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

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Abstract

We examine how geographically dispersed subsidiary information affects a firm's information environment using the pandemic-induced, localized lockdowns that restricted analysts' access to nearby subsidiaries. Linking analysts to subsidiary locations, we compare analysts covering the same firm in the same month who do versus do not have local access. In a difference-in-differences design, losing local access attenuates analysts' overweighting of salient but non-representative subsidiary signals and reduces the same analyst's forecast errors. This reallocation of attention away from local subsidiary signals aggregates to more accurate consensus forecasts, faster incorporation of news into prices, reduced insiders' trading advantages, and improved stock liquidity. Effects are stronger when subsidiaries are less representative of the firm and absent for headquarters-only proximity. Our findings support an attention-bias channel: localized subsidiary information can distort attention and delay information incorporation, and restricting such access fosters more efficient markets.

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Introduction

Information is fundamental to financial markets, and a standard view is that more information leads to more informative prices (Grossman and Stiglitz, 1980; Admati and Pfleiderer, 1988; Verrecchia, 2001). Attention-based theories, however, caution that more signals can misallocate attention: agents overweight proximate, or personally acquired information and underweight broader, aggregate fundamentals (Daniel, Hirshleifer, and Subrahmanyam, 1998; Sims, 2003; Peng and Xiong, 2006; Veldkamp, 2006). In such settings, more signals may crowd out processing of representative firm-level news, reducing rather than improving informational efficiency. [1]

We take this tension to the setting of geographically dispersed subsidiaries. A useful way is to think about the firm as a Christmas tree and each subsidiary as a light. Local subsidiary signals are private and granular, often accessed through on-the-ground observation, much like parking-lot scans, or card-swipe panels. We compare two alternative hypotheses. The "information advantage hypothesis" says that many lights, when aggregated, illuminate better the whole tree, and improve both forecasts and prices. The "attention bias hypothesis" instead cautions that observers may stare at the nearest bulbs, overweighting salient local cues and neglecting the outline of the tree, which distorts aggregation and slows price adjustment.

The stakes are large. In our sample, firms with subsidiaries account for more than 80 percent of U.S. market capitalization, and most subsidiaries are private, so access to subsidiary-related information is uneven and often mediated by in person interactions with local employees, customers, suppliers, and regulators. [2] These localized information flows differ sharply from those available at corporate headquarters (García and Norli, 2012; Bernile, Kumar, and Sulaeman, 2015; Addoum, Kumar, and Law, 2020).[3] Whether such local signals enhance the information environment or bias it is therefore an empirical question with direct policy relevance for the design of disaggregated disclosures and the use of alternative data.

These two hypotheses yield opposite predictions that map to debates about alternative data. Both predict that restricting local subsidiary access should shift analysts' attention away from subsidiary topics and toward firm-, industry-, and macro-level content. They differ, however, in what follows. Under the information advantage hypothesis, restricting local access removes useful signals, so analysts'forecasts should worsen, and price efficiency should decline. Under the attention bias

[1] For example, Dessaint, Foucault, and Frésard (2024) show that alternative data can sharpen near-term forecasts while degrading attention to longer-horizon fundamentals.
[2] In our sample, the average firm operates ten subsidiaries across six states and ten ZIP codes. Nearly all (over 99%) 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.
[3] 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.

hypothesis, restricting local access removes distortionary signals, so forecasts should improve, and, if these local biases are correlated across analysts, price efficiency should improve.

Studying geographically dispersed information is hard because access is often private, idiosyncratic, and endogenous to firm characteristics.[4] We exploit the COVID-19 pandemic's localized subsidiary lockdowns as a natural experiment. Our identification comes from within-firm, within-month variation in analysts' geographic exposure to lockdown-affected subsidiaries. These lockdowns restrict physical access to subsidiary-level information for analysts located near affected subsidiaries. Although analyst and subsidiary locations may be endogenous, the drop in local mobility at the subsidiary location is not chosen by the analysts or the firm and varies across subsidiaries of the same firm.[5] Accordingly, within-firm, within-month variation in who loses access provides a plausibly exogenous shock to analysts' ability to acquire local subsidiary information.

We measure lockdown intensity using reductions in foot traffic at subsidiary locations. We define treatment at the analyst-firm-time level when nearby subsidiaries cross a mobility threshold. The use of percentage change in foot traffic helps us mitigate concerns that results reflect baseline differences in local traffic levels or local economic activity rather than lockdown-related disruptions.

Using analyst- and subsidiary-level geolocation data, we implement a generalised difference-in-differences approach that, within same firm and time, compares forecasts of analysts located near affected subsidiaries with three benchmarks: (i) analysts located near unaffected subsidiaries (ii) analysts located near the firm's headquarters, and (iii) analysts far from both headquarters and subsidiaries. This comparison isolates the effect of restricted access to local subsidiary information from broader pandemic-related shocks.

We start by testing the premise that restricting local subsidiary access reallocates analyst attention. Specifically, we examine whether analysts near affected subsidiaries devote a smaller share of their reports to subsidiary topics and a larger share to firm, industry, and macro content. Using textual analysis of analyst reports, we find exactly this pattern during lockdowns: analysts near affected subsidiaries shift report content away from subsidiary topics and toward broader information categories.

[4] 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.

[5] The probability of a lockdown been strictly implemented depended not only on the spread of the COVID virus, but also on the political affiliation of the local governors. For example, Democratic governors were generally faster in issuing stay-at-home orders and implementing social distancing measures than their Republican counterparts. Instead, states having both a Republican governor and a Republican senate majority were more likely to delay the implementation of shelter-in-place orders. Given the plausibly weak correlation between the relative location between the analysts and the subsidiaries and the spread of the virus, the covid lockdowns provide a reasonably exogenous shock.

Next, we test the two hypotheses, starting with the cleanest level of identification: individual analysts' forecast behavior. We examine whether losing local subsidiary access affects individual analysts' forecast accuracy and forecast 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 analysts with heterogeneous geographic exposures in the same period, 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 baseline difference-in-differences specification, 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. Across alternative specifications and comparison groups, the reduction ranges from 4%–19%. The absence of similar effects when headquarters are locked down underscores that subsidiary-specific information, not centralized headquarter information, that drives the result. When analysts lose access to nearby subsidiaries, the same analysts' pre-earnings announcement revisions become less extreme and less reactive to local cues, consistent with reduced over-precision and greater reliance on broader firm-level signals.

These analyst-level results are consistent with an attention-bias mechanism rather than a local-information advantage. First, the effects are concentrated among analysts near affected subsidiaries and are absent around headquarters lockdowns. Second, they remain robust in specifications with rich firm-by-time and analyst-by-time controls, which absorb broader COVID-related shocks and analyst-specific disruptions.[6] Third, we find no material treatment-induced changes in firm productivity (e.g., ROA, sales, or EPS), which is more consistent with an information-processing rather than a real-effects channel. Finally, the forecast improvement does not increase with the number of nearby analysts; if the effect were driven by superior local information, one would expect more nearby analysts should strengthen the effects through aggregation. Instead, the evidence points to a reduction in localized bias rather than a gain in local information.

Next, we directly test whether "switching off the light" reduces analysts' bias in reacting to public news. We use the methodology of Coibion and Gorodnichenko (2015) to identify informational inefficiency through the relationship between forecast revisions and subsequent forecast errors.[7] We

[6] Firm-by-time fixed effects absorb differential COVID disruptions, 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).

[7] A significant correlation between forecast revisions and subsequent forecast errors indicates biased updating. A negative relationship reflects underreaction to new public information, whereas a positive relationship reflects

document that the lockdowns reduce this bias by 8%–19% for analysts near the affected subsidiaries, with no significant change for analysts farther away. This confirms that that proximity to subsidiaries fosters overreliance on local signals, and lockdowns curb that bias.

Taken together, these results point to geographic proximity as a source of attention-driven bias rather than superior information. The concentration of effects among nearby analysts, the absence of effects around headquarters lockdowns, and unchanged firm fundamentals all argue against localized real shocks as the driver.

The next assess whether this analyst-level bias aggregates to the firm level or washes out in consensus forecasts. In principle, idiosyncratic errors should cancel in the consensus. In our setting, however, they need not for two reasons. First, analyst coverage is geographically uneven, making some subsidiaries exert disproportionate influence on consensus forecasts. Second, proximity and familiarity can induce correlated weighting of local cues, which creates systematic, rather than idiosyncratic errors. If subsidiary access distorts assessments in a correlated way, then restricting that access should improve consensus accuracy.

Consistent with this prediction, we find that subsidiary lockdowns significantly reduce both consensus forecast errors and consensus forecast dispersion. The magnitude of these gains is economically meaningful and mirrors the individual-level gains, reinforcing the interpretation that the improvements stem from mitigating systematic biases rather than removing valuable information.

Moreover, the effects are strongest where subsidiary information is most likely to mislead, i.e., firms diversified across many industries and firms with more hierarchical vertical firms. For multi-industry firms, any one subsidiary is less representative of the overall performance, so curbing attention to local cues yields larger accuracy gains. For firms with hierarchical structures, information flows are more centralized, and firm-level hard information is of higher quality, so analysts benefit more when lockdowns redirect attention away from informal local insights and toward centralized guidance, as in Stein (2002). 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 aggregate and enhance consensus forecast accuracy by mitigating biases, we examine whether these gains translate into more informative stock prices. Specifically, we assess stock price response to public signals, and shifts in the composition of information embedded in stock prices.

overreaction. Such biases may arise when analysts place excessive weight on localized subsidiary-specific information, leading to systematic distortions in their forecast updates.

We begin by evaluating price informativeness *around earning announcements* 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. Consistent with the attention bias hypothesis, we find that subsidiary lockdowns significantly reduce the jump ratio, indicating faster pre-announcement incorporation once access to biased local cues is curtailed. One more subsidiary under lockdown is associated with a between 1.1% and 2% decrease in jump ratio.

If prices better reflect information before public disclosures because pre-announcement expectations are more accurate, then announcement-window price reactions should be more muted. Consistent with this, we find that market reactions to management earnings guidance are significantly dampened during subsidiary lockdowns: each additional affected subsidiary is associated with a 9%–19% reduction in the responsiveness of cumulative abnormal returns to earnings guidance releases, over 3- to 7-day windows. These weaker market responses are consistent with cleaner expectations rather than with a loss of valuable private information.[8]

A lower jump ratio also suggests that sophisticated investors act earlier, before earnings announcements. We investigate this prediction by studying short sellers' responses to analyst forecast revisions. Short sellers typically increase shorting after downward revisions and cover after upward revisions. If lockdowns improve informativeness of analysts' revisions by reducing localbias, sophisticated investors should react more strongly to those revisions. Consistent with this mechanism, we find that the inverse relationship between forecast revisions and short-selling volume becomes four to five times stronger during subsidiary lockdowns. Taken together with the muted announcement-window CARs, these short-selling results point to faster pre-announcement incorporation by informed traders rather than more informative signals arriving at the announcement itself. In other words, subsidiary lockdowns appear to improve the timing and quality of information incorporation into prices.

We further explore how subsidiary lockdowns shift the type of information embedded in stock prices. If localized access induces 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 (the extent to which a firm's returns co-move with the broader market/industry) and idiosyncratic return volatility. Consistent with the attention bias hypothesis, we find that subsidiary lockdowns increase return synchronicity by 3.3% and reduce idiosyncratic volatility

[8] 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.

by 5.5% (relative to their respective sample standard deviations). These results suggest that investors place less weight on noisy, subsidiary-specific cues and more weight on cleaner aggregate sources.

Together, these findings provide converging evidence that limiting subsidiary access reduces noise, accelerates pre-announcement incorporation by sophisticated traders, and leaves less to be revealed at announcements. Prices become more informative even as announcement-window reactions shrink, which supports the attention bias hypothesis rather than the information advantage hypothesis.

Improved forecast accuracy and price informativeness should also translate into a less asymmetric information environment, reducing the scope for trading on private or misinterpreted signals. And indeed, we find that as the adverse selection risks faced by liquidity providers declines, bid–ask spreads narrow, and market liquidity improves (e.g., Glosten and Milgrom, 1985; Kyle, 1985; Amihud, 2002, Easley and O'Hara, 2004). This pattern holds across several standard liquidity proxies. 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 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 lockdowns weaken 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. In the data, we detect no treatment induced shifts in ROA, sales, or EPS, and our results are unchanged when we control for these fundamentals. This supports the interpretation that the gains arise from reduced informational distortions.

Finally, mandated disclosures of public subsidiaries could attenuate the importance of geographic proximity. Such subsidiaries are rare in our sample and excluding them does not alter our findings. Together, these tests reinforce a causal reading: restricting access to localized subsidiary information improves the information environment rather than reflecting coincident changes in operations or disclosure.

2 Related Literature and Contribution

We contribute to several strands of literature on the corporate information environment by providing analyst-level, within-firm causal evidence on how geographically dispersed, subsidiary-level signals shape analysts' forecasts and firm outcomes. Our identification compares analysts covering the same firm in the same month who differentially lose access to nearby subsidiary.

First, we contribute to the relation between geographic dispersion of analysts, local information acquisition and the firm's information environment. 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 isolate proximity to subsidiaries (not overall dispersion) as the margin that flips the sign: dispersion helps among analysts far from subsidiaries but not among those near subsidiaries, who overweight salient local cues. Our lockdown design provides causal support: when local subsidiary access is curtailed, nearby analysts' forecasts improve, showing that dispersion aids aggregation only when it is not paired with concentrated exposure to subsidiary information.

Second, we add to the fast-growing literature showing that novel, alternative data (e.g. satellite imagery) can enhance forecasting, trading, and price discovery (Cao, Jiang, Wang and Yang (2024), Chi, Hwang and Zheng (2024), and Katona, Painter, Patatoukas and Zeng (2024)). While *more* signals may improve informational efficiency, we document an attention-distortion margin where incremental, highly local signals degrade aggregation. Our natural experiment qualifies the "more data is better" view by showing that restricting skewed local feeds can improve forecasts and pricing.

Third, 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 highlighting the attention allocation channel, namely, how local proximity diverts analyst attention toward distortionary signals.

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 extend this link 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, organizational structure 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. Others show that hierarchical structures facilitate coordination but can hinder flexibility and responsiveness (e.g., Stein, 2002; Alonso, Dessein, and Matouschek, 2008). We provide evidence using private subsidiaries: localized signals can distort firm-level forecasts and pricing; the distortions are largest when subsidiaries are less representative, and hierarchical structure mitigates them by strengthening centralized guidance and reducing reliance on informal local cues.

Finally, our findings have implications for policies on disaggregated disclosures. We show that increasing the number of localized signals 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 may produce more informative signals than decentralized, unevenly acquired local data. Regulators weighing the costs and benefits of expanded plant- or subsidiary-level disclosure should consider that more granular signals are not uniformly beneficial if they redirect attention away from representative firm-level fundamentals.

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 levels of "ultimate parent", "parent", and "subsidiaries." Subsidiaries may themselves act as parents to other entities within the same network.[9] The vast majority (99.9%) of subsidiaries are private, though some public subsidiaries serve as ultimate parents.[10] 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 years prior to the pandemic is 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 allows us to establish pre-lockdown baselines and control for time-invariant firm characteristics in our analyses.[11]

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

[9] The original database includes 14 legal entity types, such as parent, subsidiary, plant, and branch. For our analysis, we filter and retain five types of potential subsidiaries: subsidiary, parent, affiliate, holding, and group.
[10] We consider excluding cases where public subsidiaries are considered ultimate parents from our sample. Our results remain intact.
[11] 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.

consistent across these alternative definitions, as shown in Table A2 to A5. Subsidiaries located in these lockdown ZIP codes are designated as under lockdown. Using this classification, we define the variable $\#Subs\_Lockdown$, representing the number of a firm's subsidiaries affected by lockdown. To isolate the information effects of subsidiaries from those of headquarters, $\#Subs\_Lockdown$ 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 the employment history of registered individuals. For all the analysts issuing quarterly earnings forecasts in I/B/E/S between January 1, 2016, and December 31, 2020, we follow Law (2023) to extract 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 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, representing 201 of the 337 broker IDs.

Finally, we gather analyst reports authored by the equity research departments of 10 large investment banks[12] 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.

3.2 Variable Construction

[12] The 10 large investment banks are Barclays, Citi, Deutsche Bank, Evercore ISI, Goldman Sachs, J.P. Morgan, Morgan Stanley, RBC Capital Markets, UBS, and Wells Fargo Securities, LLC. These institutions are among the largest investment banks operating in the U.S. with established equity research departments.

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 are detailed in the results session as well as in the Variable Definition Table.

All our specifications control for firm-specific characteristics unless these are 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) Book-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 ($Monthly\_Vol$) is the monthly standard deviation of a stock's daily adjusted returns within a given month; (iv) Past-month return ($Monthly\_Ret$) is the stock's return over the previous month; (v) Past-year return ($Annual\_Ret$) 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 access to 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 level. 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 likely affected by 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:

$$Y_{i,j,t} = \beta_1 Analyst\_Near\_Subs\_Lockdown_{i,j,t} + \beta_2 Analyst\_Near\_Subs_{i,j,t} + Firm\_Mth\_Year_{i,t} + Firm\_Analyst_{i,j} + Analyst\_Mth\_Year_{j,t} + \epsilon_{i,j,t}, (1)$$

where $Y_{i,j,t}$ is the outcome of interest (e.g., forecast error) for analyst j for firm i in month t. $Analyst\_Near\_Subs\_lockdown_{i,j,t}$ is a dummy variable equal to 1 if all the subsidiaries of firm i located in the same state as analyst j are under lockdown in month t, and 0 otherwise. $Analyst\_Near\_Subs_{i,j,t}$ is a dummy variable equal to 1 if analyst j is located in the same state as the subsidiary of firm i in month t. 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.[13] 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.

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

[13] $Analyst\_Near\_Subs_{i,j,t}$ 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 (1), the variable $Analyst\_Near\_Subs\_lockdown_{i,j,t}$ effectively represents the interaction between being near a subsidiary and experiencing a lockdown, as $Analyst\_Near\_Subs\_Lockdown_{i,j,t} = 1$ only when $Analyst\_Near\_Subs_{i,j,t} = 1$. 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.

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.

To the extent that, in the absence of the COVID-19 lockdowns, outcomes for analysts covering the same firm—with and without nearby locked-down subsidiaries—would have followed parallel trends, the coefficient of interest β_1 identifies the average treatment effect on the treated (ATT) – i.e., the impact of nearby subsidiary lockdowns on analysts' forecasts for the target firm.[14] To validate the *parallel-trends assumption*, we estimate an event-study version of Equation (1):

$$Y_{i,j,t} = \sum_{s \neq -2} \beta_s Analyst\_Near\_Subs\_lockdown_{i,j} * D_{s(i,j,t)} + \alpha Analyst\_Near\_Subs_{i,j,t} + Firm\_Mth\_Year_{i,t} + Firm\_Analyst_{i,j} + \epsilon_{i,j,t}, \quad (2)$$

where $D_{s(i,j,t)}$ are event-time dummies indicating the months before and after the first lockdown of one of analyst j's nearby subsidiaries of firm i, with $s = -2$ serving as the benchmark period. We use $s = -2$ (rather than $s = -1$) as the reference because the lockdown onset is defined by a threshold rule (a footprint declines exceeding 30%). If restrictions and behavioral adjustments start gradually, the period immediately preceding the threshold crossing ($s = -1$) may already be partially affected, which would contaminate the last pre-treatment period. Using $s = -2$ as the baseline mitigates this concern and provides a cleaner pre-treatment reference for assessing parallel trends. $Analyst\_Near\_Subs\_lockdown_{i,j}$ equals one if any subsidiary of firm *i* near analyst *j* ever experiences a lockdown and zero otherwise. We include firm-by-month-year fixed effects and firm-by-analyst fixed effects to absorb time-varying firm-level shocks and persistent analyst–firm relationship heterogeneity.[15] Standard errors are three-way clustered by firm, analyst, and year-month. By plotting the estimated β_s coefficients over event time, we verify that (i) there is no differential pre-trend (i.e., analysts 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.

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

[14] Another assumption allowing us to identify ATT is that there is no anticipation. Specifically, COVID-19 has no effect prior to the treatment.
[15] Our results are robust if we incorporate firm-by-month-year and analyst-by-month-year fixed effects.

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:

$$Y_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} + \beta_2 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Time_t + \varepsilon_{i,t}, \quad (3)$$

where $Y_{i,t}$ denotes the outcome of interest for firm i during time t. $\#Subs\_Lockdown_{i,t}$ 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 the standard c variables that control for effects could influence firm performance and information environment, including $Size$, BM(Book-to-Market), $Monthly\_Ret$(Monthly Stock Return), $Annual\_Ret$ (Annual Stock Return), $Monthly\_Vol$ (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 (3) using OLS and cluster standard errors by firm and time to account for cross-sectional and temporal dependencies.

Assuming parallel trends—i.e., absent COVID-19 lockdowns, firms with and without locked-down subsidiaries would have evolved similarly—the coefficient β_1 identifies the ATT of subsidiary lockdowns on the firm's information environment. We assess this assumption using the event-study:

$$Y_{i,t} = \sum_{s \neq -2} \beta_s \, \#Subs\_Lockdown_i * D_{s(i,t)} + \alpha HQ\_Lockdown_{i,t} + \gamma \, Control_{i,t} + Firm_i + Time_t + \varepsilon_{i,t}, \quad (4)$$

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. $\#Subs\_Lockdown_i$ is the average number of subsidiaries under lockdown during the lockdown period, capturing the intensity of the treatment.[16] We include firm and time fixed effects, and standard errors are clustered by firm and time. The β_s should show no pre-trends and effects only after lockdown onset.

[16] The number of subsidiaries under lockdown can fluctuate substantially within a firm over time due to staggered and intermittent local restrictions across states, which would mechanically induce high-frequency variation in "treatment intensity" within event time. Aggregating this variation into a firm-level average over the firm-specific

Our baseline firm-level lockdown intensity measure is a firm-level count of subsidiaries under lockdown for firm i in period t. A natural concern is that a high subsidiary-lockdown count may not translate into information frictions for analysts if few (or no) analysts covering the firm are geographically proximate to the locked-down subsidiaries. To address this, we construct two alternative, analyst-linked measures that explicitly incorporate analysts' locations.

First, we compute the number of locked-down subsidiaries that have nearby analyst coverage, $\#Subs\_Lockdown\_Near\_Analyst$, defined as subsidiaries under lockdown that are located in the same state as at least one analyst covering the firm in period t. Second, we compute the number of analysts covering the firm who are themselves located in lockdown areas in period t $\#Analyst\_Lockdown$. These measures map lockdown exposure more directly to the analysts who potentially rely on localized subsidiary-level signals. We report results using these alternative measures as robustness checks. We report robustness results using these two analyst-linked measures in Appendix Table A6.

4 The Impact of Subsidiary Lockdowns on Individual Analysts' Forecasts

4.1 Attention Allocation in Analyst Reports: A Textual Analysis

We start by investigating the premise that lockdowns restrict access to local information and reduce local attention. Limited access to subsidiary-specific information should redirect analysts' attention towards more aggregated sources. We therefore look at the analyst report content to gauge analyst attention allocation. Indeed, 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. We expect lockdown-induced restrictions to shift analysts' attention towards aggregate-level information away from subsidiary-level details in analyst reports.

We gather analyst reports authored by the 10 large sell-side equity research firms from Thomson One's Investext, extracting details including document date, brokerage contributor, author, language, and report length, as well as firm names and tickers. To classify the content of analyst reports, we employ a large language model (LLM) approach using the 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), and *other* (unrelated content). When a sentence contains multiple topics, it is assigned to nearby_subsidiary if it includes any nearby-subsidiary-related

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lockdown window yields a stable measure of exposure intensity and facilitates a transparent event-study interpretation of β_s as the dynamic response per unit of overall lockdown exposure.

discussion; otherwise, it is assigned to the most salient remaining category. 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 and next to 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), 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:

$$Subs\_Topic\_Intensity_{i,j,t} = \beta_1 Analyst\_Near\_Subs\_Lockdown_{i,j,t} + \beta_2 Analyst\_Near\_Subs_{i,j,t} + Controls_{i,t} + Firm_i + Analyst_j + Mth\_Year_t + \epsilon_{i,j,t}. \quad (5)$$

Here, $Subs\_Topic\_Intensity_{i,j,t}$ 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, as well as the total word count of the analyst's report to account for baseline report length. All the control variables are lagged by one month. We also include firm, month-year, and analyst fixed effects.

Table 2 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.

The results confirm our expectations: subsidiary lockdowns significantly reduce subsidiary-related content in analyst reports. Specifically, subsidiary-level information content decreases by 92% in sentence counts, while the other content increases by 4.5%. We emphasize direction and materiality rather than exact magnitudes, consistent with analysts reallocating attention from subsidiary-specific signals toward more aggregate information. This preliminary evidence shows that lockdowns impact analyst attention/information inducing them to pay more attention to non-localized subsidiary information. We now test whether this leads to changes in the quality of analyst forecasts and in their revisions.

4.2 Analyst Location and Forecast Accuracy

We first look 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. The *attention bias hypothesis* posits that analysts near subsidiaries over-rely on localized, salient information, which distorts forecasts. Consequently, when subsidiaries near analysts go under lockdown, the removal of this distorted information source should enhance forecast accuracy. The opposite is posited by the *information advantage 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.[17] The left-hand variable is individual analyst-level earnings forecast error, 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 prediction is more precise. The results, reported in Model 1 of Table 3 Panel A, show that subsidiary lockdowns reduce forecast error by 3.6% (t-stat=2.46), relative to the average forecast error.[18] 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 nearby headquarters being under lockdown ($HQ\_Nearby\_Lockdown$) in Models 3 and 4 is insignificant, suggesting that centralized information from headquarters does not distort forecast accuracy the same way decentralized 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 3, 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 $Analyst\_Near\_Subs$ 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 advantage.

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 comparisons yield statistically significant results, confirming that the improvement in forecast accuracy is driven by differential exposure to locked-down local information rather than analyst-firm matching.

[17] 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 most recent forecast. Detailed description of the variables constructed using I/B/E/S data is in the Variable Definition Table. To account for stock splits and reverse splits, we adjust EPS values using the cumulative factor (CFACSHR) from CRSP.

[18] Here, the economic magnitude is computed as the coefficient of $\#Subs\_Lockdown$ 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.

Finally, the event-time estimates in Figure 1 show little evidence of pre-trends: coefficients in the months leading up to the first nearby subsidiary lockdown are small and generally statistically indistinguishable from zero. Around the onset of lockdown (month 0), the estimates turn negative, and the post-treatment coefficients remain below zero for most subsequent months. The decline is economically meaningful and appears persistent through at least six months after treatment, with several post-lockdown months exhibiting confidence intervals that exclude zero. Overall, the pattern is consistent with nearby subsidiary lockdowns improving analysts' forecast outcomes for the focal firm, with effects emerging only after lockdown onset.

These results are consistent with the ones of the previous section documenting that subsidiary lockdowns reduce subsidiary-related content in analyst reports and increase the relative emphasis on more aggregate topics. However, these patterns alone do not establish that the change in report content mediates the improvement in forecast accuracy. A natural concern is that lockdowns could simultaneously affect both forecast errors and report composition through broader shifts in the information environment during the pandemic, without the content shift being the operative channel.

Therefore, to provide more direct evidence consistent with a attention reallocation mechanism, we exploit heterogeneity in analysts' pre-pandemic tendency to emphasize nearby subsidiaries in their written research. Using the LLM-based sentence classification of the previous section, we construct an analyst–level measure of subsidiary emphasis based on reports written before the onset of COVID-related restrictions. Specifically, for each analyst j, we compute the average share of sentences classified as *nearby_subsidiary* across all of the analyst's available pre-period reports in 2019 in our Investext–I/B/E/S matched sample. We then define $High\_Subs\_Topic\_Intensity_{i,j,t}$ as an indicator equal to one if this analyst-level measure is above the sample median, and zero otherwise. This measure captures whether an analyst historically "overweights" localized subsidiary narratives when producing research.

Using these variables, we test whether the effect of subsidiary lockdown exposure on forecast accuracy is concentrated among analysts who previously emphasized nearby subsidiary information. Concretely, we interact $Analyst\_Near\_Subs\_lockdown_{i,j,t}$ with $High\_Subs\_Topic\_Intensity_{i,j,t}$. The results in Table 4 show that the coefficient of interaction term is significantly negative, while the main effect of $Analyst\_Near\_Subs\_lockdown_{i,j,t}$ is weak or not statistically distinguishable from zero. This pattern implies that forecast improvements are driven primarily by analysts who, *ex ante*, devoted substantial attention to nearby subsidiary stories in their reports. For analysts who did not emphasize subsidiary-level narratives before the pandemic, subsidiary lockdown exposure has little impact on forecast errors. This further supports the *attention bias hypothesis.*

4.3 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 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 in response to quarterly earnings announcements. Specifically, we ask whether analysts reduce the magnitude of their revisions when nearby subsidiaries are locked down and their usual localized cues become unavailable. Accordingly, we re-estimate Equation (1) with dependent variable $Y_{i,j,t}$ measured as $Forecast\_Revision\_EA_{i,j,t}$, which is the absolute magnitude of analyst j's first annual-forecast revision for firm i following its quarterly earnings announcement in month t[19].

The results, reported in column 1 of Table 5, show 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. Interpreted through the bias lens, restricting localized access reduces over-precision: before lockdowns, localized cues appear to induce excessively confident forecasts that require larger post-EA adjustments; during lockdowns, analysts issue more balanced forecasts requiring smaller revisions, consistent with the accuracy gains documented above.

Both sets of tests – forecasts and revisions – support the attention bias hypothesis as opposed to the information advantage hypothesis. 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.

Finally, we note an important caveat: the pandemic period featured unusually synchronized, macro-dominant shocks and mobility restrictions that may not characterize normal market conditions. Accordingly, the observed attenuation of local bias may reflect a crisis-induced reallocation of attention

[19] In this specification, $Analyst\_Near\_Subs\_Lockdown_{i,j,t}$ equals to 1 only if all subsidiaries of firm i located in the same state as analyst j were continuously under lockdown throughout the earnings-announcement window, namely: (i) the month of the analyst's last pre-announcement forecast, (ii) the earnings-announcement month t, and (iii) the month of the analyst's first post-announcement forecast revision. It equals 0 otherwise. We impose the same window-based definition for $Analyst\_Near\_Subs_{i,j,t}$. Analyst locations are highly persistent over time.

under binding constraints rather than a permanent structural efficiency gain. Future work can assess whether similar heterogeneity patterns arise under more localized or idiosyncratic disruptions

5 Understanding the Mechanism

The preceding analysis documents that restricting access to subsidiary-level information enhances forecast accuracy and reduces the bias. We now investigate the mechanism driving these improvements. We have already shown that lockdowns redirect analysts' attention toward more aggregate-level information. We now directly test if it is a bias.

We analyze the bias in analyst forecasts by testing whether subsidiary lockdowns reduce analysts' tendency to overreact to new firm-level information. While prior literature documents underreaction (see Lim (2001), Bradshaw, Richardson and Sloan (2006) and Coibion and Gorodnichenko (2015)), analysts are also prone to overreaction, a pattern where they place excessive weight on salient signals and revise forecasts too aggressively (e.g., Bordalo et al. 2019). In our setting, if subsidiary-level information acts as a localized signal that analysts overweigh (treating local noise as firm-wide trends), then lockdowns that restrict access to such information should attenuate this overreaction. In contrast, if local subsidiary access provides valuable private information, lockdowns would exacerbate forecasting inefficiency by removing a critical information channel.

We test this by using the methodology of Coibion and Gorodnichenko (2015) who show that rational 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, while biased analysts overreact to new information. This leads to a negative relationship between forecast revisions and subsequent forecast bias is expected, where analysts update forecasts too aggressively, leaving a predictable error. Therefore, following Coibion and Gorodnichenko (2015), we estimate:

$$Forecast\_Bias_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} \times Forecast\_Revision_{i,t} + \beta_2 Forecast\_Revision_{i,t} + \beta_3 \#Subs\_Lockdown_{i,t} + \beta_4 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Qtr\_Year_t + \varepsilon_{i,t}, \quad (6)$$

where $Forecast\_Bias_{i,t}$ is constructed using one-quarter-ahead earnings per share (EPS) forecasts and is defined as the difference between actual EPS and the forecasted EPS consensus scaled by the stock price two days prior to the earnings announcement. The forecasted EPS consensus is the average of all one-quarter-ahead EPS forecasts issued in the calendar month immediately preceding the earnings announcement associated with quarter t. $Forecast\_Revision_{i,t}$ is also based on one-quarter-ahead forecasts and is defined as the change in the price-scaled one-quarter-ahead EPS forecast consensus from the preceding month to that month (i.e., month t minus month $t-1$), where both consensus refer to forecasts for the same fiscal quarter whose earnings are announced in quarter t.

A statistically significant negative coefficient ($\beta_2 < 0$) indicates overreaction bias. In our setting, this captures the analyst's tendency to place excessive weight on subsidiary-level information. For instance, consider an analyst observing a positive signal from a nearby subsidiary. If they over-rely on this localized cue, they will revise their forecast upward too aggressively (a positive $Forecast\_Revision$). However, if the firm-wide fundamentals do not match this local optimism—or even move in the opposite direction—the realized earnings will fall short of the inflated forecast, resulting in a negative $Forecast\_Bias$. This systematic inverse relationship confirms that analysts are over-extrapolating localized signals, treating them as representative of the firm.

To examine the role of geographic proximity, we separately estimate this regression separately for (i) analysts located near subsidiaries but outside the firm's headquarters state, (ii) analysts located far from both subsidiaries and headquarters, and (iii) analysts located in the firm's headquarters state. Models 1 to 3 of Table 6 report the results (based on $Forecast\_Bias_{i,t}$ and $Forecast\_Revision_{i,t}$) for analysts near subsidiaries, Model 4 reports the results for analysts located far from subsidiaries and headquarters, and Model 5 reports the results for analysts near headquarters.[20]

Table 6 reports the results of which the key coefficients of interest are β_1 and β_2 . Across Models 1 to 4, β_2 is negative (between -0.24 to -0.29) and highly significant (t-stat below -5.3), confirming that, unconditionally, when analysts revise their earnings forecasts, they do so too aggressively, leaving systematic bias in their forecasts. This confirms overreaction to new information.

In Model 1, the interaction term, β_1 is positive and significant (β_1=0.021, t-stat=2.83), suggesting that when the subsidiaries are locked down, analysts reduce their overreaction bias. Specifically, the positive coefficient indicates that for locked-down subsidiaries, the analysts' forecast revisions are less aggressive relative to the subsequent forecast errors, meaning they adjust their forecasts less drastically and thus reduce the systematic tendency to overshoot the actual outcome. 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 the 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 overreaction 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 β_1 is insignificant (β_1 = 0.011, t-stat= 1.50), suggesting that the overreaction bias only matters for the analysts who are geographically exposed to subsidiary-level

[20] 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.

information. Overall, these findings suggest that access to subsidiary information introduces a behavioral overreaction bias, and lockdowns improve analysts' efficiency by reducing their reliance on these localized, noisy signals and dampening the resulting over-adjustment. This evidence favors the bias explanation over a pure information loss.

Model 5 reports the estimates for analysts located in the firm's headquarters state. In contrast to the subsidiary-near group, we find no statistically significant effect of lockdown exposure on either forecast bias or the overreaction measure in this subsample. Importantly, the interaction term of interest is also economically small and statistically indistinguishable from zero. Taken together, the evidence suggests that the attenuation of the forecasting bias documented in Models 1–3 is not a general "lockdown effect," but is instead concentrated among analysts whose information environment is closely tied to localized subsidiary-level signals; by comparison, lockdowns affecting headquarters do not appear to systematically alter analysts' forecast updating behavior.

6 Do Individual Biases Aggregate?

We now extend our analysis to evaluate whether limiting access to subsidiary-level information through COVID-19 lockdowns aggregates at the firm level, enhancing the precision of firm-level *consensus* forecasts. We first test whether the individual local attention bias aggregate across analysts and then investigate whether there is some cross-sectional variation.

6.1 Does the Bias Aggregate?

Random noise in individual forecasts should average out in consensus forecasts; but localized bias should survive aggregation for two reasons. First, analysts are unevenly distributed geographically, enabling specific subsidiaries to disproportionately influence consensus forecasts. Second, analysts clustered around a subsidiary *co-weight the same local information*, leading to correlated 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 ("*Forecast_Error*") as the absolute difference between the actual EPS and the consensus forecast (mean of all the forecasts), deflated by the stock price two days before the earnings announcement.[21] Our main results remain unchanged when we use the median forecast instead of the mean. Forecast dispersion

[21] 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.

("*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 (3) using $Forecast_{i,t}$ as the dependent variable, where $Forecast_{i,t}$ is either the consensus 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 7 show a negative relationship between the number of subsidiaries under lockdown and forecast error (Model 1, $\beta_1 = -0.017$ $(t - stat = 3.99)$) 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 *Forecast_Error* (0.91)) decrease in consensus forecast error and a 2.20% decrease in forecast dispersion, relative to the sample average.

Appendix Table A6 re-estimates our Equation (3) using the two analyst-linked exposure measures described above. Reassuringly, the results remain qualitatively unchanged: lockdown exposure continues to predict improvements in forecast accuracy and reductions in forecast dispersion. This evidence alleviates the concern that our findings are driven by a purely firm-level lockdown count that may be weakly connected to analysts' actual information environment.

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 nearby subsidiaries within a state should not reduce lockdown effect on forecast errors. To investigate this, we estimate:

$$Forecast\_Error_{i,s,t} = \beta_1 \#State\_Subs\_Lockdown_{i,s,t} \times High\_Analyst\_Num_{i,s,t} + \beta_2 \#State\_Subs\_Lockdown_{i,s,t} + \beta_3 High\_Analyst\_Num_{i,s,t} + \gamma Controls_{i,t} + Firm_i + Mth\_Year_t + \varepsilon_{i,s,t}, \quad (7)$$

where $Forecast\_Error_{i,s,t}$ represents the consensus forecast error for firm *i* in state *s* at time *t*, $\#State\_Subs\_Lockdown_{i,s,t}$ 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.

The results reported in Table 8 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 consensus forecast errors decrease when subsidiaries are under lockdown. The interaction terms in Columns 2 and 4 between state-level lockdowns and number of analysts, which capture the effect of analyst concentration during lockdowns, are statistically insignificant. This supports the presence of systematic bias among analysts near subsidiaries that does not wash out with more nearby analysts.

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

Having documented that subsidiary lockdowns enhance the accuracy of the consensus forecast, 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 with a stronger firm-level signal from the headquarters. Biases reducing the value of such signals will have a more detrimental effect. Therefore, both sides of complexity – heterogeneity across industries and within the firm organizational structure – 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 differences, we augment our baseline regression from Equation (3) by adding interaction terms that capture organizational complexity. Specifically, we estimate:

$$Forecast_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} + \beta_2 \#Subs\_Lockdown_{i,t} \times D_{i,t} + \beta_3 HQ\_Lockdown_i \times D_{i,t} + \beta_4 HQ\_Lockdown_{i,t} + \beta_5 \#Subs\_Lockdown_{i,t} \times \#Segment\_Ind_{i,t} + \gamma Control_{i,t} + Firm_i + Qtr\_Year_t + \varepsilon_{i,t}, \quad (8)$$

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) $Firm\_Hierarchy_{i,t}$: 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 $\#Subs\_Lockdown_{i,t} \times \#Segment\_Ind_{i,t}$, where $\#Segment\_Ind_i$ is the number of industry segments covered by firm i, in the year before the lockdown. When $D_{i,t} = Firm\_Hierarchy_{i,t}$, 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 7, we examine the role of $Industry\_Dispersion_{i,t}$. Consistent with the *attention bias hypothesis*, the impact of subsidiary lockdowns on consensus forecast accuracy and forecast dispersion is strongest in firms with higher industry dispersion. In Models 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 consensus forecast accuracy is stronger in firms with higher hierarchical complexity. Specifically, for high-hierarchy firms, a one-unit increase in $\#Subs\_Lockdown$ 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. Taken jointly, these results further support the bias hypothesis and show how organizational complexity magnifies their impact.

6.3 Information Diversity vs Local Bias: A Discussion

At this point, one key point of the discussion is the link to the existing view/literature. We show that analysts bias do not wash out. This would suggest that having more analysts does not necessarily improve the information environment. 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.

However, two key elements affect the informational role of the subsidiaries: their dispersion across different geographies and *their proximity to market participants*. While prior literature focuses on the *geographic distribution of analysts*, our results specifically focus on the implications of analysts' *proximity to geographically dispersed corporate subsidiaries*. Whereas Gerken and Painter (2023) 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 subsidiaries 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:

$$Forecast\_Error_{i,t} = \beta_1 Geo\_Dispersion_{i,t} + \sum_k \beta_k Controls_{i,t} + Industry_i + Mth\_Year_t + \epsilon_{i,t}, \quad (9)$$

where $Forecast\_Error_{i,t}$ is the absolute consensus analyst forecast error for firm i in month t, and $Geo\_Dispersion_{i,q}$ 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 9. 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 $Geo\_Dispersion_{i,t}$ 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.

7 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 firm's information environment.

7.1 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 proxies: the price jump ratio and the stock price reaction to the information contained in the analysts' forecast revisions and in the management forecast surprises. We find that subsidiary lockdowns reduce distortions 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 $T\text{-}1$ 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 (3), to 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, $\#Subs\_Lockdown_{i,t}$ and $HQ\_Lockdown_{i,t}$, and the set of controls are defined in Sections 3.1 and 3.2.

Table 10, 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 the COVID-19 pandemic. One more subsidiary under lockdown 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 (4) using *Jump* as the dependent variable. Figure 2 presents the estimated β_s coefficients from Equation (4) 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 management 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:

$$CAR_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} \times Mgt\_Earnings\_Surprise_{i,t} + \beta_2 \#Subs\_Lockdown_{i,t} + \beta_3 Mgt\_Earnings\_Surprise_{i,t} + \beta_4 HQ\_Lockdown_{i,t} \times Mgt\_Earnings\_Surprise_{i,t} + \beta_5 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Month\_Year_t + \varepsilon_{i,t}, \quad (10)$$

where $CAR_{i,t}$ is the cumulative abnormal return for firm i around a given 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.[22] $Mgt\_Earnings\_Surprise_{i,t}$ 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 forecast is below the range of the latest available consensus analyst forecast. Other specifications remain the same as in the earlier models.

The estimates are presented in Panel B of Table 10. In column 1, subsidiary lockdowns reduce the market's reaction to management forecast surprises with $\beta_1 = -0.006$ $(t - stat = 2.13)$ 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. Headquarter lockdowns, on the other hand, have weak effects, reinforcing that the channel operates through local subsidiary access.

Overall, these findings point to the conclusion that curtailing access to subsidiary-level signals improves price informativeness by removing distortionary local bias, not by destroying valuable information. When nearby subsidiaries are locked down, a larger share of earnings-relevant news is impounded before the announcement, and the market reacts less to public signals.

7.2 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 earnings announcements. We know that short sellers react to public information by interpreting it better (Engelberg, Reeb, and Ringgenberg, 2012). If lockdowns make nearby analysts' revisions more informative, sophisticated traders should respond more strongly to those revisions before earnings announcements, leading to lower *Jump.* We test this by relating short-selling activity around each forecast revision interacted with nearby-subsidiary lockdown exposure:

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[22] 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.

$$Short\_Volume_{i,j,t} = \beta_1 Forecast\_Revision_{i,j,t} \times Analyst\_Near\_Subs\_Lockdown_{i,j,t} + \beta_2 Forecast\_Revision_{i,j,t} + \beta_3 Analyst\_Near\_Subs\_Lockdown_{i,j,t} + \beta_4 Analyst\_Near\_Subs_{i,j,t} + \beta_5 Forecast\_Revision_{i,j,t} \times Analyst\_Near\_Subs_{i,j,t} + \beta_6 HQ\_Lockdown_{i,j,t} + \beta_7 Forecast\_Revision_{i,j,t} \times HQ\_Lockdown_{i,j,t} + \gamma Control_{i,t} + Firm_i + Mth\_Year_t + \varepsilon_{i,t}, \quad (11)$$

where $Short\_Volume_{i,j,t}$ is defined either as the ratio or the log of one plus the ratio of 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. $Forecast\_Revision_{i,j,t}$ represents the change in analyst *j*'s scaled one-year-ahead EPS forecast for firm *i*, computed as the difference between the forecast issued on day *t* and the analyst's most recent prior forecast for the same firm.

The results, shown in Table 11, indicate that short volume moves inversely with forecast revisions by nearby analysts: it rises after downward revisions and falls after upward revisions. Crucially, the slope of this relation becomes four to five times steeper when the revision comes from an analyst located near subsidiaries under lockdown. The same pattern does not arise around headquarters lockdowns and is robust across event windows and controls. Hence, by curbing local bias, lockdowns raise the information content of nearby analysts' revisions, prompting faster pre-announcement trading by short sellers and helping explain the lower jump ratios documented above.

7.3 Systematic and Idiosyncratic Components of Stock Returns

Next, we test whether cutting off localized subsidiary signals changes what information gets impounded into prices. Our premise is that subsidiary signals are local, idiosyncratic, and noisy, so overweighting them should raise firm-specific noise and reduce co-movement with aggregate (market or industry-wide) factors. If lockdowns curb that overweighting, investor attention should tilt toward broader signals. This implies higher stock price synchronicity (SYNCH) and lower idiosyncratic return volatility (IdioVol) during subsidiary lockdowns.[23] *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.

[23] 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 et al. (2003), 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.

Empirically, we estimate Equation (3) using either *SYNCH* or *IdioVol* as the dependent variables. In Table 12, Model 1 shows that more nearby subsidiaries under lockdown significantly increase *SYNCH* (β_1=0.018, t-stat=5.81). Model 2 shows that a significant decline in *IdioVol* (β_1=-0.261, t-stat=7.82). In standardized terms, 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 at headquarters (HQ) do not produce similar effects; the coefficients on HQ Lockdown are small and insignificant across all specifications, indicating that headquarters lockdowns do not meaningfully influence *SYNCH* or *IdioVol*. These results remain robust when we purge industry-wide movements from both measures as shown in Models 3 and 4 of Table 12. Hence, our findings highlight high noise in subsidiary-level signals; restricting access to them is associated with less noise in firm-specific price movements and greater incorporation of aggregate-level information.

To confirm the causal interpretation of our results, we assess the parallel trends by estimating the dynamic specification from Equation (4) using either *SYNCH* or *IdioVol* as the dependent variable. Figure 3 presents the event-study estimates of β_s coefficients from Equation (4) 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. This pattern is robust to controlling for industry returns (see Panels C and D of Figure 3). Taken together, these results indicate that subsidiary access tends to inject idiosyncratic noise into prices and restricting that access shifts the composition of priced information toward broader, aggregate fundamentals.

7.4 Stock Liquidity

We now investigate whether the improvement in the firm's information environment documented above translates to better stock liquidity. When information is higher-quality and less distorted, information asymmetry declines, adverse selection costs decreases and market liquidity improves (Glosten and Milgrom, 1985; Easley and O'Hara, 2004). Accordingly, we estimate Equation (3) with illiquidity as the outcome variable.

We employ three widely used illiquidity proxies and a composite index: (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) is 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.[24]

[24] 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 return volatility to account for confounding firm characteristics

Table 13 indicates that subsidiary lockdowns are followed by significantly better liquidity across all measures. For example, the regression coefficient associated with subsidiary lockdown in Equation (3), β_1, is -1.12 (t-stat=2.72) when illiquidity is proxied by the composite illiquidity index. 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 illiquidity measure, all economically meaningful estimates. Unlike subsidiary lockdowns, HQ lockdowns have weak and mostly insignificant effects, confirming the mechanism operating through subsidiary specific access.

Event-study estimates from Equation (4), as shown in Figure 4, show no pre-trends in any liquidity proxy. Specifically, prior to the lockdown, the key variable ("$\#Subs\_Lockdown_i$") which proxy for the intensity of the treatment effects exhibit no statistically significant impact. After lockdowns begin, the liquidity improves and the effects persist. These results align with our expectations: restricting access to distortionary subsidiary-level signals reduce information asymmetry, consistent with the documented gains in earnings forecast accuracy and price informativeness.

7.5 Insider Trading and Information Advantage

Finally, we examine whether the cleaner information environment documented above narrows insiders' informational edge. 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)). If subsidiary lockdowns reduce localized distortions and move more information into prices, insiders should find fewer mispricing opportunities. Consistent with the evidence presented so far, we expect two key effects: (i) a decline in opportunistic insider trading volume, and (ii) a decrease in insider trading profitability.

To test these hypotheses, we estimate the specification from Equation (3) using monthly data, where $Insider\_Opp\_TradingVol_{i,t}$ is the dependent variable. We define $Insider\_Opp\_TradingVol_{i,t}$ 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 Section 3 for the definitions of insiders and their trades). Table 14, 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 increase (β_1= -0.52 (t-stat=2.03) in Model 2). One more subsidiary under

that may influence liquidity. In the main table, we do not control for size as size is potentially a proxy of liquidity. However, our results remain significant if we include size, as shown in Table A7.

lockdown 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.[25] 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. As shown in Figure 5, we find no evidence of pre-trends and a clear post-lockdown decline in activity, supporting a causal interpretation.

Panel B of Table 14 investigates whether subsidiary lockdowns affect insiders' ability to generate profits from their trades. We estimate Equation (3) using $Insider\_Opp\_Profit_{i,t}$ as the dependent variable, where $Insider\_Opp\_Profit_{i,t}$ 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 a much weaker effect on insider profits. These results indicate that subsidiary lockdowns reduce distortionary subsidiary-level signals, improve price informativeness and lower insiders' informational advantage.

7.6 Addressing Alternative Explanations

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

One concern is that subsidiary lockdowns might mechanically simplify firms, for example, by temporarily reducing output and operational uncertainty which could by itself raise price informativeness and lower forecast error. 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

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

lockdown periods. As reported in Internet Appendix Table A8, we find no significant changes in these firm output measures during lockdown periods. 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 A9 through A11, 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 artifact 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), we still 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 Tables A12-A14, 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

We show that more information is not always better: when analysts lean too heavily on localized subsidiary signals, attention is distorted, and the firm's information environment deteriorates. Using the COVID-19 pandemic as a natural experiment, we show that restricting physical access to subsidiary operations, via localized lockdowns, improves nearby analysts' forecast accuracy. For analysts located near subsidiaries, lockdown reduces forecast errors coming from overreliance on noisy, local signals, attenuates their overreaction to firm-level information, and shifts their attention from subsidiary to non-local information. These individual analyst level gains aggregate: consensus forecasts become more accurate and less dispersed when subsidiary access is restricted, particularly for firms where local signals are most misleading (firms in multiple industries or with deep hierarchical complexity). Together, the evidence points to an attention-allocation channel rather than an information-destruction channel.

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 the now-cleaner revisions in earnings forecasts of nearby analysts, 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. The improved information environment leads to greater liquidity and lower information asymmetry. Moreover, 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 fundamentals or public subsidiary disclosures.

Our results reconcile two strands of the geography literature. Diversity of viewpoints across space can aid aggregation, but proximity to operating subsidiaries can push analysts toward salient, non-representative signals that skew beliefs. When access to those localized cues is curtailed, attention rebalances toward firm-level and aggregate information, improving both forecasts and prices. The effects are economically meaningful, robust to rich firm-time and analyst-time controls, and absent when headquarters—not subsidiaries—are restricted, underscoring that the mechanism runs through subsidiary-specific access rather than broad, firm-wide disruptions.

These insights open several promising directions for future research. First, understanding the heterogeneity in local information's effects across industries and 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

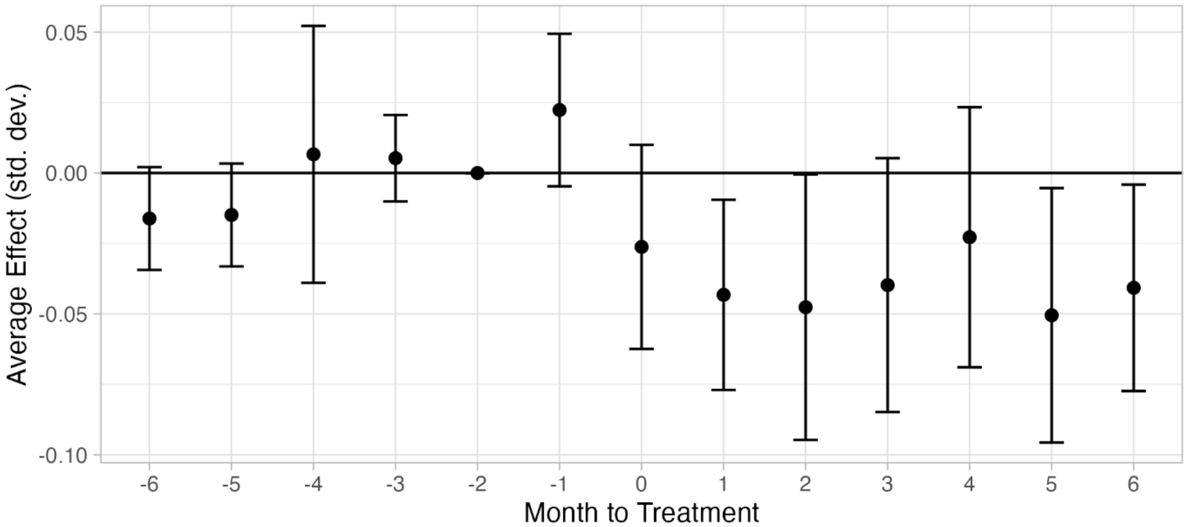
Variable Name	Definition
Subsidiary-Related Variables	
#Subsidiaries	The number of subsidiaries of a firm. Only subsidiaries outside of the same state of the headquarter are counted. The subsidiary data is from LexisNexis.
#Subs_Lockdown	The number of subsidiaries under lockdown of a firm. Only subsidiaries 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).
Analyst_Near_Subs_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.
Analyst_Near_Subs	Dummy variable equal to 1 if analyst *j* is located in the same state as at least one subsidiary of firm *i* at time *t*, and 0 otherwise.
HQ_Nearby_Lockdown	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*.
#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_Subs_Lockdown	The number of subsidiaries under lockdown of a firm in certain state.
#Subs_Lockdown_Near_Analyst	The number of subsidiaries under lockdown of a firm *i*. Only subsidiaries outside of the same state of the headquarter, and in the same state as at least one analyst covering the firm in month *i* are counted.
#Analyst_Lockdown	The number of analysts covering the firm *i* who are themselves located in lockdown areas in month *t*.
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 Pástor and Stambaugh (2003). It is the ordinary least squares estimate of $\gamma_{i,t}$ in the regression $$r^e_{i,d+1,t} = \theta_{i,t} + \phi_{i,t} r_{i,d,t} + \gamma_{i,t} sign(r^e_{i,d,t}) \cdot v_{i,d,t} + \epsilon_{i,d+1,v}, d = 1, \ldots, D,$$ 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: $R_{i,t} = \beta_{i,0} + \beta_{i,1} \times Mkt\_RF_t + \beta_{i,2} \times SMB_t + \beta_{i,3} \times HML_t + \beta_{i,4} \times RMW_t + \beta_{i,5} \times CMA_t + \varepsilon_{i,t}$, 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. 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 market-level and industry-level news, and less 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, $\left\vert CAR_{it}^{(T-21,T+2)} \right\vert > \sqrt{24}\hat{\sigma}_{it}$. 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 stockholder'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_Vol	The monthly volatility equals to monthly standard error of a stock's daily adjusted return in that month. The data is from CRSP.
Insider Trading/Short Selling-Related Variables	

Oppo 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. Oppo Sell is defined as the monthly opportunistic selling volume.
Oppo Buy	Similar to opportunistic sell. The buy transaction done by opportunistic traders is opportunistic buy. Oppo Buy is defined as the monthly opportunistic buying volume.
Insider_Opp_TradingVol	Monthly opportunistic trading volume scaled by the insider's existing holding position before the trade, following Massa, Qian, Xu, and Zhang (2015), multiplied 1000. The numerator can be *Oppo Sell*, *Oppo Buy*, or *Oppo Buy+Sell,* for firm *i* in month *t*. We also construct an alternative measure, defined as *log(Oppo Sell+ 1)*. Our main results are robust across different measures.
Short_Volume	We download the daily short sale volume files from FINRA (see website link here). We aggregate the short sale volume across all reporting exchanges (e.g. Nasdaq and NYSE). We construct short-sale measures by aggregating the daily short-sale volume over the relevant window and scaling it by total trading volume over the same window. Our main variable is the ratio of short-sale volume to total volume, or its log transformation, log(1 + short volume 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. At the firm level, following Thomas (2002) and Guo and Mota (2021), for firm *i* in month *t*, 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. Although the forecast horizon is quarterly, the month index reflects the timing of when the analyst issues the forecast. Consequently, forecasts for the same fiscal quarter may be recorded in different months across analysts. 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. Firm-level Forecast_Error is multiplied by 100 for ease of interpretation. At the analyst level, for firm *i* analyst *j* in quarter *t*, 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	At the firm level, 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. At the analyst level, following Stickel (1991), we look at the difference between the revised forecast and the most recent forecast by the same analyst. Forecast_Revision is defined as the change in analyst *j*'s one-year-ahead EPS forecast for firm *i*. It is computed as the difference between (i) the one-year-ahead EPS forecast issued by analyst *j* on day *t*, scaled by the stock price two days prior to the earnings announcement, and (ii) the most recent prior one-year-ahead EPS forecast for the same firm issued by the same analyst for that same fiscal year, scaled in the same way. For firm *i*, analyst *j*, and month *t*, *Forecast_Revision_EA* is defined as the absolute change in analyst *j*'s one-year-ahead EPS forecast for firm *i* for the same fiscal year around the quarterly earnings announcement. 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). Revisions are assigned to the earnings-announcement month to match the monthly panel structure.
Analyst Coverage	Similar to Chan and Hameed (2006), analyst coverage is defined as the number of analysts covering the firm i in quarter t.
Subs_Topic_Intensity	Subs_Topic_Intensity is measured as the count of sentences in analyst *j*'s report on firm *i* in month *t* 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.
Firm_Topic_Intensity	Firm_Topic_Intensity is the count of sentences in analyst *j*'s report on firm *i* in month *t* 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.
Macro_Topic_Intensity	Macro_Topic_Intensity is the count of sentences in analyst *j*'s report on firm *i* in month *t* 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.
Industry_Topic_Intensity	Industry_Topic_Intensity is the count of sentences in analyst *j*'s report on firm *i* in month *t* 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_Subs_Topic_Intensity	For each analyst j, we compute the average share of sentences classified as *nearby_subsidiary* across all of the analyst's available pre-period reports in 2019 in our Investext–I/B/E/S matched sample. *High_Subs_Topic_Intensity* is defined as an indicator equal to one if this analyst-level measure is above the sample median, and zero otherwise.
High_Analyst_Num	Dummy variable equal to 1 if at least three analysts located in state j cover firm i in month t.
Mgt_Earnings_Surprise	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 Nearby Subsidiary Lockdowns on Analysts' Forecast Error

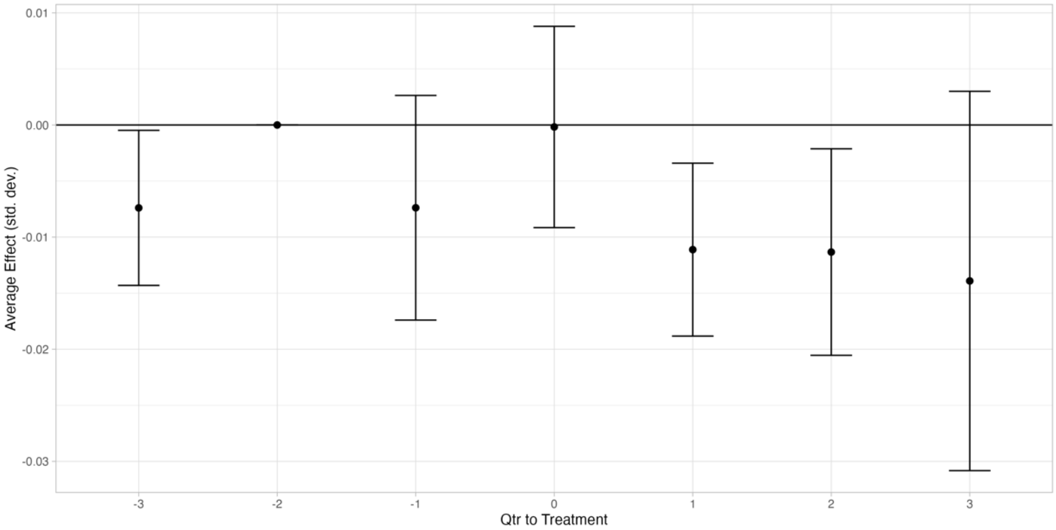


In this figure, we plot the event-time estimates (β_s) from the following equation for the effect of nearby subsidiary lockdowns on analysts' forecast outcomes for the focal firms:

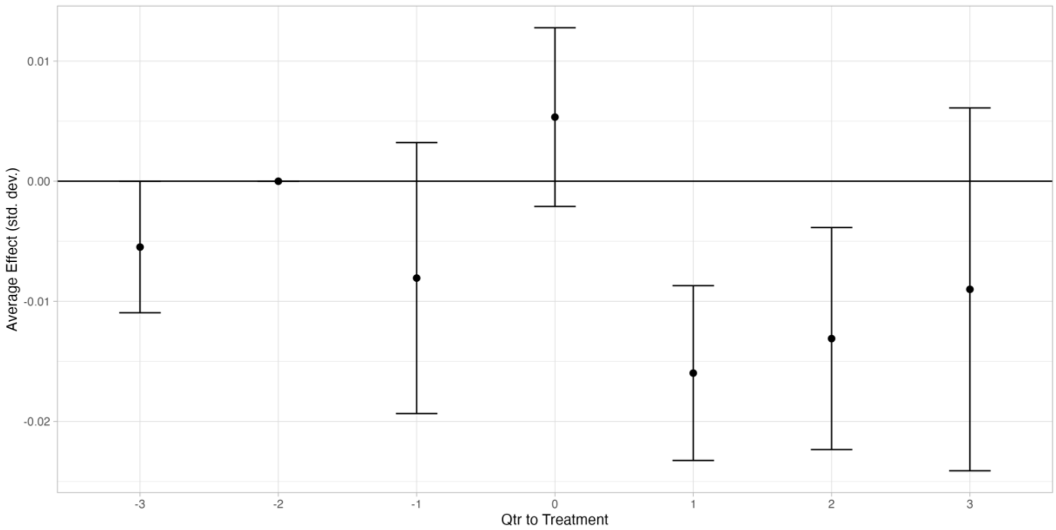
$$Y_{i,j,t} = \sum_{s \neq -2} \beta_s Analyst\_Near\_Subs\_Lockdown_{i,j} * D_{s(i,j,t)} + \alpha Analyst\_Near\_Subs_{i,j,t} + Firm\_Mth\_Year_{i,t} + Firm\_Analyst_{i,j} + \epsilon_{i,j,t},$$

where $D_{s(i,j,t)}$ are event-time dummies indicating the months before and after the first lockdown of one of analyst j's nearby subsidiaries of firm i, with $s = -2$ serving as the benchmark period. $Analyst\_Near\_Subs\_lockdown_{i,j}$ equals one if any subsidiary of firm i near analyst j ever experiences a lockdown and zero otherwise. We include firm-by-month-year fixed effects and firm-by-analyst fixed effects. Standard errors are three-way clustered by firm, analyst, and year-month. The x-axis reports months relative to the first lockdown among an analyst's nearby subsidiaries (month 0). The dots denote point estimates and the bars indicate 95% confidence intervals.

Figure 2: The Effects of Subsidiaries Under Lockdown on Jump



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



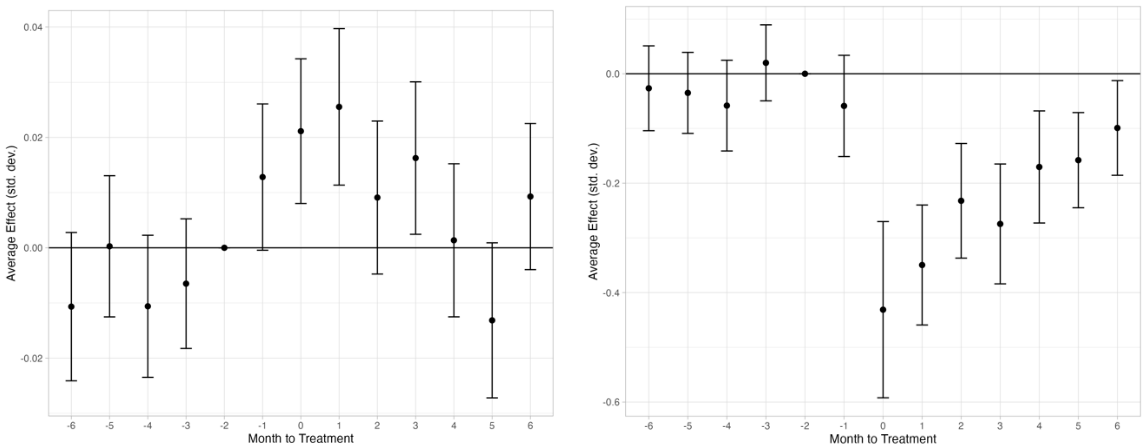
Panel B: Effects of Lockdown on *Jump*^{FF5}

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

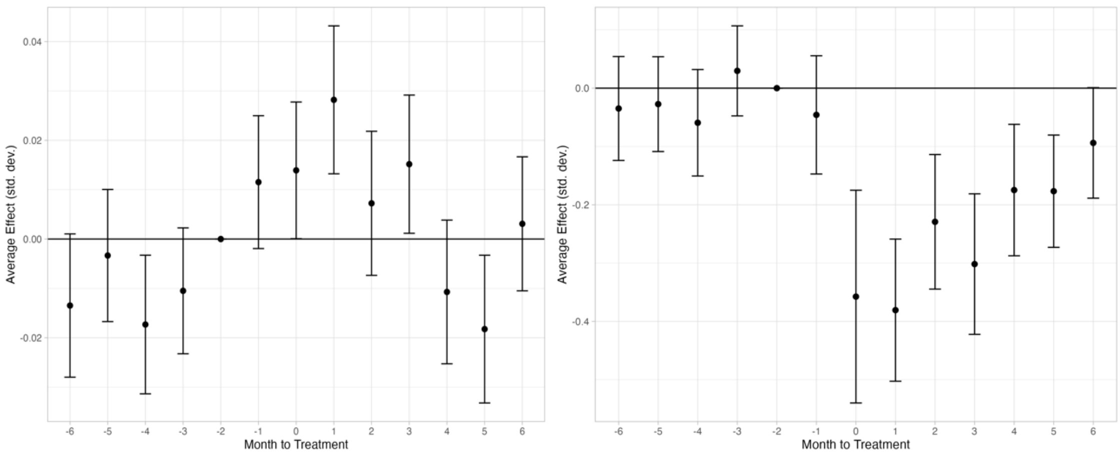
$$Jump_{i,t} = \sum_{s \neq -2} \beta_s \ \#Subs\_Lockdown_i * D_{s(i,t)} + \alpha HQ\ Lockdown_{i,t} \ + \gamma\ Control_{i,t} + Firm_i + Qtr\_Year_t + \varepsilon_{i,t}$$

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. $\#Subs\_Lockdown_i$ 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 3: The Effects of Subsidiaries Under Lockdown on SYNCH and IdioVol



Panel A: Effects of Lockdown on SYNCH **Panel B: Effects of Lockdown on IdioVol**



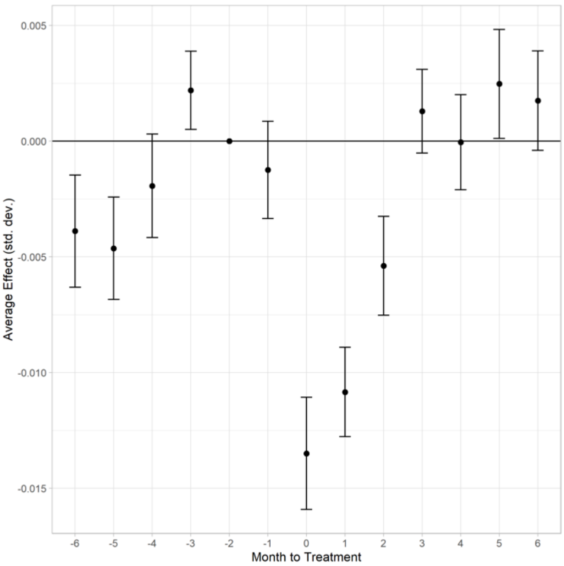
Panel C: Effects of Lockdown on SYNCH **Panel D: Effects of Lockdown on IdioVol**

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

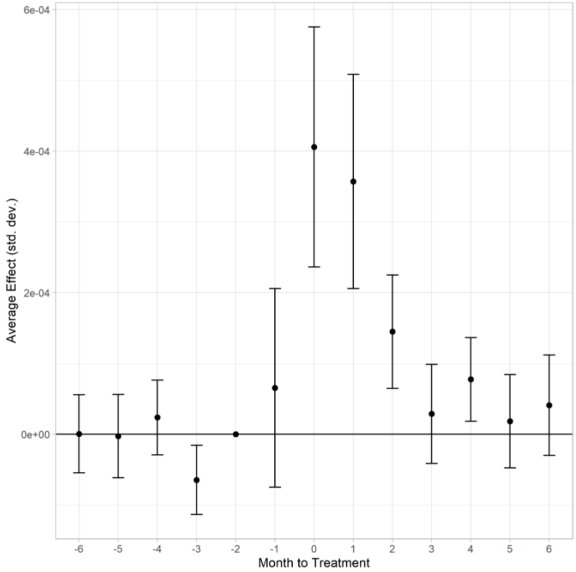
$$Y_{i,t} = \sum_{s\neq -2} \beta_s \ \#Subs\_Lockdown_i * D_{s(i,t)} + \alpha HQ\_Lockdown_{i,t} + \gamma \ Control_{i,t} + Firm_i + Mth\_Year_t + \varepsilon_{i,t}$$

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. $\#Subs\_Lockdown_i$ 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.

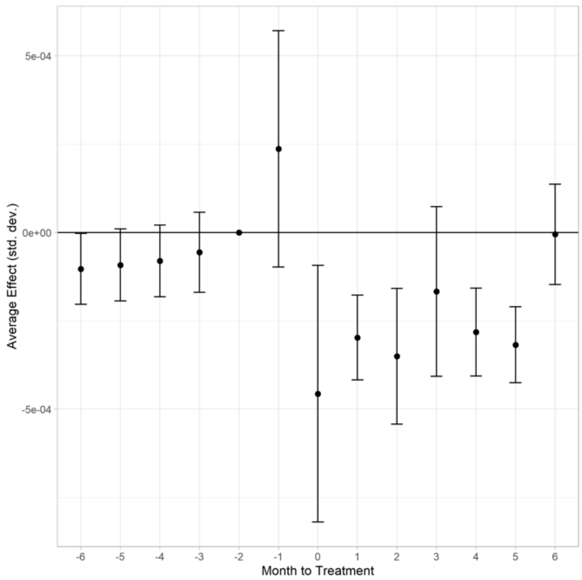
Figure 4: The Effects of Subsidiaries Under Lockdown on Liquidity



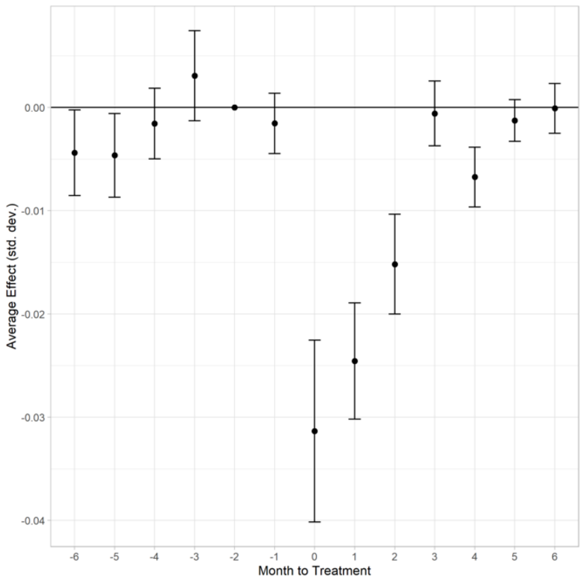
Panel A: Effects of Lockdown on Amihud Illiquidity



Panel B: Effects of Lockdown on PS Gamma



Panel C: Effects of Lockdown on Roll's Spread



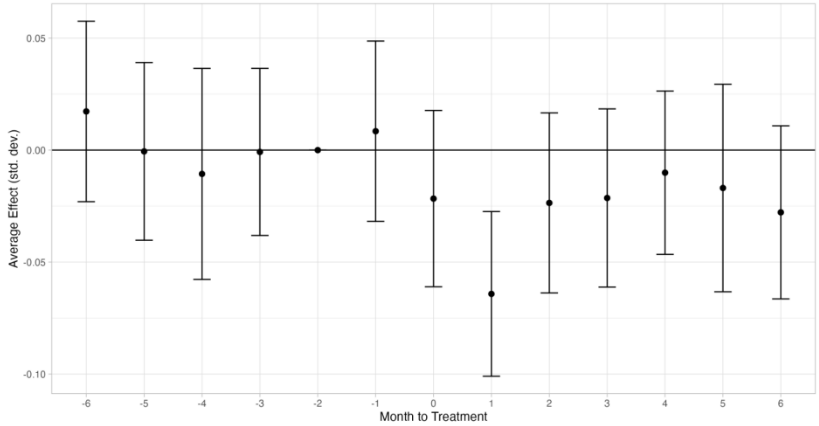
Panel D: Effects of Lockdown on Composite Illiquidity

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

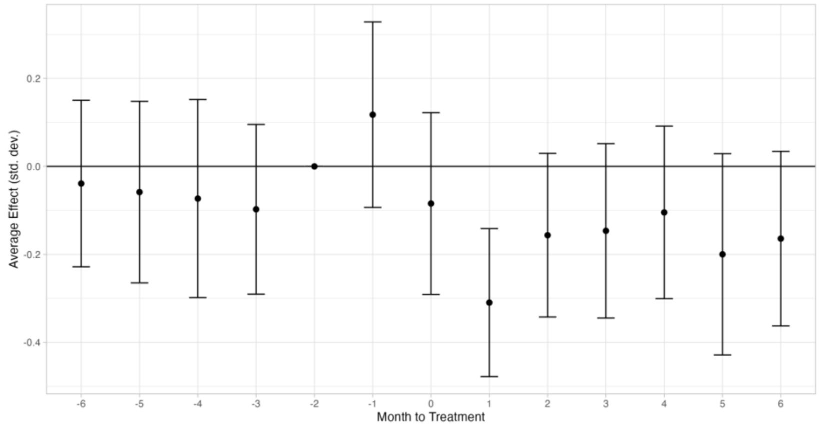
$$Liquidity_{i,t} = \sum\nolimits_{s \neq -2} \beta_s \, \#Subs\_Lockdown_i * D_{s(i,t)} + \alpha HQ\_Lockdown_{i,t} + \gamma \, Control_{i,t} + Firm_i + Mth\_Year_t + \varepsilon_{i,t}$$

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. $\#Subs\_Lockdown_i$ 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 5: 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:

$$Insider\_Opp\_TradingVol_{i,t} = \sum_{s\neq-2} \beta_s \#Subs\_Lockdown_i * D_{s(i,t)} + \alpha HQ\_Lockdown_{i,t} + \gamma\ Control_{i,t} + Firm_i + Mth\_Year_t + \varepsilon_{i,t}$$

where the dependent variable $Insider\_Opp\_TradingVol_{i,t}$ 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. $\#Subs\_Lockdown_i$ 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 Pá stor and Stambaugh (2003). 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.

	Mean	SD	P10	Median	P90	# of firms
# Subsidiary	10.077	16.384	1.000	4.000	24.980	1812
# Subs_Lockdown	0.331	0.399	0.000	0.167	0.850	1812
# Subs_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_Ret	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_Vol (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
Forecast_Error (Firm-Level) (%)	0.91	1.69	0.08	0.31	2.33	1320
Forecast_Dispersion (%)	0.50	0.84	0.06	0.20	1.21	1245
Forecast_Error (Analyst-Level) (%)	0.784	1.162	0.089	0.352	2.073	1734
Forecast_ Revision_EA	0.461	2.427	0.070	0.197	0.663	1681
Analyst_Near_Subs_Lockdown	0.015	0.035	0.000	0.000	0.051	1734
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 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 ten investment banks who cover firms with subsidiaries and are based outside the firm's headquarters state. We estimate the following Possion regression using reports written by analysts located outside the headquarters state:

$$Subs\_Topic\_Intensity_{i,j,t} = \beta_1 Analyst\_Near\_Subs\_Lockdown_{i,j,t} + \beta_2 Analyst\_Near\_Subs_{i,j,t} + Controls_{i,t} + Firm_i + Analyst_j + Mth\_Year_t + \epsilon_{i,j,t},$$

where $Subs\_Topic\_Intensity_{i,j,t}$ is measured as the count of sentences related to nearby subsidiary information extracted from analyst *j* 's report about firm *i* in month *t*. 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, as well as the total word count of the analyst's report to account for baseline report length. We also include the firm, analyst, year-month fixed effects. In Column 2, we run the same specification but replace the outcome with the count of sentences related to more aggregate level (firm, industry, and market level). Detailed variable definitions are provided in the appendix. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level.

Dependent Variables	**Topic Intensity**	
	Subsidiary/Local	Firm+Industry+Macro/Market
	(1)	(2)
Analyst_Near_Subs_Lockdown	-0.919***	0.045*
	(-3.32)	(1.67)
Analyst_Near_Subs	0.271	0.147***
	(0.547)	(5.14)
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 3: 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:

$$Forecast\_Error_{i,j,t} = \beta_1 Analyst\_Near\_Subs\_Lockdown_{i,j,t} + \beta_2 Analyst\_Near\_Subs_{i,j,t} + Firm\_Mth\_Year_{i,t} + Firm\_Analyst_{i,j} + Analyst\_Mth\_Year_{j,t} + \epsilon_{i,j,t},$$

where $Forecast\ error_{i,j,t}$ is analyst j's forecast error for firm i in month t, and $Analyst\_Near\_Subs\_Lockdown_{i,j,t}$ 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. $Analyst\_Near\_Subs_{i,j,t}$ 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 excludes analysts near HQ (includes analysts near subsidiaries and analysts located far away from subsidiaries and HQ). 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)
Analyst_Near_Subs_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)
Analyst_Near_Subs	-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)
Analyst_Near_Subs_Lockdown	-0.053***	-0.096***	-0.075**	-0.144**
	(-2.89)	(-2.70)	(-2.21)	(-2.57)
Analyst_Near_Subs	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 4: The Effects of Subsidiaries under Lockdown on Individual Analyst Forecast Errors: Heterogeneity by Pre-Period Subsidiary Mentions

This table reports the heterogeneous effects of nearby subsidiaries under lockdowns on individual analysts' forecast errors, conditional on analysts' pre-period propensity to emphasize nearby subsidiaries in their written research. The sample spans from 2019 to 2020 and includes only analysts from the ten investment banks who cover firms with subsidiaries and are based outside the firm's headquarters state. We estimate the following regression at the analyst–firm–time level:

$$Forecast\_Error_{i,j,t} = \beta_1\ Analyst\_Near\_Subs\_Lockdown_{i,j,t} + \beta_2\ High\_Subs\_Topic\_Intensity_{i,j} + \beta_3\ Analyst\_Near\_Subs\_Lockdown_{i,j,t} \times High\_Subs\_Topic\_Intensity_{i,j} + \beta_4 Analyst\_Near\_Subs_{i,j,t} + \gamma Control_{i,t} + Firm_i + Analyst_j + Mth\_Year_t + \epsilon_{i,j,t},$$

where $High\_Subs\_Topic\_Intensity_{i,j}$ is an indicator equal to one if average share of sentences classified as *nearby_subsidiary* across all of the analyst j's available pre-period reports in 2019 in our Investext–I/B/E/S matched sample is above the sample median, and zero otherwise. We 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 variable	**Analyst Forecast Error**			
	(1)	(2)	(3)	(4)
Analyst_Near_Subs_Lockdown	-0.125	0.020	-0.094	0.069
	(-0.861)	(0.104)	(-0.633)	(0.345)
Analyst_Near_Subs_Lockdown × High_Subs_Topic_Intensity		-0.586**		-0.608**
		(-2.70)		(-2.71)
HQ_Nearby_Lockdown			-0.116	-0.161
			(-0.826)	(-1.26)
Analyst_Near_Subs	0.069	0.075	0.065	0.055
	(0.945)	(1.10)	(0.910)	(1.10)
Control	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Analyst FE	Y	Y	Y	Y
Number of observations	7,849	7,849	7,849	7,849
R-squared	0.52	0.52	0.52	0.52

Table 5: 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:

$$Forecast\_Revision\_EA_{i,j,t} = \beta_1 Analyst\_Near\_Subs\_Lockdown_{i,j,t} + \beta_2 Analyst\_Near\_Subs_{i,j,t} + Firm\_Mth\_Year_{i,t} + Firm\_Analyst_{i,j} + \epsilon_{i,j,t},$$

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 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 $Analyst\_Near\_Subs\_Lockdown_{i,j,t}$ is a dummy variable set to 1 if all subsidiaries of firm i in the same state as analyst j are under lockdown 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. $Analyst\_Near\_Subs_{i,j,t}$ 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-month 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 fixed effects. In column 3, we run a similar regression 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 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-month, 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)
Analyst_Near_Subs_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_Nearby_Lockdown			0.208* (1.84)	0.155* (1.90)		
Analyst_Near_Subs	-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 6: 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:

$$Forecast\_Bias_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} \times Forecast\_Revision_{i,t} + \beta_2 Forecast\_Revision_{i,t} + \beta_3 \#Subs\_Lockdown_{i,t} + \beta_4 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Qtr\_Year_t + \varepsilon_{i,t},$$

where $Forecast\_Bias_{i,t}$ 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. $Forecast\_Revision_{i,t}$ is defined as the one-quarter-ahead EPS forecast consensus scaled by prices in month t minus the scaled EPS forecast consensus in month $t-1$. $\#Subs\_Lockdown_{i,t}$ 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, analyst far away from subsidiaries or HQ in Column 4, and analyst within HQ state in Column 5. 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	Analysts Near HQ
	(1)	(2)	(3)	(4)	(5)
#Subs_Lockdown× Forecast_Revision	0.021**			0.011	0.027
	(2.83)			-1.5	(1.66)
#Analyst_Lockdown (Any) ×Forecast_Revision		0.053***			
		(3.32)			
#Analyst_Lockdown (All) ×Forecast_Revision			0.053***		
			(3.51)		
HQ_Lockdown ×Forecast_Revision					-0.094
					(-0.438)
Forecast_Revision	-0.267***	-0.290***	-0.280***	-0.236***	-0.150
	(-9.58)	(-10.8)	(-11.3)	(-5.29)	(-1.08)
#Subs_Lockdown	0.000			0.000	-6.63e-5
	(-0.250)			(-0.045)	(-0.462)
#Analyst_Lockdown (Any)		-0.0003**			
		(-2.37)			
#Analyst_Lockdown (All)			-0.0003*		
			(-2.03)		
HQ Lockdown	-0.0004	-0.0004	-0.0006	-0.001	-0.0008
	(-0.220)	(-0.199)	(-0.286)	(-1.02)	(-0.444)
Firm FE	Y	Y	Y	Y	Y
Time FE	Y	Y	Y	Y	Y
Number of observations	4,777	4,777	4,777	10,362	1,769
R-squared	0.27	0.28	0.28	0.28	0.27

Table 7: 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:

$$Forecast_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} + \beta_2 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Qtr\_Year_t + \varepsilon_{i,t},$$

where $Forecast_{i,t}$ is analyst forecast error or forecast dispersion for firm i in the quarter of month t, $\#Subs\_Lockdown_{i,t}$ 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:

$$Forecast_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} + \beta_2 \#Subs\_Lockdown_{i,t} \times D_{i,t} + \beta_3 HQ\_Lockdown_i \times D_{i,t} + \beta_4 HQ\_Lockdown_{i,t} + \beta_5 \#Subs\_Lockdown_{i,t} \times \#Segment\_Ind_{i,t} + \gamma Control_{i,t} + Firm_i + Qtr\_Year_t + \varepsilon_{i,t},,$$

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 $\#Subs\_Lockdown_{i,t} \times \#Segment\_Ind_{i,t}$, and $D_{i,t}$ is $Firm\_Hierarchy_{i,t}$, 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)**
#Subs_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)
#Subs_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)
#Subs_Lockdown ×			0.002	0.003		
#Segments_Ind			(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 8: 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:

$$Forecast\_Error_{i,s,t} = \beta_1 \#State\_Subs\_Lockdown_{i,s,t} \times High\_Analyst\_Num_{i,s,t} + \beta_2 \#State\_Subs\_Lockdown_{i,s,t} + \beta_3 High\_Analyst\_Num_{i,s,t} + \gamma Controls_{i,t} + Firm_i + Mth\_Year_t + \varepsilon_{i,s,t}$$

where $Forecast\_Error_{i,s,t}$ is the consensus forecast error for firm i in state s in month t, $\#State\_Subs\_Lockdown_{i,s,t}$ represents the number of subsidiaries under lockdown in state s for firm i in month t, and $High\_Analyst\_Num_{i,s,t}$ is a dummy equals to 1 if at least three analysts located in state s cover firm i in month t. In columns 2 and 4, we include an interaction term between $\#State\_Subs\_Lockdown_{i,s,t}$ and $High\_Analyst\_Num_{i,s,t}$. 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_Subs_Lockdown × High_Analyst_Num		-0.027		0.027
		(-1.19)		(0.768)
#State_Subs_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 9: 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:

$$Forecast\_Error_{i,t} = \beta_1 Geo\_Dispersion_{i,t} + \sum_k \beta_k Controls_{i,t} + Industry_i + Mth\_Year_t + \epsilon_{i,t},$$

where $Forecast\_Error_{i,t}$ is the absolute consensus analyst forecast error for firm i in the month t, and $Geo\_Dispersion_{i,t}$ 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)
Geo_Dispersion	-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 10: 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 ratios around quarterly earnings announcements:

$$Jump_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} + \beta_2 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Qtr\_Year_t + \varepsilon_{i,t},$$

where $Jump_{i,t}$ is the price jump ratio for firm i in quarter t, $\#Subs\_Lockdown_{i,t}$ 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. $Jump^{FF3}$ ($Jump^{FF5}$) 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)
#Subs_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:

$$CAR_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} \times Mgt\_Earnings\_Surprise_{i,t} + \beta_2 \#Subs\_Lockdown_{i,t} + \beta_3 Mgt\_Earnings\_Surprise_{i,t} + \beta_4 HQ\_Lockdown_{i,t} \times Mgt\_Earnings\_Surprise_{i,t} + \beta_5 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Mth\_Year_t + \varepsilon_{i,t},$$

where $CAR_{i,t}$ is the cumulative abnormal return during period [t-1, t+1], [t-2, t+2] or [t-3, t+3], for firm i who releases its management forecast on day t. *Mgt_Earnings_Surprise* 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 management 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）
#Subs_Lockdown ×	-0.006**	-0.005*	-0.008**	-0.006**	-0.009**	-0.007**
Mgr_Earnings_Surprise	(-2.13)	(-1.79)	(-2.54)	(-2.10)	(-2.52)	(-2.17)
#Subs_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)
Mgr_Earnings_Surprise	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 ×	0.029	0.015	0.052*	0.033	0.068*	0.048
Mgr_Earnings_Surprise	(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 11: 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:

$$Short\_Volume_{i,j,t} = \beta_1 Forecast\_Revision_{i,j,t} \times Analyst\_Near\_Subs\_Lockdown_{i,j,t} + \beta_2 Forecast\_Revision_{i,j,t} + \beta_3 Analyst\_Near\_Subs\_Lockdown_{i,j,t} + \beta_4 Analyst\_Near\_Subs_{i,j,t} + \beta_5 Forecast\_Revision_{i,j,t} \times Analyst\_Near\_Subs_{i,j,t} + \beta_6 HQ\_Lockdown_{i,j,t} + \beta_7 Forecast\_Revision_{i,j,t} \times HQ\_Lockdown_{i,j,t} + \gamma Control_{i,t} + Firm_i + Mth\_Year_t + \varepsilon_{i,t},$$

where $Short\_Volume_{i,j,t}$ is measured as either the ratio of short selling volume to total 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]. $Forecast\_Revision_{i,j,t}$ 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)
Forecast_Revision × Analyst_Near_Subs_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)
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)
Analyst_Near_Subs_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)
Forecast_Revision × Analyst_Near_Subs	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)
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)
Analyst_Near_Subs	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 12: 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:

$$I_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} + \beta_2 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Mth\_Year_t + \varepsilon_{i,t},$$

where $I_{i,t}$ is SYNCH measure or IdioVol for firm i in month t, $\#Subs\_Lockdown_{i,t}$ 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)
#Subs_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 13: 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:

$$L_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} + \beta_2 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Mth\_Year_t + \varepsilon_{i,t},$$

where $L_{i,t}$ is a liquidity measure for firm i in the quarter of month t, $\#Subs\_Lockdown_{i,t}$ 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 Pástor and Stambaugh (2003), 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)
#Subs_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 14: 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:

$$Insider\_Opp\_TradingVol_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} + \beta_2 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Mth\_Year_t + \varepsilon_{i,t},$$

where $Insider\_Opp\_TradingVol_{i,t}$ 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:

$$Insider\_Opp\_Profit_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} + \beta_2 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Mth\_Year_t + \varepsilon_{i,t},$$

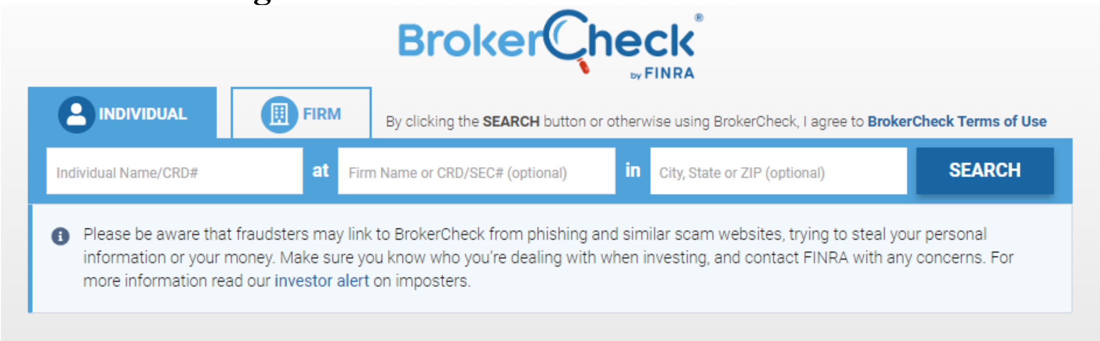
where $Insider\_Opp\_Profit_{i,t}$ is excess profit of opportunistic insider buying minus selling, measured by trading dollar volume multiplying abnormal return scaled by dollar volume of insiders' holding position, multiplied by 1000. The abnormal return is stock return minus CRSP value-weighted market 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	**Insider Opportunistic Trading Volume**		
	(Oppo buy+sell) / Insider holding	**Oppo sell / Insider holding**	**Oppo buy / Insider holding**
	(1)	**(2)**	**(3)**
#Subs_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

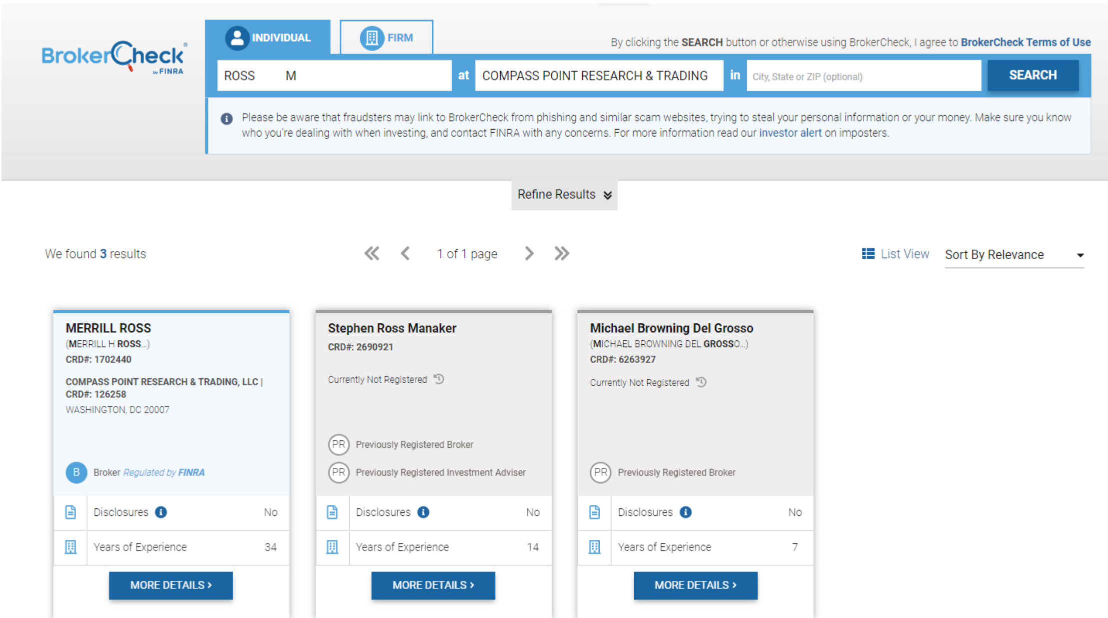
Panel B	**Insider Opportunistic Trading Profits**		
	[+1, +1]	**[+1, +3]**	**[+1, +5]**
	(1)	(2)	(3)
#Subs_Lockdown	-0.069*	-0.22**	-0.272***
	(-1.79)	(-2.26)	(-4.32)
HQ_Lockdown	1.166	2.91	3.07*
	(1.58)	(1.59)	(1.87)
Controls	Y	Y	Y
Firm FE	Y	Y	Y
Time FE	Y	Y	Y
Number of observations	18900	18900	18900
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 3, 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)
Analyst_Near_Subs _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)
Analyst_Near_Subs	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 7, 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)
#Subs_ 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 10, with the dependent variable being price jump ratio, $Jump^{FF3}$ ($Jump^{FF5}$), 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)
#Subs_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 12. 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)
#Subs_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 Analyst Forecasts: Robustness to Analyst-Linked Lockdown Exposure Measures

This table reports robustness tests using analyst-linked lockdown exposure measures in place of the baseline firm-level count of locked-down subsidiaries. The table examines the effects of subsidiary lockdown exposure on analyst forecast errors and forecast dispersion. The sample spans 2016 – 2020 and includes only firms with subsidiaries. We estimate the following regression using monthly data:

$Forecast_{i,t} = \beta_1 Lockdown\_Exposure_{i,t} + \beta_2 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Qtr\_Year_t + \varepsilon_{i,t}$,

In columns (1) and (2), $Lockdown\_Exposure_{i,t}$ is $\#Subs\_Lockdown\_Near\_Analyst$, defined as the number of subsidiaries under lockdown in month $t$for firm $i$that have at least one nearby analyst covering the firm. In columns (3) and (4), $Lockdown\ Exposure_{i,t}$ is $\#Analyst\_Lockdown$, defined as the number of analysts covering firm $i$who are themselves located in lockdown areas in month t. We use the same control variables, fixed effects, and clustered standard errors by firm and month. 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, respectively.

Dependent variables	**Analyst Forecast Error**	**Forecast Dispersion**	**Analyst Forecast Error**	**Forecast Dispersion**
	(1)	(2)	(3)	(4)
#Subs_Lockdown_Near_Analyst	-0.108***	-0.060***		
	(-4.89)	(-3.88)		
#Analyst_Lockdown			-0.073***	-0.043***
			(-3.43)	(-2.93)
HQ Lockdown	-0.076**	-0.059*	-0.103**	-0.074*
	(-2.16)	(-1.96)	(-2.44)	(-2.09)
Control	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Number of observations	72,887	69,109	72,887	69,109
R-squared	0.51	0.57	0.51	0.57

Table A7: 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:

$$L_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} + \beta_2 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Mth\_Year_t + \varepsilon_{i,t},$$

where $L_{i,t}$ is a liquidity measure for firm i in the quarter of month t, $\#Subs\_Lockdown_{i,t}$ 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 Pástor and Stambaugh (2003), 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)
#Subs_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 A8: 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:

$$P_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} + \beta_2 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Mth\_Year_t + \epsilon_{i,t},$$

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, $\#Subs\_Lockdown_{i,t}$ 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)
#Subs_Lockdown	0.0112	-0.5610	0.0520
	(1.008)	(-0.6843)	(1.558)
HQ_Lockdown	-0.2206	-5.846	0.2986
	(-1.638)	(-1.079)	(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 A9: 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:

$$I_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} + \beta_2 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Mth\_Year_t + \varepsilon_{i,t},$$

where $I_{i,t}$ is SYNCH measure or IdioVol for firm i in month t, $\#Subs\_Lockdown_{i,t}$ 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)
#Subs_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 A10: 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:

$$Jump_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} + \beta_2 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Mth\_Year_t + \varepsilon_{i,t},$$

where $Jump_{i,t}$ is the price jump ratio for firm i in month t, $\#Subs\_Lockdown_{i,t}$ 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. $Jump^{FF3}$ ($Jump^{FF5}$) 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)
#Subs_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 A11: 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:

$$L_{i,t} = \beta_1 \#Subs\_Lockdown_{i,t} + \beta_2 HQ\_Lockdown_{i,t} + \gamma Control_{i,t} + Firm_i + Mth\_Year_t + \varepsilon_{i,t},$$

where $L_{i,t}$ is a liquidity measure for firm i in the quarter of month t, $\#Subs\_Lockdown_{i,t}$ 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 Pástor and Stambaugh (2003), 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)
#Subs_Lockdown	-0.709***	-0.839***	-1.12***	-0.490***
	(-3.09)	(-2.78)	(-4.84)	(-3.98)
	-2.24	-8.32**	-3.93	0.186
HQ_Lockdown	(-0.856)	(-2.19)	(-1.52)	(0.200)
	(1.97)	(0.706)	(1.57)	(-1.90)
ROA	-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 A12: 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 12, 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)
#Subs_Lockdown	0.017***	-0.262***	0.013***	-0.254***
	(5.45)	(-7.95)	(4.43)	(-8.44)
HQ_Lockdown	-0.040	-0.266	-0.023	-0.379
	(-1.35)	(-0.668)	(-0.811)	(-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 A13: 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 10, 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)
#Subs_Lockdown	-0.007**	-0.006**	-0.010***	-0.010***
	(-2.71)	(-2.38)	(-3.60)	(-3.58)
HQ_Lockdown	0.030	0.045	-0.099*	-0.083
	(0.847)	(1.23)	(-1.91)	(-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 A14: 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 13, 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)
#Subs_Lockdown	-0.4488*	0.0162**	-0.0251**	-0.8166*
	(-1.714)	(2.226)	(-2.284)	(-1.819)
HQ_Lockdown	0.8601	0.0505	-0.1883**	-4.596
	(0.8689)	(0.7843)	(-2.643)	(-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.