WHEN SYNCHRONY CROWDS OUT INFORMATION: BROADBAND INTERNET AND STOCK PRICE EFFICIENCY

Hosein Maleki [*] Darius Palia [†] Cayman Seagraves [‡]

Abstract

Increasing access to broadband can synchronize investor attention, raise return comovement and decrease firm-specific content of stock prices. Exploiting staggered 3G rollouts across U.S. counties as exogenous access shocks, we study consequences for price discovery. After local 3G entry, idiosyncratic volatility declines, yet price efficiency worsens—measured by higher cross-autocorrelation with lagged market returns. Search activity rises, and deterioration is strongest where attention scales —markets with high social-media and retail adoption, in major financial centers, and for meme/attention-prone "newsy" stocks; within the shortable universe it is larger for non-special (easier-to-short) names—and remains pronounced where institutional participation is greater, weighing against a purely retail channel. Patterns are consistent with coordinated attention rather than firm-specific news. The results clarify that lower idiosyncratic volatility need not signal a better information environment and caution against equating broader access with higher market quality. Technologies that centralize attention can expand participation yet erode price discovery, with implications for disclosure policy, short-sale frictions and lendable supply, platform design, and investor welfare.

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[*]Rutgers University. hmaleki@business.rutgers.edu
[†]Rutgers University. dpalia@business.rutgers.edu, *corresponding author.*
[‡]University of Tulsa. cayman-seagraves@utulsa.edu

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1 Introduction

Digital infrastructure has reshaped how markets absorb and propagate information. The rollout of third-generation (3G) mobile broadband in the United States has transformed low-bandwidth, intermittent connectivity into continuous, on-the-go access to the web, with images, and video—functionality that catalyzed the rise of social platforms and broadened participation in information production and consumption beyond traditional intermediaries. Outside asset markets, quasi-experimental evidence shows that 3G materially altered behavior and beliefs: it intensified reputational monitoring of firms and reduced misconduct, and it eroded political approval by exposing corruption and mobilizing citizens, validating 3G availability as a first-order shock to the information environment. These findings motivate an asset-pricing question: did mobile broadband improve stock price efficiency, and through what mechanisms (Enikolopov, Makarin, and Petrova, 2020; Guriev, Melnikov, and Zhuravskaya, 2021; Hirshleifer, 2020)?[1]

Competing theoretical perspectives generate sharp, testable predictions in our setting. In one view, lower access costs expand the supply of useful signals to retail traders who act as marginal information producers, facilitating the incorporation of public and quasi-public news into stock prices. In this view, efficiency should improve, and the effect on idiosyncratic volatility is ambiguous ex ante because better information can either displace noise (lowering volatility) or add firm-specific signal (raising it) (Ang, Hodrick, Xing, and Zhang, 2006, 2009; Hou and Moskowitz, 2005a; Merton, 1987; Saffi and Sigurdsson, 2011a). A contrasting view emphasizes attention and salience rather than information. If 3G primarily amplifies synchronized attention, headline chasing, and social amplification (for example, through increased use of social media platforms) without commensurate gains in signal quality, then prices can move more with market-wide narratives and short-lived sentiments, react with delay to systematic news, and comove more strongly. In this case price efficiency deteriorates, and idiosyncratic volatility may fall as trading concentrates on

[1]Early studies show that online access and simplified interfaces spur trading and turnover among individuals (Barber and Odean, 2002; Choi, Laibson, and Metrick, 2002). Subsequent evidence connects online information flows to retail participation and attention cycles, using search-based proxies and platform data (Barber, Huang, Odean, and Schwarz, 2022; Da, Engelberg, and Gao, 2011; Hirshleifer, Peng, and Wang, 2025; Hvide, Meling, Mogstad, and Vestad, 2024a; Mansi, Peng, Qi, and Shi, 2025a).

common signals: in short, prices co-move more often with less information incorporated in them (Barberis, Shleifer, and Wurgler, 2005; Chabakauri and Rytchkov, 2021; Da et al., 2011). A third perspective stresses the social process by which ideas spread. Social transmission can magnify accurate signals, but it can also propagate distortions and fads; whether 3G strengthens learning or amplifies bias is therefore an empirical question that turns on the interaction between information quality and the capacity to arbitrage mispricing (Hirshleifer, 2020).

We study the staggered rollout of third-generation (3G) mobile broadband across U.S. counties, a technological shift that transformed intermittent, stationary access into always-on, on-the-go connectivity for text, images, and video. The key premise—visible in the data and conceptually grounded in the adoption of mobile social platforms—is that 3G materially lowers the marginal cost of discovering, producing, and sharing unstructured content. 3G was the first mobile generation that enabled social media at scale, concentrating always-on attention around feeds and video (Guriev et al., 2021). Unlike studies centered on individual users of platforms or broker apps—save for registry evidence in Norway (Hvide et al., 2024a)—our infrastructure-based design captures market-wide effects that reach both retail and institutional traders, who face similar attention constraints and, at times, similar biases.

Using Google Trends at the county level, we first show that the arrival of 3G is followed by a discrete rise in search intensity for finance-relevant queries (e.g., tickers, market terms, and news keywords). The timing and persistence of these increases indicate that 3G/broadband changes how and how often people acquire information: it enables and motivates more frequent, real-time search and expands the set of sources they access relative to the pre-3G environment. Consistent with the broader literature linking online search to investor attention (Da et al., 2011), this behavioral response provides a first-stage validation that the infrastructure shock operates through information acquisition.

Our empirical design implements an interaction-weighted event-study differences-in-differences estimator that is robust to staggered adoption timing, with firm and state-year fixed effects and clustered inference. We study two outcomes that theory links to information: an efficiency metric based on conditional cross-autocorrelations between individual

stock returns and lagged market returns (capturing the speed and symmetry with which market-wide news is impounded) and idiosyncratic volatility (capturing the mix of firm-specific information and noise) (Ang et al., 2006; Hou and Moskowitz, 2005b). To assess the mechanisms, we examine heterogeneity by the tightness of short-sale constraints, geographic financial sophistication, and institutional block ownership—dimensions that theory predicts should modulate the tradability and diffusion of information (Bris, Goetzmann, and Zhu, 2007; Diamond and Verrecchia, 1987; Miller, 1977; Prado, Saffi, and Sturgess, 2016; Saffi and Sigurdsson, 2011a).

The evidence points to a striking tension. Following local 3G arrival, county search intensity rises with flat pre-trends and persistent post-adoption increases, which confirms a genuine escalation in information acquisition rather than a one-time burst of attention (Da et al., 2011). Further, in response to 3G availability, idiosyncratic volatility declines (Ang et al., 2006, 2009). Yet, in the same window, the cross-autocorrelation-based efficiency metric worsens: individual stocks load more on lagged market moves, indicating slower, more delayed incorporation of systematic news into prices (Hou and Moskowitz, 2005a). The joint pattern—higher cross-autocorrelation (meaning lower price efficiency) and lower idiosyncratic volatility—rules out the simple informed-trading hypothesis in which broader access improves efficiency, and it is difficult to reconcile with the standard "more noise" story that would typically raise, not reduce, idiosyncratic volatility. Instead, the results are consistent with a synchronized-attention mechanism: mobile broadband concentrates investor focus on common, high-salience narratives circulating on social media and news feeds, compressing firm-specific variation while delaying the translation of market-wide information into the cross-section. Event-study dynamics show clean parallel trends, an onset around adoption, and persistence thereafter, pointing to a structural change in the information environment rather than a transitory cycle.

Further tests sharpens this interpretation. Where short-sale constraints are looser (non-special stocks), the rise in cross-autocorrelation is actually larger, and the accompanying decline in idiosyncratic volatility is more pronounced. This surprising pattern indicates that even when securities are easier to borrow and arbitrage should, in principle, mitigate attention-driven drift, the effect of synchronized attention is not only present but

amplified. One interpretation is that precisely in these more liquid and less constrained settings, the scale of trading tied to the common narratives becomes greater, so that arbitrage capacity is overwhelmed by the volume of attention-driven flows. By contrast, where borrowing frictions are tighter and shorting is more difficult (special stocks), the deterioration in efficiency is smaller, perhaps because constrained arbitrageurs avoid these names altogether, limiting the extent to which attention-driven trading feeds into measurable comovement (Bris et al., 2007; Saffi and Sigurdsson, 2011a).

Geography reinforces this view: in major financial centers, where information circulates quickly and market participation is deeper, shared narratives are magnified across many securities, further concentrating trading on common signals (Hou and Moskowitz, 2005b; Pirinsky and Wang, 2006). Taken together, these findings suggest that synchronized attention can dominate even in markets with fewer formal frictions, producing more correlated price movements precisely where trading is most active.

We also find that the pattern is stronger for firms with high institutional presence: using 13F blockholder filings, we classify firms with more than sample median block ownership as institutionally dominated, and for these stocks the efficiency decline is larger. This weighs against a "retail-only" explanation. Institutions are themselves wired into the same information firehose that 3G amplified—benchmark-linked dashboards, vendor alerts, and social/news streams—and their mandates (benchmarking, risk limits, and passive replication) concentrate flows on common factors. Passive blocks also add price-insensitive demand that can amplify factor comovement, while active institutions often react to the same headline set (Barberis et al., 2005; Chabakauri and Rytchkov, 2021). Although blockholders supply safer lendable inventory that lowers dynamic lending risk, in our data that liquidity is not sufficient to neutralize coordinated attention; instead, institutional presence magnifies the shift of order flow toward market-wide stories, reinforcing the measured deterioration in efficiency (Palia and Sokolinski, 2024; Prado et al., 2016). Consistent with this attention-based view, high-attention "meme" stocks exhibit even larger declines in efficiency.[2]

[2]We do not run the "opposite" exercise of isolating retail order flow from TAQ using the Boehmer, Jones, Zhang, and Zhang (2021) subpenny-signing approach because subsequent experimental validation shows that, in contemporary market structure, the BJZZ method both identifies only a subset of retail trades and mis-signs a nontrivial share of those it does identify, especially when spreads exceed one

The results also square with what is known about how 3G changes the mix of arriving signals. In non-financial settings, 3G exposure intensifies reputational monitoring and brings adverse news to light. If mobile broadband raises the salience of such content while channeling attention toward a narrower set of common narratives, then even as investors learns more, trading can become more macro-narrative-driven and less idiosyncratic, producing lower firm-specific volatility but slower diffusion of systematic information into the cross-section. That is precisely the configuration we observe. Moreover, within a social-finance perspective, our findings illustrate that the net market impact of social connectivity depends not only on the quantity of accessible information but also on how attention is allocated and on institutional capacity to arbitrage transmission-driven distortions (Guriev et al., 2021; Hirshleifer, 2020).

Our findings relate to, yet is distinct from, the investor-level literature. Studies of individuals demonstrate that easier online access and platform design shape portfolio choices, trading intensity, and reaction to salient content Barber et al., 2022; Choi et al., 2002, with newer work documenting the amplification of retail participation through social media and broker apps (Barber et al., 2022; Hirshleifer et al., 2025; Hvide et al., 2024a; Mansi et al., 2025a). By contrast, we treat 3G as a market-level infrastructure shock and study its consequences for price discovery and volatility. In doing so, we capture effects on both retail investors and institutions exposed to the same information environment. Our tests confirm this broader scope: the deterioration in price efficiency is larger in counties with greater retail adoption and social-media penetration and is pronounced for "newsy," hard-to-short firms. Notably, the decline is amplified among firms with high institutional presence (e.g., block ownership greater than sample median), indicating that institutions, like retail traders, are drawn into synchronized information flows once mobile access lowers the cost of consuming and propagating narratives.

Two broader implications follow. First, lower idiosyncratic volatility is not synonymous

cent. Barber, Huang, Jorion, Odean, and Schwarz (2024) execute ∼85,000 retail trades and report ∼35% identification, a ∼28% signing error using subpenny digits, and uninformative imbalances for ∼30% of stocks; they recommend quote-midpoint signing instead. Given these limitations and the joint-testing problems they create for retail-imbalance inference, we rely on block ownership > sample median as our proxy for institutional trading intensity and interpret the stronger deterioration in efficiency for these stocks as evidence that institutions—rather than only retail—amplify synchronized attention in the 3G era (Barber, Huang, Jorion, Odean, and Schwarz, 2024; Boehmer et al., 2021).

with better information environments. In our setting, the fall in idiosyncratic volatility accompanies a measurable decline in price efficiency as defined by the speed with which market-wide news is impounded into individual prices. Second, digital access is market infrastructure whose effects depend critically on complementary trading institutions. The same rollout that increased information acquisition also worsened a standard efficiency metric; in cross-section, deterioration is largest where shorting is more constrained and among stocks with Block Ownership of greater than sample median, indicating that arbitrage capacity does not fully counteract synchronized attention and, in some segments, participates in it (Diamond and Verrecchia, 1987; Palia and Sokolinski, 2024; Saffi and Sigurdsson, 2011a).

We contribute to the literature in four ways. First, we link a foundational change in digital access to the core question of price discovery. When 3G expands, it coordinates investor attention on common narratives, raising return comovement while thinning firm-specific information in prices—an implication that is consistent with attention-based asset pricing, category learning, and information-acquisition trade-offs (Barberis et al., 2005; Hirshleifer, 2020). Second, exploiting plausibly exogenous cross-market and over-time variation in local 3G rollout, we provide *causal* evidence that prices become less informative even as idiosyncratic volatility falls. Event-study dynamics and standard market-quality tests show stronger synchronicity, weaker incorporation of firm-specific cash-flow news, and greater reliance on common factors, establishing that access-driven coordination can degrade price discovery despite improved connectivity.

Third, we clarify what idiosyncratic volatility measures. A decline in firm-level volatility need not signal a better information environment; it can reflect attention shifting from private, firm-specific signals toward shared stories, complementing evidence that higher synchronicity coincides with less firm-level information (Morck, Yeung, and Yu, 2000). Finally, we connect market-quality research to work showing that 3G reshaped governance, corporate behavior, and political beliefs, providing a unified view of digital access as a coordinator of attention across domains (Guriev et al., 2021). Taken together, our results caution against equating broader access with better market quality and carry implications for retail intermediation in the AI and big-data era, where technologies that

synchronize attention can raise correlation while diluting discovery (Da et al., 2011).

2 Related Literature

Our study connects three strands of research: how internet access reshapes individual trading, how online attention propagates through markets, and how infrastructure shocks alter information environments.

Individual investors and the internet. Early work establishes that reducing online frictions changes household portfolios and trading intensity. Using brokerage data, Barber and Odean (2002) show that online access increases turnover and worsens performance through overconfidence and rapid rebalancing. In a complementary experiment, Choi et al. (2002) demonstrate that simplified online interfaces and default choices materially affect investment behavior, highlighting the sensitivity of retail decisions to platform design. Subsequent papers move beyond the mere presence of the internet to the *modes* through which individuals consume information and trade: Da et al. (2011) validate Google search volume as an attention proxy tightly linked to retail interest; Barber et al. (2022) exploit Robinhood data to show that salient events and app design induce coordinated retail buying; Hirshleifer et al. (2025) emphasize social transmission and cognitive frictions that bias how individuals attend to and act on information; Hvide, Meling, Mogstad, and Vestad (2024b) leverage Norwegian account-level records to trace how individuals respond to social narratives in a setting with detailed identities; and Mansi, Peng, Qi, and Shi (2025b) examine how waves of online participation shape cross-sectional trading and predictability. Together, this literature shows that the internet amplifies retail trading, channels attention toward salient content, and synchronizes investor behavior—yet it largely stops at *investor-level outcomes*.

From attention to market-wide consequences. A second strand uses online traces to infer attention and study price formation. By grounding attention in Google Trends, Da et al. (2011) open a path for measuring how investors *discover* assets and stories. Subsequent work on broker apps and social media —e.g., Barber et al. (2022)– documents

that bursts of online engagement coincide with crowded trades and short-horizon return patterns consistent with attention shocks, rather than with the swift aggregation of dispersed fundamentals. Hirshleifer et al. (2025) formalize why socially transmitted narratives can yield correlated beliefs and actions, distinct from classic salience: diffusion frictions can delay or distort how common information is impounded into prices. The common methodological feature across these papers is reliance on user- or platform-level exposure as the margin of variation. What remains underexplored is whether a broad infrastructure improvement that lowers access costs across the board—affecting both content consumption and production—moves markets toward or away from efficiency.

Infrastructure as an information shock. Our setting—staggered county-level entry of third-generation (3G) mobile broadband—treats infrastructure as the causal lever. Conceptually, 3G transitions users from intermittent, stationary connections to continuous, on-the-go access to text, images, and video; as argued in a broader political-economy context, 3G is the first mobile generation that enabled social media at scale (Guriev et al., 2021). This matters for finance because it transforms the *cost* and *timing* of exposure to unstructured information and increases the frequency with which investors can search, scroll, and share. Consistent with that mechanism, our first-stage evidence shows that local Google search intensity rises after 3G arrives, particularly for finance-relevant queries, echoing the attention interpretation in Da et al. (2011). Unlike prior papers that identify off platform adoption or user cohorts (e.g., Barber et al., 2022; Barber and Odean, 2002; Choi et al., 2002; Hvide et al., 2024b), we leverage plausibly exogenous variation in infrastructure timing to study market-level outcomes —price efficiency and idiosyncratic volatility— across the cross-section of stocks.

The literature implies three ex-ante predictions for a mobile-broadband shock. First, the informed-trading view predicts faster incorporation of fundamentals as information acquisition becomes cheaper and more frequent; retail investors should react more quickly to cash-flow news, and institutions should arbitrage residual predictability. Second, the salience/attention view predicts trading concentrated on vivid, widely discussed signals—tickers trending on search or social feeds—without necessarily improving the cross-sectional diffusion of systematic news; retail coordination documented on platforms would

extend to mobile-enabled contexts. Third, the social-transmission view predicts correlated beliefs and actions as narratives diffuse through networks, potentially slowing the impounding of common information if diffusion frictions, echo chambers, or intermittent arbitrage limit cross-stock propagation (Hirshleifer, Welch, Levi, and Teoh, 2023). The investor-level findings cited above establish these channels at the micro margin; what is unknown is which channel dominates when the information environment itself is reconfigured by infrastructure.

Retail versus institutions under ubiquitous connectivity. Prior studies emphasize retail because user-level data are abundant on brokerage apps and social media (Barber et al., 2022), while registry-based settings such as Norway provide rich micro evidence (Hvide et al., 2024b). Yet institutions share the same ambient information environment once access becomes mobile and continuous. If institutions consume similar feeds and face human attention constraints, synchronized narratives could shape their trades as well—either by crowding their screens with market-wide themes or by reallocating attention away from stock-specific signals. Our cross-sectional tests are designed with this insight in mind, examining whether institutional presence magnifies or dampens the effects identified in retail-heavy environments, thereby extending the scope of the prior literature beyond individual accounts to the joint retail–institutional equilibrium.

We contribute in three ways relative to these literatures. *First*, we move from user-level exposure to an *infrastructure shock*, exploiting the staggered, plausibly exogenous rollout of 3G across U.S. counties to identify causal effects on market-level outcomes. *Second*, we link *Google Trends* responses directly to market quality, showing that mobile broadband raises local search intensity—evidence of increased information acquisition—yet leads to *worse* cross-sectional price efficiency and *lower* idiosyncratic volatility. This joint pattern is difficult to reconcile with pure informed trading but accords with attention and social-transmission mechanisms (Da et al., 2011; Hirshleifer et al., 2025). *Third*, we document that effects are stronger where retail adoption and social-media penetration are high and that *institutional presence amplifies* the decline in efficiency, indicating that synchronized attention is not confined to retail investors (Hvide et al., 2024b; Mansi et al., 2025a). By recasting 3G as an infrastructure-driven coordination shock, we provide new evidence that

more access can—counterintuitively—*decrease* efficiency even as it *increases* information acquisition.

3 Data and Methodology

We construct a firm–county–year level panel to study how the staggered rollout of third-generation (3G) mobile broadband shapes public attention and, in turn, market outcomes. The empirical setting exploits cross-county, cross-time variation in 3G availability as a plausibly exogenous shift in information access. We integrate geospatial coverage maps with measures of firm-level visibility derived from online search and social media, align these with standard CRSP/Compustat variables, and merge county demographics for conditioning and heterogeneity. Below, we detail the data sources and the procedures used to construct them,

3G coverage and treatment variables. We measure the timing and intensity of broadband access using Collins Bartholomew's *Mobile Coverage Explorer*, which reports annual GIS polygons for 3G footprints from 1999–2018. We intersect coverage shapes with U.S. county boundaries (TIGER/Line) in QGIS/OGR and construct two county–year exposure measures: (i) $Any3G_{ct}$, an indicator equal to one if any portion of county c is covered in year t; and (ii) $Share3G_{ct}$, the fraction of county land area covered, our continuous treatment. Following the literature that treats mobile broadband rollouts as plausibly orthogonal to short-run local firm dynamics (e.g., Guriev et al., 2021), we use this staggered adoption as the core source of identifying variation.

Linking treatment to firms. We assign firms to counties by headquarters (HQ) location in year t and map county exposure to firm-year observations. The baseline relies on the HQ county FIPS; in robustness we consider (i) MSA-level exposure, (ii) alternative footprints for multi-establishment firms when available, and (iii) the exclusion of firm-years around HQ moves. This construction yields a firm–year panel in which treatment varies with local broadband availability over time.

Public attention (Google Trends). To proxy for firm-specific public attention, we retrieve monthly search interest from Google Trends for firm names and common aliases from January 2004 to August 2024. We use topic codes and suggested terms to seed queries and apply an AI-assisted screen using OpenAI's GPT models to filter ambiguous strings (e.g., homonyms or entertainment references), retaining only industry-relevant variants. We average the monthly indices to the calendar year to align with our treatment period. The resulting measure, $Attention_{it}$, captures the intensity of information-seeking about firm i in year t across all search categories.[3] Figure 4 illustrates the behavioral mechanism through which 3G broadband influences investor attention and information acquisition. Panel A displays the temporal evolution of Google Trends search intensity for finance-relevant queries, showing a discrete upward shift coinciding with local 3G rollout that persists through our sample period. Panel B presents the cross-sectional distribution of search intensity, revealing substantial heterogeneity across firms that our empirical models account for. Together, these patterns provide direct evidence of increased information-seeking behavior following mobile broadband availability, validating the attention-based mechanism underlying our price efficiency findings.

Financials, identifiers, and county covariates. We use the merged CRSP/Compustat to obtain returns and characteristics (size, book-to-market, past returns, industry codes) and compute the price-based outcomes used in later sections. County-level demographics and economic conditions (population, income, education, urbanity) are aligned to each county–year to control for local trends correlated with technology adoption and to examine heterogeneity. Specifications with geography-by-time fixed effects absorb broad local shocks; remaining controls serve primarily to improve precision.

To summarize, four features of the panel are central. *Timing:* we track the first year any 3G footprint reaches a county and subsequent growth in $Share3G_{ct}$, constructing leads/lags for dynamic estimates and using shares for dose–response. *Aggregation:* higher-frequency attention/sentiment series are aggregated to annual measures for the baseline; we use higher frequency in event windows around first coverage in extensions. *Linking:*

[3] See Figure 4 and the methodology documentation for detailed procedures on data collection, cleaning, and quality thresholds.

the HQ-based assignment is simple and transparent; alternative mappings yield similar results.

Measurement and validation. Two measurement concerns deserve attention. The first is *name ambiguity*: Google Trends data sometimes capture unrelated entities that share the same name as a firm. To address this, we apply an AI-assisted screen using OpenAI's GPT models to filter ambiguous strings (e.g., homonyms or entertainment references), retaining only industry-relevant variants. Results are robust when we drop firms with persistent ambiguity or restrict attention to Google's topic-coded series. The second concern is *data quality*: Google Trends series may exhibit zero-inflation and temporal instability. We address this by documenting coverage, zero-inflation, and stability diagnostics in Appendix A and drop series that fail pre-specified quality thresholds. Robustness checks confirm that our results are similar when we (i) use alternative transformations like $\ln(GTrends + 1)$ or $GTrends_Rep0$ (replacing zeros with missing), (ii) restrict to firms with complete series, and (iii) use different aggregation periods. Along with placebo tests on pre-period trends and stacked estimators for staggered timing, these exercises suggest that measurement error is unlikely to drive our main findings.

3.1 3G Coverage Data

Our principal source for third-generation (3G) availability is Collins Bartholomew's Mobile Coverage Explorer (MCE), which reports annual 3G footprints from 2007–2017 and is standard in prior work (e.g., Guriev et al., 2021; Heese and Pacelli, 2024).[4] Because U.S. deployment began earlier—Heese and Pacelli (2024) documents initial rollouts in the early 2000s—we extend coverage backward using Collins Bartholomew legacy layers (available from 1999). For counties that appear covered in 2007, we additionally hand-collect pre-2007 launch evidence (carrier announcements, trade publications, archived press releases) and, when corroborated, advance the county's first-coverage year.

We transform the maps into a county–year panel via GIS. For each year t, we intersect the national 3G polygon S_t with county boundaries C_c and compute the share of land

[4]Collins Bartholomew is a long-standing cartographic publisher—also behind *The Times World Atlas*—with nearly two centuries in the industry.

area within the 3G footprint,

$$\text{Coverage}_{ct} \;=\; 100 \times \frac{\text{Area}(S_t \cap C_c)}{\text{Area}(C_c)}\,.$$

This yields a continuous exposure measure that respects within-county heterogeneity, rather than switching a county "on" when any sliver is served.

Figure 1 displays the expansion of 3G coverage across U.S. counties between 2006 and 2018, shown in three-year intervals. The maps highlight the staggered nature of the rollout, with no clear concentration along any specific corridor (e.g., East Coast versus West Coast). The snapshots also reveal three major expansion waves: an early phase around 2008 (preceding panel (a)), a sharp nationwide surge in 2011 (before the widespread growth depicted in panel (c)), and a final broadening in 2014 (just prior to the 2015 coverage illustrated in panel (d)).

Table 2 reports summary statistics for the main variables. The average county in our sample has 3G coverage of 0.604, implying that about 60% of county land area is covered by 3G on average. Firm-level characteristics are broadly comparable to the overall Compustat universe: mean assets are $6.6 billion, average leverage is 20%, and the mean Z-score is -0.02. The lower panel presents county-level demographics, including (log) population, the share of white residents, the unemployment rate, and a diversity index.

4 Empirical Results

Our identification exploits the staggered arrival of third-generation (3G) mobile broadband across U.S. counties as a plausibly exogenous reduction in information-access costs. Because rollout timing is governed by engineering, spectrum, and terrain constraints rather than local firm fundamentals at annual horizons, this variation is well-suited to difference-in-differences and cohort-robust event-study designs in the spirit of Sun and Abraham (2021). To validate the mechanism that 3G reshapes *information acquisition*, we first examine a behavioral "first stage" using county-level Google Trends. Building on the interpretation of search intensity as an investor-attention proxy in Da et al. (2011), we test whether local coverage increases the frequency and breadth of finance-relevant

search in treated counties.

Table 3 reports results from our baseline specification, which relates the county-year mean of Google Trends index scores (aggregated across all categories for firms headquartered in the county) to an indicator for 3G coverage:

$$Ln\Big(MeanCntyGTrendsAllCats_{i,t}+1\Big) = \delta_1\, 3\mathrm{G}_{i,t} \;+\; \gamma^{\top} X_{i,t} \;+\; \alpha_i \;+\; \alpha_t \;+\; \varepsilon_{i,t}. \quad (1)$$

where i, j, s, and t denote county, firm, state, and year, respectively. $3\mathrm{G}_{i,t}$ equals one if county i has any 3G coverage in year t; $X_{i,t}$ includes time-varying county controls; α_i and α_t denote county and year fixed effects, respectively; and errors are clustered at the county level. Column (1) presents the baseline with no controls beyond fixed effects; Column (2) adds county demographics; and Column (3) replaces year fixed effects with state-by-year fixed effects, $\alpha_{s,t}$, to absorb state-specific time shocks.

Because staggered adoption can bias two-way fixed-effects estimates when treatment effects vary over event time, we complement equation (1) with an interaction-weighted event-study that traces dynamic responses around first coverage following Sun and Abraham (2021). In Table 3 we report leads and lags relative to the first year of local 3G availability, which allows a direct assessment of (i) pre-trends and (ii) the persistence of post-adoption effects. Two features of the estimates support a causal interpretation. First, the pre-treatment coefficients (e.g., $k = -3, -2$) are economically small and statistically indistinguishable from zero, consistent with parallel trends. Second, at and after adoption, the coefficients turn positive and remain elevated across specifications, indicating a discrete and persistent rise in search intensity once mobile broadband becomes available locally.

Across all columns, the effect of 3G availability on company-specific Google search trends manifests in year 0, and persists large and significantly for up to at least four years after the beginning of the exposure. The Post–pre differences are positive and significant (≈ 0.34 to 0.50), with steadily increasing post-event coefficients. These patterns depict the behavioral changes implied by the mechanism articulated in the introduction: the arrival of 3G enables and motivates residents/investors to search more frequently and to access newer online sources than in the pre-3G environment, thereby increasing the measured

intensity of finance-relevant queries at the county level. In the context of Da et al. (2011), the evidence is consistent with an infrastructure-driven increase in attention. Establishing this first stage is essential because it links the infrastructure shock to investor behavior for both retail and institutional participants exposed to the same information environment. Methodologically, it justifies treating 3G timing as a shock to the information environment that precedes the stock-level outcomes we study next. With the first stage in place, the remainder of the section investigates whether the same coverage that raises search improves price discovery or, instead, coordinates attention on common narratives that slow the cross-sectional impounding of market-wide information.

As a robustness, Table 4 re-estimates the Google-Trends specification using a Poisson pseudo-maximum-likelihood (PPML) estimator with high-dimensional fixed effects, following the guidance in Cohn, Liu, and Wardlaw (2022) and the econometric rationale of Silva and Tenreyro (2006). PPML is well-suited to our outcome for three reasons emphasized by this literature: (i) it is consistent under general forms of heteroskedasticity, which are prevalent in right-skewed, "count-like" outcomes; (ii) it naturally accommodates zeros without ad hoc transformations (contrast with log-specified OLS that must drop or shift zero observations); and (iii) it requires only correct specification of the conditional mean. Conceptually, the PPML design complements our log specification in Table 3 by preserving the level scale of the Google-Trends index while controlling for the same multi-way fixed effects.

Formally, we estimate

$$\mathbb{E}\Big[\text{Mean Cnty GTrends AllCats}_{i,t} \,\Big|\, X_{i,t}, \alpha_i, \alpha_{st}\Big] = \exp\Big(\beta\, 3\text{G}_{i,t} + \gamma^\top X_{i,t} + \alpha_i + \alpha_{st}\Big), \quad (2)$$

where $3\text{G}_{i,t}$ indicates any 3G coverage in county i during year t, $X_{i,t}$ includes the same demographic controls as in Table 3, α_i are county fixed effects, and α_{st} are state-by-year fixed effects (we also report a version with year fixed effects). Relative to equation (1), the PPML specification avoids log-transforming the dependent variable and thereby retains all county-year observations with zero or near-zero search intensity, a common feature of place-level Google-Trends series. This design follows the broader finance literature's recommendation to analyze count and count-like variables with PPML-FE rather than

log-OLS with constants added to accommodate zeros (Cohn et al., 2022). We continue to interpret Google search intensity as an attention proxy in the spirit of Da et al. (2011).

The estimates in Table 4 show that the 3G coefficient is positive and statistically significant across specifications (point estimates from 0.064^{*} to 0.086^{***}). Interpreted as semi-elasticities in the PPML model, these magnitudes imply economically meaningful increases in county-level finance-related search following 3G availability—approximately 6.6% to 9.0% increases, computed as $\exp(\hat{\beta}) - 1$.

4.1 Firm-Level Idiosyncratic Volatility

We next examine how mobile broadband affects the *firm-level* information environment by studying idiosyncratic return volatility. Building on the behavioral first stage, in which 3G increases local search intensity (Tables 3–4), we ask whether greater connectivity is associated with changes in the firms' stock return's idiosyncratic volatility in year t, $IVOL_{i,t}$, computed from daily returns as described in Section 3. The event-study design, based on Sun and Abraham (2021), is as follows:

$$IVOL_{i,t} = \sum_{k=-4}^{+4} \delta_k \cdot 3\mathrm{G}_{i,t+k} + \gamma^{\top} X_{i,j,t} + \alpha_i + \alpha_{st} + \varepsilon_{i,t}, \tag{3}$$

where $3\mathrm{G}_{i,t}$ is an indicator for any 3G coverage in i's headquarter county in year t, $X_{i,j,t}$ is a vector of time-varying firm and county-level controls, α_j are firm fixed effects, and α_{st} are state-by-year fixed effects. Standard errors are clustered at the county level. Coefficients δ_k capture the path of $IVOL$ from four years before to four years after adoption; the omitted category is the pre-adoption reference year.[5]

Table 5 reports three specifications with increasing saturation of controls. Across all columns, the pre-trend coefficients ($k \leq -2$) are economically small and statistically indistinguishable from zero, supporting parallel trends. Beginning at the year of adoption ($k = 0$), several post-event coefficients are negative, and the post-minus-pre contrast is negative and statistically significant (approximately -0.004^{**}). The magnitude implies a

[5]Column (2) augments $X_{i,t}$ with *lagged* idiosyncratic volatility $IVOL_{i,t-1}$ to absorb persistence in volatility; Column (3) retains $IVOL_{i,t-1}$ and replaces year fixed effects with state-by-year fixed effects α_{st}.

decline of roughly 0.4 percentage points in annualized idiosyncratic volatility relative to the pre-period benchmark.

Two features of the estimates are noteworthy. First, the negative shift in $IVOL$ is robust to the inclusion of its lagged value, $IVOL_{i,t-1}$, indicating that the results are not an artifact of volatility mean reversion. Second, the pattern is stable when replacing year fixed effects with state-by-year fixed effects, which absorb state-specific shocks to information environments or regulation. Together, these findings indicate that local 3G availability is followed by a persistent reduction in firm-specific return variation.

4.2 Price Efficiency

Having shown that idiosyncratic volatility declines after local 3G arrival (Table 5), we next ask whether price efficiency improves as well. Following the short-sale constraints literature, we measure efficiency using conditional cross-autocorrelations between market and individual stock returns. Specifically, we construct an annual firm-level measure based on the difference between downside and upside cross-autocorrelations of market returns with next-period stock returns, a proxy for the speed and symmetry with which common information is impounded into prices in the spirit of Hou and Moskowitz (2005a) and as implemented by Bris et al. (2007) and Saffi and Sigurdsson (2011a), consistent with the predictions of Diamond and Verrecchia (1987). Larger values of this difference indicate that negative market-wide news propagates more slowly than positive news—i.e., worse price efficiency.

We estimate dynamic treatment effects using the cohort-robust event-study of Sun and Abraham (2021):

$$\mathrm{XAC}^{\pm}_{i,t} = \sum_{k=-4}^{+4} \delta_k \cdot 3\mathrm{G}_{i,t+k} \; + \; \gamma^{\top} X_{i,t} \; + \; \alpha_i \; + \; \alpha_{st} \; + \; \varepsilon_{i,t}, \tag{4}$$

where $\mathrm{XAC}^{\pm}_{i,t}$ is the firm-year cross-autocorrelation measure (downside minus upside; higher values = lower efficiency) computed from daily returns within year t; $3\mathrm{G}_{i,t}$ equals one if the firm's headquarter county has any 3G coverage in year t; $X_{i,t}$ includes the same time-varying controls as in Table 5; α_i and α_{st} denote firm and state-by-year fixed effects;

and we cluster standard errors at the county level to match the treatment's variation. The omitted event-time bin is the pre-adoption reference year.

Table 6 reports three specifications with increasing saturation of controls. Pre-trend coefficients ($k \leq -2$) are small and statistically indistinguishable from zero, supporting parallel trends. Beginning in the adoption year ($k = 0$) and strengthening in later post years, the event-time coefficients turn *positive*, implying higher downside-minus-upside cross-autocorrelation after 3G arrives. The post-minus-pre contrast is positive and statistically significant (approximately 0.10^{**}), with larger effects in the later post windows, and remains stable as we add controls and replace year fixed effects with state-by-year fixed effects.

Because higher values of the cross-autocorrelation difference indicate *slower* incorporation of common (especially negative) information into individual stock prices (Bris et al., 2007; Hou and Moskowitz, 2005a; Saffi and Sigurdsson, 2011a), the positive post-pre shift in Table 6 signals a deterioration in price efficiency following 3G adoption—consistent with theories of asymmetric adjustment under constraints (Diamond and Verrecchia, 1987). Taken together with the decline in idiosyncratic volatility in Table 5, these results align with a *synchronized-attention* channel: mobile broadband increases information acquisition (Tables 3–4) and compresses firm-specific variation, yet coordinates attention on shared narratives so that market-wide news diffuses more slowly across the cross-section.

Discussion of the Results: The deterioration in the cross-autocorrelation efficiency measure is consistent with a reallocation of scarce attention toward *shared*, high-salience stories that 3G makes continuously visible. Mobile access lowers the marginal cost of checking feeds and push alerts, directing both retail and institutional screens toward big headlines (macro releases, mega-cap earnings, sector shocks) and episodic "meme" narratives that coordinate order flow across many investors (see, inter alia, Barber et al., 2022; Da et al., 2011; Hirshleifer et al., 2025). This mechanism dovetails with the media-economics evidence that traditional outlets shape behavior through editorial pipelines and broadcast constraints (see the surveys in DellaVigna and Gentzkow, 2010; DellaVigna and La Ferrara, 2010; Enikolopov and Petrova, 2015), but the Internet—and especially social

media—operate differently: as synthesized by Zhuravskaya, Petrova, and Enikolopov (2020), two distinguishing features are *low entry barriers* and *user-generated content*. Low entry barriers allow previously excluded voices to broadcast and amplify grievances; user-generated content, combined with effortless copying and reposting, accelerates diffusion and raises reputational stakes for firms (e.g., Diermeier, 2011; Sifry, 2011). In practice, this environment can rapidly mobilize attention around corporate controversies or "meme-stock" episodes (e.g., GameStop, AMC), concentrating trades on shared narratives rather than idiosyncratic signals. Such synchronized attention naturally compresses firm-specific variation (the $IVOL$ decline in Table 5) while slowing the cross-sectional impounding of adverse common news—especially when short-sale frictions impede downside adjustment—yielding the post-3G increase in our downside-minus-upside cross-autocorrelation measure (Bris et al., 2007; Diamond and Verrecchia, 1987; Hou and Moskowitz, 2005b; Saffi and Sigurdsson, 2011a).

5 Mechanisms

5.1 Heterogeneity by Short-Sale Constraints: Special vs. Non-Special Firms

Table 7 examines whether the 3G effect on price efficiency varies with short-selling frictions. We split firms annually into *special* (top 20% of borrowing fees; high shorting constraints) and *non-special* (bottom 80%; lower constraints) and estimate cohort-robust event-study DiD models over 2007–2017:

$$\mathrm{XAC}^{\pm}_{i,t} \;=\; \sum_{k=-4}^{+4} \delta_k \cdot 3\mathrm{G}_{i,t+k} \;+\; \gamma^{\top} X_{i,t} \;+\; \alpha_i \;+\; \alpha_{st} \;+\; \varepsilon_{i,t}, \tag{5}$$

where $XAC^{\pm}_{i,t}$ is the (downside–upside) cross-autocorrelation measure of efficiency (higher values = *lower* efficiency). We report *average* post-minus-pre effects (ATEs) for parsimony. All columns include firm controls (size, R&D intensity, age, leverage, sales growth, advertising), firm fixed effects, and state-by-year fixed effects; standard errors are two-way clustered (county, firm).

We find that for **non-special** stocks (Cols. 4—6), the post-minus-pre effect of 3G

on the efficiency measure is *positive* and statistically significant (coefficient estimates are similar across columns (0.027) with the lowest t-statistics being 2.30 (in Col 2)), with the largest effects concentrated in +1 to +4 event years (unreported). By construction, this indicates a *deterioration* in price efficiency after local 3G arrival. By contrast, **special** stocks (Cols. 1–3) display muted and generally insignificant post-pre shifts.

Table 8 complements the split-sample analysis in Table 7 by estimating a single event-study with interactions that allow the 3G effect on price efficiency to differ for high short-selling–constraint firms. Consistent with Table 7, the baseline 3G effect is positive and significant, indicating a deterioration in the cross-autocorrelation efficiency measure among non-specials. The interaction term is small (often statistically weak) and negative, implying that the *net* 3G effect for specials (baseline + interaction) is attenuated and, in various specifications, economically close to zero. Read alongside Table 7, these interaction results reinforce the pattern that 3G-driven, shared attention worsens efficiency where shorting frictions are lower (non-specials), but adds little incremental drag where prices are already slow to adjust due to binding borrowing constraints (specials).

These findings align with the concerted attention mechanism discussed before. Where short-sale frictions are *low* (non-specials), 3G's always-on access can reallocate attention toward shared narratives —big headlines and "meme" themes—raising the downside–upside cross-autocorrelation and thus lowering efficiency, even though idiosyncratic volatility falls (Table 5). When arbitrage is feasible, synchronized attention —not binding short-sale limits— emerges as the dominant friction that slows the cross-sectional impounding of common (especially negative) news. Where frictions are high (hence, *specials*), baseline constraints already impede rapid downside adjustment; the incremental attention shock from 3G adds relatively little on top of existing limits, producing attenuated average effects.

Put simply, when short selling is already hard (*specials*), prices tend to be slow to adjust downward even before 3G. Adding 3G —which pushes more investors to watch the same stories— does not slow them much further, so the average effect is small. Where short selling is easier (*non-specials*), the same 3G-driven, shared attention steers trading toward headline narratives and away from stock-specific signals, so efficiency deteriorates

more. This pattern supports the *concerted-attention* view and motivates our next tests in especially "newsy" and meme-prone settings, where coordination on shared stories should be strongest.

5.2 Institutional Ownership Heterogeneity

Table 9 evaluates whether institutional presence amplifies the 3G effect on price efficiency using an interaction between 3G exposure and an indicator for *High Block Ownership* (blockholders > sample median of 30.6% in the prior year). In this event-study with interactions (Sun and Abraham 2021), the post–pre average 3G effect for *low-block* firms is economically small and statistically indistinguishable from zero, whereas the incremental effect for *high-block* firms is *positive and significant* at the event year and at +1, with modest persistence thereafter. These results imply that 3G-induced deterioration in efficiency is concentrated where institutional block ownership is higher. This pattern is consistent with the mechanism that mobile connectivity coordinates attention on common narratives; institutions, facing the same real-time feeds and bandwidth limits as retail participants, contribute to synchronized trading rather than offsetting it.

Table 10 sharpens this result by restricting to the top and bottom quartiles of block ownership (measured in $t-1$) and estimating a single interacted event-study. The baseline 3G effect captures the bottom-quartile (low-block) response; the interaction term measures the *difference* for the top-quartile (high-block) firms. In the most saturated specification (Col. 3), the differential post–pre effect for the top quartile is 0.025^{***} ($t = 3.75$), indicating a materially larger increase in the inefficiency measure relative to the bottom quartile. Taken together with (i) the first-stage rise in local search (Tables 3–4), (ii) the decline in idiosyncratic volatility (Table 5), and (iii) the stronger efficiency deterioration among non-special (easier-to-short) stocks (Tables 7–8), the ownership heterogeneity suggests that institutional participation *amplifies* the 3G-driven shift toward market-wide, high-salience stories. Rather than uniformly improving information aggregation, greater connectivity appears to coordinate attention—across retail and institutional investors alike—thereby worsening cross-autocorrelation-based efficiency precisely where institutional presence is heavier.

5.3 Attention Heterogeneity: High "Meme" Exposure

Table 11 parallels the prior interaction designs by allowing the 3G effect on price efficiency to differ for firms with *high* sustained retail-attention exposure—measured as having an above-median share of "meme months" (based on Google Trends deviations) in t–1. In the interacted event-study, the post–pre average of $\sum_k \delta_k$ captures the baseline 3G effect for *lower* meme exposure, while $\sum_k \gamma_k$ traces the *incremental* effect for *high* meme firms. Two patterns emerge. First, the baseline (lower-meme) group shows small, positive, and generally modest post–pre shifts in the downside–upside cross-autocorrelation measure, indicating mild efficiency deterioration consistent with our aggregate results (Table 6). Second, for high-meme firms, the differential effect is limited near the event but turns *negative* at medium horizons (about +2 to +3 years), implying that the initial coordination on salient stories is *transient*: as meme attention fades and arbitrage capital re-enters, the cross-autocorrelation measure improves relative to the lower-meme baseline.

Combined with Tables 7–10, these results suggest that 3G-enabled attention shocks operate along two margins. Episodic, meme-like bursts concentrate order flow quickly but tend to wash out over medium horizons, producing a relative improvement for high-meme firms once the spike subsides. By contrast, the more persistent deterioration we document appears to be driven by the *broader*, non-meme segment where shared narratives are weaker but more continuous, and is *amplified* when institutional ownership is higher. In short, meme episodes are sharp but temporary; the lasting efficiency drag comes from synchronized, always-on attention in the rest of the market.

5.4 Geography of Attention: Hub Core Cities

Table 12 evaluates whether the 3G effect on price efficiency is amplified in major information-production centers ("hub core" counties). We define hub core cities as the eighteen largest financial and economic centers in the United States: New York (Manhattan), Chicago, San Francisco, Boston, Los Angeles, San Jose, Washington DC, Philadelphia, Miami, Dallas, Houston, Atlanta, Seattle, Charlotte, Minneapolis, Denver, Phoenix, and San Diego. This comprehensive selection captures the full spectrum of major metropolitan areas that serve as primary hubs for financial information produc-

tion and dissemination, hosting concentrations of financial intermediaries, media outlets, institutional investors, and corporate headquarters that collectively shape market-wide information flows. By including both traditional financial centers (New York, Chicago, Boston) and emerging hubs (Seattle, Charlotte, Phoenix), we ensure our analysis reflects the contemporary geographic distribution of U.S. financial activity. We estimate an interacted event-study in which the post–pre average of $\sum_k \delta_k$ captures the baseline 3G effect for non-hub firms, while the post–pre average of $\sum_k \gamma_k$ measures the incremental effect for hub-core firms. Consistent with the concerted attention mechanism, the hub interaction is *positive and significant*—i.e., the downside–upside cross-autocorrelation (our "higher = lower efficiency" measure) increases more for firms headquartered in hub cities, with effects that are immediate and persist over the post window. The baseline (non-hub) effect is smaller.

Hub cities concentrate both sides of the attention supply chain: content producers (financial media, sell-side analysts, IR/PR teams, large firms with constant newsflow) and content consumers (dense populations of institutions, professionals, and retail traders). When 3G makes feeds and push alerts truly continuous, these dense networks synchronize around shared narratives more readily than elsewhere. This produces exactly the pattern we document throughout the paper: (i) higher local search intensity (Tables 3–4), (ii) lower idiosyncratic volatility as attention shifts from stock-specific to market-wide stories (Table 5), and (iii) a deterioration in cross-autocorrelation efficiency (Table 6). The hub amplification dovetails with our ownership heterogeneity (Tables 9–10): where institutional presence is heavier—as in hub cores—synchronized attention is stronger, not weaker, and efficiency worsens more. It also connects to our special vs. non-special results (Tables 7–8): hub firms tend to be larger and more liquid, with fewer binding short-sale constraints, so the incremental drag from coordinated narratives is more visible where arbitrage is otherwise feasible. Finally, relative to short-lived "meme" bursts (Table 11), hub cores generate a steady pipeline of headlines, events, and commentary that sustain coordination over longer horizons. In short, the geography of information production matters: by lowering the cost of always-on connectivity in the places that set the news agenda, 3G magnifies concerted attention and further degrades our cross-

autocorrelation–based measure of price efficiency.

6 Conclusion

This paper shows that ubiquitous mobile connectivity reshapes markets not only by raising information acquisition but also by coordinating what investors attend to. When 3G arrives, local search for company news increases, firm-level idiosyncratic volatility falls, and a cross-autocorrelation measure of price efficiency deteriorates. The efficiency decline is stronger where short-selling frictions are lower (non-specials) and where institutional block ownership is higher, while meme-like attention is sharp but transient. The message is straightforward: more access does not automatically deliver more efficiency when access synchronizes attention onto shared, salient narratives.

These patterns align with theories in which limited attention is an allocative choice. When information becomes cheaper, investors tilt toward coarse or high-salience signals rather than processing all firm-specific information symmetrically (Gabaix, 2014; Peng and Xiong, 2006). Such tilts can compress idiosyncratic variation while slowing the cross-sectional impounding of common shocks, which is the joint decline in IVOL and rise in downside–upside cross-autocorrelation we document. The results also resonate with social-transmission views in which narratives propagate through networks, generating correlated beliefs and actions that need not track fundamentals one-for-one (Hirshleifer, 2020; Shiller, 2020; Zhuravskaya et al., 2020). Mobile broadband, the first generation to make social media truly always-on (Guriev et al., 2021), lowers the marginal cost of repeated checking and sharing, amplifying coordination on headline stories. Finally, the asymmetric deterioration in our efficiency metric, rooted in conditional cross-autocorrelations, fits classic insights on short-sale frictions and limits to arbitrage (Diamond and Verrecchia, 1987; Miller, 1977; Shleifer and Vishny, 1997) and is consistent with empirical work using cross-autocorrelations to diagnose slow adjustment to adverse information (Bris et al., 2007; Hou and Moskowitz, 2005b; Saffi and Sigurdsson, 2011b).

Our contribution is threefold. First, we provide causal evidence—based on a plausibly exogenous infrastructure shock—that expanding mobile access can worsen a standard measure of price efficiency even as attention (search) rises. Second, we map where this

effect is strongest: in segments with easier shorting and greater institutional presence, indicating that institutions share the same attention bottlenecks and do not systematically arbitrage away synchronized narratives. Third, we foreground a behavioral first stage with Google Trends, connecting access-induced attention to market-level outcomes and integrating investor-attention measures with limits-to-arbitrage diagnostics.

Two sets of implications follow. For asset pricing, connectivity is not a monotone sufficient statistic for efficiency; the composition and correlation of attention matter as much as its level. Models that marry rational inattention with social diffusion and capital frictions can reconcile lower IVOL with slower cross-sectional adjustment to common news. For corporate finance, always-on attention raises reputational stakes and may alter disclosure timing, investor-relations strategy, and the dynamics of crisis response as narratives propagate rapidly through networks.

We see several avenues for future work. First, microstructure: track how synchronized attention manifests in intraday order flow, inventory, and shorting activity across venues and market makers. Second, platforms and design: study how ranking algorithms and push-notification regimes steer attention under ubiquitous connectivity, and whether design nudges can deconcentrate attention back toward firm-specific signals. Third, external validity: compare 3G/4G/5G rollouts and short-sale regimes across countries to test the boundary conditions of our mechanism. Fourth, real effects: examine how firms adapt capital-raising, disclosure, and advertising policies to an information environment that compresses idiosyncratic noise yet slows the diffusion of common shocks. Finally, welfare: quantify who gains and who bears the costs when synchronized attention trades off lower idiosyncratic risk against slower price discovery.

In sum, infrastructure can coordinate attention. The 3G natural experiment reveals that when investors look more—and look together—markets can become less efficient even as they become, arguably, more informed.

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Appendix

Variable Definitions

Variable	Definition and Source
Main Dependent Variables	
Price Efficiency	Difference between downside and upside cross-autocorrelations of market returns and stock returns. Calculated as downside correlation minus upside correlation (downside_corr − upside_corr) using up to 3 lags. More negative values indicate greater price efficiency (weaker downside correlations relative to upside correlations). Values closer to zero indicate lower price efficiency. Data available 2007–2017. Source: CRSP daily returns, calculated following Hou and Moskowitz (2005), Bris, Goetzmann, and Zhu (2007), Saffi and Sigurdsson (2011), Palia and Sokolinsky (RoFS 2024).
Idiosyncratic Volatility (IVOL)	Annualized standard deviation of residuals from Fama-French 3-factor model regression: $R_{i,t} - R_{f,t} = \alpha_i + \beta_{i,1}(R_{M,t} - R_{f,t}) + \beta_{i,2}SMB_t + \beta_{i,3}HML_t + \epsilon_{i,t}$. Source: CRSP daily returns and Fama-French factors.
Main Independent Variables	
3G Coverage	Indicator variable equal to 1 if county has any 3G coverage (coverage $> 0\%$), 0 otherwise. Source: Collins 3G dataset.
GTrends	Annual average of monthly Google Trends search volume index (scaled 0-100; geo = "US") for all categories associated with firm i in year t. Data collection involved: (1) standardizing company names by removing corporate suffixes and special characters; (2) AI-assisted selection of optimal keywords using topic codes to disambiguate homonyms; (3) retrieving monthly search interest data with "All Categories" filter; and (4) aggregating to annual averages. The index is calculated at the county-level and then linked to firms. Begins in 2004. Source: Google Trends API.
Firm Classification Variables	
Mean Borrowing Fee	Annual mean of quarterly borrowing fees for stock lending, used to classify special stocks and measure short-selling constraints. Higher fees indicate greater difficulty borrowing shares for short selling. Source: Quarterly borrowing fee data aggregated to annual level.
Special Stock (Top 20%)	Indicator variable equal to 1 if firm is in the top 20% of borrowing fees by year, 0 otherwise.
Block Ownership Percentage	Percentage of total institutional ownership held by institutional investors with positions exceeding 10% of total shares outstanding. Used to measure institutional ownership concentration. Source: 13F institutional holdings data aggregated to annual level.

Continued on next page

Variable Definitions (Continued)

Variable	Definition and Source
Top 25% Block Ownership	Indicator variable equal to 1 if firm's block ownership percentage is above the 75th percentile in year t-1 relative to treatment timing, 0 otherwise. Used to examine heterogeneity in price efficiency effects for firms with extreme institutional concentration. Source: Calculated from 13F institutional holdings data aggregated to annual level.
Bottom 25% Block Ownership	Indicator variable equal to 1 if firm's block ownership percentage is below the 25th percentile in year t-1 relative to treatment timing, 0 otherwise. Used to examine heterogeneity in price efficiency effects for firms with low institutional concentration. Source: Calculated from 13F institutional holdings data aggregated to annual level.
Percent Meme Months (Primary)	Percentage of months in a year where a firm shows high retail investor attention, measured as months with Google Trends search intensity above historical norms using the primary z-score threshold ($\geq$ 2.0). Ranges from 0 to 1, where higher values indicate more sustained retail interest throughout the year. Source: Google Trends data processed using firm-specific deviation measures from 24-month rolling averages.
Percent Meme Months Above Median (Pre-3G)	Indicator variable equal to 1 if the firm's percent meme months (primary definition) is above the sample median in the year before 3G treatment, 0 otherwise. Used to examine heterogeneity in 3G effects for firms with high vs. low sustained retail attention. Source: Calculated from percent meme months measure compared to sample median in year prior to 3G adoption.
Hub Core Cities	Indicator variable equal to 1 if firm's county of headquarters is one of the major core finance hubs, including: New York (Manhattan), Chicago, San Francisco, Boston, Los Angeles, San Jose, Washington DC, Philadelphia, Miami, Dallas, Houston, Atlanta, Seattle, Charlotte, Minneapolis, Denver, Phoenix, and San Diego. The list includes the following county FIPS codes: 36061 (NY), 17031 (IL), 06075 (CA), 25025 (MA), 06037 (CA), 06085 (CA), 11001 (DC), 42101 (PA), 12086 (FL), 48113 (TX), 48201 (TX), 13121 (GA), 53033 (WA), 37119 (NC), 27053 (MN), 08031 (CO), 04013 (AZ), 06073 (CA). Used to examine heterogeneity across major financial and economic centers. Source: County FIPS codes matched to Compustat firm headquarters data.
Firm Control Variables	
Size	Natural logarithm of total assets (AT). Source: Compustat.
Leverage	Sum of long-term debt (DLTT) and debt in current liabilities (DLC), divided by total assets (AT). Source: Compustat.
R&D	Research and development expenses (XRD) scaled by total assets (AT). Missing values replaced with zero. Source: Compustat.

Continued on next page

Variable Definitions (Continued)

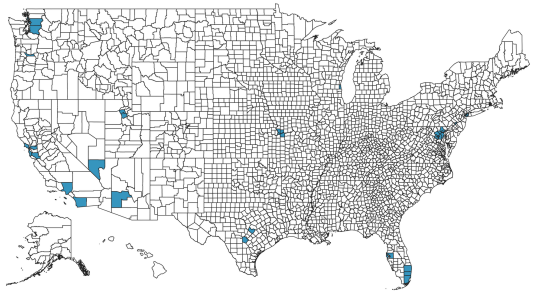
Variable	Definition and Source
Firm Age	Number of years since firm's first appearance in Compustat. Source: Compustat.
Sales Growth	Annual percentage change in sales revenue: $(\text{SALE}_t - \text{SALE}_{t-1})/\lvert\text{SALE}_{t-1}\rvert$. Source: Compustat.
Advertising Cost	Advertising expenses (XAD) scaled by total assets (AT). Missing values replaced with zero. Source: Compustat.
County Demographic Control Variables	
Population	Total county population. Source: U.S. Census Bureau.
White %	Percentage of county population that is White. Source: U.S. Census Bureau.
% 4+ Years College	Percentage of county population with 4-year college degree or higher. Source: U.S. Census Bureau.
Unemployment Rate	County-level unemployment rate as percentage of labor force. Source: U.S. Census Bureau/Bureau of Labor Statistics.
Entropy (Ethnic Diversity)	Racial/ethnic diversity index calculated using the entropy formula: Entropy $= -\sum(p_i \times \ln(p_i))$ where p_i is the proportion of each racial/ethnic group in the county. Higher values indicate greater ethnic diversity. Source: U.S. Census Bureau demographic data.

Notes: All firm-level variables are measured annually unless otherwise specified. Lagged versions (denoted as "Lag 1") use the previous year's value. Sample period is primarily 2000-2020, with some measures having more restricted availability as noted. All continuous variables are winsorized at the 1st and 99th percentiles to reduce the influence of outliers.

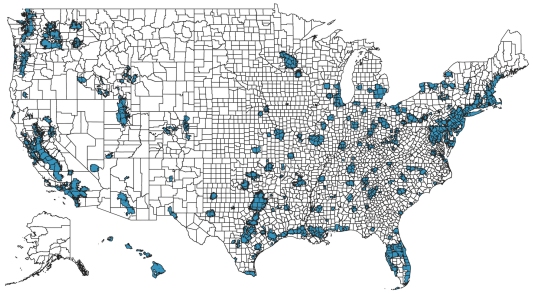
Figure 1:
The Distribution of 3G Coverage by County: USA

This figure illustrates the percentage of 3G coverage in each county in the United States over time from 2006 to 2018, providing six snapshots every three years. The blue color in the figures represents 3G coverage.

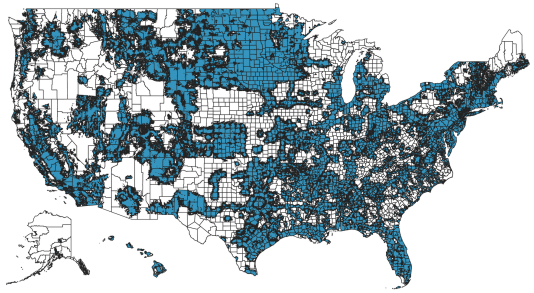
(a) 2006



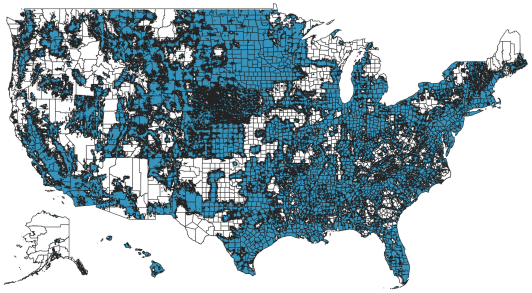
(b) 2009



(c) 2012



(d) 2015



(e) 2018

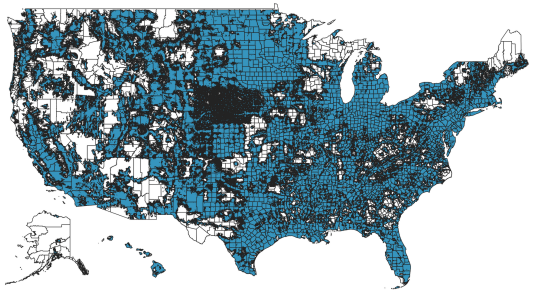
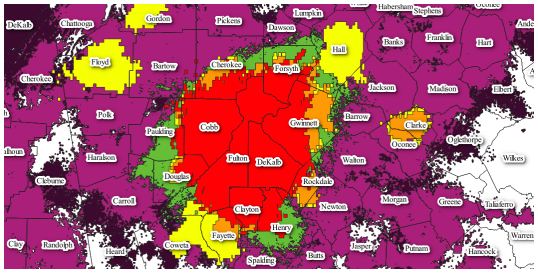


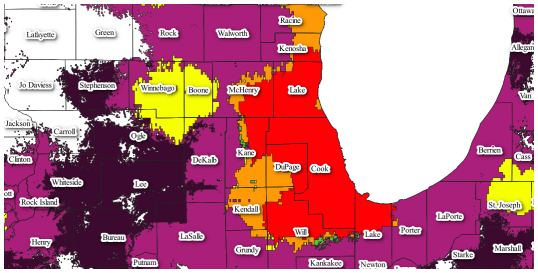
Figure 2:
The Distribution of 3G Coverage by County: Cities

This figure illustrates the percentage of 3G coverage in six select geographic areas: Atlanta, Chicago, Nashville, New York/ Philadelphia, Indianapolis/ Cincinnati, and San Francisco. The red color indicates the first wave of expansions that took place in 2007. Orange indicates the second wave (2008), and yellow, green, blue, purple, and black regions are those with 3G expansion in 2007, 2008, 2009, 2010, 2011, 2012, and 2013 (the last wave).

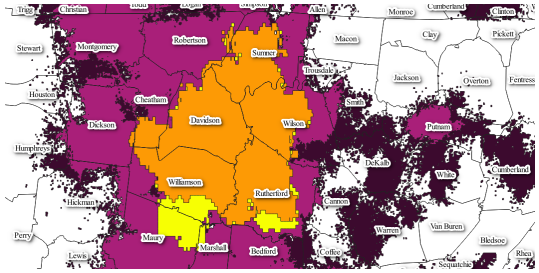
(a) Atlanta



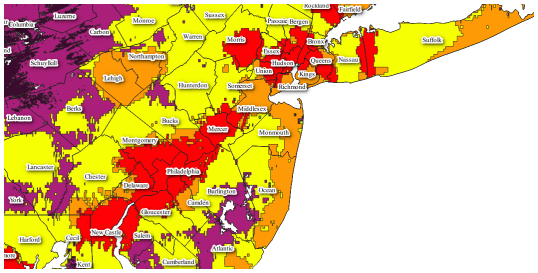
(b) Chicago



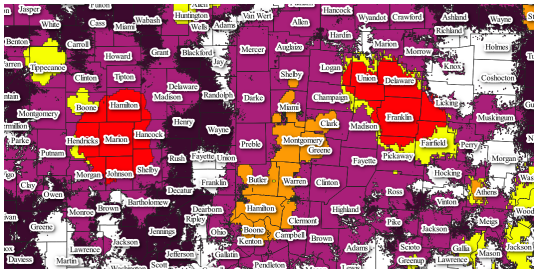
(c) Nashville



(d) New York, Philadelphia



(e) Indianapolis, Cincinnati



(f) San Francisco

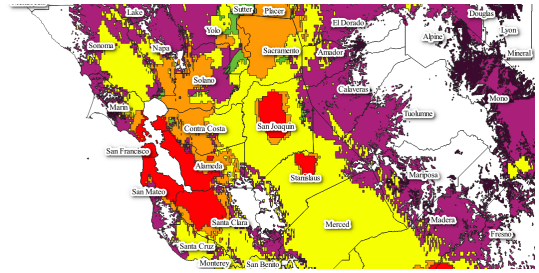
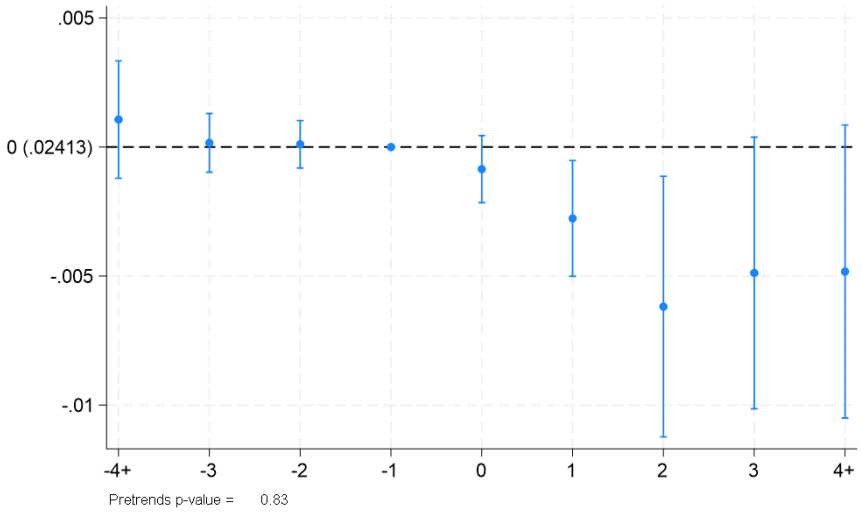


Figure 3:
Event Study of 3G Rollout: Idiosyncratic Volatility and Price Efficiency

This figure reports event-study estimates around the introduction of 3G using the Sun and Abraham (2021) interaction-weighted estimator with firm and time fixed effects. Panel A shows the response of idiosyncratic volatility (IVOL); Panel B shows the response of our price efficiency measure (downside minus upside cross-autocorrelation at lag 1). Point estimates and 95% confidence intervals are plotted; pretrends p-values are displayed within each panel.

(a) IVOL



(b) Price Efficiency

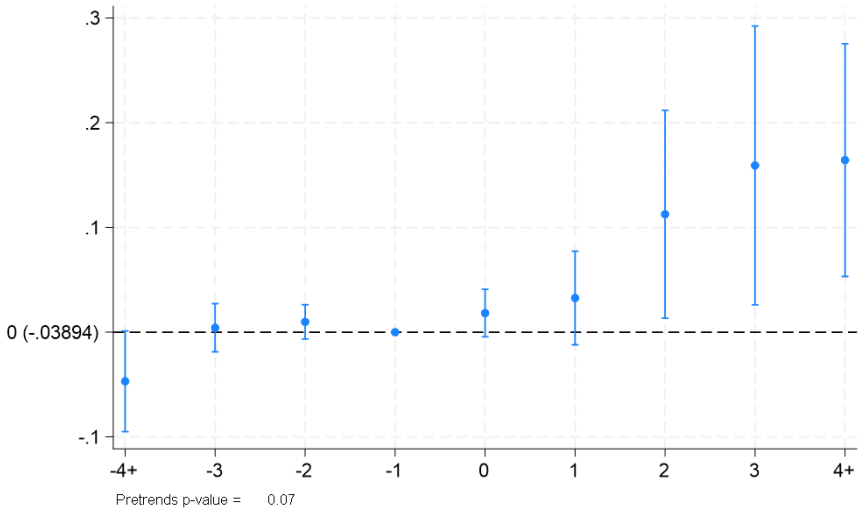
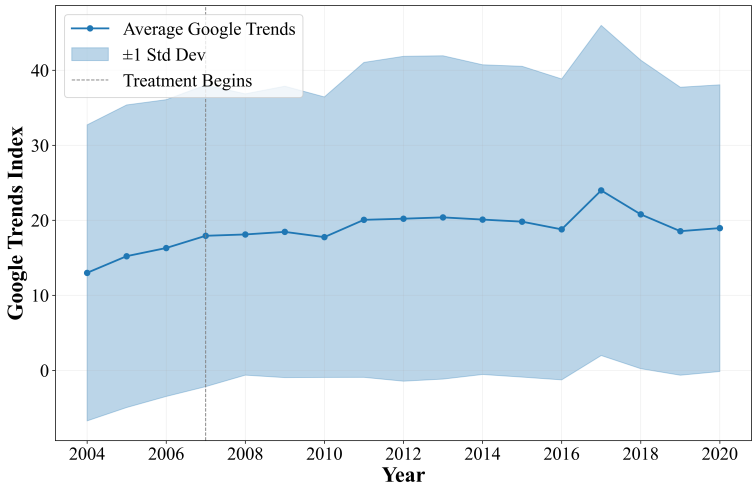


Figure 4:
Google Trends: Time Evolution and Cross-Sectional Distribution

Panel (top) shows the annual Google Trends index with shaded ±1 standard-deviation bands to illustrate temporal variation and the timing of treatment; Panel (bottom) displays the cross-sectional distribution of the Google Trends index (histogram with KDE). Figures use observations with non-missing Google Trends and are intended as validation and descriptive evidence of attention dynamics around local 3G rollout.

(a) Google Trends Time Evolution



(b) Google Trends Index Distribution

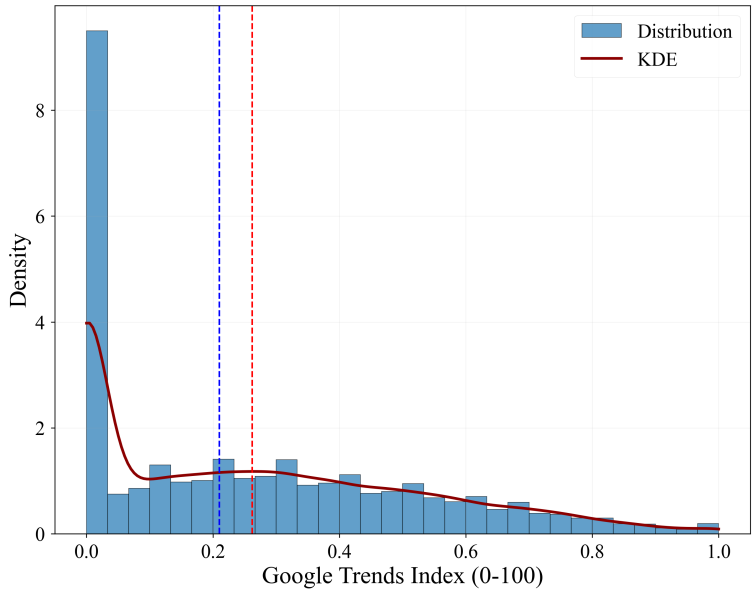


Table 2:
Firm-Level Summary Statistics

This table shows the summary statistics of firm characteristics constructed using data from Compustat and CRSP. Sample period: 2004–2020. The details of variable construction for firm characteristics are described in the Appendix.

	# of Obs.	Mean	Std. Dev.	P1	Q1	Median	Q3	P99
3G Coverage	50,541	0.604	0.489	0.000	0.000	1.000	1.000	1.000
IVOL	50,452	0.026	0.016	0.007	0.015	0.022	0.033	0.083
Price Efficiency	50,541	-0.004	0.138	-0.329	-0.095	-0.004	0.088	0.309
Mean Borrowing Fee	21,223	0.011	0.036	0.003	0.004	0.004	0.005	0.150
Assets ($ B)	50,518	6.629	21.024	0.011	0.202	0.818	3.295	120.406
R&D	50,541	0.040	0.093	0.000	0.000	0.000	0.034	0.511
Age	50,541	24.827	16.077	4.000	13.000	20.000	34.000	68.000
Leverage	50,336	0.203	0.202	0.000	0.031	0.154	0.313	0.900
Inst Block Own %	50,541	0.252	0.219	0.000	0.000	0.251	0.400	0.835
Avg Z-Score	31,790	-0.022	0.761	-1.554	-0.365	0.000	0.189	2.039
Pct Meme Months	31,790	0.040	0.088	0.000	0.000	0.000	0.000	0.417
Log Population	50,541	13.515	1.296	9.538	12.934	13.698	14.297	16.106
% White	50,541	76.486	13.359	37.451	67.315	77.771	86.665	98.227
% 4+ Years College	50,458	36.479	11.187	13.248	28.719	34.997	44.643	61.315
Unemployment Rate	43,484	0.059	0.021	0.027	0.044	0.053	0.070	0.122
Entropy	43,484	58.564	19.459	10.171	45.582	60.733	77.231	88.900
Observations	50,541							

Table 3: Impact of 3G Coverage on County-Level Google Trends

This table presents the results of a differences-in-differences analysis using the Sun and Abraham (2021) event-study specification, examining the effect of 3G coverage on county-level Google Trends index scores across all categories. Sample period: 2004–2020. The baseline regression model is specified as:

$$Ln(Mean\ Cnty\ GTrends\ AllCats + 1)_{i,t} = \delta_1 3G_{i,t} + \gamma X_{i,t} + \alpha_i + \alpha_{st} + \epsilon_{i,t}$$

where the dependent variable represents the natural logarithm of the county mean of Google Trends index scores across all categories for firms headquartered in that county. $3G_{i,t}$ is a binary indicator equal to 1 if county i has any 3G coverage in year t, 0 otherwise. The table reports three specifications with increasing controls. All specifications include county fixed effects (α_i) and year fixed effects (α_t). Column (1) reports baseline results. Column (2) adds county-level demographic controls. Column (3) introduces state-by-year fixed effects (α_{st}) instead of year fixed effects. Standard errors are clustered at the county level.

	Event Study: County Google Trends		
	(1)	(2)	(3)
Before$^{=-4}$	-0.385***	-0.363**	-0.478**
	(-2.64)	(-2.39)	(-2.07)
Before$^{=-3}$	-0.035	-0.048	-0.073
	(-0.47)	(-0.64)	(-0.69)
Before$^{=-2}$	-0.002	-0.016	-0.053
	(-0.04)	(-0.31)	(-0.64)
At$^{=0}$	0.061*	0.070*	0.128**
	(1.71)	(1.92)	(2.46)
After$^{=+1}$	0.165*	0.183*	0.262**
	(1.71)	(1.85)	(2.44)
After$^{=+2}$	0.283**	0.296**	0.388**
	(2.14)	(2.15)	(2.29)
After$^{=+3}$	0.328*	0.331*	0.433*
	(1.81)	(1.75)	(1.84)
After$^{=+4}$	0.347*	0.403**	0.510
	(1.90)	(1.97)	(1.57)
Post-Pre Difference	0.343**	0.363**	0.495**
	(2.20)	(2.20)	(2.05)
County Controls		×	×
County FE	×	×	×
Year FE	×	×	
State × Year FE			×
Observations	10,810	9,092	9,071
Adj. R^2	0.685	0.723	0.721

Table 4: Impact of 3G Coverage on County-Level Google Trends: PPML Estimation

This table presents results from Poisson Pseudo-Maximum Likelihood (PPML) estimation examining the effect of 3G coverage on county-level Google Trends index scores. The dependent variable is the mean county-level Google Trends All Categories index. $3G_{i,t}$ is a binary indicator equal to 1 if county i has any 3G coverage in year t, 0 otherwise. Model (1) includes county and year fixed effects. Model (2) adds county demographic controls (lagged). Model (3) replaces year fixed effects with state-by-year fixed effects. Standard errors are clustered at the county level. Sample period: 2004–2020.

	Mean Cnty Google Trends		
	(1)	(2)	(3)
3G Coverage	0.064*	0.085***	0.086**
	(1.87)	(2.84)	(2.43)
County Controls		×	×
County FE	×	×	×
Year FE	×	×	
State × Year FE			×
Observations	6,736	5,521	5,472

Table 5:
Impact of 3G Coverage on Firm-Level Idiosyncratic Volatility: Controlling for Prior Volatility

This table presents the results of a differences-in-differences analysis using the Sun and Abraham (2021) event-study specification, examining the effect of 3G coverage on firm-level idiosyncratic volatility. Sample period: 2004–2020. The baseline regression model is specified as:

$$IVOL_{i,t} = \sum_{k=-4}^{4} \delta_k \cdot 3G_{i,t+k} + \gamma X_{i,t} + \alpha_i + \alpha_{st} + \epsilon_{i,t}$$

where $IVOL_{i,t}$ is the firm's idiosyncratic volatility calculated from daily stock returns in year t. The event study framework examines the dynamic effects around the 3G adoption event, with $k = 0$ representing the year of adoption. $3G_{i,t}$ is a binary indicator equal to 1 if the county where firm i is headquartered has any 3G coverage in year t, 0 otherwise. The table reports three specifications with increasing controls. All specifications include firm fixed effects (α_i) and state-by-year fixed effects (α_{st}). Columns (1) reports baseline results. Column (2) adds firm-level controls (natural log of total assets, R&D intensity, firm age, leverage, sales growth, advertising cost). Column (3) introduces county-level demographic controls, including natural log of population, percentage White, percentage with college degree, unemployment rate, and entropy. Standard errors are clustered at both the county and firm level.

	Event Study: IVOL		
	(1)	(2)	(3)
Before$^{=-4}$	0.001	0.001	0.001
	(0.93)	(0.95)	(0.92)
Before$^{=-3}$	0.000	0.000	0.000
	(0.13)	(0.32)	(0.28)
Before$^{=-2}$	0.000	0.000	0.000
	(0.06)	(0.24)	(0.23)
At$^{=0}$	-0.001	-0.001	-0.001
	(-1.57)	(-1.27)	(-1.29)
After$^{=+1}$	-0.003***	-0.003**	-0.003**
	(-2.70)	(-2.43)	(-2.42)
After$^{=+2}$	-0.006**	-0.006**	-0.006**
	(-2.35)	(-2.41)	(-2.40)
After$^{=+3}$	-0.005**	-0.005*	-0.005*
	(-2.00)	(-1.82)	(-1.82)
After$^{=+4}$	-0.005*	-0.005*	-0.005*
	(-1.82)	(-1.67)	(-1.67)
Post-Pre Difference	-0.004**	-0.004**	-0.004**
	(-2.06)	(-1.98)	(-1.98)
County Controls			×
Firm Controls		×	×
Firm FE	×	×	×
Year FE	×	×	×
Observations	40,118	39,714	39,643
Adj. R^2	0.652	0.659	0.659

Table 6: Impact of 3G Coverage on Firm-Level Price Efficiency: Controlling for Prior Efficiency

This table presents the results of a differences-in-differences analysis using the Sun and Abraham (2021) event-study specification, examining the effect of 3G coverage on firm-level price efficiency. Sample period: 2004–2020. The baseline regression model is specified as:

$$PE_{i,t} = \sum_{k=-4}^{4} \delta_k \cdot 3G_{i,t+k} + \gamma X_{i,t} + \alpha_i + \alpha_{st} + \epsilon_{i,t}$$

where $PE_{i,t}$ is calculated using the cross-autocorrelation methodology by Hou and Moskowitz (2005a), computing the difference between downside and upside cross-correlations of stock returns with lagged market returns. The variable ranges from -1 to 1, with values closer to zero indicating more efficient price discovery. The event study framework examines the dynamic effects around the 3G adoption event, with $k = 0$ representing the year of adoption. $3G_{i,t}$ is a binary indicator equal to 1 if the county where firm i is headquartered has any 3G coverage in year t, 0 otherwise. The table reports three specifications with increasing controls. All specifications include firm fixed effects (α_i) and state-by-year fixed effects (α_{st}). Columns (1) reports baseline results. Column (2) adds firm-level controls. Column (3) introduces county-level demographic controls. Standard errors are clustered at both the county and firm level.

	Event Study: Price Efficiency		
	(1)	(2)	(3)
Before$^{=-4}$	-0.033	-0.047*	-0.047*
	(-1.37)	(-1.92)	(-1.91)
Before$^{=-3}$	0.007	0.005	0.004
	(0.58)	(0.46)	(0.36)
Before$^{=-2}$	0.010	0.010	0.010
	(1.23)	(1.25)	(1.18)
At$^{=0}$	0.018	0.018	0.018
	(1.55)	(1.60)	(1.58)
After$^{=+1}$	0.028	0.034	0.033
	(1.29)	(1.48)	(1.43)
After$^{=+2}$	0.092*	0.114**	0.113**
	(1.90)	(2.28)	(2.23)
After$^{=+3}$	0.128**	0.162**	0.159**
	(1.97)	(2.41)	(2.35)
After$^{=+4}$	0.126**	0.166***	0.164***
	(2.22)	(2.96)	(2.91)
Post-Pre Difference	0.082*	0.107**	0.106**
	(1.88)	(2.39)	(2.34)
County Controls			×
Firm Controls		×	×
Firm FE	×	×	×
Year FE	×	×	×
Observations	38,476	38,160	38,089
Adj. R^2	0.095	0.095	0.094

Table 7: Impact of 3G Coverage on Price Efficiency: Special vs. Non-Special Firms

This table presents the results of a differences-in-differences analysis using the Sun and Abraham (2021) event-study specification, examining heterogeneous effects of 3G coverage on firm-level price efficiency across firms with different short-selling constraints. Sample period: 2007–2017. Special stocks are defined as firms in the top 20% of short-selling borrowing fees by year, indicating high short-selling constraints. The baseline regression model is specified as:

$$PE_{i,t} = \sum_{k=-4}^{4} \delta_k \cdot 3G_{i,t+k} + \gamma X_{i,t} + \alpha_i + \alpha_{st} + \epsilon_{i,t}$$

where $PE_{i,t}$ is the price efficiency measure calculated using cross-autocorrelation methodology. The event study framework examines the dynamic effects around the 3G adoption event, with $k = 0$ representing the year of adoption. The reported 3G coefficients represent the average difference between post-event and pre-event estimated effects. All specifications include firm-level controls (natural log of total assets, R&D intensity, firm age, leverage, sales growth, advertising cost), firm fixed effects (α_i), and state-by-year fixed effects (α_{st}). Columns (1)-(3) show results for the special stocks subsample (high borrowing fees). Columns (4)-(6) show results for the non-special stocks subsample (low borrowing fees). Standard errors are clustered at both the county and firm level. This table reports only the average treatment effects (post-pre differences) without showing the full event study dynamics.

	Event Study: Price Efficiency					
	(1)	(2)	(3)	(4)	(5)	(6)
3G	-0.052	-0.076*	-0.078*	0.027**	0.027**	0.027**
	(-1.37)	(-1.95)	(-1.97)	(2.36)	(2.30)	(2.31)
County Controls			×			×
Firm Controls		×	×		×	×
Firm FE	×	×	×	×	×	×
Year FE	×	×	×	×	×	×
Observations	2,072	2,020	2,020	22,796	22,588	22,541
Adj. R^2	-0.302	-0.323	-0.327	0.056	0.055	0.054

Table 8: 3G Coverage and Price Efficiency: Top Special Stocks Interaction

This table presents event-study estimates using the Sun and Abraham (2021) specification, examining heterogeneous 3G effects on price efficiency for firms with high short-selling constraints (top 20% of borrowing fees) relative to other firms. Sample period: 2007–2017. Special stocks are defined as firms in the top 20% of annual borrowing fees, indicating high short-selling constraints where stocks are expensive or difficult to borrow. The model includes an interaction term:

$$PE_{i,t} = \sum_{k=-4}^{4} \delta_k \cdot 3G_{i,t+k} + \sum_{k=-4}^{4} \gamma_k \cdot (3G_{i,t+k} \times Special20_i) + \phi X_{i,t} + \alpha_i + \alpha_{st} + \epsilon_{i,t}$$

where $Special20_i$ equals 1 if the firm is in the top 20% of borrowing fees in that year. The γ_k coefficients show the differential 3G effect for firms with high short-selling constraints. Price efficiency uses cross-autocorrelation methodology. All specifications include firm controls, firm fixed effects, and state-by-year fixed effects. Standard errors clustered at county and firm level. The reported 3G coefficients represent the average difference between post-event and pre-event estimated effects. This table reports only the average treatment effects (post-pre differences) without showing the full event study dynamics.

	Event Study: Price Efficiency		
	(1)	(2)	(3)
3G	-0.028***	-0.028***	-0.028***
	(-3.29)	(-3.22)	(-3.22)
County Controls			×
Firm Controls		×	×
Firm FE	×	×	×
Year FE	×	×	×
Observations	26,277	26,041	25,985
Adj. R^2	0.047	0.047	0.047

Table 9: 3G Coverage and Price Efficiency: High Block Ownership Interaction

This table presents event-study estimates using the Sun and Abraham (2021) specification, examining heterogeneous 3G effects on price efficiency for firms with high block ownership (>30%, the sample median) relative to lower block ownership firms. Sample period: 2004–2020. Block ownership measures the percentage of institutional ownership held by investors with positions >10% of shares outstanding. The model includes an interaction term:

$$PE_{i,t} = \sum_{k=-4}^{4} \delta_k \cdot 3G_{i,t+k} + \sum_{k=-4}^{4} \gamma_k \cdot (3G_{i,t+k} \times HighBlock_i) + \phi X_{i,t} + \alpha_i + \alpha_{st} + \epsilon_{i,t}$$

where $HighBlock_i$ equals 1 if the firm had block ownership >30% in the year before 3G treatment. The γ_k coefficients show the differential 3G effect for high block ownership firms. Price efficiency uses cross-autocorrelation methodology. All specifications include firm controls, firm fixed effects, and state-by-year fixed effects. Standard errors clustered at county and firm level. The reported 3G coefficients represent the average difference between post-event and pre-event estimated effects. This table reports only the average treatment effects (post-pre differences) without showing the full event study dynamics.

	Event Study: Price Efficiency		
	(1)	(2)	(3)
3G	0.016***	0.017***	0.017***
	(3.93)	(3.98)	(3.97)
County Controls			×
Firm Controls		×	×
Firm FE	×	×	×
Year FE	×	×	×
Observations	40,173	39,748	39,677
Adj. R^2	0.089	0.089	0.088

Table 10: 3G Coverage and Price Efficiency: Block Ownership Percentile Analysis (Top 25% vs Bottom 25% Sample)

This table presents event-study estimates using the Sun and Abraham (2021) specification, examining 3G effects on price efficiency for firms in the top and bottom quartiles of block ownership distribution. The analysis is restricted to firms that are either in the top 25% or bottom 25% of block ownership in the year before 3G treatment. Sample period: 2004–2020. Block ownership measures the percentage of institutional ownership held by investors with positions >10% of shares outstanding. The model estimates:

$$PE_{i,t} = \sum_{k=-4}^{4} \delta_k \cdot 3G_{i,t+k} + \gamma_k \cdot Top25PctBlock_i + \sum_{k=-4}^{4} \eta_k \cdot (3G_{i,t+k} \times Top25PctBlock_i) + \phi X_{i,t} + \alpha_i + \alpha_{st} + \epsilon_{i,t}$$

where $Top25PctBlock_i$ equals 1 if the firm's block ownership was above the 75th percentile (top 25%) in the year before 3G treatment. The η_k coefficients capture the differential 3G effect for top quartile firms relative to bottom quartile firms within this restricted sample. Price efficiency uses cross-autocorrelation methodology. All specifications include firm controls, firm fixed effects, and state-by-year fixed effects. Standard errors clustered at county and firm level. The reported 3G coefficients represent the average difference between post-event and pre-event estimated effects. This table reports only the average treatment effects (post-pre differences) without showing the full event study dynamics.

	Event Study: Price Efficiency		
	(1)	(2)	(3)
3G	0.024***	0.025***	0.025***
	(3.70)	(3.76)	(3.75)
County Controls			×
Firm Controls		×	×
Firm FE	×	×	×
Year FE	×	×	×
Observations	14,898	14,796	14,774
Adj. R^2	0.067	0.066	0.066

Table 11: 3G Coverage and Price Efficiency: Percent Meme Months Above Median Interaction

This table presents event-study estimates using the Sun and Abraham (2021) specification, examining heterogeneous 3G effects on price efficiency for firms with high percent meme months (above median) relative to lower percent meme months firms. Sample period: 2004–2020. Percent meme months measures the percentage of months with high retail investor attention based on Google Trends deviations from historical averages. The model includes an interaction term:

$$PE_{i,t} = \sum_{k=-4}^{4} \delta_k \cdot 3G_{i,t+k} + \sum_{k=-4}^{4} \gamma_k \cdot (3G_{i,t+k} \times HighPctMeme_i) + \phi X_{i,t} + \alpha_i + \alpha_{st} + \epsilon_{i,t}$$

where $HighPctMeme_i$ equals 1 if the firm had percent meme months above the sample median in the year before 3G treatment. The γ_k coefficients show the differential 3G effect for high percent meme months firms. Price efficiency uses cross-autocorrelation methodology. All specifications include firm controls, firm fixed effects, and state-by-year fixed effects. Standard errors clustered at county and firm level. The reported 3G coefficients represent the average difference between post-event and pre-event estimated effects. This table reports only the average treatment effects (post-pre differences) without showing the full event study dynamics.

	Event Study: Price Efficiency		
	(1)	(2)	(3)
3G	0.286***	0.277***	0.283***
	(18.02)	(16.30)	(16.10)
County Controls			×
Firm Controls		×	×
Firm FE	×	×	×
Year FE	×	×	×
Observations	40,221	39,806	39,735
Adj. R^2	0.087	0.087	0.086

Table 12: 3G Coverage and Price Efficiency: Hub Core Cities Interaction

This table presents event-study estimates using the Sun and Abraham (2021) specification, examining heterogeneous 3G effects on price efficiency for firms in major hub cities relative to other locations. Sample period: 2004–2020. Hub core cities are defined as major metropolitan areas that serve as financial and economic centers, including counties such as New York County (NY), Cook County (IL), San Francisco County (CA), Suffolk County (MA), Los Angeles County (CA), and Miami-Dade County (FL). The model includes an interaction term:

$$PE_{i,t} = \sum_{k=-4}^{4} \delta_k \cdot 3G_{i,t+k} + \sum_{k=-4}^{4} \gamma_k \cdot (3G_{i,t+k} \times HubCore_i) + \phi X_{i,t} + \alpha_i + \alpha_{st} + \epsilon_{i,t}$$

where $HubCore_i$ equals 1 if the firm is headquartered in a hub core city. The γ_k coefficients show the differential 3G effect for hub core city firms. Price efficiency uses cross-autocorrelation methodology. All specifications include firm controls, firm fixed effects, and state-by-year fixed effects. Standard errors clustered at county and firm level. The reported 3G coefficients represent the average difference between post-event and pre-event estimated effects. This table reports only the average treatment effects (post-pre differences) without showing the full event study dynamics.

	Event Study: Price Efficiency		
	(1)	(2)	(3)
3G	0.322***	0.312***	0.322***
	(22.16)	(20.77)	(22.44)
County Controls			×
Firm Controls		×	×
Firm FE	×	×	×
Year FE	×	×	×
Observations	39,931	39,554	39,483
Adj. R^2	0.089	0.089	0.088