomerate discount, our focus is fundamentally different. Conglomerate discount refers to valuation effects arising from diversification and inefficiencies in conglomerates' resource allocation across divisions. In contrast, our study centres on the flow of localized, subsidiary-level information and its impact on the firm's overall information environment.

locked down, underscoring that it is subsidiary-specific information, not general pandemic disruption, that drive the result.

Our identification strategy rules out broader COVID-19 pandemic shocks and a set of alternative explanations by including firm-by-time and analyst-by-time fixed effects. Firm-by-time fixed effects isolate within-firm variations, addressing firm-specific influences such as differential impacts of government support (e.g., CARES Act) or operational fragility related to subsidiary structures. Analyst-by-time fixed effects control analyst-specific concerns, including superior insights into COVID-19 impacts or pandemic-related personal disruptions (Du, 2023). Additional robustness checks confirm that subsidiary lockdowns do not significantly affect firm productivity, further strengthening our conclusion that the information gains stem from reduced reliance on noisy local signals rather than changes in fundamentals.

Next, we examine the underlying mechanism by documenting that lockdowns reduce underreaction bias, a common forecasting inefficiency where analysts adjust too conservatively to new firm-level news (Coibion and Gorodnichenko, 2015).[4] While underreaction is present for all analysts, lockdowns reduce it by 8%–19% for those near affected subsidiaries, with no significant change for analysts farther away. This suggests that proximity to subsidiaries fosters incomplete incorporation of firm-wide news, and that lockdowns facilitate more complete, timely forecast adjustments.

Textual analysis of analyst reports further supports this attention reallocation channel: during lockdowns, analysts near affected subsidiaries significantly reduce subsidiary-related content. This shift away from local cues aligns with the observed improvement in forecast accuracy. Additionally, content related to firm-, industry-, and market-level information increases. These findings confirm that subsidiary lockdowns encourage analysts to focus more on aggregate information, thereby enhancing forecast accuracy.

Taken together, these results point to geographic proximity as a channel through which analysts disproportionately weight localized, noisy signals, generating systematic forecast errors that are mitigated when access to such information is restricted.

A natural question is whether the analyst-level biases we document aggregate out at the firm level. In principle, idiosyncratic errors should cancel in consensus forecasts. In our setting, however, the geographic distribution of analysts is highly uneven, so certain subsidiaries exert disproportionate influence on the consensus. Moreover, proximity and familiarity lead analysts to weight local information differently, creating correlated, systematic forecast errors that do not wash out in

[4] Underreaction in the forecast revisions to new centralized information represents analysts' failure to fully incorporate newly available information into their forecasts. This may occur when analysts place excessive weight on localized subsidiary-specific information, leading to systematic errors in their forecast updates. In other words, underreaction to public centralized information reflects a behavioral bias where analysts fail to adjust their forecasts sufficiently in response to new firm-wide information.

aggregation. If localized subsidiary information distorts analysts' assessments of firm-wide fundamentals, and those distortions are correlated, then restricting access should improve consensus accuracy. Consistent with this prediction, we find that subsidiary lockdowns significantly reduce both consensus forecast errors and forecast dispersion. The magnitude of these gains is economically meaningful and mirrors the individual-level patterns, reinforcing the interpretation that the improvements stem from mitigating systematic biases rather than removing valuable information.

We also find that the effect is strongest where subsidiary information is most likely to mislead—firms with greater industry diversification and those with more hierarchical structures. Diversified firms operate subsidiaries across multiple industries, making local subsidiary signals less representative of overall performance. In hierarchical firms, information flows are more centralized, reducing reliance on informal, localized insights (Stein, 2002). When subsidiaries in such firms are locked down, analysts are compelled to shift their focus away from noisy local signals toward firm-level hard information, which is expected to be of higher quality in a hierarchical structure. In both cases, subsidiary lockdowns produce especially large improvements in consensus accuracy, underscoring that the benefits of restricting localized access are amplified when firm-wide performance is harder to infer from piecemeal local signals.

Having established that subsidiary lockdowns enhance consensus forecast accuracy by mitigating local biases, we next examine whether these gains translate into more informative stock prices. Specifically, we assess whether restricting access to subsidiary-level information affects price informativeness, alters stock price response to public signals, and shifts the composition of information embedded in stock prices.

We begin by evaluating price informativeness using the price jump ratio (Weller, 2018), which measures the fraction of total earnings-related stock price adjustment that occurs at the announcement rather than before it. A lower jump ratio implies that more price-relevant information is incorporated into stock prices prior to the announcement, i.e., greater price informativeness. Under the bias/attention allocation hypothesis, local subsidiary signals introduce noise and distort investor attention, delaying the incorporation of firm-level information into prices. Restricting access to such information, by "turning off" subsidiary-level cues, should reduce noise and enable markets to more accurately reflect firm-wide fundamentals. Consistent with this hypothesis, we find that subsidiary lockdowns significantly reduce the jump ratio. This result complements our earlier analyst-level findings and suggests that biased local signals not only distort forecasts but leads to slower incorporation of firm-relevant news.

We reinforce this interpretation by examining how the stock market responds to new information when subsidiary access is restricted. Specifically, we examine market reactions to public disclosures such as analyst forecast revisions and management earnings guidance. If prices already reflect the

information in these announcements, due to more accurate pre-announcement expectations, then price reactions should be more muted. Consistent with this, we find that market reactions are significantly dampened during subsidiary lockdowns: each additional subsidiary under lockdown is associated with a 12%–15% decline in the cumulative abnormal return (CAR) following forecast or guidance releases. This muted response and improved information incorporation supports the bias hypothesis that more of the relevant information was already priced in before the announcement.[5]

A lower jump ratio indicates most price adjustment occurs before earnings announcements, implying "smart" investors act beforehand. We investigate whether sophisticated investors, specifically short sellers, react more to analyst forecast revisions when subsidiaries are under lockdown. We expect short sellers to recognize that lockdowns reduce analyst bias, increasing forecast accuracy and driving a stronger response to revisions. Short sellers typically short after downward revisions and cover after upward revisions. This inverse relationship between forecast revisions and short-selling volume is 4–5 times stronger during lockdowns, highlighting that improved forecast quality makes revisions more informative, prompting more decisive actions from short sellers.

We further explore how subsidiary lockdowns shift the type of information that becomes embedded in prices. If localized access leads to narrow focus and information distortion, then restricting such access should reorient investor attention toward broader signals. We test this idea using two return-based measures: stock price synchronicity (SYNCH) and idiosyncratic return volatility (IdioVol). SYNCH captures the extent to which a firm's returns move with the broader market, while IdioVol reflects the share of price movements unique to the firm. Under the bias hypothesis, lockdowns reduce investor fixation on subsidiary-specific signals and encourage a broader, market-oriented information processing. Consistent with this, we find that subsidiary lockdowns increase return synchronicity by 3.3% and reduce idiosyncratic volatility by 5.5% (relative to their respective sample standard deviations). These results suggest that limiting access to localized signals not only improves the accuracy of the information incorporated into prices but also shifts the composition of that information toward less noisy, aggregate sources.

Together, these findings provide converging evidence that geographically proximate subsidiary-level information, while seemingly rich, can distort how information is processed by analysts and investors. When access to this information is reduced, the resulting improvements in price informativeness support the central claim of the bias hypothesis: more information is not always better, especially when it distracts from more representative firm-wide fundamentals.

[5] The latter results are consistent with Di Maggio et al. (2023) who document that institutional investors reduce their exposure to stocks before earnings announcements due to the sensitivity of fund flows to individual stock returns. In our case, COVID-19 lockdowns prompt institutional investors to acquire new information and trade before earnings announcements, leading to lower jump ratios.

Improved forecast accuracy and price informativeness should translate into a less asymmetric information environment, where prices more fully reflect firm-specific fundamentals and fewer informational advantages remain unexploited. Informed traders, such as corporate insiders, typically profit when prices lag fundamentals due to noise or inattention. But when such distortions are removed, the scope for trading on private or misinterpreted signals diminishes. Moreover, when information is more widely and accurately impounded into prices, adverse selection risks faced by liquidity providers decline, reducing bid–ask spreads and improving market liquidity (e.g., Glosten and Milgrom, 1985; Kyle, 1985; Amihud, 2002, Easley and O'Hara, 2004).

We test this prediction by examining several standard liquidity proxies and find broad-based improvements during subsidiary lockdowns. For example, one additional locked-down subsidiary is associated with a 1.1 standard deviation decline in Amihud illiquidity, with similarly significant improvements across Pastor-Stambaugh gamma, Roll's spread, and a composite liquidity index. These results suggest that suppressing localized, distortionary information sources leads to more symmetric information environments, which facilitate smoother trading.

As further evidence of reduced information asymmetry, we examine insider trading behavior, since insiders' abnormal profits often reflect private informational advantages (Cohen, Malloy, and Pomorski, 2012). Consistent with this view, we find that subsidiary lockdowns significantly reduce both opportunistic insider trading activity and its profitability. Specifically, one additional subsidiary under lockdown leads to a 2.5% drop in opportunistic insider sales and a 49% decline in 1-day abnormal trading profits, with even larger reductions over longer windows. By reducing access to potentially distortionary local information, subsidiary lockdown weakens insiders' informational edge, supporting our broader conclusion that such restrictions improve informational symmetry in markets.

Our findings are robust across a battery of specifications and tests that address potential identification concerns and alternative explanations. A key assumption underlying our firm-level difference-in-differences design is that, absent lockdowns, firms with and without affected subsidiaries would have followed similar trends. We test this assumption using dynamic event study analyses and find no evidence of differential pre-trends across any of our main outcomes. Furthermore, the effects are largely absent when firm headquarters are locked down, reinforcing that the mechanism operates through subsidiary-level disruptions rather than broader firm-wide shocks.

We also consider whether real-side operational changes might confound our results. For instance, if subsidiary lockdowns reduced firm output, making earnings more predictable, improvements in forecast accuracy or price informativeness could reflect simpler fundamentals rather than changes in information flow. However, we find no significant effects of lockdowns on firm-level performance metrics such as ROA, sales, or EPS. Our results remain unchanged when controlling for these

fundamentals, suggesting that the improvements stem from reduced informational distortions rather than changes in firm operations.

Finally, we examine the potential role of public subsidiaries, whose mandated disclosures may mitigate the importance of geographic proximity. These subsidiaries are rare in our sample, and excluding them does not alter our findings, further supporting our interpretation that the observed effects are driven by analysts' differential access to localized, privately acquired information.

2 Related Literature

We contribute to several strands of literature on the corporate information environment, with a central focus on geographically dispersed information sources. First, we provide a new perspective on the role of geographic dispersion in affecting informational efficiency. A key innovation of our analysis is to disentangle the effects of having more geographically dispersed operations from the proximity of analysts to those subsidiaries. Gerken and Painter (2023), for instance, highlight how geographically dispersed analysts gain valuable local signals and generate less correlated forecast errors due to a set of diverse insights. However, their analysis does not consider how proximity to dispersed operations shapes information acquisition and attention allocation. We demonstrate that while geographic dispersion among analysts can improve information aggregation, analysts located near subsidiaries are more prone to overreliance on localized signals, leading to systematic forecast errors. By contrast, analysts far from subsidiaries remain unaffected, suggesting that geographic diversity improves informational efficiency only when it avoids concentrated exposure to local noise. This duality, where dispersion aids aggregation but proximity fosters distortion, refines our understanding of the complex role geography plays in corporate information processing.

Second, our study contributes to the literature on geographic proximity and the corporate information environment. Prior research emphasizes the value of local information access in enhancing forecast accuracy and investment decisions (e.g., Coval and Moskowitz, 2001; Bae, Stulz, and Tan, 2008; Giroud, 2013; Bernile, Kumar, and Sulaeman, 2015; Bernstein, Giroud, and Townsend, 2016). More recently, Kang, Stice-Lawrence, and Wong (2021) caution that institutional investors may over-rely on low-quality local information, but do not examine the consequences of such overreliance. We complement this literature by showing that access to subsidiary-level information can reduce forecast accuracy and increase forecast dispersion, particularly for nearby analysts, thereby lowering stock price informativeness. Unlike Kang et al. (2021), who focus on the quality of local information, our results emphasize the attention allocation channel—namely, how local proximity diverts analyst attention toward distortionary signals.

Third, we deepen the literature on geographic factors and analyst behavior. While Gerken and Painter (2023) find that geographic diversity among analysts improves forecast accuracy, Addoum,

Kumar, and Law (2020) document that analysts struggle to incorporate information from geographically dispersed operations. We build on these findings by isolating the role of analyst proximity to subsidiaries, rather than overall dispersion. We show that analysts near subsidiaries, who rely heavily on local cues, produce more accurate forecasts when access to such information is restricted during lockdowns. This evidence highlights a novel mechanism through which geographic proximity can hinder, rather than help, analyst performance.

Fourth, we contribute to the literature linking the corporate information environment and stock liquidity. Prior research shows that greater voluntary disclosure and transparency enhance stock liquidity by reducing information asymmetry (Diamond and Verrecchia, 1991; Leuz and Verrecchia, 2000; Healy, Hutton, and Palepu, 1999; Balakrishnan et al., 2014). We provide a new perspective by identifying subsidiary-level information as a distinct source of information asymmetry. While subsidiary insights may contain value, they can introduce distortions when relied upon unevenly by analysts and insiders. We show that restricting access to this information reduces adverse selection, improves liquidity, and limits insiders' ability to profit from informational advantages, underscoring the market-wide consequences of localized informational frictions.

Fifth, we expand the literature on subsidiaries and firm-level information aggregation. Most prior studies focus on public subsidiaries (e.g., Slovin and Sushka, 1997; Vijh, 2006), financial institutions (Cornett, Ors, and Tehranian, 2002), or transaction-specific contexts such as equity carve-outs and M&A (e.g., Smith and Schipper, 1986; Slovin and Sushka, 1998; Jaffe et al., 2015). A smaller theoretical literature (e.g., Kahn and Winton, 2004; Banerjee and Noe, 2017) explores how internal capital markets aggregate subsidiary information. Our study contributes empirical evidence by focusing on private subsidiaries and demonstrating how their localized information can distort firm-level forecasts and pricing, especially when analysts are in close geographic proximity.

Sixth, we contribute to the organizational theory literature linking firm complexity to information processing and market outcomes. Prior work shows that hierarchical structures facilitate coordination but can hinder flexibility and responsiveness (e.g., Stein, 2002; Alonso, Dessein, and Matouschek, 2008). We extend this line of research by showing that geographically dispersed, hierarchical firms face greater challenges in aggregating subsidiary-level information. Our results reveal that hierarchical structure mitigates the adverse effects of subsidiary-level information by providing stronger centralized guidance and reducing reliance on informal local cues.

Finally, our findings have normative and policy implications. We show that increasing the number of signals, particularly localized ones, can reduce overall informational efficiency, challenging the assumption that more disaggregated data necessarily improves market functioning. Even after accounting for potential managerial manipulation (e.g., Chen, Cohen, and Lou, 2016), our evidence suggests that centralized aggregation at the headquarters level often produces more informative signals

than decentralized market-based aggregation. This insight raises important questions about the design of corporate disclosures and the regulation of subsidiary-level transparency.

3 Data and Methodology

3.1 Data Sources: Subsidiary Location, Analyst Location and Lockdowns

We draw on multiple data sources, including CRSP, Compustat, I/B/E/S, SafeGraph, LexisNexis, Thomson Reuters, BrokerCheck and FINRA. Information on subsidiaries and their geographic location is obtained from LexisNexis, which provides detailed data on U.S. firms at the local legal entity level. To ensure accuracy, LexisNexis conducts phone verifications and annual updates for its Corporate Affiliations database. This database provides the firms' hierarchical structure, detailing it at the level of "ultimate parent", "parent", and "subsidiaries." Subsidiaries may themselves act as parents to other entities within the same network.[6] The vast majority (99.9%) of subsidiaries are private, though some public subsidiaries serve as ultimate parents.[7] Between 2016 and 2019, the database contains 62,044 unique global legal entities. After applying our requirement for subsidiary and ultimate parent classifications, our final dataset consists of 52,681 unique U.S. legal entities.

We identify lockdown periods using "foot traffic data" from SafeGraph Places Patterns dataset, which tracks human mobility. This dataset is derived from GPS pings collected from over 45 million mobile devices across 3.6 million commercial points of interest in the United States. It records the number of visits to specific locations during fixed time intervals. The sample is well balanced across U.S. demographics and geographies, covering approximately 10% of the U.S. population. During the COVID-19 lockdown, foot traffic—measured as the total number of visits within a month for a given ZIP code—dropped significantly, reaching its lowest point in April 2020, followed by a slow recovery in May and June 2020. Bai and Massa (2023) provide a detailed description of this dataset. We match the foot traffic data with the locations of firm subsidiaries and headquarters obtained from LexisNexis.

Stock market and accounting data are sourced from CRSP and Compustat, respectively. Our subsidiary data covers 2016 to 2019, and subsidiary information from the prior year used to reflect pre-existing corporate structures. Foot traffic data from SafeGraph is available for 2019 and 2020, enabling us to measure the impact of lockdown during the COVID-19 pandemic. Our final merged dataset spans January 2016 to December 2020. While no lockdowns occurred before 2020, including earlier years

[6] The original database includes 14 legal entity types, such as parent, subsidiary, plant, and branch. For our analysis, we filter and retain five types as potential subsidiaries: subsidiary, parent, affiliate, holding, and group.
[7] We consider excluding cases where public subsidiaries are considered ultimate parents from our sample. Our results remain intact.

allows us to establish pre-lockdown baselines and control for time-invariant firm characteristics in our analyses.[8]

We classify a ZIP code area as being under lockdown if its monthly footprint activities decline by at least 30% compared to the same month in the previous year. Following Bai and Massa (2021), we adopt the 30% threshold based on both its economic significance and the frequency of such declines in digital footprints. To ensure that our findings are not sensitive to this specific threshold, we perform robustness tests using alternative cutoff values of 40%, 50%, 60%, and 70%. The main results remain consistent across these alternative definitions, as shown in Table A2 to A5. Subsidiaries located in these lockdown ZIP codes are designated as a subsidiary under lockdown. Using this classification, we define the variable *#Subsidiaries_Lockdown,* representing the number of a firm's subsidiaries affected by lockdown. To isolate the information effects of subsidiaries from those of headquarters, *#Subsidiaries_Lockdown* excludes subsidiaries in the same state as the firm's headquarters. To measure the effect of headquarters lockdown, we define *HQ_Lockdown*, a dummy variable equal to 1 if the ZIP code of the firm's headquarters is under lockdown and 0 otherwise.

We obtain analyst location data from BrokerCheck by FINRA, a mandatory registration platform for brokerage firms and individuals conducting transactions and business with the investing public. BrokerCheck provides detailed information, including employment history of registered individuals. For all analysts issuing quarterly earnings forecasts in I/B/E/S between January 1, 2016, and December 31, 2020, we follow Law (2023) to reverse-engineer anonymized analysts by extracting their first initial and last name. We match the numeric broker ID in the Unadjusted Detail History File (ESTIMATOR) with the string broker ID in the Recommendations Detail File (ESTIMID). Next, we manually search for each recovered analyst name on BrokerCheck to link their broker ID to the corresponding brokerage firm name. For each non-anonymized analyst forecast record with a verified analyst name and broker name, we locate the corresponding analyst and brokerage firm on BrokerCheck and extract the analyst's state-level location (See Appendix Figure A1 for the BrokerCheck interface).

To ensure accuracy, we implement the following validation rules: (1) The analyst's surname and first-initial on BrokerCheck must match exactly with I/B/E/S records; (2) Their employment period must align with our sample timeframe; (3) Due to multiple potential matches from BrokerCheck, particularly with common names at large brokers, we discard ambiguous matches to prevent misidentification. By applying these procedures, we successfully recover location information for approximately 82.9% of the quarterly analyst forecasts in our sample (this represents 201 of the 337 broker IDs).

[8] We start from a total of 2,077,787 firm-month observations from LexisNexis Corporate Affiliations Data. After applying various data screens, our final sample includes 89,169 observations, covering 1,812 unique firms during the period from 2016 to 2020. Table A1 shows the sample creation process.

3.2 Variable Construction

To empirically assess the impact of subsidiary-level information on the firm's overall information environment, we construct key outcome variables that capture different dimensions of market efficiency. Specifically, we focus on (i) stock price incorporation of information, (ii) the accuracy and informativeness of earnings forecasts, (iii) the relationship between stock returns and systematic versus idiosyncratic factors, (iv) stock liquidity, and (v) insider trading. The construction and measurement of each outcome variable will be detailed in the results session as well as in the Variable Definition table.

All our regression analyses control for the firm-specific characteristics if not absorbed by the fixed effects. These control variables include (i) Size ("Size") is the natural logarithm of a firm's market capitalization at the end of the previous month; (ii) Bok-to-market ratio ("BM") is the natural logarithm of book equity divided by market equity, measured at the end of the previous month; (iii) Monthly return volatility ($MonthlyVol$) is monthly standard deviation of a stock's daily adjusted returns within a given month; (iv) Past-month return ($MonthlyRet$) is the stock's return over the previous month; (v) Past-year return ($AnnualRet$) is the stock's cumulative return over the 11 months preceding the most recent month (excluding the past-month return) For brevity, these control variables are collectively represented as "$Control$" in our regression specifications rather than being explicitly listed in the equations.

We provide summary statistics for our sample in Table 1. We first calculate the time-series average of each variable at the firm level, and then report the cross-sectional mean across firms. The Variable Definition Table provides a detailed description of a list of variables used in our paper. The firms in the sample have, on average, 10 subsidiaries, and operate across 2 industries, but there is significant cross-firm variation, indicating substantial heterogeneity in organizational complexity. The mean number of subsidiaries under lockdown for the year 2020 is 1.63, confirming that lockdowns meaningfully restricted subsidiary operations. The distribution of the firm characteristics of our sample aligns with those reported in broader datasets.

3.3 Methodology

Our empirical analysis is conducted at both individual analyst and firm levels. At the individual analyst level, the primary objective is to causally identify how analysts respond to localized information from nearby subsidiaries.

A direct correlation between analyst proximity to subsidiaries and forecast quality is potentially biased due to endogeneity concerns, such as reverse causality (better analysts locating near more subsidiaries) or omitted variables (favorable local economic conditions attracting both analysts and subsidiaries). To mitigate these concerns, we exploit COVID-19 lockdowns across the United States in 2020 as a quasi-natural experiment. The timing and location of these lockdowns, driven by public health

policies rather than firm-specific factors, provide an exogenous disruption to subsidiary-level information flows. Effectively, lockdowns "switch off" localized subsidiary information, allowing us to compare changes in forecast accuracy between treated analysts (located near locked-down subsidiaries) and control analysts (covering the same firm but distant from locked-down subsidiaries).

We specifically focus on firm-analyst pairs that differ *solely by analysts' geographic proximity to subsidiaries:* if the effects were due to disruptions to subsidiary-level information, they should be strongest among nearby analysts. To formally test this hypothesis, we estimate the following firm-analyst-level regression:

$$Forecast\ error_{i,j,t} = \beta_1 Subsidiaries_nearby_lockdown_{i,j,t} + \beta_2 NearbySubsidiaries_{i,j,t} + Firm_Mth_Year_{i,t} + Firm_Analyst_{i,j} + Analyst_Mth_Year_{i,j} + \epsilon_{i,j,t}, \quad (1)$$

where $Forecast\ error_{i,j,t}$ is the forecast error of analyst j for firm i in month t. $Subsidiaries_nearby_lockdown_{i,j,t}$ is a dummy variable equal to 1 if all subsidiaries of firm i located in the same state as analyst j are under lockdown in month t, and 0 otherwise. $NearbySubsidiary_{i,j,t}$ is a dummy variable equal to 1 if analyst j is located in the same state as the subsidiary of firm i. We incorporate firm-by-month-year, analyst-by-month-year, and firm-by-analyst fixed effects to control for time-varying firm-specific, analyst-specific, and firm-analyst relationship dynamics.[9] The standard errors are clustered by firm, analyst, and year-month.

This three-dimensional data structure and the use of granular fixed effects help mitigate several concerns on the firm side. First, contemporaneous shocks during the COVID-19 period, such as government interventions (e.g., the CARES Act), might differentially affect firms depending on their subsidiary structures. Firms with many locked-down subsidiaries might have received more relief, influencing analysts' forecasts independently. Second, firms might fundamentally differ on the basis of their number of locked-down subsidiaries, which could correlate with operational fragility, uncertainty, or differences in profitability. The inclusion of firm-by-time fixed effects ensures our estimation captures only within-firm variations, effectively comparing analysts covering the same firm, thereby isolating the disruption in subsidiary-level information.

[9] $NearSubsidiary_{i,j,t}$ is not fully absorbed by the $Firm_Analyst_{i,j}$ fixed effect, as there are instances where existing subsidiaries close, new subsidiaries open, or analysts relocate. Although we do not explicitly include an interaction term in Equation (3), the variable $Subsidiaries_nearby_lockdown_{i,j,t}$ effectively represents the interaction between being near a subsidiary and experiencing a lockdown, as $Subsidiaries_nearby_lockdown_{i,j,t} = 1$ only when $NearSubsidiary_{i,j,t} = 1$. Also, due to data limitations (short panel with numerous firms and analysts but limited periods), firm-month-year and analyst-month-year fixed effects are not simultaneously included.

Concerns from the analyst side also warrant attention. One potential issue is that the analysts located near severely impacted subsidiaries might better assess COVID-19 impacts on the firm and industry, potentially improving forecast accuracy independently of subsidiary-level information. To address this issue, we conduct robustness checks verifying that subsidiary lockdowns do not significantly alter firm productivity measures (ROA, Sales, EPS), and control for these metrics explicitly (detailed in Section 6.6). Another concern is that pandemic-related disruptions (e.g., childcare responsibilities due to school closures) might alter analyst behaviors. For example, Du (2023) shows that female analysts with children are less likely to issue timely forecasts after school closures caused by the COVID-19 pandemic. However, these analyst-specific effects should be invariant across firms covered by the same analyst, and thus are controlled by our analyst-by-time fixed effects. This specification allows us to compare the forecasts for firms with and without subsidiary lockdowns for the same analyst.

Next, we turn to firm-level analysis, aiming to causally identify how subsidiary-level information influences a firm's overall information environment. A simple correlation between the number of subsidiaries and firm-level information quality could be subject to endogeneity due to reverse causality (firms with poorer information environments prompting reliance on local subsidiary observations) or omitted variable bias (firms with hierarchical structures both establishing more subsidiaries and exhibiting lower information transparency). To address this issue, we rely on a generalized difference-in-difference strategy that compares *pre*- and *post*-lockdown changes in key outcomes—such as price informativeness and analyst forecast accuracy—between the treated and control firms. This framework allows us to isolate the impact of subsidiary-level information loss on the firm's broader information environment. In particular, our baseline specification is the following two-way fixed effect (TWFE) model:

$$Y_{i,t} = \beta_1 \#Subsidiary\ Lockdown_{i,t} + \beta_2 HQ\ Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Time_t + \varepsilon_{i,t}, \quad (2)$$

where $Y_{i,t}$ denotes the outcome of interest for firm i during time t. $\#Subsidiary\ Lockdown_{i,t}$ is the number of subsidiaries of firm i under lockdown in period t (a nonnegative integer); $HQ\ Lockdown_{i,t}$ is a dummy variable equal to 1 if the firm's headquarters is under lockdown during period t, to isolate the effect of subsidiary closures from headquarter-specific disruptions. $Control_{i,t}$ is a list of common control variables that could influence firm performance and information environment, including $Size$, BM (Book-to-Market), $MonthlyRet$ (Monthly Stock Return), $AnnualRet$ (Annual Stock Return), $MonthlyVol$ (Monthly Stock Return Volatility), and firm and time fixed effects account for unobserved time-invariant heterogeneity and common trends. Time fixed effects are at Year-Quarter or Year-Month level depending on the frequency of the dependent variable. We estimate Equation (2) using OLS and cluster standard errors by firm and time to account for cross-sectional and temporal dependencies.

To the extent that, in the absence of the COVID-19 lockdowns, the outcome of firms with and without locked-down subsidiaries would have evolved along parallel trends, the coefficient of interest β_1 identifies the average treatment effect on the treated (ATT) – i.e., the impact of subsidiary lockdowns on firms' information environment.[10] To validate the *parallel-trends assumption*, we estimate an event study-version of Equation (2):

$$Y_{i,t} = \sum_{s \neq -2} \beta_s \, Avg\#Subsidairy\ Lockdown_i * D_{s(i,t)} + \alpha HQ\ Lockdown_{i,t} + \gamma\ Control_{i,t} + Firm_i + Time_t + \varepsilon_{i,t}, \quad (3)$$

where $D_{s(i,t)}$ are event-time dummies indicating the quarters (or months) before and after the first lockdown of firm i 's subsidiaries, with $s = -2$ serving as the benchmark period. $Avg\#Subsidiary\ Lockdown_i$ is the average number of subsidiaries under lockdown during the lockdown period, capturing the intensity of the treatment. We include Firm and Time fixed effects and standard errors are clustered by firm and time. By plotting the estimated β_s coefficients over time, we verify that (i) there is no significant pre-trend (i.e., firms with locked-down subsidiaries did not exhibit differential trends before the lockdowns), and (ii) the observed effects emerge only after lockdowns begin, supporting a causal interpretation.

4 The Impact of Subsidiary Lockdowns on Individual Analysts' Forecasts

4.1 Analyst Location and Forecast Accuracy

We start by looking at the effect of subsidiary lockdowns on individual analyst forecasts, considering the geographic proximity between analysts and subsidiaries. Financial analysts gather, process, and disseminate firm-specific information, making their earnings forecasts a key measure of the firm's information environment. If proximity to a subsidiary leads analyst to over-rely on localized information, the *bias hypothesis* posits that analysts near subsidiaries are more likely to incorporate distorted information into their forecasts. Consequently, when subsidiaries near analysts go under lockdown, the removal of this distorted information source should enhance forecast accuracy. The opposite should happen in the case of the *information hypothesis*.

We therefore estimate Equation (1) using individual analyst-level forecast data, incorporating the geographic locations of both subsidiaries and analysts. We obtain earnings and analyst EPS forecasts data from the I/B/E/S Unadjusted Detail files and I/B/E/S Unadjusted Detail History files.[11] The

[10] Another assumption allowing us to identify ATT is that there is no anticipation. Specifically, COVID-19 has no effect prior to the treatment. Moreover, most of the firms in our sample experience COVID lockdowns starting from the first quarter of 2020. Hence, our setting resembles a one-time shock instead of a staggered treatment experiment.

[11] We extract forecasts of EPS with Forecast Period Indicator 6: i.e. Quarter 1, adjusted for stock splits etc (see Payne and Thomas (2003)). When an analyst issues multiple EPS forecasts for the same actual EPS, we retain the

individual analyst-level earnings forecast error is calculated as the absolute difference between the actual EPS and the individual analyst's forecasted EPS, deflated by the stock price two days prior to the announcement. A lower forecast error indicates that the analyst's earnings predictions is more precise. The results, reported in Model 1 of Table 2 Panel A, subsidiary lockdown reduces forecast error by 3.6% (t-stat=2.46), relative to the average forecast error.[12] Such reduction increases to 11.5% when replacing firm-by-year month fixed effects with analyst-by-year month fixed effects in Model 2 of the same panel. The effect of headquarters nearby being under lockdown (*HQ_nearby_lockdown*) in Models 3 and 4 is insignificant, suggesting that centralised information from headquarters does not influence forecast accuracy the same way decentralised information does.

To further isolate the source of these effects, we conduct subsample analyses focusing on analysts located outside the firm's headquarters state, as shown in Table 2, Panel B. We compare: (i) analysts near subsidiaries under lockdown with those near subsidiaries not under lockdown and those far from any subsidiaries, and (ii) analysts near locked-down subsidiaries with those near but unaffected by lockdowns. Models 1 and 2 document even larger effects among non-HQ-state analysts, with forecast errors declining by 6.8% (t-stat = 2.89) and 12.3% (t-stat = 2.70), relative to the average forecast error. This suggests that analysts outside the HQ state—who likely rely more on subsidiary-level information—experience greater forecast accuracy improvements when access to local information is restricted.

Importantly, the coefficient on the *NearSubsidiary* variable, which captures proximity to any subsidiary regardless of lockdown status, is insignificant across specifications. This implies that the improvement stems from restricted access to subsidiary information rather than proximity itself.

In Models 3 and 4, we restrict the sample further by excluding analysts located far from any subsidiaries. Forecast errors decline by 9.6% to 18.5% relative to the sample average. Both sets of comparisons yield statistically significant results, reinforcing that the improvement in forecast accuracy is driven by differential exposure to locked-down local information.

4.2 Analyst Location and Forecast Revisions

Next, we examine how subsidiary lockdowns influence the magnitude of analyst forecast revisions around earnings announcements, focusing on analysts located near the subsidiaries. A common assumption is that geographic proximity enhances information access. However, if proximity also

most recent forecast. Detailed description of variables constructed using I/B/E/S data are in Internet Appendix C. To account for stock splits and reverse splits, we adjust EPS values using the cumulative factor (CFACSHR) from CRSP.

[12] Here, the economic magnitude are computed as the coefficient of $\#Subsidiary\ Lockdown$ multiplied by the lockdown dummy (moving from 0 to 1) divided by the sample average ($0.028 \times 1 \div 0.784 \approx 0.036$). This method applies to other similar cases.

induces over-reliance on local information, especially when that information is noisy or non-representative, then analysts may exhibit over-precision bias, producing more extreme but less accurate forecasts (Hilary and Hsu (2011)). Subsidiary lockdowns, by restricting access to such localized signals, may reduce this overconfidence and lead to smaller, more tempered revisions. To test this, we focus on revisions to annual forecasts made in response to quarterly earnings announcements. Specifically, we ask: when analysts lose access to nearby subsidiary information due to lockdowns, do they revise their forecasts by a smaller amount, consistent with reduced over-precision? We estimate the following regression:

$$Forecast\ revision_EA_{i,j,t} = \beta_1 All_subsidiaries_nearby_lockdown_EA_{i,j,t} + \beta_2 NearbySubsidiary_EA_{i,j,t} + Firm_Mth_Year_{i,t} + Firm_Analyst_{i,j} + \epsilon_{i,j,t}, \quad (4)$$

where $Forecast\ revision_EA_{i,j,t}$ is the absolute magnitude of analyst *j*'s first revision of the annual forecast for firm i following its quarterly earnings announcement in month *t*. $All_subsidiaries_nearby_lockdown_EA_{i,j,t}$ is an indicator equal to 1 if all subsidiaries of firm i located in the same state as analyst j were under lockdown at the time of the analyst's pre-announcement forecast. $NearbySubsidiary_EA_{i,j,t}$ is an indicator equal to 1 if analyst *j* is located in the same state as any subsidiary of firm i.

The results, reported in column 1 of Table 3, shows that analysts revise their forecasts less aggressively when all nearby subsidiaries are under lockdown: the magnitude of the revision decreases by 0.63% relative to the sample mean. This effect is significant and robust to controls for headquarters lockdowns and across alternative specifications. These results suggest a reduction in over-precision bias among nearby analysts. Before lockdowns, access to localized subsidiary information may have led analysts to place excessive confidence in biased signals, resulting in overconfident forecasts. Lockdowns restrict this access, and analysts issue more balanced forecasts that require smaller revisions after earnings announcements, consistent with improved forecast accuracy and less bias.

5 Understanding the Mechanism

The preceding analysis documented that restricting access to subsidiary-level information enhances forecast accuracy and reduces forecast revision frequency. We now investigate the mechanism driving these improvements. We propose that lockdowns redirect analysts' attention toward more aggregate-level information, mitigating excessive reliance on localized signals under normal conditions. To test this issue, we first test whether it is a bias and second whether such bias aggregates across analysts.

5.1 Is it a Bias?

We consider two tests of bias. First, we directly investigate changes in analyst bias and secondly, we look at how lockdown changes analysts' attention allocation.

5.1.1 Underreaction-Driven Forecast Bias

We analyse the bias in analyst forecasts by testing whether subsidiary lockdowns reduce analysts' tendency to underreact to new firm-level information. A common behavioral pattern in forecasts is underreaction, analysts revise too little in response to new signals, leading to predictable forecast errors (see Lim (2001), Bradshaw, Richardson and Sloan (2006) and Coibion and Gorodnichenko (2015)). If subsidiary-level information introduces noise or bias, then lockdowns that restrict access to such information should attenuate this underreaction. In contrast, if local subsidiary access provides valuable private information, lockdowns would exacerbate underreaction by removing an information channel.

We test this using the methodology of Coibion and Gorodnichenko (2015) who show that under rational expectations, analysts' forecasts fully incorporate all the available information at time t, and their forecast revisions between time $t-1$ and t should be uncorrelated with subsequent forecast bias. Conversely, if analysts underreact to new information, a positive relationship between forecast revisions and subsequent forecast bias is expected, where analysts update forecasts insufficiently, leaving a predictable error. We follow Coibion and Gorodnichenko (2015) and estimate:

$$Forecast\ Bias_{i,t} = \beta_1 \#Subsidiary_Lockdown_{i,t} * Forecast\ Revision_{i,t} + \beta_2 Forecast\ Revision_{i,t} + \beta_3 \#Subsidairy_Lockdown_{i,t} + \beta_4 HQ\ Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Quarter_Year_t + \varepsilon_{i,t}, \quad (5)$$

where $Forecast\ Bias_{i,t}$ is based on one-quarter-ahead forecasts and defined as the difference between actual earnings per share (EPS) and the forecasted EPS consensus, which is the average of all forecasted EPS in month t, scaled by the stock price two days prior to the earnings announcement. $Forecast\ Revision_{i,t}$ is the difference between the price-scaled EPS forecast consensus in month t and month $t-1$. To examine the role of geographic proximity, we separately estimate this regression for analysts located near subsidiaries but outside the HQ state and those located far from both subsidiaries and HQ. The results based on $Forecast\ Bias_{i,t}$ and $Forecast\ Revision_{i,t}$ for analysts near subsidiaries are in Models 1 to 3 of Table 4 while Model 4 reports the results for analysts located away from subsidiaries.[13]

[13] We also construct alternative lockdown measures: $\#Analyst\ Lockdown\ (Any)_{i,t}$, the number of analysts in states where at least one subsidiary is under lockdown, and $\#Analyst\ Lockdown\ (All)_{i,t}$, the number in states where all subsidiaries are under lockdown. These measures focus on analysts near subsidiaries under lockdown while excluding scenarios where subsidiaries are under lockdown but lack surrounding analysts, as no effect is expected in such cases.

Table 4 reports the results, where the key coefficients of interest are β_1 and β_2 in Equation (5). Across Models 1 to 4, β_2 is positive (between 0.24 to 0.29) and highly significant (t-stat above 5.3), confirming that, unconditionally, when analysts revise their earnings forecasts, they do so incompletely, leaving systematic bias in their forecasts, confirming underreaction to new information.

In Model 1, the interaction term, β_1 is negative and significant (β_1=-0.021, t-stat=2.83), suggesting that when subsidiaries are locked down, analysts reduce their underreaction bias—they revise forecasts more aggressively and incorporate new firm-level information more efficiently. In Models 2 and 3, we estimate the interaction effects for analysts located outside the headquarter state, with any (Model 2) or all nearby subsidiaries (Model 3) under lockdown. When all subsidiaries in an analyst's state (outside the HQ) are under lockdown, the impact is stronger (β_1=-0.053, t-stat = -3.51), reinforcing the idea that proximity to subsidiaries distorts forecasts. The reduction in underreaction is economically significant, decreasing by 7.86% in Model 1, 17.93% in Model 2, and 18.64% in Model 3 (calculated as β_1/β_2).

In Model 4, the interaction term $\beta_1$is insignificant (β_1 = -0.011, t-stat= -1.50), suggesting that the underreaction bias only matters for the analysts who are geographically exposed to subsidiary-level information. Overall, these findings suggest that access to subsidiary information introduces forecast bias, and lockdowns improve analysts' responsiveness to firm-wide information and reduces the bias. This evidence favors the bias explanation over a pure information loss.

5.1.2 Attention Allocation as Evidenced by Analysts' Information Production

The preceding analysis demonstrates that geographic proximity to subsidiaries contributes to analysts' underreaction to firm-level news, while subsidiary lockdowns alleviate this underreaction. Given that analysts face limited information-processing capacity (Blankespoor et al., 2020), these findings suggest that proximity induces misallocation of attention – analysts may devote excessive attention to localized, subsidiary-level information at the expense of attention to broader firm- or market-level signals. Consequently, lockdowns that limit access to subsidiary-specific information should redirect analysts' attention towards more aggregated sources.

While directly measuring analysts' attention allocation is empirically challenging, we use analyst report content, as a proxy for attention allocation. Analyst reports represent the primary output of analysts' information-processing activities. Thus, changes in the relative representation of subsidiary-, firm-, industry-, and macro-level content should reflect how analysts reallocate cognitive resources in response to information access constraints. Therefore, we expect lockdown-induced restrictions to shift analysts' attention towards aggregate-level information and a decreased emphasis on subsidiary-level details in analyst reports.

We gather analyst reports authored by the top ten brokerage houses from Thomson One's Investext, extracting details including document date, brokerage contributor, author, language, and report length,

as well as firm names and tickers. Matching these reports with I/B/E/S data yields a sample comprising 1475 unique analyst reports covering 521 unique firms from 2019 to 2020.

To classify the content of analyst reports, we employ a large language model (LLM) approach using DeepSeek-V3-0324 model. We segment each report into chunks of 10 sentences to provide sufficient context for accurate classification while maintaining computational efficiency. Each sentence is classified into one of five categories: nearby_subsidiary (discussions of subsidiaries in or near the analyst's location), whole_firm (company-wide strategy and financials), industry (market trends and competitive analysis), macro_economy (regulatory and macroeconomic factors), or other (unrelated content). The classification prioritizes nearby_subsidiary content when multiple topics are present in a sentence. Full technical details of the prompt engineering and classification procedure are provided in Appendix B.

We focus on the analysts located outside the firm's headquarters state who cover firms with subsidiaries. To present the estimates as a semi-elasticity (a percentage change in the frequency of related content in response to a unit change in lockdown exposure), but without dropping observations when log- transforming the dependent variable, we follow Cohn et al. (2022) and estimate the following panel relationship using a Poisson model with fixed effects:

$$Sent\ Freq\ NearbySub_{i,j,t} = \beta_1 Subsidiaries_nearby_lockdown_{i,j,t} + \beta_2 NearSubsidiary_{i,j,t} + \beta_3 WordCount_{i,j,t} + Controls_{i,t} + Firm_Mth_Year_{i,t} + Firm_Analyst_{i,j} + \epsilon_{i,j,t}. \quad (6)$$

Here, $Sent\ Freq\ NearbySub_{i,j,t}$ measures the number of sentences related to nearby subsidiary information extracted from analyst j's report about firm i in month t. $Controls_{i,t}$ represents a set of firm-level characteristics, including size, book-to-market ratio, past month return, past year return, monthly volatility, and analyst coverage. All the control variables are lagged one month. We also include firm, month-year, and analyst fixed effects.

Table 5 summarizes our empirical findings. Column 1 presents results using nearby subsidiary sentence counts as the dependent variable, while column 2 examines the allocation to other content categories (combining whole_firm, industry, and macro_economy topics) as a robustness check, controlling for nearby subsidiary content.

Results confirm our expectations: subsidiary lockdowns significantly reduce subsidiary-related content in analyst reports. Specifically, subsidiary-level information content decreases by 91.9% in sentence counts, while other content increases by 4.5%. These findings confirm that subsidiary lockdowns prompt analysts to reallocate attention from subsidiary-specific signals toward more aggregate information, consistent with our hypothesis that reduced local information access enhances forecast accuracy through improved focus on firm-wide fundamentals.

Therefore, both sets of tests lead to a bias/attention allocation story as opposed to a pure information one. Indeed, the fact that subsidiary lockdowns may improve the quality of local subsidiary-level information, consequently benefiting nearby analysts is inconsistent with a reallocation of attention to macro-level and firm-related content. In sum, our evidence points to attention misallocation as a central mechanism through which geographic proximity introduces forecast bias. Lockdowns improve information aggregation not by reducing information flow per se, but by rebalancing attention toward broader, more representative signals.

5.2 Does the Bias Aggregate?

Next, we extend our analysis to evaluate whether limiting access to subsidiary-level information through COVID-19 lockdowns enhances the precision of firm-level consensus forecasts. Traditional theory posits that random noises in individual forecasts should average out across analysts; however, two factors challenge this assumption in our context. First, analysts are unevenly distributed geographically, enabling specific subsidiaries to disproportionately influence consensus forecasts. Second, analysts clustered around particular subsidiaries collectively overreact to the same localized information while underreacting to broader data, leading to correlated and systematic forecast errors. Consequently, an overemphasis on subsidiary-level information can introduce systematic biases into aggregated firm-level forecasts, compromising their accuracy. Thus, subsidiary lockdowns, by reducing reliance on localized information, are expected to improve firm-level consensus forecast accuracy.

Following Thomas (2002) and Guo and Mota (2021), we define firm-level analyst forecast error ("Analyst Forecast Error") as the absolute difference between the actual EPS and the consensus forecast (mean of all forecasts), deflated by the stock price two days before the earnings announcement.[14] Our main results remain unchanged when we use the median forecast instead of the mean. Forecast dispersion ("Forecast Dispersion") captures dispersion in earnings forecasts among analysts and is defined as the standard deviation of all analyst forecasts for firm i in quarter t, requiring a minimum of three forecasts, following Thomas (2002) and Zhang (2006). A higher Forecast Dispersion implies greater disagreement in analysts' expectations.

We estimate Equation (2) using $Forecast_{i,t}$ as the dependent variable, where $Forecast_{i,t}$ is either the analyst earnings forecast error or the dispersion in analyst forecasts for firm i in month t (As these forecasts pertain to one-quarter-ahead earnings, the monthly values for forecast errors and dispersion are the same within a quarter). Models 1 and 2 of Table 6 show a negative relationship between the number of subsidiaries under lockdown and forecast error (Model 1, $\beta_1 = -0.017$ $(t - stat = 3.99)$)

[14] As shown in Table 1, the mean analyst forecast error in our sample is 0.91%, higher than the 0.44% reported in Guo and Mota (2021). This discrepancy likely reflects the greater forecasting challenges associated with firms that operate through subsidiaries, as subsidiary-level complexity may introduce additional sources of complexity in earnings prediction.

and forecast dispersion (Model 2, $\beta_1 = -0.011$ ($t - stat = 3.69$)). One more subsidiary under lockdown is associated with a 1.87% (i.e. the value of β_1(-0.017)/the sample mean of *Analyst Forecast Error* (0.91)) decrease in forecast error and a 2.20% decrease in forecast dispersion, relative to the sample average.

Furthermore, we examine state-level consensus forecast errors conditioned on the number of analysts within the state. Given the logic outlined above, individual analyst biases should persist when aggregating forecasts at the state level, particularly as analysts from the same state focus similarly on local subsidiary information. Consequently, a greater number of analysts within a state should not reduce forecast errors. To investigate this, we estimate:

$$Forecast\ error_{i,s,t} = \beta_1 \#State\ Subsidiary\ Lockdown_{i,s,t} * High\ Analyst\ Num_{i,s,t} + \beta_2 \#State\ Subsidiary\ Lockdown_{i,s,t} + \beta_3 High\ Analyst\ Num_{i,s,t} + \gamma Controls_{i,t} + Firm_i + Month_Year_t + \varepsilon_{i,s,t}, (7)$$

where $Forecast\ error_{i,j,t}$ represents the consensus forecast error for firm *i* in state *s* at time *t*, $\#State\ Subsidiary\ Lockdown_{i,s,t}$ is the count of subsidiaries under lockdown in state *s*, and $High\ Analyst\ Num_{i,s,t}$ is an indicator variable set to 1 if at least three analysts within the state cover firm i. The analysis covers 2016–2020, limited to states with at least one analyst covering a subsidiary in the same state.

Results in Table 7 confirm our hypothesis: analyst biases persist at the state level and do not diminish with more analysts present. Specifically, Columns 1 and 3 show that forecast errors increase when subsidiaries are under lockdown. The interaction terms in Columns 2 and 4 between state-level lockdowns and analyst numbers, which capture the effect of analyst concentration during lockdowns, are statistically insignificant, further supporting the presence of persistent, systematic biases among analysts concentrated around subsidiaries.

5.3 The Heterogeneous Effects of Industry Dispersion of Subsidiaries and Hierarchical Structure

Having documented that subsidiary lockdowns enhance accuracy of forecast consensus, we now focus on cross-sectional variations to identify potential heterogeneous effects across different firm characteristics. We consider two key dimensions of organizational complexity: the breadth of industries in which its subsidiaries operate and the firm's hierarchical structure. We hypothesize that these features magnify the impact of subsidiary lockdowns on the information environment through distinct mechanisms.

First, when subsidiaries span multiple industries, idiosyncratic subsidiary-level information may be misleading, as the deviation from firm-level signals may increase and those local signals become less

predictive of firm-wide performance. Analysts who over-rely on these fragmented signals risk producing biased or less accurate forecasts—a distortion that intensifies with greater industry diversity. Second, hierarchical organizations, which integrate hard information more effectively through centralized decision-making (Stein, 2002), provide analysts a stronger firm-level signal from the headquarters. Biases reducing the effect of such signal will have a more detrimental effect. Therefore, both sides of organizational complexity will magnify the detrimental effect of the bias. As direct access to subsidiary-level data becomes restricted, analysts covering firms spanning multiple industries as well as highly hierarchical firms will benefit more from the firm's centralized disclosures, thereby achieving larger improvements in forecast accuracy.

To examine these cross-sectional effects, we augment our baseline regression from Equation (2) by adding interaction terms that capture organizational complexity. Specifically, we estimate:

$$Forecast_{i,t} = \beta_1 \#Subsidiary\ Lockdown_{i,t} + \beta_2 \#Subsidiary\ Lockdown_{i,t} * D_{i,t} + \beta_3 HQ\ Lockdown_i * D_{i,t} + \beta_4 HQ\ Lockdown_{i,t} + \beta_5 \#Subsidiary\ Lockdown_{i,t} * \#Segment\ Ind_{i,t} + \gamma Control_{i,t} + Firm_i + Qtr_Year_t + \varepsilon_{i,t}, \quad (9)$$

where $D_{i,t}$ is our measure of organizational complexity, which can take one of two forms: (1) $Industry\ Dispersion_{i,t}$: the number of distinct industries (using Fama-French 48 classification) in which firm i has subsidiaries in the year before the lockdown; (2) $Hierarchy_{i,t}$: a dummy variable equal to 1 if the total number of organizational layers across all subsidiaries of firm i is above the sample median in year t. To ensure robustness, when $D_{i,t} = Industry\ Dispersion_{i,t}$, we control the interaction $\#Subsidiary\ Lockdown_{i,t} * \#Segment\ Ind_{i,t}$, where $\#Segment\ Ind_i$ is the number of industry segments covered by firm i, in the year before the lockdown. When $D_{i,t} = Hierarchy_{i,t}$, we omit this term because $\#Segment\ Ind_{i,t}$ as industry segmentation is not central to hierarchical complexity.

In Models 3 and 4 of Table 6, we examine the role of $Industry\ Dispersion_{i,t}$. Consistent with the *bias hypothesis*, the impact of subsidiary lockdowns on forecast accuracy and forecast dispersion is strongest in firms with higher industry dispersion. In Model 3 and 4, the interaction term shows a negative effect on forecast error, with $\beta_2 = -0.003$ $(t - stat = 1.83)$ and forecast dispersion with $\beta_2 = -0.004$ $(t - stat = 3.28)$. Quantitatively, a one-unit increase in $\#Subsidiary\ Lockdown$, interacted with a one-standard-deviation increase in $Industry\ Dispersion$, is associated with a 0.76% decrease in forecast error and a 1.85% decrease in forecast dispersion relative to the sample mean.

In Models 5 and 6, we turn to hierarchical complexity and find further support for our hypothesis: the impact of subsidiary lockdowns on analyst forecast accuracy is stronger in firms with higher hierarchical complexity. Specifically, for high-hierarchy firms, a one-unit increase in $\#Subsidiary\ Lockdown$ is associated with a 3.85% reduction in forecast error in firms that have high

hierarchical layers. However, there is no evidence of significant interaction effects for forecast dispersion. Jointly taken, these results further support the bias hypothesis and show how organizational complexity magnifies their impact.

5.4 Geographic Dispersion of Analysts: Information Diversity vs Local Bias

Two key elements affect the informativeness role of the subsidiaries: their dispersion across different geographies and their proximity to market participants. Indeed, Gerken and Painter (2023) argue that geographically dispersed market participants acquire unique firm-specific information through interactions with local employees, customers, suppliers, local media, or through personal connections with executives. They document that greater geographic concentration of analysts is associated with higher consensus forecast errors and reduced firm liquidity, highlighting the benefits of geographic diversity in improving the firm's information environment.

While their analysis focuses on the *geographic distribution of analysts*, our results specifically focus on the implications of analysts' *proximity to geographically dispersed corporate subsidiaries*. While they show that geographic dispersion among analysts mitigates correlated errors by integrating diverse perspectives—thus improving overall forecast accuracy— our focus is to show that proximity to subsidiaries induces localized biases, as analysts near subsidiaries overweight subsidiary-specific information, leading to systematic distortions in their forecasts.

This misalignment is particularly pronounced under normal conditions but diminishes when access to subsidiary is restricted, as observed during lockdowns. By restricting access to localized subsidiary information, lockdowns effectively reduce analysts' reliance on potentially misleading local insights, leading to improved forecast accuracy. Thus, our study reconciles two seemingly conflicting perspectives: while a geographically dispersed analyst network enhances the firm's overall information environment, excessive proximity to firm operations can distort information processing, leading to biased forecasts.

To further examine the implications of such misalignment linked to geographic dispersion, we replicate the analysis in Table 3 of Gerken and Painter (2023), which examines the effect of geographic dispersion on consensus analyst forecast errors. We estimate:

$$Forecast\ error_{i,t} = \beta_1 GeoDispersion_{i,q} + \sum_k \beta_k Controls_{i,q} + Industry_I + Month_Year_t + \epsilon_{i,q}, \quad (8)$$

where $Forecast\ error_{i,t}$ is the absolute consensus analyst forecast error for firm i in month t, and $GeoDispersion_{i,q}$ is a Herfindahl index measuring the concentration of analysts in metropolitan statistical areas, multiplied by -1 for ease of interpretation. The model follows the specifications of Gerken and Painter (2023), including industry and year-quarter fixed effects, and controlling firm size,

book-to-market, turnover, institutional ownership, firm age, and analyst coverage. Standard errors are clustered by firm.

We present the results in Table 8. In Model 1, we confirm the findings in Gerken and Painter (2023) that higher geographic dispersion of analysts lowers consensus forecast errors. However, when we decompose the $GeoDispersion_{i,q}$ measure into two components—analysts near subsidiaries and analysts far from subsidiaries—a more nuanced pattern emerges. If we focus on analysts near the subsidiaries (Model 2), we find that the coefficient on geographic dispersion changes sign and loses statistical significance. This suggests that the benefits of dispersion do not hold when analysts are located near subsidiaries. Moreover, proximity to subsidiaries may introduce distractions or lead analysts to overweight localized information, ultimately reversing the expected benefits of geographic diversity. When focusing on analysts who are geographically dispersed but are located far from the subsidiaries (Model 3), the results in Gerken and Painter (2023) reappear, indicating that the improvements in forecast accuracy are primarily driven by geographic dispersion of analysts who are not overly exposed to subsidiary-level information.

Taken together, these findings suggest a more complex effect of geographic dispersion on information processing. On the one hand, a geographically dispersed group of information acquirers benefits from diverse perspectives and local knowledge, reducing correlated errors, thereby improving the firm's overall information environment. On the other hand, when analysts are located close to subsidiaries, the benefits of geographic dispersion are offset by behavioral biases that lead them to overemphasize localized information, potentially distorting their overall assessment of the firm.

6 Implications for Corporate Information Environment

Having established that analyst forecast accuracy improves during subsidiary lockdowns, we now move on to assess whether there is an impact on the information environment of the firm.

6.1 Stock Price Informativeness

We start by looking at the impact of subsidiary lockdowns on stock price informativeness. Specifically, we investigate whether subsidiary lockdowns enhance stock price informativeness, by reducing local biases that divert attention from broader firm-level information or impede price informativeness by reducing access to granular subsidiary-level information. We examine two sets of measures: the price jump ratio and the stock price reaction to the information contained in the analyst forecast revisions and in the management forecast surprises. We find that subsidiary lockdowns reduce distortion due to local, fragmented information and enhance stock price informativeness.

The price jump ratio ("*Jump*") introduced by Weller (2018) quantifies the proportion of stock price movement occurring at earnings announcements relative to total price variation before and after the earnings announcement. A lower *Jump* implies that more information is incorporated into stock prices before earnings announcements, which reflects greater price informativeness. We define the *Jump* as $Jump_{it}^{(a,b)} = \frac{CAR_{it}^{(T-1,T+b)}}{CAR_{it}^{(T-a,T+b)}}$, where CAR_{it} represents the cumulative abnormal returns for firm i in quarter t, $(T-1,T+b)$ is the post-announcement window capturing price movements immediately after the earnings announcement from days *T-1* to *T+b*, $(T-a, T+b)$ is the total window including both pre- and post-announcement movements. Following Weller (2018), we set the pre-announcement window to 21 days (a=21) to balance resolution on earnings announcement effects with the potential for information leakage. The post-announcement window is set to two trading days (b=2) to account for post-earnings announcement drift. We estimate the abnormal stock returns using either the Fama and French three-factor model (FF3) or five-factor model (FF5), based on daily returns over a 365-calendar day window ending 90 days before the quarterly earnings announcement. The resulting price jump ratios based on FF3 and FF5 are denoted as $Jump^{FF3}$ and $Jump^{FF5}$, respectively.

Weller distinguishes between "informational efficiency" and "informativeness", emphasizing that "prices may reflect acquired information well (high informational efficiency), but nonetheless summarize a low absolute level of information (low informativeness)." A low *Jump* indicates that a large portion of the price adjustment occurs before the announcement, suggesting a stronger information environment and high price informativeness. The key intuition behind this measure is that it reflects how much information enters prices early versus how much is incorporated eventually. This measure is particularly relevant in the COVID-19 context, as the pandemic may have disrupted the availability of acquirable information in the market. We argue that acquisition of (potentially biased) information from subsidiaries gets curtailed when these subsidiaries are under lockdown, and our goal is to quantify this impact.

We apply our baseline model, Equation (2), on quarterly data, using *Jump* as the outcome variable. *Jump* is either $Jump^{FF3}$ or $Jump^{FF5}$, which are based on the Fama-French three-factor and five-factor models, respectively. The main explanatory variables— $\#Subsidiary\ Lockdown_{i,t}$ and $HQ\ Lockdown_{i,t}$—and the set of controls are defined in Sections 3.1 and 3.2.

Table 9 Panel A reports the results. As shown in Model 1, $Jump^{FF3}$ decreases significantly by 0.005 (t-stat=2.16) when subsidiaries go under lockdown due to COVID-19 pandemic. One more subsidiary under lockdowns is associated with a 1.1% decrease in $Jump^{FF3}$ (i.e. the value of β_1(-0.005)/the sample mean of $Jump^{FF3}$ (0.44)). The effect is more pronounced in Model 2, using $Jump^{FF5}$ registering a decrease of 2.0% for one more subsidiary under lockdown. Hence, firms with greater exposure to subsidiary lockdowns incorporate a larger fraction of firm-level information into prices before public

disclosure, supporting the idea that limiting access to subsidiary-level information increases price informativeness.

To confirm the causal interpretation of our results, we assess the "parallel trends" by estimating Equation (3) using *Jump* as the dependent variable. Figure 1 presents the estimated β_s coefficients from Equation (3) over the period $s = -3$ to $s = 3$. Panel A plots the estimates when the dependent variable is $Jump^{FF3}$, while Panel B plots the estimates when the dependent variable is $Jump^{FF5}$. We find no evidence of pre-trends and *Jump* declines significantly after earnings announcements during periods of subsidiary lockdowns. Hence, these results suggest subsidiary-level information diverts market attention from broader firm-wide information, and limiting access to this information (via subsidiary lockdowns) causes a lower jump ratio and increases stock price informativeness.

The second set of proxies of informativeness examines the stock price reaction to the information contained in the managerial forecast surprises. When stock prices are more informative, market reactions to new information should be less pronounced, as much of the information is already incorporated into prices. In the extreme case of full information incorporation, earnings forecasts from firm management should have minimal impact on stock prices. We therefore estimate:

$$CAR_{i,t} = \beta_1 \#Subsidiary\ Lockdown_{i,t} * MgtEarningsSurprise_{i,t} + \beta_2 \#Subsidiary\ Lockdown_{i,t} + \beta_3 MgtEarningsSurprise_{i,t} + \beta_4 HQ\ Lockdown_{i,t} * MgtEarningsSurprise_{i,t} + \beta_5 HQ\ Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Month_Year_t + \varepsilon_{i,t}, \quad (9)$$

where $CAR_{i,t}$ is the cumulative abnormal return for firm i around certain analyst forecast revision on date t. We measure $CAR_{i,t}$ over three event windows: a 3-day period [t-1, t+1], a 5-day period [t-2, t+2], and a 7-day period [t-3, t+3]. Following Hirshleifer, Lim, and Teoh (2009), the cumulative abnormal returns are defined as the difference between the firm's buy-and-hold return and the return of a size and book-to-market (B/M) matched portfolio over the specified event windows. [15] $MgtEarningsSurprise_{i,t}$ is a categorical variable set to 1 if the current management forecast is above the latest available consensus analyst forecast, 0 if the current management forecast is within the range of the latest available consensus analyst forecast, -1 if the current management is below the range of the latest available consensus analyst forecast. Other specifications remain the same as in the earlier models.

The estimates of Equation (9) are presented in Panel B of Table 9. In column 1, subsidiary lockdowns reduce the market's reaction to management forecast surprises with $\beta_1 = -0.006$ ($t -$

[15] Each stock is matched with 1 of 25 size-B/M portfolios every day based on the market capitalization at the end of last month and the B/M at the end of December of the prior year. We calculate the value-weighted daily returns of the 25 size-B/M portfolios using all listed stocks in NYSE, NASDAQ, and AMEX. Our results are also robust if the daily abnormal returns are computed by subtracting the daily CRSP value-weighted return from the daily raw returns.

$stat = 2.13$) over the 3-day event window. Controlling for firm characteristics, we find that each additional subsidiary under lockdown is associated with an 8.8% to 14.9% decrease in market reactions, depending on the event window, as shown in columns 2, 4 and 6.

Overall, these findings point in the direction that subsidiary lockdowns reduce information distortions and increase informativeness. Indeed, stock prices consistently react less to the public disclosure of new information, enhancing price informativeness.

6.2 Do Short Sellers Follow Improved Forecasts?

The key intuition of the Weller measure is that a low *Jump* indicates that a large portion of the price adjustment occurs before the earnings announcements, suggesting that "smart" investors have already acted before such announcements. We now investigate whether this is the case by looking at how sophisticated investors ("short sellers") react to analyst forecast revision before earning announcements. We know that short sellers react to public information better interpreting it (Engelberg, Reeb, and Ringgenberg, 2012). We therefore expect that they will realize that analysts suffer from a lower bias when the subsidiaries are under lockdown and therefore will react more to their revisions, realizing the higher informational content. This will impound information in prices before the earnings announcement and will lead to a lower *Jump.* We test this hypothesis by estimating the following regression:

$$Short\ Volume_{i,j,t} = \beta_1 Analyst\ Forecast\ Revision_{i,j,t} Subsidiaries_nearby_lockdown_{i,j,t} + \beta_2 Analyst\ Forecast\ Revision_{i,j,t} + \beta_3 Subsidiaries_nearby_lockdown_{i,j,t} + \beta_4 NearSubsidiary_{i,j,t} + \beta_5 Analyst\ Forecast\ Revision_{i,j,t} NearSubsidiary_{i,j,t} + \beta_6 HQ_lockdown_{i,j,t} + \beta_7 Analyst\ Forecast\ Revision_{i,j,t} HQ_lockdown_{i,j,t}, \quad (10)$$

where $Short\ Volume_{i,j,t}$ is defined either as the ratio or the log of one plus the ratio of monthly short-selling volume to total trading volume for firm i, measured over various event windows ([t+1, t+3], [t+1, t+5], [t-3, t+3], [t-3, t+5]) around day t, when analyst j issues a forecast revision. $Analyst\ Revision_{i,j,t}$ represents the magnitude of analyst j's forecast revision for firm i on day t.

The results, shown in Table 10, indicate how lockdowns of nearby subsidiaries influence short selling behavior. In general, larger positive forecast revisions correspond to lower short-selling volumes and vice versa. Crucially, this negative relationship intensifies substantially (by approximately 4-5 times) when forecast revisions occur near subsidiaries experiencing lockdowns. This suggests that the improved forecasts of the analysts get incorporated into prices the piece of information which would otherwise take longer to get incorporated.

6.3 Systematic and Idiosyncratic Components of Stock Returns

Subsidiary lockdowns will also affect the way information is reflected in the stock prices and their movements. We employ two key metrics – stock price synchronicity (*SYNCH*) and idiosyncratic return volatility (*IdioVol*)—to assess how subsidiary-level information influences stock returns. We hypothesize that subsidiary-level information is localized, idiosyncratic, noisy and privately held, and may not closely reflect firm-wide performance. If analysts and investors overweight subsidiary-level information, restricting access to this information (via lockdowns) shifts investors' attention to broader market signals (e.g., industry reports or market trends), leading to higher *SYNCH* (greater reliance on market-wide information)[16] and lower *IdioVol* (less idiosyncratic noise in stock prices).[17] *SYNCH* measures the extent to which a firm's stock price co-moves with market returns. A higher *SYNCH* suggests that stock prices reflect more market-wide information and less firm-specific information (including firm-level and subsidiary-level information). *IdioVol*, instead captures the firm-specific component of stock return variability, is lower when stock prices move independently of the broader market.

Empirically, we examine these effects by estimating Equation (2) using either *SYNCH* or *IdioVol* as dependent variables. As shown in Model 1 of Table 11, subsidiary lockdowns significantly increase *SYNCH* (β_1=0.018, t-stat=5.81), indicating that stock returns become more correlated with the broader market when subsidiary information is restricted. Model 2 shows that firm-specific volatility reduces when subsidiaries are locked down (β_1=-0.261, t-stat=7.82). Quantitatively, one additional subsidiary under lockdown is associated with a 3.28% rise in *SYNCH* and a 5.50% reduction in *IdioVol*, relative to the sample standard deviation.

Lockdowns affecting headquarters (HQ) do not produce similar effects. The coefficient on HQ Lockdown is statistically insignificant across all specifications, indicating that headquarters lockdowns do not meaningfully influence *SYNCH* or *IdioVol*. This suggests that subsidiary-level information plays a distinct role in shaping firm-specific stock return behavior. These results remain robust when we adjust for industry-wide returns in the measurement of *SYNCH* and *IdioVol* as shown in Models 3 and 4 of Table 11. Hence, our findings highlight that higher noise, and that restricting access to subsidiary-level information access through lockdowns is associated with less noise in firm-specific price movements and incorporation of more aggregate level information.

[16] Admittedly, investors may also exert greater reliance on firm-wide information. However, assuming fixed amount of information incorporated, we should expect the systematic component to increase as long as market-wide information increases proportionally.

[17] Prior research has used these measures to study stock price informativeness, though their interpretation remains debated. Some argue that higher *SYNCH* indicates lower informativeness, as firm-specific information is not incorporated into stock prices (Morck, Yeung, and Yu (2000), Durnev, Morck and Yeung (2004), Chan and Hameed (2006)), while others suggest that it indicates more informative prices (Dasgupta, Gan and Gao (2010), and Chan and Chen (2014)). Hou, Peng and Xiong (2013), on the other hand, argue that *SYNCH* is independent of the amount of information incorporated into stock prices. To remain agnostic in this debate, we do not interpret *SYNCH* and *IdioVol* as direct measures of informativeness. Instead, we use them to capture how different types of information affect stock returns.

To confirm the causal interpretation of our results, we assess the parallel trends by estimating the dynamic specification from Equation (3) using either *SYNCH* or *IdioVol* as the dependent variable. Figure 2 presents the event-study estimates of β_s coefficients from Equation (3) over the period $s = -6$ to $s = 6$. Panel A (B) plots the estimates when the dependent variable is *SYNCH* (*IdioVol*). Prior to lockdowns, the β_s coefficients (for $s = -6$ to $s = -1$) are not statistically significant, indicating the absence of pre-trends. After lockdowns begin, *SYNCH* increases and *IdioVol* declines, supporting the interpretation that subsidiary lockdowns shift the type of information incorporated into stock prices – from subsidiary-driven idiosyncratic signals to broader market-level factors. Our findings remain robust to controlling for industry returns (see Panels C and D of Figure 2).

6.4 Stock Liquidity

We now investigate whether the improvement in the firm's information environment may also enhance stock liquidity. When market participants have access to higher-quality, less distorted information, informational asymmetry declines, reducing adverse selection costs and enhancing market liquidity (Glosten and Milgrom, 1985; Easley and O'Hara, 2004). To assess this hypothesis, we estimate Equation (2) using illiquidity as the dependent variable.

We employ three widely used illiquidity proxies and a composite measure: (i) Amihud Illiquidity (Amihud, 2002), (ii) PS Gamma (Pástor and Stambaugh, 2003), (iii) Roll's spread (Roll, 1984), and (iv) composite illiquidity index, defined as the average z-scores of the three measures. The distribution of the illiquidity measures in our sample (see Table 1) are broadly comparable to the values reported in Goyenko, Holden, and Trzcinka 2009; Næs, Skjeltorp, and Ødegaard 2011, Koch, Ruenzi, and Starks 2016; and Goyal, Subrahmanyam, and Swaminathan 2023, confirming the representativeness of our sample in capturing market liquidity. We include firm and year-month fixed effects, along with a set of lagged firm-specific control variables, including BM, past-month return, and monthly returns volatility to account for confounding firm characteristics that may influence liquidity.[18]

The results in Table 12 indicate that subsidiary lockdowns significantly enhance stock liquidity across all measures. For example, the regression coefficient associated with subsidiary lockdown in Equation (2), β_1, is -1.12 (t-stat=2.72) when illiquidity is proxied by the composite illiquidity index. The liquidity effects are economically significant. One more subsidiary under lockdown is associated with a 1.06-standard-deviation decrease in Amihud Illiquidity, a 0.22%-standard-deviation increase in the PS Gamma, a 3.34-standard-deviation decrease in Roll's Spread, and a 2.29-standard-deviation decrease in the composite measure of illiquidity measure. Unlike subsidiary lockdowns, the effect of

[18] We don't control for size as size is potentially one proxy of liquidity. However, our results remain significant if we include size, as shown in Table A6.

HQ lockdown on firm liquidity is weaker and insignificant in most cases, confirming the effect of subsidiary specific disruptions.

To assess whether liquidity trends differ before and after lockdown events and if our liquidity results are driven by pre-trends, we estimate Equation (3), with results shown in Figure 3. Prior to the lockdown, the key variables ("$Average\ Lockdown_i$") —which proxies for the intensity of the treatment effects— exhibits no statistically significant impact, confirming the absence of pre-existing trends, across all liquidity proxies. In contrast, once the lockdown begins, the liquidity improves significantly. These results align with our expectations: restricting the access to subsidiary-level information—manifested through improved price informativeness, and more precision of analyst forecasts—lowers informational asymmetry and ultimately improve stock liquidity.

6.5 Insider Trading and Information Advantage

Finally, we examine how subsidiary lockdowns affect the corporate insiders. Insiders possess private insights into firm performance, operations, and strategic decisions, often allowing them to time their trades profitably at the expense of less-informed investors (Cohen, Malloy, and Pomorski (2012)). The evidence so far suggests that subsidiary lockdowns reduce information asymmetry, which should diminish insiders' informational advantage. Consequently, we expect two key effects: (i) a decline in opportunistic insider trading volume as insiders engage in fewer trades due to reduced mispricing opportunities, and (ii) a decrease in insider trading profitability as insiders face fewer opportunities to exploit mispricing.

To test these hypotheses, we estimate the specification from Equation (2) using monthly data, where $Opportunistic\ TradingVol_{i,t}$ is the dependent variable. We define $Opportunistic\ TradingVol_{i,t}$ as the monthly opportunistic trading volume (buying and/or selling), scaled by the insider's holding position at the end of the previous month and multiplied by 1000 (see the definitions of insiders and their trades are detailed in Section 3).

Table 13, Panel A, shows a negative relationship between the number of subsidiaries under lockdown and both opportunistic selling and total insider trading (opportunistic buying + selling). Specifically, as the number of subsidiaries under lockdown increases, there is a decrease in opportunistic trades, β_1 for opportunistic buys plus sells in Model 1 is -0.71 (t-stat=2.16). Breaking down this effect into buying and selling, Model 2 shows opportunistic selling decreases significantly as subsidiary lockdowns increases (β_1= -0.52 (t-stat=2.03) in Model 2). One more subsidiary under lockdowns is associated with a 2.48% decrease in opportunistic selling and a 2.40% decrease in total opportunistic trades, relative to the sample average. However, there is no significant effect on opportunistic buys (Model 3). These results support our hypothesis that as insiders face diminished informational advantage, they abstain from trading—particularly from selling, where they traditionally

exploit their informational edge.[19] Interestingly, HQ Lockdown has no significant effect on either opportunistic selling or buying, reinforcing the idea that the effect is driven specifically by the restriction of subsidiary-level information rather than broader firm-wide constraints.

To further validate these findings, we assess the parallel trends by estimating the dynamic specification from Equation (3) and report the results in Figure 4. Again, there is no statistically significant effect of the key variables ("$Average\ Lockdown_i$") –our proxy for the intensity of the treatment effects—before the lockdowns. In contrast, the effect becomes statistically significant once the lockdowns take effect, reinforcing our multivariate findings and alleviating endogeneity concerns.

Panel B of Table 13 investigates whether subsidiary lockdowns affect insiders' ability to generate profits from their trades. We estimate Equation (2) using $Opportunistic\ TradingProfit_{i,t}$ as the dependent variable, where $Opportunistic\ TradingProfit_{i,t}$ is the excess profit of opportunistic insider trading, calculated as the dollar volume of insider buys minus sells, multiplied by the stock's abnormal return, and scaled by the dollar volume of insiders' holding position (multiplied by 1000). The abnormal return is the stock return minus the CRSP value-weighted market portfolio return, and the 1-day, 3-day and 5-day event windows are defined over the interval days starting from one-day after the insider trade day, i.e. [+1, +1], [+1, +3], and [+1, +5] respectively.

The results show a consistent negative relationship between subsidiary lockdowns and insider trading profits, with the effect increasing over the horizon. For example, over the 5-day window [+1, +5], the decline in insider trading profits is significantly related to subsidiary lockdowns in Model 3 (β_1= -0.27 (t-stat=-4.32). One more subsidiary under lockdown is associated with a 49.34% decrease in insider trading profits over the [+1, +1] horizon, a 121.35% decrease over the [+1, +3] horizon and a 77.33% decrease over the [+1, +5] horizon, relative to the average profits. Similar to previous findings, the HQ lockdown has much weaker effect of insider profits. These results indicate that as subsidiary lockdowns reduce insiders' informational advantage, insiders are less able to trade profitably.

6.6 Addressing Alternative Explanations

To ensure robustness of our findings, we conduct a series of tests to rule out alternative explanations that could confound our interpretation of the results.

One potential concern is that subsidiary lockdowns might affect the firm's real operations in ways that indirectly improve the information environment. Specifically, lockdowns at the subsidiary level reduce the firm's overall output, leading to lower business complexity, less operational uncertainty and consequently an improved information environment. In this case, the observed increase in stock price

[19] Notably, before COVID-19, on average, sells accounted for 76% of total insider trades.

informativeness and analysts forecast accuracy will arise not only from restricted access to subsidiary-level information but also fom lower firm-level output.

To test this alternative explanation, we examine key firm level output measures (return on assets (ROA), Sales, and earnings per share (EPS)) before and during lockdown periods. As reported in Internet Appendix Table A7, we find no significant changes in these firm output measures during lockdown periods. If the improvements in informativeness were driven by reductions in firm activity, we would expect to observe declines in firm output. The lack of significant effects on these measures suggests that the observed improvements in price informativeness and forecast accuracy are unlikely to be driven by firm-wide operational slowdowns.

Additionally, we re-estimate our main regressions while explicitly controlling for firm output measures to account for any potential confounding effects. As shown in Internet Appendix Tables A8 through A10, the impact of subsidiary lockdowns on stock price informativeness (jump ratio), liquidity, return synchronicity and idiosyncratic volatility remains statistically and economically significant after controlling for firm output. These results confirm that our findings are not simply an artefact of lower firm level activity, but rather reflect a shift in the nature and accessibility of firm-level information.

A second concern relates to the presence of publicly listed subsidiaries, which introduce additional complexities in interpreting the effects of subsidiary-level lockdowns. Unlike private subsidiaries, publicly listed subsidiaries are subject to mandatory financial disclosures, which may weaken the reliance on geographically proximate subsidiary information. If some subsidiaries in our sample are publicly listed, this could alter the interpretation of our results in several ways. First, since public subsidiaries disclose financial data through regulatory filings, the role of local analysts in gathering private subsidiary information may be diminished. Second, analysts may overweight publicly disclosed subsidiary information, potentially reinforcing local biases rather than reducing them. Third, public subsidiaries may act as both subsidiaries and ultimate parent firms, creating a greater degree of interdependence across firms in our sample and potentially influencing correlations in information spillovers. These considerations suggest that having publicly listed subsidiaries may influence our results.

While this is not a major issue, as the number of listed subsidiaries is very limited (less than 1% of the sample), still we directly address this issue by redefining our sample excluding all publicly listed subsidiaries and reconstructing our key measures based solely on private subsidiary information. We then re-estimate our main regressions using this refined sample. As shown in Table A11-A13, our main results remain robust after this adjustment. This robustness check confirms that the documented effects are primarily driven by information sourced from geographically proximate private subsidiaries, where local analysts and investors play a more significant role in extracting firm-specific insights.

Conclusion

This study provides new evidence on how access to geographically dispersed subsidiary-level information affects corporate information processing and stock market efficiency. Using the COVID-19 pandemic as a natural experiment, we show that restricting physical access to subsidiary operations, via localized lockdowns, improves analyst forecast accuracy, enhances price informativeness, boosts liquidity, and reduces insiders' trading profits. These findings support the bias/attention allocation hypothesis: when analysts and investors focus too heavily on localized information, they introduce systematic noise into forecasts and prices. Restricting access to such information reduces distortion, enabling more accurate aggregation of firm-wide fundamentals.

At the analyst level, we find that proximity to subsidiaries, normally seen as an advantage, induces forecast errors due to overreliance on noisy, local signals. In particular, analysts located near subsidiaries, who typically rely on direct access to subsidiary-level insights, exhibit significant reductions in forecast errors relative to analysts located farther away, when access is restricted. We also find that the analysts near subsidiaries underreact less to new firm level information when the nearby subsidiaries are locked down, which in turn contributes to their improved forecasts. Textual analysis of analyst reports confirms that these analysts shift attention away from subsidiary content during lockdowns, aligning with the observed improvements in forecast quality. These gains aggregate to the firm level: consensus forecasts become more accurate and less dispersed when subsidiary access is restricted, especially for firms where local signals are more likely to mislead, those with high diversification or hierarchical complexity.

In financial markets, restricted subsidiary access improves stock price informativeness by reducing pre-announcement noise and increasing the efficiency of investor response to public signals. Short sellers respond more decisively to forecast revisions, and market reactions to earnings guidance become more muted, consistent with higher-quality information being priced in sooner. Return-based measures show a rebalancing away from idiosyncratic volatility toward market-wide co-movement, suggesting that subsidiary lockdowns reorient investor attention toward broader, less noisy signals.

Finally, the improved information environment leads to greater liquidity and lower information asymmetry. Bid–ask spreads narrow, and insider trading becomes both less frequent and less profitable, indicating a decline in the informational advantage previously conferred by local access. These patterns are robust to alternative specifications and are not driven by real-side changes in firm operations or public subsidiary disclosures.

Taken together, our results reveal that more information is not always better—particularly when it is noisy, localized, and disproportionately weighted. Geographic proximity can be a double-edged sword: while dispersion facilitates information diversity, proximity can foster distortion. Our findings offer a new lens on the role of geographic structure in shaping market efficiency and call for a rethinking of how disaggregated firm information is accessed, disclosed, and interpreted.

These insights open several promising directions for future research. First, understanding the heterogeneity in local information's effects across industries or subsidiary types—operational, strategic, or financial—may refine our understanding of when local access is helpful versus harmful. Second, the analyst bias we identify may extend to institutional investors, rating agencies, or even corporate managers, suggesting a broader behavioral mechanism worth exploring. Finally, future work could investigate how firms structure internal communication and reporting lines to mitigate localized bias and ensure that firm-wide decisions and external market assessments are based on more balanced, representative signals.

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Variable Definition

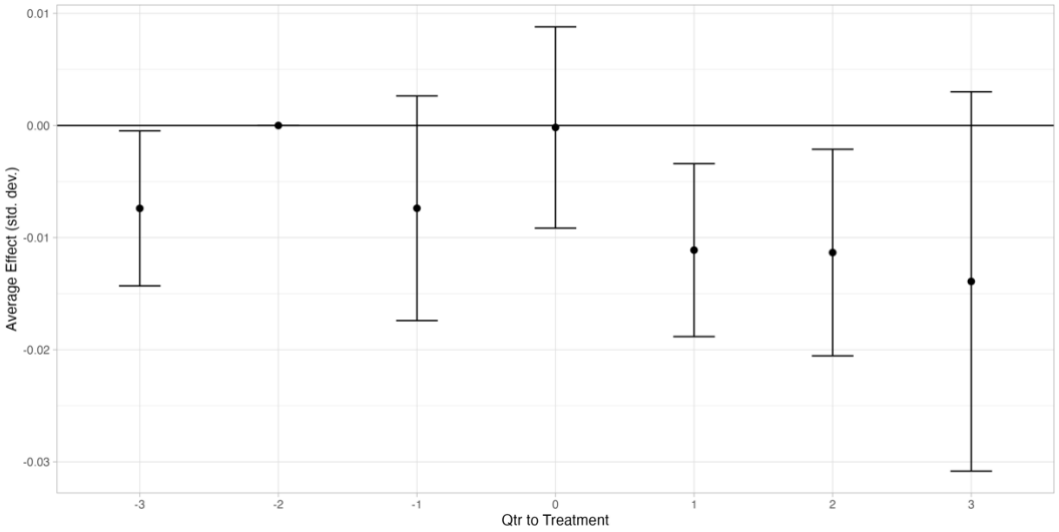
Variable Name	Definition
Subsidiary-Related Variables	
#Subsidiaries	The number of subsidiaries of a firm. Only subsidiaries outside of the same state of the headquarter are counted. The subsidiary data is from LexisNexis.
#Subsidiaries_Lockdown	The number of subsidiaries under lockdown of a firm. Only subsidiaries outside of the same state of the headquarter are counted. The subsidiary data is from LexisNexis. The lockdown is identified following Bai and Massa (2020) using the foot traffic data from SafeGraph Places Patterns dataset. We select data from January 2019 to December 2020. For a zip code in 2020, if its monthly footprint activities contracted 30% relative to the activities in the same zip code in the same month of 2019, it is identified as being under lockdown. Any subsidiaries located in the zip code is identified as being under lockdown.
HQ_Lockdown	Dummy variable of headquarter lockdown state, which equals to 1 when headquarter is under lockdown and equals to 0 when headquarter is not under lockdown. The headquarter data is from LexisNexis. The lockdown data is from SafeGraph.
Industry_Dispersion	Industry dispersion is defined as the number of different industries of subsidiaries other than the primary industry of the firm. We follow the industry classification of SIC (4-Digits) and Fama-French 48 industries (see website link here).
All subsidiaries near analyst under lockdown	Dummy variable for lockdown status of all subsidiaries in the same state as an analyst, which equals to 1 if all subsidiaries of firm i in the same state as analyst j are under lockdown in month t, and 0 otherwise.
All subsidiaries near analyst under lockdown (EA)	Dummy variable for lockdown status of all subsidiaries in the same state as an analyst around firm's earning announcement, which equals to 1 if all subsidiaries of firm i in the same state as analyst j are under lockdown during analyst j's last forecast before the earning announcement, firm i's earning announcement, and analyst j's first forecast after the earning announcement and 0 otherwise.
#Analyst Lockdown (Any)	#Analyst Lockdown (Any) is defined as number of analysts outside HQ state and in whose states at least one of the subsidiaries is under lockdown.
#Analyst Lockdown (All)	#Analyst Lockdown (All) is defined as number of analysts outside HQ state and in whose states all the subsidiaries are under lockdown.
#State Subsidiary Lockdown	The number of subsidiaries under lockdown of a firm in certain state.
Firm-Level Characteristics	
Amihud Illiquidity (x10^6)	Amihud Illiquidity from Amihud (2002). It is the monthly average value of daily illiquidity, which equals to absolute daily adjusted return divided by dollar volume. The data is from CRSP.
PS_Gamma	PS Gamma from Pastor and Stambaugh (2002). It is the ordinary least squares estimate of $\gamma_{i,t}$ in the regression $r^e_{i,d+1,t} = \theta_{i,t} + \phi_{i,t} r_{i,d,t} + \gamma_{i,t} sign\left(r^e_{i,d,t}\right) \cdot v_{i,d,t} + \epsilon_{i,d+1,v}, d = 1, \dots, D,$ where quantities are defined as follows: $r_{i,d,t}$ is the return on stock i on day d in month t; $r^e_{i,d,t} = r_{i,d,t} - r_{m,d,t},$ where $r_{m,d,t}$ is the return on the CRSP value-weighted market return on day d in month t; and $v_{i,d,t}$ is the dollar volume for stock i on day d in month t. A stock's liquidity is computed in a given month only if there are more than 15 observations with which to estimate the regression (D>15). The data is from CRSP.
Roll's Spread	Roll's Spread from Roll (1984). It is the monthly covariance between successive daily price changes, $Cov(\Delta p_t, \Delta p_{t+1})$, where Δp_t is the daily price change on day t and Δp_{t+1} is the daily price change on day $t+1$. The data is from CRSP.
Composite Illiquidity	The average value of z-score of Amihud Illiquidity, negative z-score of PS Gamma, and z-score of Roll's Spread. Here is the formula for calculating z-score for each liquidity measure: $z = \frac{data\ point - mean}{standard\ deviation}.$ For every month, composite illiquidity (union) would be calculated as long as there is at least one non-missing value among the three measures. The composite illiquidity (intersection) would be calculated only when all three measures are non-missing in a month, otherwise the average z-score would be missing.
SYNCH	We estimate the following linear regression using daily returns for each firm in each month: $R_{i,t} = \beta_{i,0} + \beta_{i,1} \times Mkt_RF_t + \beta_{i,2} \times SMB_t + \beta_{i,3} \times HML_t + \beta_{i,4} \times RMW_t + \beta_{i,5} \times CMA_t + \epsilon_{i,t}$, where $R_{i,t}$ is the return of stock i on day t in excess of the risk-free rate, MKT_RF_t is the market return

	on day t minus the risk-free rate, and SMB_t, HML_t, RMW_t, and CMA_t are the factor returns of stock i on day t. Right-hand variables in the regression are obtained from Kenneth French's website. We further estimate the following linear regression using daily returns for each firm in each month by adding industry returns: $R_{i,t} = \beta_{i,0} + \beta_{i,1} \times Mkt_RF_t + \beta_{i,2} \times SMB_t + \beta_{i,3} \times HML_t + \beta_{i,4} \times RMW_t + \beta_{i,5} \times CMA_t + \beta_{i,6} \times Ind$ where Ind_RF_t is the industry return minus the risk-free rate of stock i on day t. We calculate industry returns based on the Fama-French classification of 48 industries. SYNCH is defined as $$\log(\frac{R^2}{1-R^2}),$$ where R^2 is the r-square obtained from the above regression for stock i in month t. We require a minimum of 20 daily observations in the month to calculate $SYNCH_{i,t}$. A high $SYNCH_{i,t}$ indicates that the individual stock price reflects more firm-specific news. Our measure of SYNCH differs from the measures in Roll (1988), Morck, Yeung, and Yu (2000), and Chan and Hameed (2006) in that we include extra factor returns. We also try alternative measures based on regressions where we put only market returns or market returns plus industry returns as the right-hand variables. We show that our results remain qualitatively similar if we use the exactly same measures from the previous literature.
IdioVol	Our measure of idiosyncratic volatility is based on the same regression specification used for SYNCH. IdioVol is then defined as the standard deviation of the residuals from the regression for stock i in month t. We require a minimum of 20 daily observations in the month to calculate IdioVol. High IdioVol means the individual stock price reflects more idiosyncratic news. IdioVol is multiplied by 10^4.
Jump	Jump is the ratio of post-announcement price variation to total variation before and including the earnings announcement: $$Jump_{it}^{(a,b)} = \frac{CAR_{it}^{(T-1,T+b)}}{CAR_{it}^{(T-a,T+b)}},$$ with a>1 to capture pre-announcement variation and b≥0 to capture post- announcement drift. Following Weller (2018), we select a 21-day pre-announcement window (a=21) to balance resolution on earnings announcement price effects against the possibility of earnings-related information entering earlier than the period considered. We define the cumulative variation associated with the quarterly earnings announcement to include an additional two trading days after the announcement (b=2) to allow for post-earnings announcement drift. We estimate abnormal returns relative to the Fama and French three-factor model and five-factor model using daily returns over a 365-calendar day window ending 90 days before the quarterly earnings announcement. Observations with estimation windows with fewer than 63 valid preceding trading days (one calendar quarter) are dropped. We also require the Jump denominator to be larger than return volatility over the preceding month, estimated simply as the square root of the daily return variance. Namely, $$\left|CAR_{it}^{(T-21,T+2)}\right| > \sqrt{24}\hat{\sigma}_{it}.$$ Events exceeding the value on the right represent large announcement period returns (relative to scaled daily volatility) indicative of material earnings announcement information. A high Jump corresponds to a large announcement-date jump relative to pre-announcement drift, whereas a low Jump corresponds to a small announcement-date jump. Aggressive informed trading drives the Jump toward 0, and the absence of informed trading precipitates a Jump close to 1. Higher values of the Jump thus represent less information in prices relative to the post-announcement information set.
CAR	Following Hirshleifer, Lim, and Teoh (2009), the cumulative abnormal returns of the event windows are defined as the difference between the buy-and-hold return of the firm and that of a size and book-to-market (B/M) matching portfolio over the windows. Each stock is matched with 1 of 25 size-B/M portfolios every day based on the market capitalization at the end of last month and the B/M at the end of December of the prior year. We calculate the value-weighted daily returns of the 25 size-B/M portfolios using all listed stocks in NYSE, NASDAQ, and AMEX.
Size	The natural logarithm of stock size by the end of last month. The stock size equals to the absolute stock price multiplying the number of shares outstanding. The data is from CRSP.
BM	The natural logarithm of book-to-market ratio by the end of last month. It equals to the book equity divided by market equity. The book equity is total stock holder's equity plus deferred taxes and investment tax credit minus preferred stock. The preferred stock equals to either preferred stock redemption value, preferred stock liquidating value, and total preferred stock (capital), depending on their availability on order. The data is from CRSP and Compustat.
Monthly Volatility	The monthly volatility equals to monthly standard error of a stock's daily adjusted return in that month. The data is from CRSP.
Conglomerate	Conglomerate is a dummy variable set to 1 if the firm has segments in more than one industries. We follow Fama-French 48 Industries or SIC 4-Digits to define industries.
Insider Trading/Short Selling-Related Variables	
Opportunistic Sell	We download the data on insider trading from Thomson Reuters. We follow Cohen, Malloy, and Pomoski (2012) to identify opportunistic insider trading. We require an insider to make at least one trade in each of the three preceding years to define her as either an opportunistic or a routine trader. Specially, we define a routine trader as an insider who placed a trade in the same calendar month for at least three

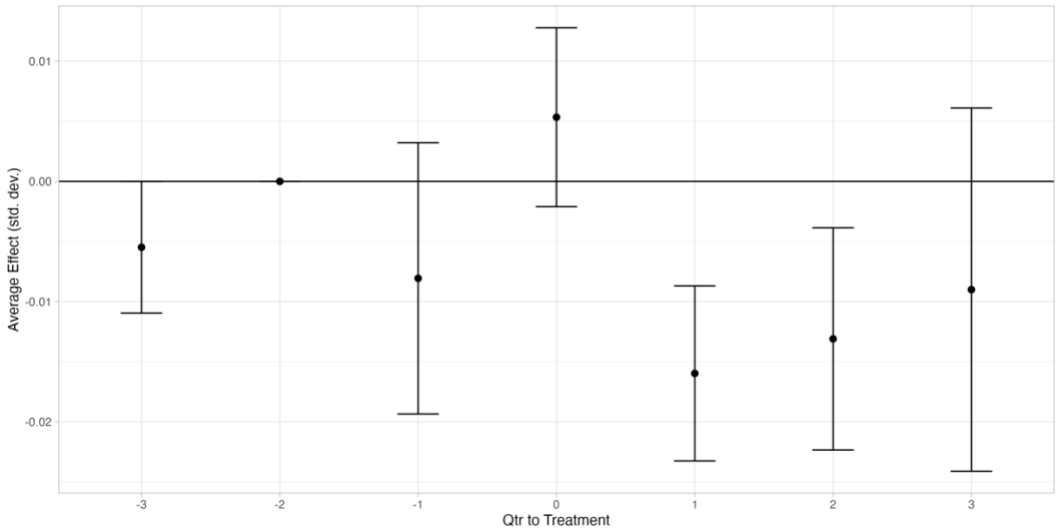
	consecutive years. We define opportunistic traders as everyone else, that is, those insiders for whom we cannot detect an obvious discernible pattern in the past timing of their trades. We thus designate all insiders as either routine traders or opportunistic traders at the beginning of each calendar year, based on their past history of trades, and then see how they trade from that point onward. Once an insider has been classified as a routine trader, she would be routine traders forever thereafter. Once an insider has been classified as an opportunistic trader, she can be reclassified as an routine trader if her trading behaviors satisfy the criteria of routine traders, otherwise she would stay as an opportunistic trader thereafter. All subsequent trades that are made after we classify each insider as either routine or opportunistic are then placed into one of two buckets: (a) "routine trades" (i.e., all trades made by routine traders), and (b) "opportunistic trades" (i.e., all trades made by opportunistic traders). The sell transaction done by opportunistic traders is opportunistic sell. We define opportunistic sell as log(opportunistic sell volume + 1). We also construct an alternative measure, defined as the opportunistic sell volume scaled by the insider's existing holding position before the trade, following Massa, Qian, Xu, and Zhang (2015). Our main results are robust if we use the raw value of opportunistic sell volume or the natural logarithm of the scaled opportunistic sell.
Opportunistic Buy	Similar to opportunistic sell. The buy transaction done by opportunistic traders is opportunistic buy. Opportunistic buy is constructed as either the natural logarithm of the opportunistic buy volume or the opportunistic buy volume divided by the insider's existing holding position before the trade.
Short Volume	We download the monthly short sale volume files from FINRA (see website link here). We aggregate the short sale volume across different exchanges (e.g. Nasdaq and NYSE). Short volume is defined as the monthly the short sale volume scaled by total trading volume or the log of one plus this ratio.
Forecast-Related Variables	
Forecast Error	All data on forecast-related variables is obtain from I/B/E/S. We obtain the data on analyst forecasts of EPS provided in the unadjusted detail history file because there is a rounding-error problem due to stock splits in the adjusted detail history file according to Payne and Thomas (2003). We use the cumulative factor to adjust shares (CFACSHR) in CRSP to adjust for stock splits or reverse splits. When one analyst makes multiple EPS forecasts for the same actual EPS for a firm, we use the most recent forecasted value. Analyst forecast error is constructed based on one-quarter ahead forecasts. Following Thomas (2002) and Guo and Mota (2021), for firm i in quarter t, firm-level analyst forecast error is defined as the absolute value of the difference between the actual earnings per share (EPS) and the forecasted EPS consensus, which is the average value of all forecasted EPS, deflated by the stock price two days prior to the announcement of the actual earnings. We also try an alternative measure, where we replace the forecasted EPS consensus with the median value of forecasted EPS. Alternative measures also doesn't harm our main results. AFE is multiplied by 100 for ease of interpretation. For firm i analyst j in quarter t, analyst-level analyst forecast error is defined as the absolute value of the difference between the actual earnings per share (EPS) and the analyst forecasted EPS, deflated by the stock price two days prior to the announcement of the actual earnings.
Forecast Dispersion	Forecast dispersion is defined as the standard deviation of all analyst forecasts for firm i in quarter t. Following Thomas (2002) and Zhang (2006), we require a minimum of three forecasts to construct forecast dispersion. We apply the same filter to analyst forecast errors. However, our results are robust if we remove the restriction.
Forecast Revision	Forecast revision is at firm level and defined as the EPS forecast consensus scaled by prices in month t minus the scaled EPS forecast consensus in month t-1.
Analyst Forecast Revision	Following Stickel (1991), we look at the difference between the revised forecast and the most recent forecast by the same analyst. In particular, analyst forecast revision is defined as EPS forecasts for firm i by analyst j on day t minus the most recent EPS forecast for firm i by analyst j, deflated by the stock price before forecast revision.
Analyst Coverage	Similar to Chan and Hameed (2006), analyst coverage is defined as the number of analysts covering the firm i in quarter t.
Analyst Forecast Revision across EA	For firm i analyst j in month t, analyst forecast revision across firm's earnings announcements is at individual analyst level and defined as the absolute amount of analyst j's first forecast revision after firm's quarterly earnings announcement for firm i in the quarter of month t. Specifically, for each quarterly earnings announcement, the forecast revision is calculated as the absolute difference between the analyst's forecast immediately before the quarterly earnings announcement and the first forecast made by the same analyst after the subsequent annual financial disclosure date (i.e., the forecast for the upcoming annual financial results).
Sent Freq NearbySub	Sent Freq NearbySub is measured as the count of sentences in analyst j's report on firm i in month that relate to nearby subsidiary information, as classified using a large language model (DeepSeek-V3-0324). A sentence is classified as nearby subsidiary-related if it discusses subsidiaries located in or near the analyst's state, or mentions the firm name of any subsidiary geographically proximate to the analyst.
Sent Freq Firm	Sent Freq Firm is the count of sentences in analyst j's report on firm i in month that relate to firm-level information, as classified using a large language model (DeepSeek-V3-0324). A sentence is classified as firm-level if it discusses company-wide strategy or financials.

Sent Freq Macro	Sent Freq Macro is the count of sentences in analyst j's report on firm i in month that relate to macroeconomic information, as classified using a large language model (DeepSeek-V3-0324). A sentence is classified as macroeconomic if it discusses regulations, GDP, or interest rates. Sent Freq Macro is the count of sentences in analyst j's report on firm i in month that relate to macroeconomic information, as classified using DeepSeek-V3-0324. A sentence is classified as macroeconomic if it discusses regulations, GDP, or interest rates.
Sent Freq Industry	Sent Freq Industry is the count of sentences in analyst j's report on firm i in month that relate to industry-level information, as classified using DeepSeek-V3-0324. A sentence is classified as industry-level if it discusses market trends, competitors, or supply chain.
High Analyst Num	Dummy variable equals to 1 if at least three analysts located in state j cover firm i in month t.
MgtEarningsSurprise	We download data on management forecasts from I/B/E/S Guidance. Similar to Rogers and Stocken (2005), management forecast surprise is a dummy variable set to 1 if the current management forecast is above the latest available consensus analyst forecast, 0 if the current management forecast is within the range of the latest available consensus analyst forecast, -1 if the current management forecast is below the range of the latest available consensus analyst forecast. The current consensus analyst forecast corresponding to a management forecast is provided by I/B/E/S.

Figure 1: The Effects of Subsidiaries Under Lockdown on Jump



Panel A: Effects of Lockdown on $Jump^{FF3}$



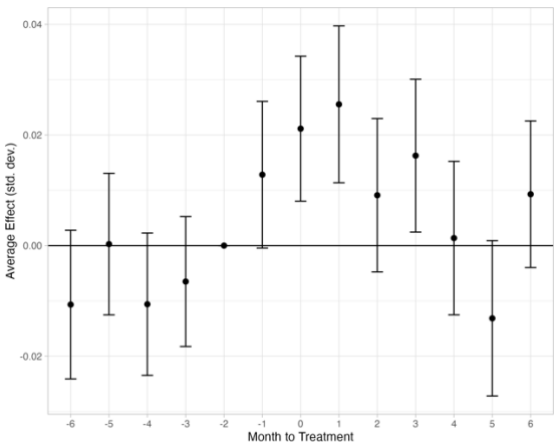
Panel B: Effects of Lockdown on $Jump^{FF5}$

In this Figure, we test the dynamic effects of lockdown on $Jump^{FF3}$ and $Jump^{FF5}$. In particular, we estimate:

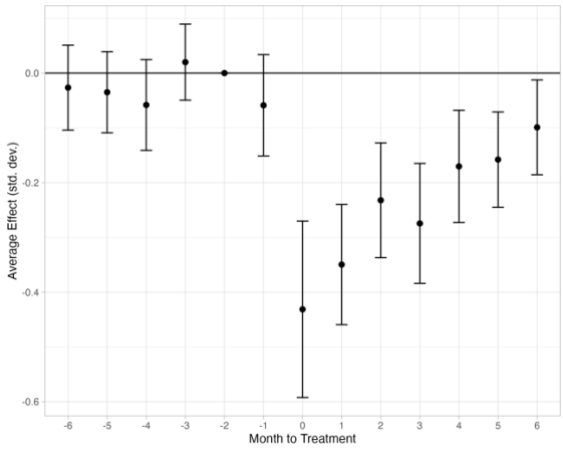
$$Jump_{i,t} = \sum_{s \neq -2} \beta_s \, Avg\#Subsidiary\ Lockdown_i * D_{s(i,t)} + \alpha HQ\ Lockdown_{i,t} + \gamma\ Control_{i,t} + Firm_i + Qtr_Year_t + \varepsilon_{i,t}$$

where the dependent variable $Jump_{i,t}$ here is the price jump ratio based on abnormal returns obtained from Fama-French 3 factor model in Panel A and from Fama-French 5 factor model in Panel B for firm i around the earning announcement in quarter t and $D_{s(i,t)}$ is a set of indicator variables that take value one if the observation corresponds to quarter t relative to the first quarter of subsidiary lockdowns for firm i. $Avg\#Subsidiary\ \ Lockdown_i$ is the average number of subsidiaries under lockdown during the lockdown period, capturing the intensity of the treatment effect. The sample spans from 2016 to 2020 and only includes firms with subsidiaries. For brevity, we only show the coefficients for the period covering the period [-3,3]. We also include a set of control variables including size, book-to-market ratio, past month return, past year return, and monthly volatility. Detailed variable definition is provided in the appendix. We include firm fixed effects and quarter fixed effects and standard errors are clustered by firm and quarter. The bars represent 95% confidence intervals.

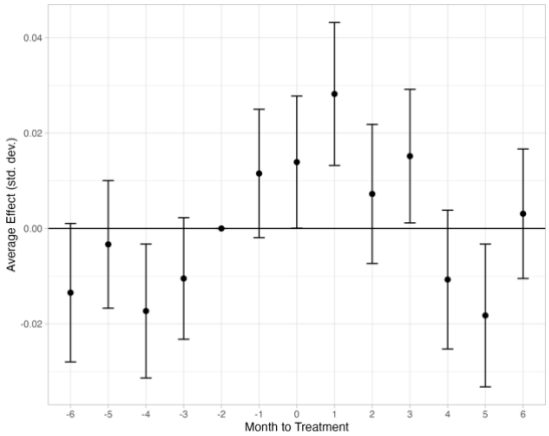
Figure 2: The Effects of Subsidiaries Under Lockdown on SYNCH and IdioVol



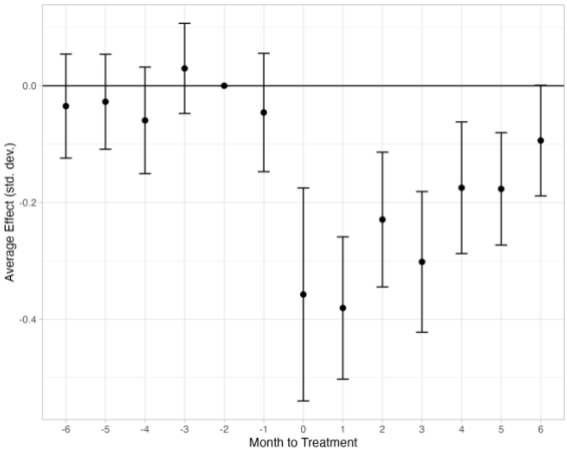
Panel A: Effects of Lockdown on SYNCH



Panel B: Effects of Lockdown on IdioVol



Panel C: Effects of Lockdown on SYNCH



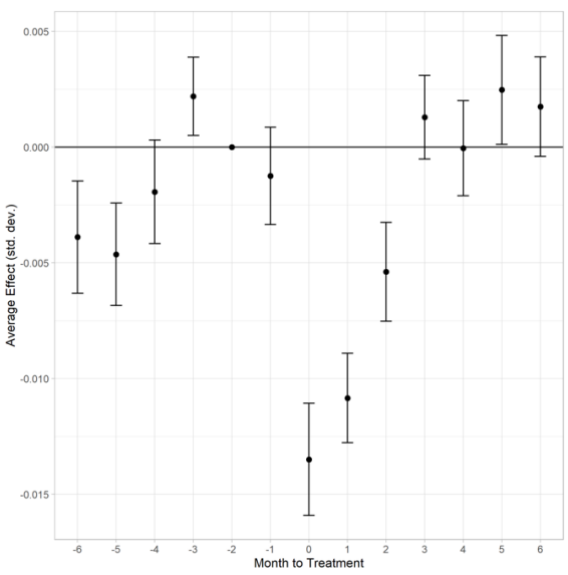
Panel D: Effects of Lockdown on IdioVol

In this Figure, we test the dynamic effects of lockdown on stock liquidity. In particular, we estimate:

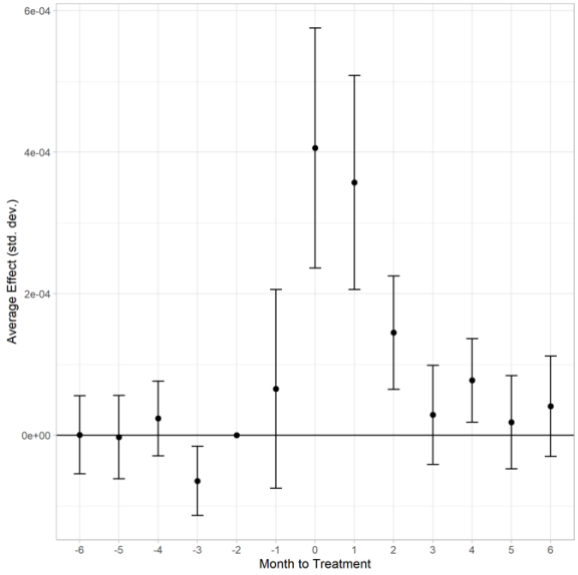
$$Y_{i,t} = \sum_{s\neq -2} \beta_s \, Average\#Subsidiary\ Lockdown_i * D_{s(i,t)} + \alpha HQ\ Lockdown_{i,t} + \gamma\ Control_{i,t} + Firm_i + Month_Year_t + \varepsilon_{i,t}$$

where the dependent variable $Y_{i,t}$ here is SYNCH with and without considering industry information in Panel A & C, and IdioVol with and without considering industry returns in Panel B & D, respectively. On the right hand side, $D_{s(i,t)}$ is a set of indicator variables that take value one if the first lockdown on the subsidiaries of firm i was s months away from the first lockdown in month t. $Average\ Lockdown_i$ measure the intensity of the treatment effects. It is defined as the average value of the number of subsidiaries under lockdown during the lockdown period and set to 0 if firm i never has any subsidiaries under lockdown. The sample spans from 2016 to 2020 and only includes firms with subsidiaries. For brevity, we only show the coefficients for the period covering the period [-6,6]. We also include a set of control variables including size, book-to-market ratio, past month return, past year return, and monthly volatility. Detailed variable definition is provided in the appendix. We include firm fixed effects and month fixed effects and standard errors are clustered by firm and month. The bars represent 95% confidence intervals.

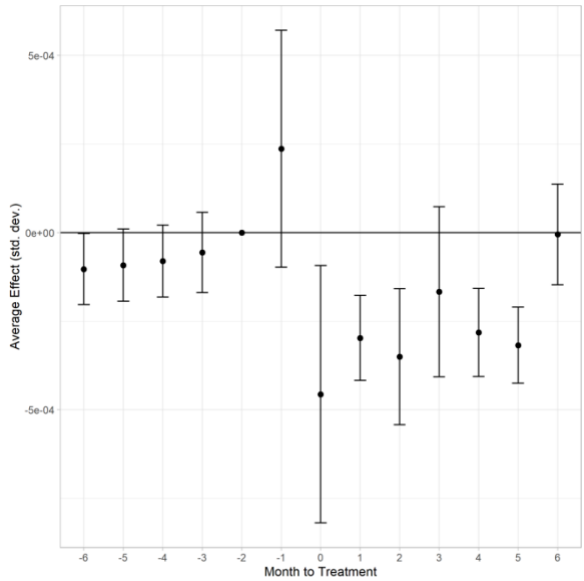
Figure 3: The Effects of Subsidiaries Under Lockdown on Liquidity



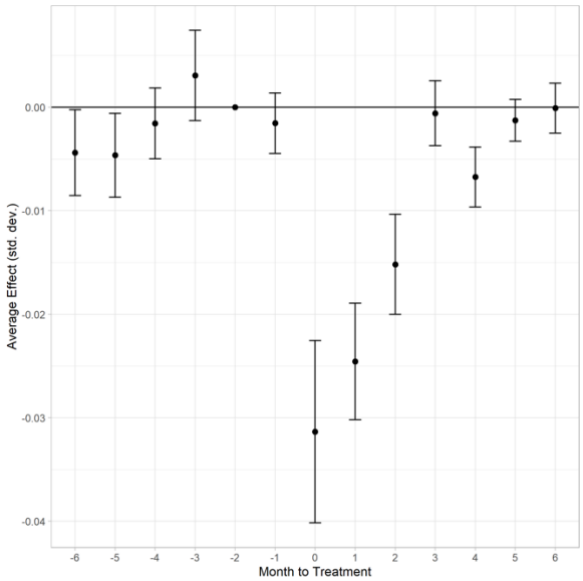
Panel A: Effects of Lockdown on Amihud Illiquidity



Panel B: Effects of Lockdown on PS Gamma



Panel C: Effects of Lockdown on Roll's Spread



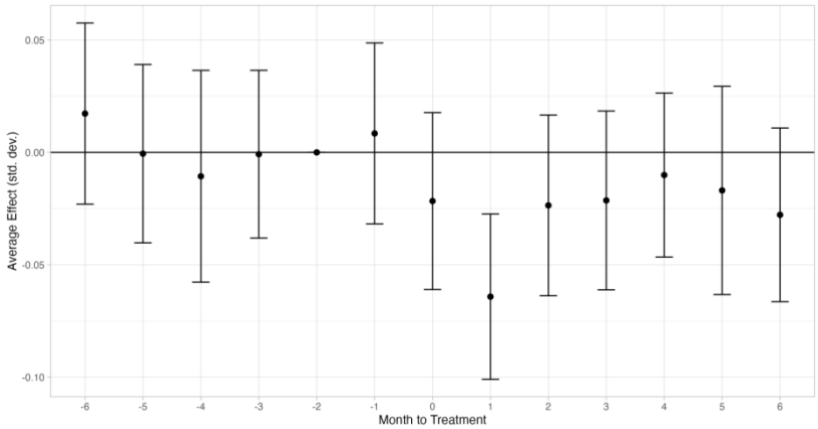
Panel D: Effects of Lockdown on Composite Illiquidity

In this Figure, we test the dynamic effects of lockdown on stock liquidity. In particular, we estimate:

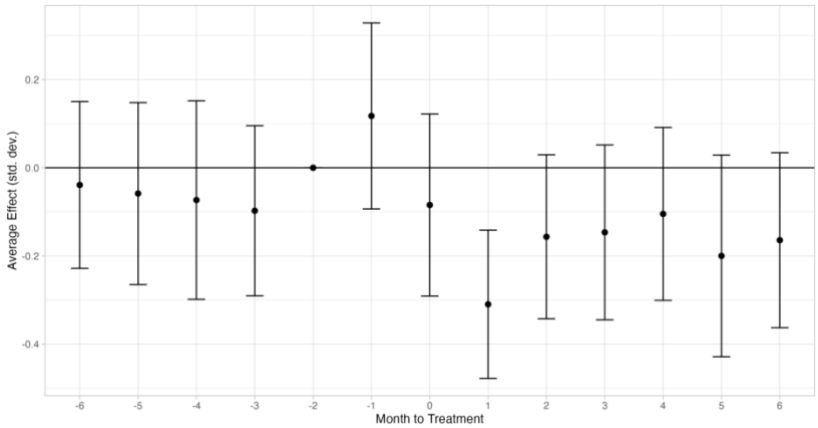
$$Liquidity_{i,t} = \sum_{s \neq -2} \beta_s \, Average\#Subsidiary\ Lockdown_i * D_{s(i,t)} + \alpha HQ\ Lockdown_{i,t} + \gamma\ Control_{i,t} + Firm_i + Month_Year_t + \varepsilon_{i,t}$$

where the dependent variable $Liquidity_{i,t}$ here is Amihud Illiquidity in Panel A, and PS Gamma in Panel B, Roll's spread in Panel C, and Composite Illiquidity, the average z-scores of these three measures, in Panel D. On the right hand side, $D_{s(i,t)}$ is a set of indicator variables that take value one if the first lockdown on the subsidiaries of firm i was s months away in month t. $Average\ Lockdown_i$ measure the intensity of the treatment effects. It is defined as the average value of the number of subsidiaries under lockdown during the lockdown period and set to 0 if firm i never has any subsidiaries under lockdown. The sample spans from 2016 to 2020 and only includes firms with subsidiaries. For brevity, we only show the coefficients for the period covering the period [-6,6]. We also include a set of control variables including size, book-to-market ratio, past month return, past year return, and monthly volatility. Detailed variable definition is provided in the appendix. We include firm fixed effects and month fixed effects and standard errors are clustered by firm and month. The bars represent 95% confidence intervals.

Figure 4: The Effects of Subsidiaries Under Lockdown on Insider Trading Volume



Panel A: Effects of Lockdown on Log(Opportunistic Selling Volume +1)



Panel B: Effects of Lockdown on Opportunistic Selling Volume (%)

In this Figure, we test the dynamic effects of lockdown on opportunistic insider selling volume. In particular, we estimate:

$$Opportunistic\ Sell_{i,t} = \sum_{s\neq -2} \beta_s Average\#Subsidiary\ Lockdown_i * D_{s(i,t)} + \alpha HQ\ Lockdown_{i,t} + \gamma\ Control_{i,t} + Firm_i + Month_Year_t + \varepsilon_{i,t}$$

where the dependent variable $Opportunistic\ Selling_{i,t}$ here is log(opportunistic selling volume + 1) in Panel A and opportunistic selling volume scaled by insider holding position in Panel B and $D_{s(i,t)}$ is a set of indicator variables that take value one if the first lockdown on the subsidiaries of firm i was s months away from the first lockdown in month t. $Average\ Lockdown_i$ measure the intensity of the treatment effects. It is defined as the average value of the number of subsidiaries under lockdown during the lockdown period and set to 0 if firm i never has any subsidiaries under lockdown. The sample spans from 2016 to 2020 and only includes firms with subsidiaries. For brevity, we only show the coefficients for the period covering the period [-6,6]. We also include a set of control variables including size, book-to-market ratio, past month return, past year return, and monthly volatility. Detailed variable definition is provided in the appendix. We include firm fixed effects and month fixed effects and standard errors are clustered by firm and month. The bars represent 95% confidence intervals.

Table 1: Summary Statistics

This table reports the summary statistics for the sample of only firms with subsidiaries. # Subsidiaries is the number of subsidiaries of a firm. # Subsidiary lockdown is the number of subsidiaries under lockdown. HQ lockdown is the dummy variable indicating the status of headquarters under lockdown. Industry dispersion (SIC4Digits) is the number of subsidiaries' industries, measured by 4-digit SIC code. Industry dispersion (FF48) is the number of subsidiaries' industries, measured by Fama French 48 industries. Amihud Illiquidity is from Amihud (2002). PS Gamma is Pastor and Stambaugh Gamma from Pastor and Stambaugh (2002). Roll's Spread is from Roll (1984). Composite Illiquidity is the average z-scores of these three liquidity measures. SYNCH is from Chan and Hameed (2006). We calculate the time-series average of each variable at firm level and then report the cross-sectional mean. Detailed variable definition is provided in the appendix.

						# of firms
	Mean	**SD**	**P10**	**Median**	**P90**	
# Subsidiary	10.077	16.384	1.000	4.000	24.980	1812
# Subsidiary Lockdown	0.331	0.399	0.000	0.167	0.850	1812
# Subsidiary Lockdown (2020 Only)	1.631	1.477	0.250	1.083	4.167	1546
HQ_Lockdown	0.058	0.045	0.000	0.056	0.125	1812
Monthly Return	0.010	0.018	-0.007	0.011	0.028	1659
Size	14.549	1.967	11.900	14.570	17.162	1659
BM	-0.854	0.934	-1.989	-0.720	0.110	1627
Monthly Volatility (x100)	2.476	1.102	1.467	2.172	3.857	1782
Industry_Dispersion (FF48)	2.001	2.318	0.000	1.357	4.512	1812
Amihud Illiquidity($x10^6$)	0.158	0.431	0.001	0.011	0.436	1659
PS Gamma	-2.721	7.334	-7.043	-0.285	-0.017	1603
Roll's Spread	0.020	0.008	0.012	0.018	0.031	1659
Composite Illiquidity	-0.047	0.491	-0.328	-0.196	0.326	1595
IdioVol	4.433	4.748	0.927	2.701	10.908	1812
SYNCH	-0.038	0.549	-0.751	-0.066	0.635	1812
IdioVol (Industry-Adjusted)	3.900	4.375	0.672	2.266	9.964	1812
SYNCH (Industry-Adjusted)	0.345	0.649	-0.472	0.305	1.234	1812
$Jump^{FF3}$	0.44	0.36	0.06	0.45	0.84	1109
$Jump^{FF5}$	0.44	0.38	0.06	0.44	0.83	1109
Analyst Forecast Error (%)	0.91	1.69	0.08	0.31	2.33	1320
Forecast Dispersion (%)	0.50	0.84	0.06	0.20	1.21	1245
Individual Analyst Forecast Error (%)	0.784	1.162	0.089	0.352	2.073	1734
Individual Analyst Revision Amount Around EA	0.461	2.427	0.070	0.197	0.663	1681
All Subs Near Analyst Under Lockdown	0.015	0.035	0.000	0.000	0.051	1734
All Subs Near Analyst Under Lockdown (EA)	0.009	0.019	0.000	0.000	0.032	1681
Opportunistic Buy Volume/Insider Holding (x1000)	2.642	7.619	0	0	7.191	1538
Opportunistic Sell Volume/Insider Holding (x1000)	21.628	48.664	0	4.135	57.630	1538
Opportunistic Buy Plus Sell Volume/Insider Holding (x1000)	28.797	59.502	0.201	7.515	72.846	1538
Scaled Insider Daily Excess Profit [+1, +1] (x1000)	13.985	444.102	-0.152	0	0.263	1565
Scaled Insider Daily Excess Profit [+1, +3] (x1000)	18.129	510.864	-0.158	0.004	0.789	1565
Scaled Insider Daily Excess Profit [+1, +5] (x1000)	35.176	1072.696	-0.220	0.004	0.863	1565
Manager Forecast Surprise (Quarterly)	-0.295	0.530	-1	-0.308	0.333	538
Manager Forecast Surprise (Annual)	-0.001	0.355	-0.4	0	0.353	754

Table 2: The Effects of Subsidiaries under Lockdown on Individual Analyst Forecast Errors

This table reports the effects of nearby subsidiaries under lockdowns on analyst forecast errors at individual level. The sample spans from 2016 to 2020 and only includes firms with subsidiaries. Panel A presents regression results using all individual analyst-level forecast data with available location information. In column 1, we estimate the following regression:

$$Forecast\ error_{i,j,t} = \beta_1 Subsidiaries_nearby_lockdown_{i,j,t} + \beta_2 NearSubsidiary_{i,j,t} + Firm_Mth_Year_{i,t} + Firm_Analyst_{i,j} + \epsilon_{i,j,t},$$

where $Forecast\ error_{i,j,t}$ is analyst j's forecast error for firm i in month t, and $Subsidiaries_nearby_lockdown_{i,j,t}$ is a dummy variable set to 1 if all subsidiaries of firm i in the same state as analyst j are under lockdown in month t and 0 otherwise. $NearSubsidiary_{i,j,t}$ is a dummy variable set to 1 if analyst j locates in the same state as any subsidiary of firm i. We include the firm and year-month interaction fixed effects and firm and analyst interaction fixed effects. In column 2, we estimate a similar regression as in column 1 but include firm-by-analyst fixed effects and analyst-by-time fixed effects, controlling firm characteristics including size, book-to-market ratio, past month return, past year return, monthly volatility. In column 3&4, we regressions similar to the specification used in column 1&2 with additional control for headquarter lockdown $HQ_nearby_lockdown_{i,j,t}$, which is a dummy variable set to 1 if analyst j is in the same state of firm i's headquarter and the headquarter is under lockdown in month t. Panel B reports the effect using sub-samples. In column 1&2, we estimate the regression from Panel A column 3&4 using a subsample which includes analysts near subsidiaries where all nearby subsidiaries are under lockdown, along with analysts located far away. In column 3&4, we estimate a similar regression but use a subsample of analysts near subsidiaries. Standard errors are clustered by firm, year-month, and analyst. Analyst forecast errors are multiplied by 100 for ease of interpretation. Detailed variable definition is provided in the appendix. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Panel A

Dependent variable	**Analyst Forecast Error**			
Sample	All Analysts			
	(1)	(2)	(3)	(4)
Subsidiaries_nearby_lockdown	-0.028**	-0.090***	-0.028**	-0.072**
	(-2.46)	(-2.70)	(-2.18)	(-2.42)
HQ_nearby_lockdown			0.0009	-0.114
			-0.073	(-1.43)
NearSubsidiary	-0.001	0.010	-0.001	0.008
	(-0.173)	(0.308)	(-0.170)	(0.232)
Control	N	Y	N	Y
Firm × Time FE	Y	N	Y	Y
Firm × Analyst FE	Y	Y	Y	N
Analyst × Time FE	N	Y	N	Y
Number of observations	274,269	257,966	274,269	257,966
R-squared	0.88	0.69	0.88	0.87

Panel B

Dependent variable	**Analyst Forecast Error**			
Sample	Analysts Outside of HQ State		Analysts Near Subsidiaries Outside of HQ State	
	(1)	(2)	(3)	(4)
Subsidiaries_ nearby_lockdown	-0.053***	-0.096***	-0.075**	-0.144**
	(-2.89)	(-2.70)	(-2.21)	(-2.57)
NearSubsidiary	0.0006	0.034		
	-0.092	(1.36)		
Control	N	Y	N	Y
Firm × Time FE	Y	N	Y	N
Firm × Analyst FE	Y	Y	Y	Y
Analyst × Time FE	N	Y	N	Y
Analyst FE	N	N	N	N
Number of observations	243,834	229,186	71,660	68,410
R-squared	0.88	0.70	0.9	0.82

Table 3: The Effects of Subsidiaries under Lockdown on Individual Analyst Forecast Revisions across Firm Earnings Announcements

This table reports the effects of nearby subsidiaries under lockdowns on individual analyst forecast revisions across firm earnings announcements at individual level. The sample spans from 2016 to 2020 and only includes firms with subsidiaries. In column 1, we estimate the following regression using all individual analyst-level forecast data with available location:

$$Forecast\ revision_EA_{i,j,t} = \beta_1 All_subsidiaries_nearby_lockdown_EA_{i,j,t} + \beta_2 NearSubsidiary_{i,j,t} + Firm_Mth_Year_{i,t} + Firm_Analyst_{i,j} + \epsilon_{i,j,t},$$

where $Forecast\ revision_EA_{i,j,t}$ is the absolute amount of analyst j's first forecast revision after firm's quarterly earnings announcement for firm i in the quarter of month t. Specifically, for each quarterly earnings announcement, the forecast revision is calculated as the absolute difference between the analyst's forecast immediately before the quarterly earnings announcement and the first forecast made by the same analyst after the subsequent annual financial disclosure date (i.e., the forecast for the upcoming annual financial results). The variable $All_subsidiaries_nearby_lockdown_EA_{i,j,t}$ is a dummy variable set to 1 if all subsidiaries of firm i in the same state as analyst j are under lockdown during analyst j's last forecast before the earning announcement, firm i's earning announcement, and analyst j's first forecast after the earning announcement and 0 otherwise. $NearSubsidiary_EA_{i,j,t}$ is a dummy variable set to 1 if analyst j locates in the same state as any subsidiary of firm i during analyst j's last forecast before the earning announcement, firm i's earning announcement, and analyst j's first forecast after the earning announcement and 0 otherwise. We include the firm and year-quarter interaction fixed effect and firm and analyst interaction fixed effect. In column 2, we estimate the same regression as in column 1 but replace the firm and analyst interaction fixed effect with analyst interaction fixed effects. In column 3, we run a similar regression with additional control for headquarter lockdown, $HQ_nearby_lockdown_EA_{i,j,t}$, which is a dummy variable set to 1 if analyst j is in the same state of firm i's headquarter and the headquarter is under lockdown during analyst j's last forecast before the earning announcement, firm i's earning announcement, and analyst j's first forecast after the earning announcement and 0 otherwise. In column 4, we estimate the same regression as in column 1 but replace the firm and analyst interaction fixed effect with analyst fixed effects. In column 5, we run the following regression using a subsample of analysts located outside the headquarter states. In column 6, we estimate the same regression as in column 1 but replace the firm and analyst interaction fixed effect with analyst fixed effects. Detailed variable definition is provided in the appendix. Standard errors are clustered by firm, year-quarter, and analyst. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Dependent variable	**Analyst Forecast Revision**					
	All Analysts				Analysts Outside of HQ State	
	(1)	(2)	(3)	(4)	(5)	(6)
All subs near analyst under lockdown	-0.290*** (-2.75)	-0.246** (-2.42)	-0.332*** (-2.81)	-0.277** (-2.40)	-0.252** (-2.66)	-0.183** (-2.11)
HQ_lockdown (near analyst)			0.208* (1.84)	0.155* (1.90)		
Analyst near subsidiary	-0.019 (-0.918)	0.0003 (0.020)	-0.016 (-0.784)	0.003 (0.186)	-0.016 (-0.889)	-0.019 (-1.41)
Firm × Time FE	Y	Y	Y	Y	Y	Y
Firm × Analyst FE	Y	N	Y	N	Y	N
Analyst FE	N	Y	N	Y	N	Y
Number of observations	144,051	144,051	144,051	144,051	126,935	126,935
R-squared	0.84	0.72	0.84	0.72	0.91	0.78

Table 4: Coibion-Gorodnichenko Regressions for Quarterly EPS Forecasts

This table reports the results from the Coibion-Gorodnichenko (forecast bias on forecast revision) regression. We estimate the following regression in Column 1 using monthly data:

$$Forecast\ Bias_{i,t} = \beta_1 \#Subsidiary\ Lockdown_{i,t} \times Forecast\ Revision_{i,t} + \beta_2 Forecast\ Revision_{i,t} + \beta_3 \#Subsidiary\ Lockdown_{i,t} + \beta_4 HQ\ Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Quarter_Year_t + \varepsilon_{i,t},$$

where forecast bias is constructed based on one-quarter-ahead forecasts and defined as the difference between actual earnings per share (EPS) and the forecasted EPS consensus, which is the average value of all forecasted EPS in month t, scaled by the stock price two days prior to the earnings announcement. Forecast revision is defined as the EPS forecast consensus scaled by prices in month t minus the scaled EPS forecast consensus in month $t-1$. $\#Subsidiary\ Lockdown_{i,t}$ represents the number of subsidiaries under lockdowns in month t for firm i, and $HQ\ Lockdown_{i,t}$ is a dummy variable set to 1 if the headquarter of firm i is under lockdown in month t and 0 otherwise. In Column 2 to 4, we further include #Analyst Lockdown (Any) and #Analyst Lockdown (All). #Analyst Lockdown (Any) is defined as number of analysts outside HQ state and in whose states at least one of the subsidiaries is under lockdown. #Analyst Lockdown (All) is defined as number of analysts outside HQ state and in whose states all the subsidiaries are under lockdown. We also include a set of control variables including size, book-to-market ratio, past month return, past year return, monthly volatility, and analyst coverage. We construct forecast bias and forecast revision based on analysts near subsidiaries outside HQ state in Columns 1 to 3 and analyst far away from subsidiaries or HQ in Column 4. We include firm and year-quarter fixed effects. Standard errors are clustered by both firm and year-quarter.

Dependent Variables	**Forecast Bias**			
	Analysts Near Subsidiaries Outside HQ			Analysts Far Away from Subsidiaries or HQ
	(1)	(2)	(3)	(4)
#Subsidiary Lockdown* Forecast Revision	-0.021**			-0.011
	(-2.83)			(-1.50)
#Analyst Lockdown (Any)*Forecast Revision		-0.053***		
		(-3.32)		
#Analyst Lockdown (All)*Forecast Revision			-0.053***	
			(-3.51)	
Forecast Revision	0.267***	0.290***	0.280***	0.236***
	(9.58)	(10.8)	(11.3)	(5.29)
#Subsidiary Lockdown	0.000			0.000
	(0.259)			(0.045)
#Analyst Lockdown (Any)		0.0003**		
		(2.37)		
#Analyst Lockdown (All)			0.0003*	
			(2.03)	
HQ Lockdown	0.0004	0.0004	0.0006	0.001
	(0.220)	(0.199)	(0.286)	(1.02)
Firm FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Number of observations	4,777	4,777	4,777	10,362
R-squared	0.27	0.28	0.28	0.28

Table 5: The Effects of Subsidiaries under Lockdown on Individual Analyst Report

This table reports the effects of nearby subsidiaries under lockdowns on analysts' discussions of nearby subsidiaries within their reports, at the individual analyst level. The sample spans from 2019 to 2020 and includes only analysts from the top ten brokerages who cover firms with subsidiaries and are based outside the firm's headquarters state. We estimate the following regression using reports written by analysts located outside the headquarters state:

$$Sent\ Freq\ NearbySub_{i,j,t} = \beta_1 Subsidiaries_nearby_lockdown_{i,j,t} + \beta_2 NearSubsidiary_{i,j,t} + \beta_3 WordCount_{i,j,t} + Controls_{i,t} + Firm_Mth_Year_{i,t} + Firm_Analyst_{i,j} + \epsilon_{i,j,t},$$

where $Sent\ Freq\ NearbySub_{i,j,t}$ is measured as the count of sentences related to nearby subsidiary information extracted from analyst *j*'s report about firm *i* in month *t*. $WordCount_{i,j,t}$ is the total number of words in the report. We include a set of firm-level control variables including size, book-to-market ratio, past month return, past year return, monthly volatility, and analyst coverage. We also include the firm, analyst, year-month fixed effects. Detailed variable definitions are provided in the appendix. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Dependent Variables	**Sentence Count**	
	Subsidiary/Local	Firm+Industry+Macro/Market
	(1)	(2)
Subsidiaries_nearby_lockdown	-0.919***	0.045*
	(-3.32)	(1.67)
NearSubsidiary	0.271	0.147***
	(0.547)	(5.14)
Word Count	0.250***	0.213***
	(5.20)	(16.9)
Controls	Y	Y
Firm FE	Y	Y
Time FE	Y	Y
Analyst FE	Y	Y
Number of observations	1494	1494
Squared Cor.	0.80	0.95
Pseudo R2	0.53	0.87

Table 6: The Effects of Subsidiaries under Lockdown on Analyst Forecast Errors and Forecast Dispersion

This table reports the effects of the number of subsidiaries under lockdowns on analyst forecast errors and forecast dispersion. The sample spans from 2016 to 2020 and only includes firms with subsidiaries. In Column 1 and 2, we estimate the following regression using monthly data:

$$Forecast_{i,t} = \beta_1 \#Subsidiary\ Lockdown_{i,t} + \beta_2 HQ\ Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Qtr_Year_t + \varepsilon_{i,t},$$

where $Forecast_{i,t}$ is analyst forecast error or forecast dispersion for firm i in the quarter of month t, $\#Subsidiary\ Lockdown_{i,t}$ represents the number of subsidiaries under lockdowns in month t for firm i, and $HQ\ Lockdown_{i,t}$ is a dummy variable set to 1 if the headquarter of firm i is under lockdown in month t and 0 otherwise. We also include a set of control variables including size, book-to-market ratio, past month return, past year return, monthly volatility, and analyst coverage. In Column 3-6, we estimate the following regression:

$$Forecast_{i,t} = \beta_1 \#Subsidiary\ Lockdown_{i,t} + \beta_2 \#Subsidiary\ Lockdown_{i,t} \times D_{i,t} + \beta_3 HQ\ Lockdown_i \times D_{i,t} + \beta_4 HQ\ Lockdown_{i,t} + \beta_5 \#Subsidiary\ Lockdown_{i,t} \times \#Segment\ Ind_{i,t} + \gamma Control_{i,t} + Firm_i + Qtr_Year_t + \varepsilon_{i,t},$$

where in Column 3-4, $D_{i,t}$ is $Industry\ Dispersion_{i,t}$, which is the number of different industries of all subsidiaries of firm i in the year before the lockdown happens and $\#Segment\ Ind_i$ is defined the number of different industries of all segments of firm i in the year before the lockdown happens. Industry classification follows the Fama-French 48-industry classification. In Column 5-6, we do not control the interaction $\#Subsidiary\ Lockdown_{i,t} * \#Segment\ Ind_{i,t}$, and $D_{i,t}$ is $Hierarchy_{i,t}$, which is a dummy variable set to 1 if the sum of layers across all subsidiaries of firm i is higher than the median value of sum layers across all subsidiaries across all firms in year t. Analyst forecast errors and forecast dispersion are multiplied by 100 for ease of interpretation. Detailed variable definition is provided in the appendix. Standard errors are clustered by firm and quarter. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Dependent variables	**Analyst Forecast Error**	**Forecast Dispersion**	**Analyst Forecast Error**	**Forecast Dispersion**	**Analyst Forecast Error**	**Forecast Dispersion**
Dummy(D)=			**Industry Dispersion**		**Firm Hierarchy**	
	(1)	(2)	(3)	(4)	**(5)**	**(6)**
#Subsidiary Lockdown	-0.017***	-0.011***	-0.006	-0.002	0.026	-0.012
	(-3.99)	(-3.69)	(-0.859)	(-0.377)	(1.51)	(-1.55)
#Subsidiary Lockdown × D			-0.003*	-0.004***	-0.035*	-0.0007
			(-1.83)	(-3.28)	(-1.95)	(-0.089)
HQ Lockdown × D			-0.015	0.022	-0.102*	0.031
			(-1.26)	(1.68)	(-1.84)	(0.789)
#Subsidiary Lockdown × #Segments Ind			0.002	0.003		
			(0.513)	(0.866)		
HQ Lockdown	-0.073*	-0.054*	-0.049	-0.107***	-0.054	-0.066*
	(-1.97)	(-1.94)	(-1.17)	(-4.40)	(-1.08)	(-2.02)
Control	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y
Number of observations	72,887	69,109	68,171	64,888	72,887	69,109
R-squared	0.51	0.57	0.50	0.57	0.51	0.57

Table 7: The Effects of Subsidiary Lockdowns on State-Level Forecast Errors Conditioning on the Number of Analysts

This table reports the effects of same-state subsidiary lockdowns and the number of analysts on consensus analyst forecast errors within the same state. The sample spans from 2016 to 2020 and includes only firm-state-month observations where at least one analyst is located in the same state as a firm subsidiary. Columns 1 and 2 use the full sample of firm-state observations with local analyst coverage. Columns 3 and 4 restrict the sample to firm-state observations with at least two analysts covering the firm located in the same state as a firm subsidiary. In columns 1 and 3, we estimate the following regression:

$$Forecast\ error_{i,j,t} = \beta_1 \#State\ Subsidiary\ Lockdown_{i,j,t} + \beta_2 High\ Analyst\ Num_{i,j,t} + \gamma Controls_{i,t} + Firm_i + Month_Year_t + \varepsilon_{i,t},$$

where $Forecast\ error_{i,j,t}$ is the consensus forecast error for firm i in state j in month t, $\#State\ Subsidiary\ Lockdown_{i,j,t}$ represents the number of subsidiaries under lockdown in state j for firm i in month t, and $High\ Analyst\ Num_{i,j,t}$ is a dummy equals to 1 if at least three analysts located in state j cover firm i in month t. In columns 2 and 4, we include an interaction term between $\#State\ Subsidiary\ Lockdown_{i,j,t}$ and $High\ Analyst\ Num_{i,j,t}$. We also include a set of control variables including size, book-to-market ratio, past month return, past year return, monthly volatility, and analyst coverage. We include firm and year-month fixed effects. Analyst forecast errors are multiplied by 100 for ease of interpretation. Detailed variable definitions are provided in the appendix. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Dependent Variables	**Analyst Forecast Error**			
	State Analyst Num>=1		State Analyst Num>=2	
	(1)	(2)	(3)	(4)
#State Subsidiary Lockdown × High Analyst Num		-0.027		0.027
		(-1.19)		(0.768)
#State Subsidiary Lockdown	-0.037**	-0.029*	-0.041**	-0.058**
	(-2.56)	(-1.83)	(-2.14)	(-2.10)
High Analyst Num	-0.026***	-0.025***	-0.015	-0.016*
	(-4.10)	(-4.01)	(-1.59)	(-1.75)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Number of observations	122,602	122,602	42,383	42,383
R-squared	0.49	0.49	0.52	0.52

Table 8: Replicating Gerken and Painter (2023) with the Decomposition of Analyst GeoDispersion

This table replicates the tests of geographic dispersion on consensus analyst forecast error from Gerken and Painter (2023) using our sample. We estimate the following regression model:

$$Forecast\ error_{i,t} = \beta_1 GeoDispersion_{i,q} + \sum_k \beta_k Controls_{i,q} + Industry_I + Month_Year_t + \epsilon_{i,q},$$

where $Forecast\ error_{i,t}$ is the absolute consensus analyst forecast error for firm i in the month t, and $GeoDispersion_{i,q}$ is a Herfindahl index of the concentration of analysts in MSAs, multiplied by negative one for ease of interpretation. The specifications follow Gerken and Painter (2023), including industry and year-quarter fixed effects, and controlling for firm size, book-to-market ratio, turnover, institutional ownership, firm age, and analyst coverage. Standard errors are clustered by firm. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable	**Analyst Forecast Error**		
	All Analysts	Analysts Near Subsidiaries (Outside of HQ State)	Analysts Far from Firm
	(1)	(2)	(3)
MSA dispersion index	-2.39**	0.222	-1.02*
	(-2.42)	(0.657)	(-1.80)
Controls	Y	Y	Y
Industry FE	Y	Y	Y
Time FE	Y	Y	Y
Number of observations	15,175	7,654	13,640
R-squared	0.008	0.06	0.008

Table 9: Jump Ratio and Market Reaction to Information Shocks

This table reports the effects of the number of subsidiaries under lockdowns on stock price jump ratios and market reaction to management forecast surprise. The sample spans from 2016 to 2020 and only includes firms with subsidiaries. We estimate the following regression focusing the Jump around quarterly earnings announcements:

$$Jump_{i,t} = \beta_1 \#Subsidiary\ Lockdown_{i,t} + \beta_2 HQ\ Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Quarter_Year_t + \varepsilon_{i,t},$$

where $Jump_{i,t}$ is the price jump ratio for firm i in quarter t, $\#Subsidiary\ Lockdown_{i,t}$ represents the number of subsidiaries under lockdowns in the announcement month of quarter t for firm i, and $HQ\ Lockdown_{i,t}$ is a dummy variable set to 1 if the headquarter of firm i is under lockdown in the announcement month of quarter t and 0 otherwise. We also include firm and year-quarter fixed effects and a set of control variables including size, book-to-market ratio, past month return, and monthly volatility. Jump is the ratio of post-announcement price variation to total variation before and including the quarterly earnings announcement. $Jump^{FF3}$ ($Jump^{FF3}$) is based on a Fama-French three-factor (five-factor) model. Detailed variable definition is provided in the appendix. Standard errors are clustered by firm and month. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Panel A: Price Jump Ratio		
Dependent Variables	**$Jump^{FF3}$**	**$Jump^{FF5}$**
	(1)	(2)
#Subsidiary Lockdown	-0.005**	-0.009**
	(-2.16)	(-2.60)
HQ Lockdown	0.052	-0.082
	(1.41)	(-1.62)
Control	Y	Y
Firm FE	Y	Y
Time FE	Y	Y
Number of observations	21,249	21,249
R-squared	0.10	0.10

Panel B focus on market's reaction to management forecast surprise. We estimate the following regression using monthly data:

$$CAR_{i,t} = \beta_1 \#Subsidiary\ Lockdown_{i,t} \times MgrEarningsSurprise_{i,t} + \beta_2 \#Subsidiary\ Lockdown_{i,t} + \beta_3 MgrEarningsSurprise_{i,t} + \beta_4 HQ\ Lockdown_{i,t} * Mgr_Surprise_{i,t} + \beta_5 HQ\ Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Month_Year_t + \varepsilon_{i,t},$$

where $CAR_{i,t}$ is either 3-day abnormal return during period [t-1, t+1], or 7-day abnormal return during period [t-3, t+3], for firm i who releases its management forecast on day t. *MgtEarningsSurprise* is a categorical variable that takes the following rules: 1 if the current consensus analyst forecast exceeds the management forecast range, 0 if the consensus analyst forecast falls within the management forecast range, -1 if the consensus analyst forecast is below the management forecast range. A positive managerial earnings surprise indicates unexpectedly high guidance, while a negative surprise suggests downward guidance shocks. We don't include controls in column 3 and 5 but include a set of control variables including size, book-to-market ratio, past month return, past year return, and monthly volatility in column 4 and 6. Detailed variable definition is provided in the appendix. The management forecast is of one year horizon. Firm and year-month fixed effects are included in the regression and standard errors are clustered by firm and month. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Panel B: Market's Reaction to Management Forecast Surprise						
Dependent variable:	**3-day CAR [t-1, t+1]**		**5-day CAR [t-2, t+2]**		**7-day CAR [t-3, t+3]**	
	（1）	（2）	（3）	（4）	（5）	（6）
#Subsidiary Lockdown × MgrEarningsSurprise	-0.006**	-0.005*	-0.008**	-0.006**	-0.009**	-0.007**
	(-2.13)	(-1.79)	(-2.54)	(-2.10)	(-2.52)	(-2.17)
#Subsidiary Lockdown	0.003	0.003	0.005	0.005	0.006*	0.006*
	(0.915)	(0.901)	(1.48)	(1.53)	(1.68)	(1.79)
MgrEarningsSurprise	0.057***	0.057***	0.052***	0.051***	0.048***	0.047***
	(5.87)	(5.84)	(4.64)	(4.64)	(4.35)	(4.30)
HQ Lockdown × MgrEarningsSurprise	0.029	0.015	0.052*	0.033	0.068*	0.048
	(0.968)	(0.499)	(1.74)	(1.10)	(1.86)	(1.33)
HQ Lockdown	-0.003	0.008	-0.023	-0.008	-0.033	-0.017
	(-0.114)	(0.281)	(-0.783)	(-0.293)	(-0.934)	(-0.509)
Controls	N	Y	N	Y	N	Y
Firm FE	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y
Number of observations	1,574	1,574	1,574	1,574	1,574	1,574
R-squared	0.64	0.67	0.62	0.66	0.63	0.68

Table 10: The Effects of Nearby Subsidiary Lockdowns on Short Selling Around Analyst Forecast Revisions

This table reports how lockdowns of nearby subsidiaries influence short selling volume around individual analyst forecast revisions. The sample covers the period from 2016 to 2020 and includes only firms with at least one subsidiary. The analysis is conducted at the individual analyst-firm level using forecast data with available analyst location information. We estimate the following regression:

$$Short\ Volume_{i,j,t} = \beta_1 Analyst\ Forecast\ Revision_{i,j,t} \times Subsidiaries_nearby_lockdown_{i,j,t} + \beta_2 Analyst\ Forecast\ Revision_{i,j,t} + \beta_3 Subsidiaries_nearby_lockdown_{i,j,t} + \beta_4 NearSubsidiary_{i,j,t} + \beta_5 Analyst\ Forecast\ Revision_{i,j,t} \times NearSubsidiary_{i,j,t} + \beta_6 HQ_lockdown_{i,j,t} + \beta_7 Analyst\ Forecast\ Revision_{i,j,t} \times HQ_lockdown_{i,j,t},$$

where $Short\ Volume_{i,j,t}$ is measured as either the ratio of monthly short selling volume to total monthly trading volume (columns 1 to 4) or the log of one plus this ratio (columns 5 to 8), for firm i over one of the following windows around the day t when analyst j issues a forecast revisions: [t+1, t+3], [t+1, t+5], [t-3, t+3], or [t-3, t+5]. $Analyst\ Revision_{i,j,t}$ is the value of analyst j's forecast revision for firm i on day t. We include a set of control variables including size, book-to-market ratio, past month return, past year return, monthly volatility, and analyst coverage. We also include firm and year-month fixed effects. Detailed variable definitions are provided in the appendix. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Dependent Variables	**Volume of Short Selling**							
	Short vol / Trading vol				log(1+Short Vol/Trading Vol)			
	[t+1, t+3]	[t+1, t+5]	[t-3, t+3]	[t-3, t+5]	[t+1, t+3]	[t+1, t+5]	[t-3, t+3]	[t-3, t+5]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Analyst Forecast Revision × Subsidiaries_nearby_lockdown	-8.31***	-11.6***	-8.79***	-11.3***	-7.00***	-9.66***	-7.37***	-9.39***
	(-2.85)	(-33.0)	(-3.50)	(-8.58)	(-2.72)	(-14.0)	(-3.35)	(-7.44)
Analyst Forecast Revision	-1.91***	-1.90***	-1.83***	-1.85***	-1.57***	-1.56***	-1.51***	-1.52***
	(-3.76)	(-3.72)	(-3.68)	(-3.83)	(-3.64)	(-3.61)	(-3.55)	(-3.70)
Subsidiaries_nearby_lockdown	2.02	2.00	2.34**	2.35**	1.78	1.76	2.05**	2.06**
	(1.51)	(1.60)	(2.06)	(2.08)	(1.52)	(1.61)	(2.05)	(2.07)
Analyst Forecast Revision × NearSubsidiary	0.096	0.023	0.016	0.032	0.093	0.040	0.020	0.036
	(0.179)	(0.044)	(0.031)	(0.064)	(0.205)	(0.088)	(0.046)	(0.086)
Analyst Forecast Revision × HQ_lockdown	-8.16	-5.25	-9.63	-8.20	-7.19	-4.73	-8.37	-7.17
	(-0.983)	(-0.909)	(-1.12)	(-1.14)	(-1.03)	(-0.977)	(-1.14)	(-1.17)
NearSubsidiary	0.402	0.187	-0.074	-0.093	0.348	0.163	-0.058	-0.075
	(1.16)	(0.569)	(-0.233)	(-0.297)	(1.14)	(0.562)	(-0.208)	(-0.272)
HQ_lockdown	0.434	0.873	0.648	0.969	0.441	0.828	0.626	0.907
	(0.176)	(0.387)	(0.268)	(0.419)	(0.203)	(0.416)	(0.295)	(0.445)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y	Y	Y
Number of observations	248,349	248,346	248,351	248,348	248,349	248,346	248,351	248,348
R-squared	0.34	0.38	0.42	0.44	0.34	0.38	0.42	0.44

Table 11: The Effects of Subsidiaries under Lockdown on Price Synchronicity and Idiosyncratic Volatility

This table reports the effects of the number of subsidiaries under lockdowns on SYNCH and IdioVol. The sample spans from 2016 to 2020 and only includes firms with subsidiaries. In Column 1 to Column 4, we estimate the following regression using monthly data:

$$I_{i,t} = \beta_1 \#Subsidiary\ Lockdown_{i,t} + \beta_2 HQ\ Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Month_Year_t + \varepsilon_{i,t},$$

where $I_{i,t}$ is SYNCH measure or IdioVol for firm i in month t, $\#Subsidiary\ Lockdown_{i,t}$ represents the number of subsidiaries under lockdowns in month t for firm i, and $HQ\ Lockdown_{i,t}$ is a dummy variable set to 1 if the headquarter of firm i is under lockdown in month t and 0 otherwise. We also include firm and year-month fixed effects and a set of control variables including size, book-to-market ratio, past month return, past year return, monthly volatility, and analyst coverage. To obtain SYNCH and IdioVol in Column 1 & 2, we estimate the following linear regression using daily returns for each firm in each month: $R_{i,t} = \beta_{i,0} + \beta_{i,1} \times Mkt_{RF_t} + \beta_{i,2} \times SMB_t + \beta_{i,3} \times HML_t + \beta_{i,4} \times RMW_t + \beta_{i,5} \times CMA_t + \varepsilon_{i,t}$, where $R_{i,t}$ is the return of stock i on day t in excess of the risk-free rate, MKT_RF_t is the market return on day t minus the risk-free rate, and SMB_t, HML_t, RMW_t, and CMA_t are the factor returns of stock i on day t. Right-hand variables in the regression are obtained from Kenneth French's website. SYNCH is defined as $SYNCH_{i,t} = \log(\frac{R^2}{1-R^2})$, where R^2 is the r-square obtained from the above regression for stock i in month t. IdioVol is then defined as the standard deviation of the residuals from the regression for stock i in month t. We require a minimum of 20 daily observations in the month to calculate $SYNCH_{i,t}$. We add industry returns to the above regression as the right-hand variable and estimate the SYNCH and IdioVol in Column 3 & Column 4. We multiply IdioVol by 10^4. Detailed variable definition is provided in the appendix. Standard errors are clustered by firm and month. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Dependent Variables	**SYNCH**	**IdioVol**	**SYNCH (Industry-Adjusted)**	**IdioVol (Industry-Adjusted)**
	(1)	(2)	(3)	(4)
#Subsidiary Lockdown	0.018***	-0.261***	0.014***	-0.255***
	(5.81)	(-7.82)	(4.74)	(-8.47)
HQ Lockdown	-0.038	-0.241	-0.021	-0.349
	(-1.29)	(-0.593)	(-0.735)	(-0.959)
Control	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Number of observations	72,435	72,435	72,435	72,435
R-squared	0.43	0.46	0.49	0.46

Table 12: The Effects of Subsidiaries under Lockdown on Liquidity

This table reports the effects of the number of subsidiaries under lockdowns on liquidity. The sample spans from 2016 to 2020 and only includes firms with subsidiaries. We estimate the following regression using monthly data:

$$L_{i,t} = \beta_1 \#Subsidiary\ Lockdown_{i,t} + \beta_2 HQ\ Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Month_Year_t + \varepsilon_{i,t},$$

where $L_{i,t}$ is a liquidity measure for firm i in the quarter of month t, $\#Subsidiary\ Lockdown_{i,t}$ represents the number of subsidiaries under lockdowns in month t for firm i, and $HQ\ Lockdown_{i,t}$ is a dummy variable set to 1 if the headquarter of firm i is under lockdown in month t and 0 otherwise. We also include a set of control variables including book-to-market ratio, past month return, past year return, and monthly volatility. We have four different measures for liquidity, Amihud Illiquidity from Amihud (2002), PS Gamma from Pastor and Stambaugh (2002), Roll's Spread from Roll (1984), and the composite illiquidity index, defined as the average z-scores of these three measures. Detailed variable definition is provided in the appendix. Standard errors are clustered by firm and month. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Dependent variables	**Amihud Illiquidity**	**PS Gamma**	**Roll's Spread**	**Composite Illiquidity**
	(1)	(2)	(3)	(4)
#Subsidiary Lockdown	-0.4569*	0.0161**	-0.0267**	-1.123***
	(-1.745)	(2.240)	(-2.373)	(-2.719)
HQ Lockdown	0.9075	0.0505	-0.1860**	-2.979
	(0.9707)	(0.8055)	(-2.632)	(-0.6435)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Number of observations	84,984	42,397	52,630	84,984
R-squared	0.789	0.485	0.606	0.517

Table 13: The Effects of Subsidiaries under Lockdown on Opportunistic Insider Volume and Profits

This table reports the effects of the number of subsidiaries under lockdowns on insider trading volumes and its predictability of returns. The sample spans from 2016 to 2020 and only includes firms with subsidiaries. In Panel A, we estimate the following regression using daily insider trading data and monthly lockdown and financial data:

$$Opportunistic\ TradingVol_{i,t} = \beta_1 \#Subsidiary\ Lockdown_{i,t} + \beta_2 HQ\ Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Month_Year_t + \varepsilon_{i,t},$$

where $Opportunistic\ TradingVol_{i,t}$ is defined as monthly $opportunistic\ trading/\ insider\ holding\ position\ by\ last\ month$, multiplied 1000. The numerator can be $Oppo\ Sell_{i,t}$opportunistic insider selling volume, $Oppo\ Buy_{i,t}$ opportunistic insider buying volume, or $Oppo\ Buy + Sell_{i,t}$opportunistic insider buying plus selling volume, for firm i in month t, $\#Subsidiary\ Lockdown_{i,t}$ represents the number of subsidiaries under lockdowns in month t for firm i, and $HQ\ Lockdown_{i,t}$ is a dummy variable set to 1 if the headquarter of firm i is under lockdown in month t and 0 otherwise. In Panel B, we estimate the following regression using daily insider trading data and monthly lockdown and financial data:

$$Opportunistic\ TradingProfit_{i,t} = \beta_1 \#Subsidiary\ Lockdown_{i,t} + \beta_2 HQ\ Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Month_Year_t + \varepsilon_{i,t},$$

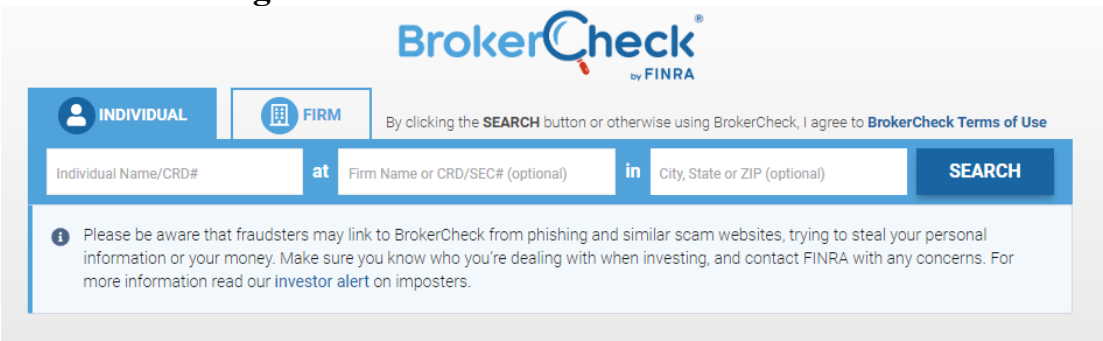
where $Opportunistic\ TradingProfit_{i,t}$ is excess profit of opportunistic insider buying minus selling, measured by trading dollar volume multiplying abnormal return scaled by dollar volume of insiders' holding position, multiplied by 1000. The abnormal return is stock return minus CRSP value-weighted market portfolio return, and the window of the profit is either [+1, +1], [+1, +3], or [+2, +5]. We also include a set of control variables including size, book-to-market ratio, past month return, past year return, and monthly volatility. Detailed variable definition is provided in the appendix. Standard errors are clustered by firm and year-month. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Panel A	**Opportunistic Trading Volume**		
	(Oppo buy+sell) / Insider holding	**Oppo sell / Insider holding**	**Oppo buy / Insider holding**
	(1)	**(2)**	**(3)**
#Subsidiary Lockdown	-0.714**	-0.52**	-0.023
	(-2.16)	(-2.03)	(-0.35)
HQ Lockdown	-0.043	0.292	-0.28
	(-0.02)	(0.09)	(-0.67)
Controls	Y	Y	Y
Firm FE	Y	Y	Y
Time FE	Y	Y	Y
Number of observations	18836	18836	18836
R-squared	0.0010	0.0020	0.0241

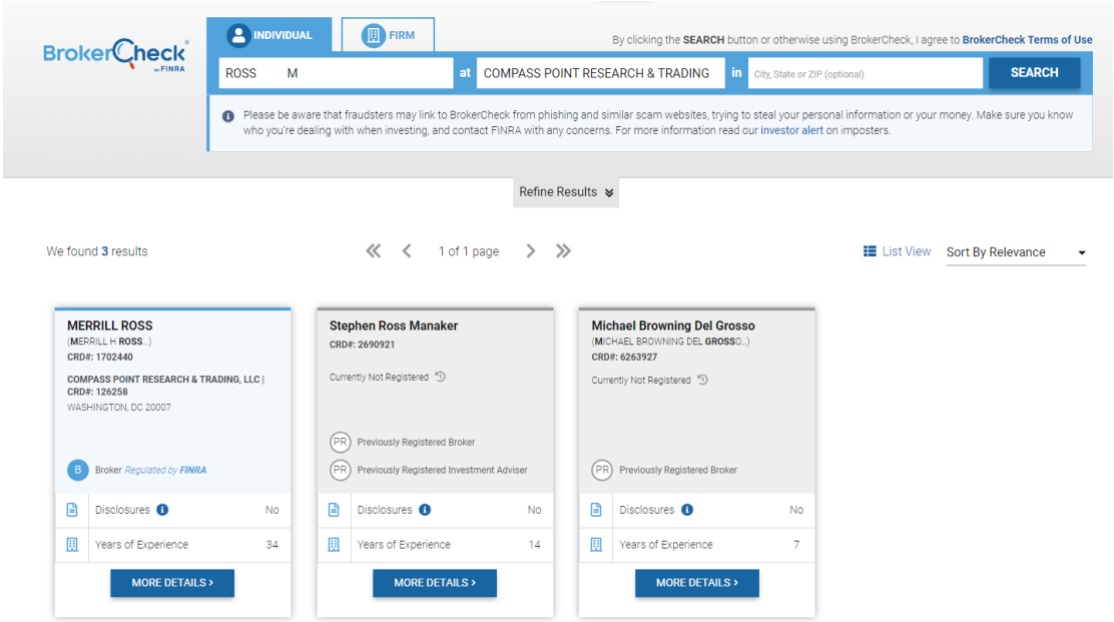
Panel B	**Opportunistic Trading Profits**		
	[+1, +1]	**[+1, +3]**	**[+1, +5]**
	(1)	(2)	(3)
#Subsidiary Lockdown	-0.069*	-0.22**	-0.272***
	(-1.79)	(-2.26)	(-4.32)
HQ Lockdown	1.166	2.91	3.07*
	(1.58)	(1.59)	(1.87)
Controls	Y	Y	Y
Firm FE	Y	Y	Y
Time FE	Y	Y	Y
Number of observations	18900	18900	18900
R-squared	1.727e-05	2.134e-05	1.741e-05

Appendix A

Figure A1: The Interface of BrokerCheck



Panel A: The Search Interface of BrokeCheck



Panel B: A Screenshot of the Searched Results for An Analyst-Broker Pair



Panel C: A Screenshot of The Working Experience of The Matched Analyst

In this figure, we present the screenshot of BrokerCheck search interface in Panel A, a screenshot of the searched results for an analyst-broker pair in Panel B, and a screenshot of the working experience of the matched analyst in Panel C.

Table A1: Sample Creation

This table reports the impact of various data filters on the initial LexisNexis sample.

	Dropped Observations	Sample Size	Number of Firms
LexisNexis Corporate Affiliations Data 2016 to 2020 (2020 is a replication of 2019)		2077787	471861
Drop if (parent) company is not traded in NYSE/NASDAQ/AMEX	1585988	491799	121300
Drop if company is not located in the U.S.	246156	245643	62044
Drop if company type is not subsidiary, parent, affiliate, holding, or group	38449	207194	52681
CRSP CUSIP match	233323	12320	2995
IBES CUSIP match	4506	7814	1812
Extend annual observation to monthly observation		93768	1812
Drop if number of subsidiaries = 0	4599	89169	1812

Table A2: The Effects of Subsidiaries under Lockdown on Individual Analyst Forecast Errors Using Alternative Lockdown Cut-offs

This table reports the effects of nearby subsidiaries under lockdowns on analyst forecast errors at individual level, focusing on a subsample of analysts located outside the headquarters states, using alternative cutoff values for defining lockdowns. To define a lockdown, we classify a ZIP code as under lockdown if its monthly mobile phone footprint declines relative to the same month in the previous year. We vary the threshold of footprint decline across columns to test robustness: ⩾40% in columns (1) and (2), ⩾50% in columns (3) and (4), ⩾60% in columns (5) and (6), and ⩾70% in columns (7) and (8). The regression specification follows that of Table 2, with the dependent variable being analyst forecast error. We use the same set of control variables, fixed effects, and standard errors clustered by firm and month. The sample spans from 2016 to 2020 and only includes firms with non-public subsidiaries. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variable	**Analyst Forecast Error**							
Lockdown Cut-off Values	40%		50%		60%		70%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Subsidiaries_nearby _lockdown	-0.058***	-0.120***	-0.049**	-0.102***	-0.042**	-0.085**	-0.010	-0.099*
	(-2.76)	(-3.36)	(-2.56)	(-2.92)	(-2.14)	(-2.15)	(-0.273)	(-1.82)
NearSubsidiary	0.0005	0.034	-0.0003	0.033	-0.001	0.031	-0.002	0.030
	(0.068)	(1.44)	(-0.041)	(1.39)	(-0.166)	(1.32)	(-0.314)	(1.32)
Control	N	Y	N	Y	N	Y	N	Y
Firm × Time FE	Y	N	Y	N	Y	N	Y	N
Firm × Analyst FE	Y	Y	Y	Y	Y	Y	Y	Y
Analyst × Time FE	N	Y	N	Y	N	Y	N	Y
Analyst FE	N	N	N	N	N	N	N	N
Number of observations	243,834	229,186	243,834	229,186	243,834	229,186	243,834	229,186
R-squared	0.88	0.70	0.88	0.70	0.88	0.70	0.88	0.70

Table A3: The Effects of Subsidiaries under Lockdown on Analyst Forecast Errors and Forecast Dispersion Using Alternative Lockdown Cut-offs

This table reports the effects of the number of subsidiaries under lockdowns on analyst forecast errors and forecast dispersion, using alternative cutoff values for defining lockdowns. To define a lockdown, we classify a ZIP code as under lockdown if its monthly mobile phone footprint declines relative to the same month in the previous year. We vary the threshold of footprint decline across columns to test robustness: ⩾40% in columns (1) and (5), ⩾50% in columns (2) and (6), ⩾60% in columns (3) and (7), and ⩾70% in columns (4) and (8).The regression follows the same specification as Table 6, with the dependent variable being consensus analyst forecast error or forecast dispersion, using the same control variables, fixed effects, and clustered standard errors by firm and month. The sample spans from 2016 to 2020 and only includes firms with non-public subsidiaries. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variables	**Analyst Forecast Error**				**Forecast Dispersion**			
Lockdown Cut-off Values	40%	50%	60%	70%	40%	50%	60%	70%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
#Subsidiary Lockdown	-0.024***	-0.032***	-0.028***	-0.072***	-0.012***	-0.016***	-0.015**	-0.032***
	(-4.66)	(-4.82)	(-2.89)	(-4.26)	(-3.44)	(-3.71)	(-2.75)	(-3.40)
HQ Lockdown	-0.044	-0.038	-0.059*	-0.015	-0.051**	-0.036	-0.016	-0.025
	(-1.25)	(-1.19)	(-1.98)	(-0.483)	(-2.38)	(-1.52)	(-1.01)	(-1.50)
Control	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y	Y	Y
Number of observations	73,281	73,281	73,281	73,281	69,509	69,509	69,509	69,509
R-squared	0.51	0.51	0.51	0.51	0.57	0.57	0.57	0.57

Table A4: Jump Ratio and Market Reaction to Information Shocks Using Alternative Lockdown Cut-offs

This table reports the effects of the number of subsidiaries under lockdowns on stock price jump ratios and market reaction to management forecast surprise, using alternative cutoff values for defining lockdowns. To define a lockdown, we classify a ZIP code as under lockdown if its monthly mobile phone footprint declines relative to the same month in the previous year. We vary the threshold of footprint decline across columns to test robustness: ⩾40% in columns (1) and (5), ⩾50% in columns (2) and (6), ⩾60% in columns (3) and (7), and ⩾70% in columns (4) and (8).The regression follows the same specification as Table 9, with the dependent variable being price jump ratio, $Jump^{FF3}$ ($Jump^{FF3}$), based on a Fama-French three-factor (five-factor) model. We use the same control variables, fixed effects, and clustered standard errors by firm and month. The sample spans from 2016 to 2020 and only includes firms with non-public subsidiaries. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variables	**$Jump^{FF3}$**				**$Jump^{FF5}$**			
Lockdown Cut-off Values	40%	50%	60%	70%	40%	50%	60%	70%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
#Subsidiary Lockdown	-0.006*	-0.007	-0.017***	-0.024*	-0.012***	-0.014***	-0.029***	-0.044***
	(-1.97)	(-1.53)	(-3.35)	(-2.06)	(-3.79)	(-3.58)	(-4.60)	(-3.42)
HQ Lockdown	0.011	-0.009	0.022	0.016	-0.068**	-0.056	0.002	-0.003
	(0.413)	(-0.298)	(1.07)	(0.812)	(-2.13)	(-1.51)	(0.052)	(-0.118)
Control	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y	Y	Y
Number of observations	21,326	21,326	21,326	21,326	21,313	21,313	21,313	21,313
R-squared	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10

Table A5: The Effects of Subsidiaries under Lockdown on Price Synchronicity and Idiosyncratic Volatility Using Alternative Lockdown Cut-offs

This table reports the effects of the number of subsidiaries under lockdowns on stock price informativeness, using alternative cutoff values for defining lockdowns. To define a lockdown, we classify a ZIP code as under lockdown if its monthly mobile phone footprint declines relative to the same month in the previous year. We vary the threshold of footprint decline across columns to test robustness: ⩾40% in columns (1) and (5), ⩾50% in columns (2) and (6), ⩾60% in columns (3) and (7), and ⩾70% in columns (4) and (8). The regression specification follows that of Table 11. The dependent variable is SYNCH and IdioVol. We use the same control variables, fixed effects, and clustered standard errors by firm and month. The sample spans from 2016 to 2020 and only includes firms with non-public subsidiaries. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent Variables	**SYNCH**				**IdioVol**			
Lockdown Cut-off Values	40%	50%	60%	70%	40%	50%	60%	70%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
#Subsidiary Lockdown	0.021*** (3.16)	0.024** (2.07)	0.028 (1.62)	0.031 (0.898)	-0.314*** (-6.43)	-0.384*** (-4.68)	-0.512*** (-3.77)	-0.781*** (-3.99)
HQ Lockdown	-0.038 (-0.774)	-0.041 (-1.10)	-0.019 (-0.993)	-0.036* (-1.82)	0.061 (0.183)	0.080 (0.204)	-0.135 (-0.390)	-0.153 (-0.583)
Control	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y	Y	Y	Y
Number of observations	72,730	72,730	72,730	72,730	72,730	72,730	72,730	72,730
R-squared	0.42868	0.42857	0.42849	0.42840	0.46722	0.46683	0.46674	0.46631

Table A6: The Effects of Subsidiaries under Lockdown on Liquidity Controlling for Size

This table reports the effects of the number of subsidiaries under lockdowns on liquidity. The sample spans from 2016 to 2020 and only includes firms with subsidiaries. We estimate the following regression using monthly data:

$$L_{i,t} = \beta_1 \#Subsidiary\ Lockdown_{i,t} + \beta_2 HQ\ Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Month_Year_t + \varepsilon_{i,t},$$

where $L_{i,t}$ is a liquidity measure for firm i in the quarter of month t, $\#Subsidiary\ Lockdown_{i,t}$ represents the number of subsidiaries under lockdowns in month t for firm i, and $HQ\ Lockdown_{i,t}$ is a dummy variable set to 1 if the headquarter of firm i is under lockdown in month t and 0 otherwise. We also include a set of control variables including size, book-to-market ratio, past month return, past year return, and monthly volatility. We have four different measures for liquidity, Amihud Illiquidity from Amihud (2002), PS Gamma from Pastor and Stambaugh (2002), Roll's Spread from Roll (1984), and the composite illiquidity index, defined as the average z-scores of these three measures. Detailed variable definition is provided in the appendix. Standard errors are clustered by firm and month. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Dependent variables	**Amihud Illiquidity**	**PS Gamma**	**Roll's Spread**	**Composite Illiquidity**
	(1)	(2)	(3)	(4)
#Subsidiary Lockdown	-0.2763	0.0152**	-0.0221*	-0.9021**
	(-1.065)	(2.053)	(-1.958)	(-2.174)
HQ Lockdown	0.7125	0.0495	-0.1955***	-3.217
	(0.8171)	(0.8009)	(-2.847)	(-0.7048)
Size	-18.96***	0.2155***	-0.4873***	-23.18***
	(-9.075)	(7.916)	(-9.912)	(-10.29)
	(-4.562)	(3.361)	(7.150)	(0.1068)
Other Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Number of observations	84,984	42,397	52,630	84,984
R-squared	0.802	0.489	0.611	0.525

Table A7: The Effects of Subsidiaries under Lockdown on Firm Productivity

This table reports the effects of the number of subsidiaries under lockdowns on firm productivity. The sample spans from 2016 to 2020 and only includes firms with subsidiaries. We estimate the following regression using monthly data:

$$P_{i,t} = \beta_1 \#SubsidiaryLockdown_{i,t} + \beta_2 HQLockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Month_Year_t + \epsilon_{i,t},$$

where $P_{i,t}$ is the latest available quarterly ROA (return on assets), or quarterly EPS (earnings per share), or quarterly Sale of firm i in month t, which measures the productivity of firm, $\#Subsidiary\ Lockdown_{i,t}$ represents the number of subsidiaries under lockdowns in month t for firm i, and $HQ\ Lockdown_{i,t}$ is a dummy variable set to 1 if the headquarter of firm i is under lockdown in month t and 0 otherwise. We also include firm and quarter fixed effects and a set of control variables including size, book-to-market ratio, past month return, and monthly volatility. ROA is defined as firm's quarterly net income divided by total assets. EPS is defined as quarterly net income divided by total share outstanding. Sales is defined as total sales divided by total assets. Standard errors are clustered by firm and quarter. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Dependent variables	**ROA**	**EPS**	**Sales**
	(1)	(2)	(3)
#Subsidiary Lockdown	0.0112 (1.008)	-0.5610 (-0.6843)	0.0520 (1.558)
HQ Lockdown	-0.2206 (-1.638)	-5.846 (-1.079)	0.2986 (1.370)
Controls	Y	Y	Y
Firm FE	Y	Y	Y
Time FE	Y	Y	Y
Number of observations	77,744	77,702	77,759
R-squared	0.337	0.536	0.934

Table A8: The Effects of Subsidiaries under Lockdown on SYNCH and IdioVol with Controlling Firm Productivity

This table reports the effects of the number of subsidiaries under lockdowns on stock price informativeness. The sample spans from 2016 to 2020 and only includes firms with subsidiaries. We estimate the following regression using monthly data:

$$I_{i,t} = \beta_1 \#Subsidiary\ Lockdown_{i,t} + \beta_2 HQ\ Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Month_Year_t + \varepsilon_{i,t},$$

where $I_{i,t}$ is SYNCH measure or IdioVol for firm i in month t, $\#Subsidiary\ Lockdown_{i,t}$ represents the number of subsidiaries under lockdowns in month t for firm i, and $HQ\ Lockdown_{i,t}$ is a dummy variable set to 1 if the headquarter of firm i is under lockdown in month t and 0 otherwise. We also include a set of control variables including size, book-to-market ratio, past month return, past year return, monthly volatility, analyst coverage, and measures of firm productivity, ROA, EPS, and Sales. To obtain SYNCH and IdioVol in Column 1 & 2, we estimate the following linear regression using daily returns for each firm in each month: $R_{i,t} = \beta_{i,0} + \beta_{i,1} \times Mkt_{RF_t} + \beta_{i,2} \times SMB_t + \beta_{i,3} \times HML_t + \beta_{i,4} \times RMW_t + \beta_{i,5} \times CMA_t + \varepsilon_{i,t}$, where $R_{i,t}$ is the return of stock i on day t in excess of the risk-free rate, MKT_RF_t is the market return on day t minus the risk-free rate, and SMB_t, HML_t, RMW_t, and CMA_t are the factor returns of stock i on day t. Right-hand variables in the regression are obtained from Kenneth French's website. SYNCH is defined as $SYNCH_{i,t} = \log(\frac{R^2}{1-R^2})$, where R^2 is the r-square obtained from the above regression for stock i in month t. IdioVol is then defined as the standard deviation of the residuals from the regression for stock i in month t. We require a minimum of 20 daily observations in the month to calculate $SYNCH_{i,t}$. We add industry returns to the above regression as the right-hand variable and estimate the SYNCH and IdioVol in Column 3 & 4. We multiply IdioVol by 10^4. Detailed variable definition is provided in the appendix. Standard errors are clustered by firm and month. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Dependent variables	**SYNCH**	**IdioVol**	**SYNCH (Industry-Adjusted)**	**IdioVol (Industry-Adjusted)**
	(1)	(2)	(3)	(4)
#Subsidiary Lockdown	0.016***	-0.242***	0.012***	-0.234***
	(4.35)	(-4.67)	(3.42)	(-3.76)
HQ Lockdown	-0.036	-0.201	-0.024	-0.287
	(-0.801)	(-0.945)	(-0.699)	(-1.48)
ROA	0.064	-2.15	-0.009	-1.93
	(0.561)	(-1.19)	(-0.095)	(-1.07)
EPS	-0.008	-0.004	-0.004	0.004
	(-1.45)	(-0.084)	(-0.786)	(0.117)
Sales	-0.256*	-2.20*	-0.147	-1.87*
	(-1.92)	(-1.97)	(-1.18)	(-1.90)
Other Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Number of observations	66,284	66,284	66,284	66,284
R-squared	0.42411	0.46813	0.48610	0.46523

Table A9: The Effects of Subsidiaries under Lockdown on Jump with Controlling Firm Productivity

This table reports the effects of the number of subsidiaries under lockdowns on stock price informativeness. The sample spans from 2016 to 2020 and only includes firms with subsidiaries. We estimate the following regression focusing the Jump around quarterly earnings announcements:

$Jump_{i,t} = \beta_1 \#Subsidiary\ Lockdown_{i,t} + \beta_2 HQ\ Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Month_Year_t + \varepsilon_{i,t}$,

where $Jump_{i,t}$ is the price jump ratio for firm i in month t, $\#Subsidiary\ Lockdown_{i,t}$ represents the number of subsidiaries under lockdowns in month t for firm i, and $HQ\ Lockdown_{i,t}$ is a dummy variable set to 1 if the headquarter of firm i is under lockdown in month t and 0 otherwise. We also include firm and quarter fixed effects and a set of control variables including size, book-to-market ratio, past month return, monthly volatility, and measures of firm productivity, ROA, EPS, and Sales. Jump is the ratio of post-announcement price variation to total variation before and including the quarterly earnings announcement. $Jump^{FF3}$ ($Jump^{FF3}$) is based on a Fama-French three-factor (five-factor) model. Standard errors are clustered by firm and quarter. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Dependent variables	**$Jump^{FF3}$**		**$Jump^{FF5}$**	
	(1)	(2)	(3)	(4)
#Subsidiary Lockdown	-0.005**	-0.007**	-0.009**	-0.010***
	(-2.16)	(-2.66)	(-2.60)	(-3.13)
HQ Lockdown	0.052	0.055	-0.082	-0.094*
	(1.41)	(1.53)	(-1.62)	(-1.74)
ROA		-0.667**		-0.582*
		(-2.53)		(-1.95)
EPS		0.0003		-0.004
		(0.038)		(-0.321)
Sales		0.002		0.158
		(0.012)		(1.02)
Other Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Number of observations	21,249	19,743	22,212	19,734
R-squared	0.10	0.10	0.09	0.10

Table A10: The Effects of Subsidiaries under Lockdown on Liquidity with Controlling Firm Productivity

This table reports the effects of the number of subsidiaries under lockdowns on liquidity. The sample spans from 2016 to 2020 and only includes firms with subsidiaries. We estimate the following regression using monthly data:

$$L_{i,t} = \beta_1 \#Subsidiary\ Lockdown_{i,t} + \beta_2 HQ\ Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Month_Year_t + \varepsilon_{i,t},$$

where $L_{i,t}$ is a liquidity measure for firm i in the quarter of month t, $\#Subsidiary\ Lockdown_{i,t}$ represents the number of subsidiaries under lockdowns in month t for firm i, and $HQ\ Lockdown_{i,t}$ is a dummy variable set to 1 if the headquarter of firm i is under lockdown in month t and 0 otherwise. We also include a set of control variables including book-to-market ratio, past month return, past year return, monthly volatility, and measures of firm productivity, ROA, EPS, and Sales. We have four different measures for liquidity, Amihud Illiquidity from Amihud (2002), PS Gamma from Pastor and Stambaugh (2002), Roll's Spread from Roll (1984), and the composite illiquidity index, defined as the average z-scores of these three measures. Detailed variable definition is provided in the appendix. Standard errors are clustered by firm and month. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Dependent variables	**Amihud Illiquidity**	**PS Gamma**	**Roll's Spread**	**Composite Illiquidity**
	(1)	(2)	(3)	(4)
#Subsidiary Lockdown	-0.709***	-0.839***	-1.12***	-0.490***
	(-3.09)	(-2.78)	(-4.84)	(-3.98)
HQ Lockdown	-2.24	-8.32**	-3.93	0.186
	(-0.856)	(-2.19)	(-1.52)	(0.200)
ROA	(1.97)	(0.706)	(1.57)	(-1.90)
	-28.4**	-25.3	-25.5**	-25.7*
	(-2.25)	(-1.47)	(-2.26)	(-1.88)
EPS	0.569**	0.432	0.572**	0.548**
	(2.29)	(1.22)	(2.45)	(2.53)
Sales	25.7***	12.9*	22.3***	18.0**
	(2.88)	(1.87)	(2.72)	(2.21)
Other Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Number of observations	77,702	20,913	77,702	77,702
R-squared	0.36	0.48	0.48	0.77

Table A11: The Effects of Subsidiaries under Lockdown on SYNCH and IdioVol for Sub-Sample Excluding Public Subsidiaries

This table reports the effects of the number of subsidiaries under lockdowns on stock price informativeness for a sub-sample of firms. In this sub-sample, we exclude all public subsidiaries (i.e., subsidiaries that are listed on a stock exchange) and all subsidiaries of the public subsidiaries from the ultimate parent company. The regression follows the same specification as Table 2, with the dependent variable being either SYNCH or IdioVol, using the same control variables, fixed effects, and clustered standard errors by firm and month. The sample spans from 2016 to 2020 and only includes firms with non-public subsidiaries. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variables	**SYNCH**	**IdioVol**	**SYNCH (Industry-Adjusted)**	**IdioVol (Industry-Adjusted)**
	(1)	(2)	(3)	(4)
#Subsidiary Lockdown	0.017*** (5.45)	-0.262*** (-7.95)	0.013*** (4.43)	-0.254*** (-8.44)
HQ Lockdown	-0.040 (-1.35)	-0.266 (-0.668)	-0.023 (-0.811)	-0.379 (-1.06)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Number of observations	82,353	82,353	82,353	82,353
R-squared	0.43041	0.45998	0.48844	0.45955

Table A12: The Effects of Subsidiaries under Lockdown on Jump for Sub-Sample Excluding Public Subsidiaries

This table reports the effects of the number of subsidiaries under lockdowns on Jump for a sub-sample of firms. In this sub-sample, we exclude all public subsidiaries (i.e., subsidiaries that are listed on a stock exchange) and all subsidiaries of the public subsidiaries from the ultimate parent company. The regression follows the same specification as Table 3, with the dependent variable being Jump, using the same control variables, fixed effects, and clustered standard errors by firm and month. The sample spans from 2016 to 2020 and only includes firms with non-public subsidiaries. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variables	**$Jump^{FF3}$**		**$Jump^{FF5}$**	
	(1)	(2)	(3)	(4)
#Subsidiary Lockdown	-0.007** (-2.71)	-0.006** (-2.38)	-0.010*** (-3.60)	-0.010*** (-3.58)
HQ Lockdown	0.030 (0.847)	0.045 (1.23)	-0.099* (-1.91)	-0.083 (-1.49)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Number of observations	21,882	20,942	21,856	20,926
R-squared	0.10	0.10	0.09	0.10

Table A13: The Effects of Subsidiaries under Lockdown on Liquidity for Sub-Sample Excluding Public Subsidiaries

This table reports the effects of the number of subsidiaries under lockdowns on stock liquidity for a sub-sample of firms. In this sub-sample, we exclude all public subsidiaries (i.e., subsidiaries that are listed on a stock exchange) and all subsidiaries of the public subsidiaries from the ultimate parent company. The regression follows the same specification as Table 11, with the dependent variable being stock liquidity measures, using the same control variables, fixed effects, and clustered standard errors by firm and month. The sample spans from 2016 to 2020 and only includes firms with non-public subsidiaries. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Dependent variables	**Amihud Illiquidity**	**PS Gamma**	**Roll's Spread**	**Composite Illiquidity**
	(1)	(2)	(3)	(4)
#Subsidiary Lockdown	-0.4488* (-1.714)	0.0162** (2.226)	-0.0251** (-2.284)	-0.8166* (-1.819)
HQ Lockdown	0.8601 (0.8689)	0.0505 (0.7843)	-0.1883** (-2.643)	-4.596 (-0.7960)
Controls	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Number of observations	83,675	41,713	51,839	71,299
R-squared	0.788	0.483	0.607	0.048

Appendix B

Analyst Report Classification Using a Large Language Model

To analyze how analysts allocate attention across different levels of information, we develop a classification pipeline based on the DeepSeek-V3-0324 large language model. We apply this method to segment and label the textual content of analyst reports by topic category.

Each analyst report is preprocessed to remove non-ASCII characters, numbers, and extraneous whitespace. We then segment the cleaned text into individual sentences using spaCy's sentence boundary detection. To maintain context and reduce token overhead, every 10 consecutive sentences are grouped into a single classification batch.

We instruct DeepSeek to classify each sentence into one of five mutually exclusive categories:

- nearby_subsidiary: Refers to any subsidiary in the same state as the analyst.
- whole_firm: Pertains to the overall firm, including operations beyond local subsidiaries.
- industry: Discusses the broader industry context, excluding firm-specific details.
- macro_economy: Covers economy-wide developments, such as interest rates or GDP.
- other: Captures irrelevant or unclassifiable content.

If a sentence includes overlapping content (e.g., both local and macro information), it is assigned to the most specific relevant category. In particular, nearby_subsidiary is prioritized to capture localized attention bias.

Below is the exact classification prompt sent to DeepSeek:

You are a research assistant helping a finance professor classify the content of analyst reports.

Your task is to classify each sentence into ONE of the following mutually exclusive categories:
1. nearby_subsidiary — If the sentence talks about any subsidiary located in the same state as the analyst. Use this category even if the sentence also includes firm-wide or macro content.
2. whole_firm — If the sentence discusses the overall firm or its strategy beyond just local subsidiaries.
3. industry — If the sentence talks about the industry the firm operates in, but not the specific firm.
4. macro_economy — If the sentence discusses economy-wide trends (e.g., GDP, inflation, interest rates).
5. other — If the sentence is unrelated to any of the above categories.

Input metadata:
- Firm Name: {FirmName}
- Analyst Location: {AnalystState}
- Subsidiaries in Analyst State: {ListOfSubsidiaries}

Below is a batch of 10 sentences. Classify each sentence and return the result in strict JSON format as follows:

[
{"sentence": "Sentence text 1", "label": "nearby_subsidiary"},
{"sentence": "Sentence text 2", "label": "whole_firm"},
...
]

Each classification batch is submitted via API calls with temperature = 0 and top_p = 0 to ensure deterministic and reproducible output. The API allows up to 800 tokens per response and retries failed calls up to three times. If a batch fails repeatedly, the result is coded as "NA".

The labelled sentences are aggregated at the report level to compute the amount of attention devoted to each category. These attention measures are used in our main empirical analyses to test whether subsidiary lockdowns shift analyst focus from localized signals (nearby_subsidiary) to more aggregate sources (whole_firm, industry, macro_economy). Manual checks of random subsamples confirm the accuracy and consistency of the classifications.