Title

Media Accessibility and the Capital Market Effects of Media Dissemination: Evidence from Digital Paywalls

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Publication Date

2024

Peer reviewed|Thesis/dissertation

Media Accessibility and the Capital Market Effects of Media Dissemination: Evidence from Digital Paywalls

By

Kimberlyn George

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Business Administration

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Associate Professor Omri Even-Tov, Chair
Professor Panos Patatoukas
Professor Sunil Dutta
Professor Steven Davidoff Solomon

Spring 2024

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Abstract

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This paper examines the capital market implications of media accessibility. I exploit the staggered adoption of digital paywalls, which charge readers for access to previously free online news content, by major U.S. local newspapers as a negative shock to media accessibility. Focusing specifically on the disclosure dissemination role of the media, I find that after the adoption of digital paywalls, firms that receive persistent earnings announcement media coverage from their local newspaper experience reduced abnormal trading volume, which is driven by a reduction in abnormal retail trading volume. Additionally, these firms experience reduced liquidity, and slower speed of price discovery. These results are driven by low-visibility firms for which local investors, the likely readers of local newspapers, are more likely to be the marginal investor. Using a placebo test, I show it is unlikely the results are driven by an unmodeled factor. While prior literature has extensively documented the capital market effects of earnings announcement media coverage, my findings underscore that the effects of media coverage depend on the accessibility of media content.

Dedication

To Santi, who made it all possible.

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Acknowledgments

First and foremost, I thank my committee chair Omri Even-Tov. This dissertation would not have been possible without his mentorship, guidance, and thoughtful advice. I thank my committee members, Panos Patatoukas, Sunil Dutta, and Steven Davidoff Solomon, along with Eric So, Michael Dambra, Ben Lourie, Xiao-Jun Zhang, Valerie Zhang, Tanya Paul, Xi Wu, Biwen Zhang, and Shawn Kim for their guidance and feedback. I also thank seminar participants at U.C. Berkeley, University of Notre Dame, London School of Economics and Political Science, University of Michigan, Texas A&M University, INSEAD, Ohio State University, University of Texas at Dallas, University of Southern California and London Business School for helpful comments and suggestions.

1. Introduction

The accessibility of news varies both across time and across media outlets. As an example, before 1996, readers could only access a *Wall Street Journal* article by acquiring a print copy of the newspaper. Today, for a $40 monthly subscription fee, they can access these articles online via the newspaper's website or on a mobile device through the newspaper's dedicated app. However, readers can often access coverage of the same underlying firm events online through *Yahoo! Finance* or *The Associated Press* for free.

Given the media's importance as a capital market information intermediary (Tetlock (2007), Miller and Skinner (2015), Ahern and Peress (2023)), it is important to understand whether, and to what extent, differences in media accessibility affect the influence of media coverage on capital market outcomes. To my knowledge, this is the first paper to examine the capital market implications of media accessibility. I focus specifically on the media's important role in disseminating firm disclosures (Bushee et al. (2010), Engelberg and Parsons (2011), Blankespoor et al. (2018), Guest (2021)) and leverage the staggered adoption of digital paywalls, which impose an acquisition cost on readers for access to previously free online news content, by major U.S. local newspapers as a negative shock to media accessibility. Using this setting, I study how changes in media accessibility affect capital market outcomes for firms that receive earnings announcement media coverage.

While paywalls have proliferated across the digital media landscape throughout the past decade, exploring their adoption by local newspapers as a shock to media accessibility provides identification benefits. Specifically, I am able to exploit cross-sectional variation in firms' exposure to the effects of a given local newspaper's paywall adoption by comparing local firms to non-local firms. I expect local newspapers to be more influential intermediaries for the investors of local firms than the investors of non-local firms for two reasons. First, because these newspapers cater to a local audience, their coverage exhibits local bias (Gurun and Butler (2012), Engelberg and Parsons (2011)). Thus, local newspapers contribute more significantly to the information environments of local firm investors than non-local firm investors. Second, the behavior of local investors, the likely readers of local newspapers, influences aggregate market outcomes of local firms to a greater extent than non-local firms. Prior literature has documented that trading by local investors affects local firm valuations, liquidity, and returns (Jacobs and Weber (2012), Loughran and Schultz (2005), Hong et al. (2008), Shive (2012)), and that local newspaper coverage affects local firm aggregate market outcomes, through its influence on local investor trading (Gurun and Butler (2012), Hillert et al. (2014)).

The impact of the decreased news accessibility caused by digital paywall adoption on market outcomes hinges two factors: (1) whether local news consumption declines due to higher acquisition costs and (2) whether such a decline is counterbalanced by alternative information sources. Media outlets select from all available information what is most valuable to distill and disseminate to their audience (Ahern and Peress (2023)). Investors have limited attention, and under rational attention theory, investors allocate their scarce attention by weighting its opportunity cost against the trading profits they expect from higher attention and superior financial information (Sims (2003)). By focusing readers' attention, media outlets reduce the analytical costs investors face when seeking investment-related information (Ahern and Peress (2023)). If readers opt to pay for local newspaper access, or replace their local news consumption with an alternative information source, the introduction of paywalls should have a negligible impact on trading behavior and market outcomes. Conversely, if the perceived benefits of local newspaper access do not justify the increased cost, or if investors are unaware of these benefits, the paywall adoption may alter the impact of local newspaper dissemination on capital market outcomes.

I examine the staggered adoption of digital paywalls by eight major, geographically dispersed local newspapers: *The Boston Globe*, *The Los Angeles Times*, *The Arizona Republic*, *The Chicago Tribune*, *The Houston Chronicle*, *The Miami Herald*, *The San Francisco Chronicle*, and *The Denver Post*.[1] Similar to most traditional news outlets, these local newspapers initially provided all news content free of charge on their websites, which were established in the late 1990s. However, they subsequently adopted a digital paywall strategy between 2011 and 2013, requiring readers to pay between $10-$16 per month for unlimited access to news content. Literature in marketing has documented the significant negative effects paywall adoptions have had on newspaper page views and unique site visitors for both local and national newspaper websites (Kim et al. (2020), Pattabhiramaiah et al. (2019)).

I employ a stacked difference-in-difference research design, comparing treated firms, defined as firms located in a city whose local newspaper has adopted a digital paywall, to control firms, defined as firms located in cities with a local newspaper that has not adopted a paywall.[2] The stacked difference-in-difference design employs a constant event window for each paywall adoption event, spanning the four quarters before and after the paywall adoption quarter. To examine the effects of paywall adoption on the influence of earnings announcement media coverage, I focus on treated and control firms within each paywall adoption event that receive coverage of earnings announcements from their local newspaper at least once during both the pre- and post-paywall adoption periods, which I categorize as *Covered Firms*. My main analyses compare *Covered Firms* in paywall-adopting cities (treated *Covered Firms*) to *Covered Firms* in non-adopting cities (control *Covered Firms*) before and after paywall adoption. I focus on capital market outcomes that prior literature has documented to be influenced by earnings announcement media coverage: trading volume, liquidity, and the speed of price discovery (Bushee et al. (2010), Guest (2021), Blankespoor

[1] As discussed in Section 3.1, to identify these local newspapers, I start with the 20 largest local newspapers by 2010 circulation and retain those with Ravenpack coverage during my sample period.

[2] As discussed in Section 3.1, within each paywall adoption event, control firms are identified as firms local to any of the eight local newspapers in my sample who's treatment status did not change during the event window. This may include both previously-treated and later-treated firms. As discussed in Section 4.6, to mitigate concerns that dynamic treatment effects are biasing my results (Baker et al. (2022)), I show that results are robust to only including later-treated control firms within each adoption event. I limit control firms to firms local to my sample of local newspapers given I require data on each firm's local newspaper earnings announcement coverage.

et al. (2018)).

I first examine whether decreased news accessibility resulting from digital paywalls impacts *Covered Firms'* trading activity. Prior literature has found that media coverage increases investor attention and the likelihood of trading (Barber and Odean (2008), Blankespoor et al. (2018)). To the extent paywall adoption reduces an outlet's audience, and if prior readers do not acquire information elsewhere, investor attention and trading activity generated from coverage may decline. I find evidence that following paywall adoption, treated *Covered Firms* experience a significant reduction in abnormal trading volume relative to control *Covered Firms*.

I further examine trading volume from different investor types - retail and institutional traders.[3] I expect retail investor trading activity to be more sensitive to digital paywall adoption for two reasons: (1) retail investors' decision-making is likely more influenced by general purpose media outlet coverage than institutional investors', who actively follow firm events (Blankespoor et al. (2018)), and (2) retail investors have less resources than institutional investors and are thus are more likely to be sensitive to the cost of news. Consistent with this expectation, I find a significant reduction in abnormal retail trading volume for treated *Covered Firms*, but no significant change in abnormal large trading volume when controlling for firm attributes and announcement characteristics. These differential effects suggest that paywalls impact the information environments and decision-making of less sophisticated investors to a greater extent than sophisticated investors. Overall, my findings suggest that digital paywalls limit the impact of local news dissemination on trading activity.

Next, I study how media accessibility affects liquidity for *Covered Firms*. I examine abnormal bid-ask spreads and abnormal depth in the earnings announcement window. Digital paywall adoption could have negative effects on liquidity if the decreased accessibility of local news dissemination increases investors' information processing costs, which can lead to more information asymmetry, greater price protection, and less willingness to trade (Blankespoor et al. (2018), Grossman and Stiglitz (1980), Diamond and Verrecchia (1991)). Additionally, the decreased trading volume associated with the paywall adoption could cause a more shallow market. On the other hand, decreased trading volume could reduce concerns about privately informed counter-parties and decrease *perceived* information asymmetry, leading to improved liquidity (Easley and O'Hara (1992)). I find evidence of the former, with treated *Covered Firms* experiencing a significant increase in abnormal bid-ask spread and significant decrease in abnormal depth following paywall adoption, relative to control *Covered Firms*.

Last, I examine the speed of price discovery. While prior literature has provided evidence that media coverage of earnings announcements improves the speed of price discovery (Guest (2021)), it has also been shown to spur attention-driven or biased trading, specifically from retail investors (Barber and Odean (2008)). Given my finding that digital paywall adoption seems to affect retail investors' media-driven trading to a greater extent than institutional

[3] I identify retail trades in TAQ following Boehmer et al. (2021), and I identify large trades likely to be executed by institutional investors as trades over $50,000, following Bushee et al. (2020).

investors, it is unclear whether digital paywall adoption will enhance or impede price discovery for *Covered Firms*. I focus on intraperiod efficiency (*IPE*) which captures the speed at which earnings information is incorporated into price (Blankespoor et al. (2018), Campbell et al. (2023)). I find that treated *Covered Firms* experience a significant reduction in the speed of price discovery following paywall adoption relative to control *Covered Firms*.

I examine two possible mechanisms that could drive the reduced speed of price discovery for *Covered Firms*. First, it is possible that the reduction in trading volume caused by digital paywall adoption is accompanied by a reduction in informed, efficiency-enhancing trading. To probe this explanation, I examine local investors' fundamental information acquisition, measured by the abnormal count of 8-K downloads by local IP addresses. A reduction in local investors' fundamental information acquisition for treated *Covered Firms* would be consistent with digital paywall adoption preventing efficiency-enhancing trading. Second, it is possible that the liquidity reduction is driving the slower speed of price discovery. Stock liquidity increases the rate at which information - especially negative information - is incorporated into price (Cheng et al. (2023)). To test this explanation, I split firms into sub-samples based on their frequency of negative earnings surprises and mean value of standardized unexpected earnings surprise (SUE) during the event window. If the reduction in *IPE* is driven by firms with the highest frequency of negative earnings news, this would be consistent with the decrease in liquidity driving the reduced speed of price discovery for *Covered Firms*.

I find evidence to support of both explanations. Treated *Covered Firms* experience a reduction in abnormal 8-K downloads from *local* IP addresses, but no change in abnormal 8-K downloads from *non-local* IP addresses relative to control *Covered Firms*, consistent with reduced fundamental information acquisition by local investors after paywall adoption. At the same time, sub-sample analyses suggest that the documented reduction in *IPE* for treated *Covered Firms* is driven by firms with the most negative earnings news, indicating that liquidity may drive the change in speed of price discovery. Given these findings, I do not draw any conclusions about the relative importance of these two mechanisms for the decreased speed of price discovery.

I next conduct various cross-sectional analyses to provide support that the effects I document are driven by the decreased accessibility of local news following paywall adoption. I expect the effects of local newspaper paywall adoption to be most pronounced for firms with a larger percentage of local investors. Given less visible firms' shares are more likely to be absorbed by local investors (Hong et al. (2008), Gurun and Butler (2012)), low-visibility firms are more likely to have a higher percentage of local investor ownership than high-visibility firms. Following prior literature, I proxy for firm visibility using firm size and national media coverage (Gurun and Butler (2012), Jacobs and Weber (2012), Shive (2012)). I find that results tend to be significantly larger for and most concentrated in firms where local investors are more likely to be the marginal investor - smaller firms and firms with lower national media coverage. Additionally, given I expect digital paywall adoption to have a more pronounced effect on retail investors' information environments than institutional investors', I predict that it will have the strongest effect on firms with a higher percentage of retail investor ownership. When splitting the sample along these lines, I find that most effects are concentrated in firms with a larger percentage of retail investor ownership. These findings provide further support that the effects of paywall adoption on trading volume, liquidity, and price discovery for *Covered Firms* are driven by changes in the influence of local media coverage on local investors' trading behavior.

Prior literature documents how incentives to appease advertisers influence local newspapers' coverage of local firms (Gurun and Butler (2012)). Thus, it is possible that the shift from reliance on advertisers to reliance on readers for revenue may alter local newspapers' coverage decisions. To alleviate concerns that changes in coverage are driving my results, I examine the total number of local newspaper earnings announcement articles and their tone before and after paywall adoption and find no significant changes in the coverage of local firms after paywall adoption. Additionally, I show that coverage by newswires and national media outlets did not significantly change for treated *Covered Firms* after paywall adoption.

An additional concern may stem from the possibility that an unmodeled factor is driving both the local newspaper's decision to adopt the digital paywall and changes in local firms' trading volume, liquidity, and speed of price discovery. To alleviate such concerns, I conduct a placebo test using the sample of treated and control firms within each paywall adoption event that receive no earnings announcement media coverage from their local newspaper in either the pre- or post-adoption periods, which I label as *Non-Covered Firms*. I do not expect paywall adoption to have an effect on earnings announcement market outcomes for local firms that receive no earnings announcement local newspaper coverage. I show that treated *Non-Covered Firms* experience no significant changes in trading volume, liquidity, speed of price discovery, or abnormal 8-K downloads by local IP addresses relative to control *Non-Covered Firms*, alleviating concerns that an unmodeled factor is driving my results.

My paper offers three main contributions to the literature. First, I add to the literature on the traditional media's role in disseminating firm disclosures. Prior literature has extensively documented the significant impacts of media coverage of earnings announcements on trading volume (Guest (2021), Engelberg and Parsons (2011)) liquidity (Bushee et al. (2010), Lawrence et al. (2018), Blankespoor et al. (2018)), and speed of price discovery (Guest (2021), Blankespoor et al. (2018)). The key finding of this study is that these effects of media coverage are dependent on the accessibility of media content. This finding complements initial work in the literature on media in accounting that documents how features of media outlets moderate the influence of media coverage on capital market outcomes. For example, Drake et al. (2017) show that media outlets' professionalism influences the effects of media coverage on price formation and call for future work to explore other attributes of information intermediaries. Rees and Twedt (2022) study the political bias of media outlets and show that the slant of media coverage influences the effect of media coverage on price discovery. Additionally, my findings augment recent survey data of corporate media relations officers that documents how firms differentially value media outlets (Flam et al. (2023)). Media coverage is often included as a determinant of outcomes of interest in accounting research. My findings suggest that researchers should consider the outlets included in measures of media coverage, their accessibility, and how their accessibility may differ over time and across investor types. Importantly, the accessibility of media content may be a correlated omitted variable in explaining outcomes of interest in accounting research.

Second, I contribute to the research on information processing costs. Extant literature has largely focused on investors' costs to acquire and interpret firm disclosures (Blankespoor et al. (2020)), and has provided ample evidence that media coverage of disclosures lowers disclosure processing costs (Blankespoor et al. (2020), Blankespoor et al. (2018), Guest (2021)). I contribute to this literature by bringing to light additional processing costs investors confront to acquire and interpret media coverage itself. Thus, I broaden the scope of investor information processing costs to include other information sources, specifically those provided by information intermediaries.

Last, the evidence presented answers calls in Blankespoor et al. (2020) to understand the capital market implications of the evolution and decline of traditional media. Thus far, attention in this area has been focused on local newspaper closures, which have been shown to affect local firm misconduct (Heese et al. (2022)), local firm toxic emissions (Jiang and Kong (2023)), profits to insider trading for local firms (Kyung and Nam (2023)), and municipal borrowing costs (Gao et al. (2020)). These studies highlight the important role of the traditional media as a monitor of firms and reveal how the loss of media monitoring affects firm behavior. I contribute to this literature by focusing on changes in the accessibility of news rather than changes in the existence of news itself. While financial pressures over the past two decades have lead many newspapers to close, they have also lead surviving newspapers to adjust their business models and impose additional costs on readers. My findings suggest that for surviving news outlets, the role of traditional media outlets in capital markets is changing as the industry has adapted to the digital era.

The paper proceeds as follows. Section 2 outlines the setting's institutional details, Section 3 introduces the research design and variable construction, Section 4 discusses results, and Section 5 concludes.

2. Institutional Details

When newspapers first launched their online counterparts in the late 1990s, most offered all content available in their print editions on their public websites free of charge.[4] By 2000, 23% of Americans reported that they went online for news at least three times a week, and by 2010, that number increased to 46% (Center (2012)). As readers moved online and stopped reading print editions, both print subscriber and print advertising revenue quickly declined (Weber (2017)). Due to competition from other popular online platforms, such as Craigslist and Google, digital advertising revenue failed to offset declines in print advertising and circulation revenue, leading many newspapers to adjust their online policies in the 2010s.

The first significant newspaper to adopt a digital paywall was *The New York Times* in March 2011.[5] Other newspapers were quick to follow suit, and by 2019, approximately 70%

[4]A notable exception is *Wall Street Journal*, which has always charged online readers for access.

[5]I do not include the *New York Times* paywall adoption event in my sample, as the *New York Times* is considered a national media outlet (Hillert et al. (2014), Fang and Peress (2009)).

of newspaper sites in the United States and Europe had adopted digital paywalls (Simon and Graves (2019)). Paywalls were typically first implemented in one of two strategies: metered or non-metered. Under a metered paywall, readers are given a certain number of articles per month, before being asked to pay for unlimited access. Under a non-metered paywall, typically weather and traffic news is moved to a separate free website, while all other content is offered exclusively to paying customers. In my sample of newspapers, *The Chicago Tribune*, *The Houston Chronicle*, and *The Boston Globe* followed the non-metered strategy, while all other papers used a metered paywall.

Literature in marketing has documented the significant effects digital paywalls have had on news readership. Kim et al. (2020) examine 42 local newspapers and find that paywall introduction has a negative impact on pageviews for 36 of the 42 newspapers, with an average decrease in newspaper daily web traffic of 30%. Pattabhiramaiah et al. (2019) study the *New York Times* paywall adoption, finding a 13% decrease in the number of unique visitors to the *New York Times* website after adoption.

It is reasonable to believe that changes in investor usage of local news may have detectable aggregate capital market effects. Prior literature has established that the tone of local newspaper coverage has a significant effect on local firm valuations (Gurun and Butler (2012)), that local media coverage contributes significantly to local firm momentum (Hillert et al. (2014)), and that local news coverage of earnings announcements has a causal effect on trading volume (Engelberg and Parsons (2011)). Additionally, over my sample period, local newspapers were a dominant source of news in the United States. A Pew Research study from March, 2010 found that on a typical day, 50% of Americans read news in a local newspaper while only 17% read news in a national newspaper such as *The New York Times* or *USA Today* (Purcell et al. (2010)).

3. Research Design and Variable Construction

3.1 Research Design

To answer my research questions, I employ a stacked difference-in-difference (DiD) research design using the staggered adoption of digital paywalls by 8 major U.S. local newspapers. To identify these newspapers, I start with the 20 largest U.S. local newspapers by 2010 daily circulation according to Editor and Publisher.[6] I keep local newspapers with Ravenpack coverage over my sample period. I remove local newspapers located in cities with multiple local newspapers in the top 20 largest newspapers by circulation.[7] Table 1 Panel A lists the eight newspapers and their paywall adoption dates, hand collected from historical news stories and press releases.

[6]Editor and Publisher is a trade news magazine that reports annual circulation data, which includes print and digital readers, via its News Media DataBook.

[7]This filter only removes New York newspapers.

The stacked DiD overcomes biases in the traditional staggered DiD design documented by Baker et al. (2022) by (1) using a constant event window for each treatment event and (2) using only clean control units within each event for comparison. For each paywall event, I examine a constant event window spanning the four calendar quarters before and after each paywall adoption quarter. Often when implementing a paywall, newspapers will offer an introductory offer to readers, with the first one to three months offered at a significantly reduced price.[8] Because of this, I remove the paywall adoption quarter, and the quarters immediately before and after the adoption quarter from my main analyses to have a constant window pre- and post- adoption.[9]

I identify treated firms as firms in Compustat with headquarters located within 100 miles of the local newspaper headquarter zip code (Gurun and Butler (2012)). I focus on local firms given prior literature on the local bias of local newspapers (Gurun and Butler (2012), Engelberg and Parsons (2011)) and local investors (Seasholes and Zhu (2010), Coval and Moskowitz (1999)) and prior literature providing evidence that the trading activity of local investors affects economic aggregates for local firms, including turnover (Jacobs and Weber (2012), Loughran and Schultz (2005)), valuations (Hong et al. (2008)), and returns (Kumar et al. (2011), Shive (2012)).

Within each paywall adoption event, I identify control firms as firms in Compustat with headquarters in cities with a local newspaper that has not adopted a digital paywall between the firms' q-4 to q+4 earnings announcement dates, where q is the event paywall adoption quarter. Similar to Cengiz et al. (2019), I allow for previously treated firms to serve as controls, as long as their treatment status did not change over the event window. However, use of previously treated units as controls can bias treatment effect estimates in the presence of dynamic treatment effects (Baker et al. (2022)). As shown in Table 12 Panel A, inferences are consistent when imposing a stricter requirement and only allowing later-treated units to serve as controls, though this approach limits the number of events that can be studied.

Table 1 Panel B reports the number of quarterly observations and the number of treated and control firms for each paywall adoption event. Given I only expect paywall adoption to have an effect on earnings announcement outcomes for local firms that receive local news earnings announcement coverage, I classify treated and control firms within each event into three categories: (1) *Covered Firms* are firms that receive earnings announcement coverage from their local newspaper at least once in both the pre- and post- paywall adoption periods; (2) *Non-Covered Firms* are firms that never receive earnings announcement coverage from their local newspaper in the event window; and (3) *Inconsistent Firms* receive earnings announcement coverage from their local newspaper pre- or post-paywall adoption, but not both. I label a firm's earnings announcement as having local newspaper earnings announcement coverage if the firm's local newspaper released an article about the firm with Ravenpack Relevance score equal to 100 in the [-1, +1] window surrounding the earnings

[8]The *Los Angeles Times*, *Chicago Tribune*, and *Boston Globe* each offered readers a price of 99 cents for an introductory period after paywall adoption.

[9]As shown in Table 12 Panel B, inferences remain consistent without dropping q-1 and q+1.

announcement date. Table 1 Panel C reports the number of quarterly observations with non-missing variables and number of firm-event observations for each category. I focus my main analyses on the sample of *Covered Firms*, and I use the sample of *Non-Covered Firms* for a placebo test and an additional difference-in-difference-in-difference specification, tabulated in Table 11 Panels A and B.

In my main analyses, the event datasets are stacked together, and a two-way fixed effect regression is estimated on the stacked dataset, with dataset specific unit- and time-fixed effects (Baker et al. (2022)) and standard errors clustered at the city-quarter level, using the following equation:

$$[Outcome_{i,q,e}] = \beta_1 \cdot Post_{q,e} \cdot Treat_{i,e} + \sum_{j=2}^{k} \beta_j \cdot Control_{i,q,e} + \psi_{i,e} + \gamma_{q,e} + \epsilon \qquad (1)$$

3.2 Variable Construction

The first $[Outcome_{i,q,e}]$ for firm i in quarter q and event e I examine is abnormal trading volume in the earnings announcement window. I define *Abnormal Volume* as the log-difference in average trading volume in the day of and day immediately after the earnings announcement and the average trading volume in a control period, defined as days [-41,-11] relative to the earnings announcement, following Dellavigna and Pollet (2009) and Hirshleifer et al. (2009).[10] I further examine abnormal volume by different investor types, measuring *Abnormal Retail Volume* and *Abnormal Large Volume* in the same manner, with retail trades identified in TAQ following the Boehmer et al. (2021) methodology and large trades identified in TAQ as trades over $50,000, following Bushee et al. (2020) and Dambra et al. (2023).

To examine liquidity, I measure *Abnormal Bid-Ask Spread* (*Abnormal Depth*) as the intraday average percent effective spread (the log of the average intraday bid and offer depth) obtained from TAQ and averaged over the two-day period beginning on the earnings announcement date, minus the same over a control period, defined as days [-41, -11], following Blankespoor et al. (2018) and Campbell et al. (2023). To measure the speed at which earnings information is incorporated into prices, I focus on the intraperiod-efficiency (*IPE*). The *IPE* measures the speed of price response over days [0, +5] relative to the earnings announcement date using an area-under-the-curve approach. Larger values of *IPE* represent faster price dicovery. I compute *IPE* following the methodology in Blankespoor et al. (2018) to correct for overreactions. I also present results for *IPE-Unadjusted*, which does not account for overreactions. Appendix I provides the formulas applied to compute each measure. For both measures of *IPE*, to reduce the influence of outliers due to small denominators, observations with one-week cumulative abnormal return of less than 2% are dropped. In untabulated analyses, I confirm that results for these variables are consistent when keeping all observations and applying ordinal logistic regression to the decile rank of the variable.

[10] Results are robust to alternative measures of abnormal trading volume, including standardized abnormal volume as in Barber et al. (2023) and standardized abnormal turnover as in Beaver et al. (2020).

In additional analyses, I examine the abnormal count of 8-K downloads on EDGAR by local and non-local investors during the earnings announcement window. I follow the methodology in Drake et al. (2015) to identify plausibly human downloads and remove automated downloads. To determine the location of downloads, I obtain historical Maxmind Geolocation data from Sood (2020).[11] MaxMind provides location-based variables such as Country, City, ZipCode, and latitidue/longitude for ranges of IP addresses. I define local IP addresses as IP addresses with a latitude/longitude within 100 miles of the latitude/longitude of the local newspaper headquarter zip code. Following deHaan et al. (2015), I measure *Abnormal Local Downloads* as the log-adjusted sum of 8-K downloads on EDGAR from local IP addresses from days [0, +1] surrounding the earnings announcement less the log-adjusted trailing average of the sum of 8-K downloads from local IP addresses on the same two weekdays over the preceding seven weeks. I compute *Abnormal Non-Local Downloads* in the same manner as *Abnormal Local Downloads*, using the total count of 8-K downloads from non-local IP addresses. I further distinguish local downloads specifically by *retail* investors with *Abnormal Local Retail Downloads*, the abnormal number of 8-K downloads from plausibly retail local investors. I identify plausibly retail investor 8-K downloads as downloads initiated from IP addressed linked to one of the top ten U.S. internet service providers (e.g. Verizon, Comcast), following Drake et al. (2020).[12]

I follow prior literature to control for firm attributes and announcement-specific variables likely to be associated with media coverage and market reactions to earnings releases (Guest (2021), Bushee et al. (2010)). Firm attributes include *Size*, the log-adjusted market capitalization, *MTB*, the market-to-book ratio, *Institutional Ownership*, the proportion of shares held by institutional owners, *Analysts*, the log-adjusted count of analysts issuing earnings forecasts, *Return Volatility*, the standard deviation of monthly returns over the prior year, *Employees*, the log-adjusted year end number of employees, and *Shareholders*, the log-adjusted year end number of shareholders. Announcement-specific variables include *SUE*, the difference between actual earnings per share (EPS) and the median forecasted EPS, obtained from I/B/E/S analyst forecasts issued within 90 days prior to the earnings announcement, scaled by quarter end stock price, $|SUE|$, the absolute value of *SUE*, *Negative Surprise*, an indicator equal to one if *SUE* is less than zero, and *National Media*, the log-adjusted count of national media articles with Ravenpack Relevance Score equal to 100 written about the firm in the [-1, +1] window surrounding the earnings announcement. [13,14]

[11]While MaxMind does not provide archival data, Sood (2020) provides historical downloads of Maxmind data via Harvard Dataverse. I use the MaxMind GeoLite City Database downloaded from April 9, 2015, the download date closest to the end of my sample period.

[12]Drake et al. (2020) find that download activity from plausibly retail investors is not associated with measures of future firm performance, consistent with these downloads stemming from less sophisticated investors.

[13]*SUE*, $|SUE|$, and *Negative Surprise* are constructed following Blankespoor et al. (2018). Results remain consistent when computing SUE as the difference between earnings before extraordinary items for quarter q and the corresponding earnings for quarter $q-4$, normalized by the share price at the end of quarter q.

[14]National media sources include *The Wall Street Journal*, *Barron's*, *Forbes*, *CNBC*, *The New York Times*, and *Bloomberg News*.

In regression analyses, the decile-rank of SUE and $|SUE|$ are used. I winsorize all continuous variables at 1% and 99% to account for the presence of outliers.

Table 2 Panel A reports descriptive statistics for the full sample of quarterly observations and Table 2 Panel B presents descriptive statistics for the sample of *Covered Firms*. Compared to the full sample, *Covered Firms* are larger, receive more national media and analyst coverage, and have larger abnormal EDGAR download activity by local investors. Table 2 Panel C focuses on the sample of *Covered Firms* and reports pre-period mean values and differences in pre-period mean values for outcome and control variables for treated and control observations. Few variables experience significant differences across treated and control observations, and significant differences when present are small in economic magnitude.

4. Results

4.1 Trading Volume

I begin my analyses by examining whether *Covered Firms* in paywall-adopting cities experience changes in *Abnormal Volume* following paywall adoption. I predict that digital paywall adoption will have a negative effect on trading volume for treated *Covered Firms*. Investors have limited attention, and prior literature has documented that media coverage increases investor attention and the likelihood of trading (Barber and Odean (2008), Blankespoor et al. (2018)). To the extent a local newspaper's audience is reduced by the introduction of the paywall, I expect investor attention and related trading activity to also be reduced by paywall adoption.

Table 3 Column (1) (Column (2)) estimates Equation (1) for *Abnormal Volume* without controls (with controls). The negative, significant coefficient for $Post * Treat$ in Table 3 Columns (1) and (2) implies that relative to *Covered Firms* in non-paywall adopting cities, *Covered Firms* in paywall adopting cities experience a significant reduction in *Abnormal Volume* following paywall adoption. The decrease in *Abnormal Volume* in Column (2) is equivalent to 7% of the mean value of *Abnormal Volume*.

Given that institutional investors are more likely than retail investors to follow firm news (Blankespoor et al. (2018)), and that retail investors are likely more sensitive to the cost of news, I expect the trading activity of retail investors to be more sensitive to the decreased accessibility of local news than the trading activity of institutional investors. I examine *Abnormal Retail Volume* and *Abnormal Large Volume* separately in Columns (3) - (6). Consistent with expectation, in Table 3 Columns (3) and (4), the negative, significant coefficient for $Post * Treat$ suggests that treated *Covered Firms* experience a significant reduction in *Abnormal Retail Volume* relative to control *Covered Firms*. The coefficient in Column (4) implies a decrease in *Abnormal Retail Volume* equivalent to 20% of the mean value of *Abnormal Retail Volume*, a decrease larger in economic magnitude than that of *Abnormal Volume*. Turning to *Abnormal Large Volume* in Columns (5) and (6) of Table 3, while I find a negative, significant coefficient for $Post * Treat$ in Column (5), the coefficient is no longer significant when control variables are included in Column (6). Overall, the evidence suggests that the decreased accessibility of local news caused by digital paywall adoption has a discernible negative impact on *Abnormal Volume* for treated *Covered Firms*, and that this effect appears to be driven by changes in retail investors' trading behavior.

4.2 Liquidity

Next, I examine how the decreased accessibility of local news affects liquidity for *Covered Firms*. In Table 4, I focus on *Abnormal Bid-Ask Spread* in the earnings announcement window in Columns (1) and (2) and *Abnormal Depth* in the earnings announcement window in Columns (3) and (4). Given market liquidity and information asymmetry can be reflected in either or both spreads or depth, it is important to examine the two in conjunction with each other (Lee et al. (1993)). On one hand, digital paywall adoption could have negative effects on liquidity if the decreased accessibility of local news increases information processing costs, leading to more information asymmetry, more price protection, and less willingness to trade (Blankespoor et al. (2018), Grossman and Stiglitz (1980), Diamond and Verrecchia (1991)). Additionally, the decreased trading volume documented in Table 3 could lead to a more shallow market. On the other hand, the decreased trading volume documented above could also reduce concerns about privately informed counter-parties and decrease *perceived* information asymmetry, leading to improvements in liquidity (Easley and O'Hara (1992)).

In Table 4 Columns (1) and (2), the positive, significant coefficient on $Post{*}Treat$ implies that *Covered Firms* in paywall-adopting cities experience a significant increase in *Abnormal Bid-Ask Spread* relative to *Covered Firms* in non-adopting cities. Table 4 Columns (3) and (4) report similar results for *Abnormal Depth*, with treated *Covered Firms* experiencing a significant decrease in *Abnormal Depth* following paywall adoption relative to control *Covered Firms*. In terms of economic magnitude, the decrease in *Abnormal Depth* is equivalent to 15% of the standard deviation of *Abnormal Depth*. The combination of increased bid-ask spreads and decreased trading depth suggest declines in liquidity for treated *Covered Firms*, consistent with digital paywall adoption increasing information asymmetry for *Covered Firms*.

4.3 Speed of Price Discovery

Next, I examine whether *Covered Firms* experience a change in the speed of price discovery following paywall adoption. It is ex-ante unclear whether digital paywall adoption will enhance or impede price discovery for *Covered Firms*. On one hand, media coverage of earnings announcements has been documented to improve the speed of price discovery (Guest (2021)). On the other hand, media coverage has been shown to drive attention-driven or biased trading, specifically from retail investors (Barber and Odean (2008)). Given the results in Table 3 which document a decrease in abnormal trading volume by retail investors in the earnings announcement window, the effect of paywall adoption on price discovery is unclear.

In Table 5, I apply Equation (1) to *IPE* and *IPE-Unadjusted*, both of which capture the speed at which earnings information is incorporated into price. For both measures, observations with one-week cumulative abnormal return of less than 2% are dropped to reduce the influence of outliers (Campbell et al. (2023), Blankespoor et al. (2018)). Larger values of *IPE* and *IPE-Unadjusted* represent faster price discovery. I focus on *IPE* rather than the earnings response coefficient (ERC) because the ERC is conceptually more a measure of earnings informativeness, precision, and credibility (Guest (2021), Holthausen and Verrecchia (1990)).

The negative, significant coefficient on $Post * Treat$ in both columns for both measures implies that treated *Covered Firms* experience a significant reduction in the speed at which earnings information is incorporated into price relative to control *Covered Firms*.[15] In terms of economic magnitude, the coefficient in Panel A Column (2) implies a decrease in *IPE* equivalent to 10% of the mean value of *IPE*.

Taken together with the results for *Abnormal Volume* in Table 3, the results in Table 5 suggest that by limiting accessibility of local news, digital paywalls reduce trading volume, specifically by retail investors, and slow the speed of price discovery. This finding is surprising in light of the substantial evidence on the under-performance of retail traders and the potential for retail traders to be susceptible to behavioral biases (Barber and Odean (2000), Barber and Odean (2008)). To reconcile these results, I examine two potential mechanisms driving the reduced speed of price discovery following paywall adoption.

First, it possible that the reduction in trading volume documented in Table 3 is accompanied by a reduction in informed or price efficiency-enhancing trading. To provide support for this mechanism, I examine local investors' fundamental information acquisition following paywall adoption. Finding that local investors reduce their fundamental information acquisition after media coverage is less accessible would be consistent with paywall adoption reducing efficiency-enhancing trading.

Second, it is possible that the reduced speed of price discovery after paywall adoption is caused by the reduced liquidity documented in Table 4. Stock liquidity increases the rate at which information is incorporated into price, and this relationship is stronger for negative news than positive news (Cheng et al. (2023)). If changes in liquidity are driving the changes in price discovery, then the effects of paywall adoption on price discovery will be strongest for firms with negative earnings news. To provide support for this mechanism, I re-examine the effects of paywall adoption on *IPE* for sub-samples of *Covered Firms* formed by measures of earnings news.

I find support for both explanations in Table 6. In Panel A of Table 6, I examine fundamental information acquisition by local and non-local investors. In columns (1)-(4) I apply Equation (1) to *Abnormal Local Downloads*, the abnormal number of 8-K downloads from local investors, identified by IP addresses with latitude/longitude within 100 miles of the local newspaper headquarter zip code, and *Abnormal Local Retail Downloads*, the abnormal number of 8-K downloads from plausibly retail local investors. In Table 6 Columns (1) and

[15] In untabulated analyses, I find consistent evidence when keeping all observations and applying an ordinal logistic regression to the decile-rank of *IPE*.

(2), I find evidence of a significant reduction in abnormal 8-K downloads by local IP addresses following paywall adoption for treated *Covered Firms* relative to control *Covered Firms*. Table 6 Columns (3) and (4) provide evidence of significant reductions in specifically local *retail* investor download activity after paywall adoption. Table 6 Columns (5) and (6) show no significant changes in abnormal 8-K downloads by non-local investors, alleviating concerns that the results in Columns (1)-(4) after driven by overall changes in download activity. These findings are consistent with local newspaper coverage of earnings announcements prompting further information acquisition by readers, and with digital paywall adoption reducing readership and thus reducing additional fundamental information acquisition and harming price discovery.

In Panel B of Table 6, I apply Equation (1) to *IPE* for sub-samples of *Covered Firms*. In Columns (1) and (2), *Covered Firms* are split into *Low SUE* and *High SUE* sub-samples by the median value of *SUE* over the event window. In Columns (3) and (4), *Covered Firms* are split into *High Fraction Negative SUE* and *Low Fraction Negative SUE* sub-samples by the median fraction of earnings announcements in the event window for which the firm misses analyst expectations (i.e. $NegSUE == 1$). I find that the negative effect of paywall adoption on *IPE* for *Covered Firms* is driven by firms with more negative earnings news, consistent with the change in liquidity driving the effect on price dicsovery. Given I find supporting evidence for both mechanisms, I don't draw any conclusions about the relative importance of each for the decrease in speed of price discovery.

4.4 Local Newspaper Dissemination and Capital Market Outcomes

The above analyses examine whether digital paywall adoption alters the impact of local newspaper earnings announcement dissemination on trading volume, liquidity, and speed of price discovery. Implicit in these tests is an assumption that local newspaper dissemination of earnings announcements does has an impact on trading volume, liquidity, and price discovery. This assumption is supported by evidence from prior literature that has documented the causal effects of local newspaper earnings announcement dissemination and general news outlet earnings announcement dissemination on capital market outcomes. Engelberg and Parsons (2011) exploit geographic variation across individual investors using data from a retail brokerage to document the causal impact of local newspaper dissemination of earnings announcements on trading activity. Guest (2021) exploit restructuring events at *The Wall Street Journal* to show that earnings announcement media coverage causes improvements to liquidity and the speed of price discovery. Similarly, Blankespoor et al. (2018) exploit the staggered implementation of robo-journalism earnings announcement dissemination by the *Associated Press* to show that media dissemination has positive causal effect on liquidity and trading volume.

To provide assurance that these documented associations exist in my sample, I examine the association between local newspaper earnings announcement coverage and trading volume, liquidity, and speed of price discovery in Table 7. To do so, I focus on the full sample of earnings announcement observations for *Non-Covered Firms*, *Covered Firms* and

Inconsistent Firms. I keep one observation per firm-quarter from the full stacked datasets and run simple regressions of *Abnormal Volume*, *Abnormal Retail Volume*, *Abnormal Large Volume*, *Abnormal Depth*, and *IPE* on *Local Coverage*, an indicator equal to one of the firm's local newspaper covered the earnings announcement, and controls. I also include firm and quarter fixed effects. The results in Table 7 show that after controlling for other determinants, earnings announcements that receive local newspaper coverage are associated with significantly higher *Abnormal Volume* and *Abnormal Retail Volume*, but not significantly different *Abnormal Large Volume*. Additionally, earnings announcements with local newspaper coverage are associated with improved liquidity and faster speed of price discovery. While this evidence cannot be awarded a causal interpretation, given a local newspaper's decision to cover a local firm's earnings announcement is endogenous to the firm's news and capital market outcomes, the associations provide confidence that the assumption that local media coverage of earnings announcements does have capital market effects is supported.

4.5 Robustness

4.5.1 Cross-Sectional Analyses

To provide supporting evidence that the above documented effects are driven by the decreased accessibility of local news following paywall adoption, I conduct various cross-sectional analyses. I start with firm visibility. I expect the impact of digital paywall adoption on market outcomes for *Covered Firms* to be more pronounced for less visible firms, as local investors, the readers affected by local newspaper paywall adoption, are more likely to be marginal investors of less visible firms (Hong et al. (2008)). Changes in the accessibility of local newspapers' dissemination of public disclosures is likely to have the largest effect on firms for which local investors are the marginal investors. I employ two measures of firm visibility. First, in Table 8 Panel A I split the sample of *Covered Firms* into large and small firms, using the median value of *Size*. Second, in Table 8 Panel B I split the sample of *Covered Firms* into high and low news firms, using the median value of the fraction of pre-period earnings announcements that receive media coverage from a national media outlet.

Across Panel A and Panel B, a consistent pattern emerges. The impact of paywall adoption on retail trading volume, liquidity, and speed of price discovery is most pronounced and largest for low visibility firms (i.e. *Low Size* and *Low News*). This evidence is consistent with the expectation that the decreased accessibility of local news will have the largest effect on firms for which local investors, the readers of local newspapers, are the marginal investor.

Next, in Table 8, Panel C, I split the sample into high and low institutional ownership firms, using the pre-period median value of *Institutional Ownership*. I expect the information environments of retail investors to be affected by paywall adoption to a greater extent than the information environments of institutional investors, given institutional investors actively follow firm news and have more resources than individual investors (Blankespoor et al. (2018)). Thus, I expect the effect of digital paywall adoption on aggregate market outcomes to be more pronounced for firms with a higher fraction of retail investor ownership. The results in Table 8 Panel C are largely consistent with this expectation. The effect of paywall adoption on retail trading and liquidity for *Covered Firms* is most pronounced for or largest for firms with low institutional ownership (high retail investor ownership).

4.5.2 Changes in Coverage

Prior literature has documented that incentives to appease advertisers influence local newspapers' coverage of local firms. Specifically, Gurun and Butler (2012) document that local newspapers use more positive language when covering local firms, and they contribute some of this media tilt to local firms' local media advertising expenditures. Thus, there is a concern that the adoption of a digital paywall and corresponding increased focus on revenue from readers rather than advertisers may cause local newspapers to alter their coverage of local firms, and that these changes in coverage are driving my results. To alleviate such concerns, I examine changes in coverage for the sample of *Covered Firms* following paywall adoption.[16] I focus on three measures of media coverage: (1) *Coverage*, an indicator equal to one if a firm's local newspaper issued an article about the firm's earnings announcement, (2) *Local News Articles*, the log-adjusted count of the number of articles written about the firm's earnings announcement by its local newspaper, and (3) *Tone*, the average tone of articles written about the firm's earnings announcement by the firm's local newspaper, using the Ravenpack Composite Sentiment Score (CSS). Given the CSS is not available for all Ravenpack observations, the sample for this test is smaller. The CSS ranges from 0-100, so I adjust the value of CSS by subtracting 50 and dividing by 50 to compute *Tone*. I apply Equation (1) to these three variables in Columns (1)-(3) of Table 9 Panel A. The coefficient on $Post * Treat$ is insignificant across all three columns, providing evidence that local newspapers are not significantly altering their coverage of local firms after paywall adoption.

An additional concern may stem from the link between local media and national media. It is possible that local media outlets are sources for national media outlets (Gurun and Butler (2012)). To the extent paywall adoption imposes information acquisition costs on the creators of national media content, the national media coverage of local firms may be affected by paywall adoption, and this change in national media coverage could be driving my results. I examine this question in Table 9 Panel B, using *Newswires*, the log-adjusted count of Dow Jones Newswires written about the firm in the [-1, +1] earnings announcement window with Ravenpack Relevance Score equal to 100, and *National Media*, the log-adjusted count of national media articles written about the firm in the [-1, +1] earnings announcement window with Ravenpack Relevance Score equal to 100. For both measures of national media attention, I find no changes in coverage for treated *Covered Firms* relative to control *Covered Firms*. This finding alleviates concerns that changes in national media coverage are driving my results.

[16] In untabulated analyses, I find similar results when examining the full sample of observations, including *Covered Firms*, *Non-Covered Firms*, and *Inconsistent Firms*.

4.5.3 Parallel Trends

In Table 10, I confirm the validity of the parallel trends assumption for the difference-in-difference design. Specifically, I alter Equation (1) and replace $Post * Treat$ with interactions of $Treat$ with indicators for each quarter relative to paywall adoption. Quarter Q-4 is removed and used as a baseline. Across the five columns, I apply this equation to my main variables of interest: *Abnormal Retail Volume*, *Abnormal Bid-Ask Spread*, *Abnormal Depth*, *IPE*, and *Abnormal Local Downloads*. Across all five variables, there are no significant differences in the difference in outcomes between treated and control users in Q-3 or Q-2 relative to the baseline (Q-4), consistent with no significant pre-trends in my outcomes of interest. The effects of paywall adoption are only observed for each of the five variables from Q+2 onward.

4.5.4 Placebo Test and Triple Difference

In Table 11 Panel A, I conduct a placebo test using the sample of *Non-Covered Firms*, which receive no earnings announcement coverage from their local newspaper in both the pre- and post- adoption periods. For *Non-Covered Firms*, I do not expect digital paywall adoption to have any effect on my earnings announcement outcomes of interest. I apply Equation (1) to *Abnormal Retail Volume*, *Abnormal Bid-Ask Spread*, *Abnormal Depth*, *IPE*, and *Abnormal Local Downloads* in Table 11 Columns (1)-(5) using the sample of *Non-Covered Firms*. Across all five columns, the coefficient on $Post * Treat$ is insignificant.

I use the sample of *Non-Covered Firms* as a placebo test to validate the assumption that the documented effects for *Covered Firms* are driven by the paywall adoption, rather than an unrelated factor that is simultaneously causing both the local newspaper to change its revenue model and the changes in local firms' trading volume, liquidity, and speed of price discovery. However, given I document that the sample of *Non-Covered* firms experience no treatment effects following paywall adoption, it is possible to consider these firms as an additional control group. In Table 11 Panel B, I analyze *Covered* and *Non-Covered Firms* together, employing a triple-difference design. Specifically, I examine whether the difference in my outcomes of interest between *Covered Firms* and *Non-Covered Firms* for firms in treated cities is significantly different than the difference in my outcomes of interest between *Covered Firms* and *Non-Covered Firms* in control cities following paywall adoption. I employ the following equation:

$$\begin{aligned}[Outcome_{i,q,e}] =&\beta_1 \cdot Post_{q,e} \cdot Treat_{i,e} + \beta_2 \cdot Post_{q,e} \cdot Covered_{i,e} \\ &+ \beta_3 \cdot Post_{q,e} \cdot Treat_{i,e} \cdot Covered_{i,e} + \sum_{j=4}^{k} \beta_j \cdot Control_{i,q,e} \\ &+ \psi_{i,e} + \gamma_{q,e} + \epsilon\end{aligned} \tag{2}$$

I tabulate the results of applying Equation (2) to the combined sample of *Covered Firms* and *Non-Covered Firms* for my main outcomes of interest in Table 11 Panel B. For brevity, I report the first three coefficients (β_1, β_2 and β_3). Consistent with my main analyses, the coefficient on $Post * Treat * Covered$ across columns suggest that treated *Covered Firms* experience a significant reduction in *Abnormal Retail Volume*, declines in liquidity and speed of price discovery and a significant reduction in *Abnormal Local Downloads*.

4.5.5 Alternative Sample Construction

In my last robustness analyses, I confirm that results are not sensitive to choices in sample construction discussed in Section 3. First, in Table 12 Panel A, I run my analyses on an alternative sample using a more strict definition for control firms. Specifically, I remove all previously treated units from each events' control group. I apply Equation (1) to my main variables of interest in Columns (1)-(6) using this strict control group sample. Results for this alternative sample are consistent with the main analyses, suggesting issues with dynamic treatment effects and the inclusion of previously treated units as controls are not driving my results (Baker et al. (2022)).

Last, as discussed in Section 2, I remove the quarters immediately prior to and following paywall adoption due to the introductory offers typically offered by local newspapers when adopting digital paywalls. In Table 12 Panel B, I examine my main outcomes of interest on a larger sample that includes Q-1 and Q+2. Results are consistent with the main analyses.

5. Conclusion

Since the advent of the internet in the 1990s, traditional media outlets have struggled to survive. While newspapers initially provided news free of charge online when they first created their websites, declines in revenues from print circulation and insufficient gains in digital advertising revenue prompted newspapers to shift their monetization strategy and begin charging readers for access to online news. In this study, I exploit the staggered adoption to digital paywalls by eight local newspapers to document the capital market implications of this shift in news accessibility, specifically as it relates to the role news outlets play in disseminating earnings announcements.

I examine aggregate market outcomes documented by prior literature to be influenced by media dissemination of earnings announcements: trading volume, liquidity, and speed of price discovery. My findings suggest that the impact of local newspaper dissemination of earnings announcements on investor attention and associated trading volume is muted after information acquisition costs are imposed on newspaper readers. I show that digital paywall adoption leads to decreased liquidity and slower speed of price discovery for firms that receive persistent coverage from adopting newspapers.

These findings are important in the context of the larger evolution and decline of traditional media, as increasing numbers of media outlets beyond local newspapers have adopted digital paywalls, reducing news accessibility. Most recently, business-focused outlets *Bloomberg News* and *Forbes* adopted digital paywalls in 2019 and 2020, respectively, and major national news sources *USA Today* and *Reuters* implemented paywalls in 2021. The findings presented suggest that these changes to news accessibility may mute the influence of traditional media outlets in capital markets and may disproportionately impact the information environments of individual investors. However, I caveat I cannot claim generalizability of my findings to digital paywall adoption by national news outlets or business-focused news outlets, as investors' willingness to pay for these outlets may be different than investors' willingness to pay for local newspapers and the level of influence for these outlets may be different than that of local newspapers.

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Appendix I
Key Variable Definitions

Variable	Description
Dependent Variables	
Abnormal Volume	The log-difference between average trading volume over the two-day period beginning on the earnings announcement and the average trading volume on days [-41, -11] relative to the earnings announcement.
Abnormal Retail Volume	The log-difference between average retail volume over the two-day period beginning on the earnings announcement and the average trading volume on days [-41, -11] relative to the earnings announcement. Retail trades are identified in TAQ following the Boehmer et al., 2021 methodology.
Abnormal Large Volume	The log-difference between average large volume over the two-day period beginning on the earnings announcement and the average trading volume on days [-41, -11] relative to the earnings announcement. Large trades are identified in TAQ as trades over \$50,000 following Bushee et al., 2020.
Abnormal Bid-Ask Spread	The intraday average percent effective spread obtained from TAQ and averaged over the two-day period beginning on the earnings announcement date, minus the same average over days [-41, 11] relative to the earnings announcement, multiplied by 100.
Abnormal Depth	The log of the average intraday bid and offer depth obtained from TAQ averaged over the two-day period beginning on the earnings announcement date, minus the same average over days [-41, -11] relative to the earnings announcement.
IPE	Intraperiod efficiency measure of the speed with which earnings information is incorporated into price, measured over the five trading days following the announcement of earnings and adjusted for overreactions following Blankespoor et al., 2018. Calculated as $IPE_{(0,+5)} = \sum_{t=0}^{5}\left(\frac{\lvert CumAR_5 - CumAR_t\rvert}{\lvert CumAR_5\rvert}\right)$, where $CumAR$ is the cumulative abnormal buy- and-hold return from day 0 to day t. The primary test sample excludes observations with an absolute $CumAR_5$ of less than 2%.
IPE-Unadjusted	Intraperiod efficiency measure, not adjusted for overreactions. Calculated as $IPE_{\text{Unadj}(0,+5)} = \sum_{t=0}^{4}\left(\frac{CumAR_t}{CumAR_5}\right) + 0.5$. The primary test sample excludes observations with an absolute $CumAR_5$ of less than 2%.
Abnormal Local Downloads	The log-adjusted sum of 8-K downloads on EDGAR from local IP addresses from days [0, +1] surrounding the earnings announcement less the log-adjusted trailing average on the same two weekdays over the preceding seven weeks, following deHaan et al., 2015. Local IP addresses are identified as IP addresses with a latitude/longitude within 100 miles of the latitude/longitude of the local newspaper headquarter zip code.

Abnormal Local Retail Downloads	The abnormal count of 8-K downloads on EDGAR from local, plausibly retail investor IP addresses, computed in the same manner as *Abnormal Local Downloads*. Plausibly retail IP addresses are identified following Drake et al., 2020 as IP addresses registered to the top-ten U.S. internet service providers.
Abnormal Non-Local Downloads	The log-adjusted sum of 8-K downloads on EDGAR from non-local IP addresses from days [0, +1] surrounding the earnings announcement less the log-adjusted trailing average on the same two weekdays over the preceding seven weeks, following deHaan et al., 2015. Non-local IP addresses are identified as IP addresses with a latitude/longitude more than 100 miles from the latitude/longitude of the local newspaper headquarter zip code.

DiD Variables

Post	An indicator variable equal to one for quarterly observations after the paywall adoption quarter and zero otherwise.
Treat	An indicator variable equal to one for firms affected by paywall adoption, defined as firms with headquarters within 100 miles of the adopting local newspaper zipcode, and zero otherwise.

Control Variables

SUE	The difference between actual EPS and the median of forecasted EPS, obtained from I/B/E/S analyst forecasts issued within 90 days prior to the earnings announcement, scaled by quarter-end stock price. In regression analyses, the decile rank of *SUE* by year is used, following Blankespoor et al., 2018.
$\lvert SUE \rvert$	The absolute value of *SUE*. In regression analyses, the decile rank of $\lvert SUE \rvert$ by year is used, following Blankespoor et al., 2018.
Negative Surprise	An indicator equal to one if *SUE* is less than zero, and zero otherwise.
Size	The log-adjusted quarter-end market value of equity.
MTB	The ratio of market value of equity to book value of equity.
Institutional Ownership	Fraction of shares held by institutional investors.
National Media	The log-adjusted count of news articles with a Ravenpack Relevance Score equal to 100 issued about the firm in the [0, +1] window surrounding the earnings announcement date from national media outlets, including *The Wall Street Journal*, *Barron's*, *Forbes*, *CNBC*, *The New York Times*, and *Bloomberg News*.
Analysts	The log-adjusted count of analysts following the firm.
Return Volatility	The standard deviation of monthly returns over the year prior to the earnings announcement.
Employees	The log-adjusted fiscal year-end number of employees, obtained from Compustat.
Shareholders	The log-adjusted fiscal year-end number of shareholders, obtained from Compustat.

Table 1

Paywall Adoption Dates, Observations by Event, and Firm Classifications

This table provides descriptive sample construction information. Panel A lists the cities, corresponding local newspapers, and paywall adoption dates used to construct the sample. Panel B reports the number of observations, number of firms, number of treated firms, and number of control firms for each adoption event. Treated firms are identified as firms with headquarters located within 100 miles of the local newspaper headquarter zipcode. Control firms are identified within each event as firms local to one of the eight cities listed in Panel A whose local newspaper did not adopt a digital paywall between the firm's q-4 to q+4 earnings announcement dates, where q is the event paywall adoption quarter. Panel C reports the number of quarterly observations and the number of firm-event observations for each earnings announcement coverage classification. *Covered Firms* receive earnings announcement coverage from their local newspaper at least once in both the pre- and post-paywall adoption periods. *Non-Covered Firms* do not receive any earnings announcement coverage from their local newspaper in both the pre- and post-paywall adoption periods. *Inconsistent Firms* receive earnings announcement coverage from their local newspaper in either the pre- or post-paywall adoption periods, but not both.

Panel A: Paywall Adoption Dates

City	Paper	Paywall Date
Boston	Boston Globe	2011-10-11
Los Angeles	Los Angeles Times	2012-03-05
Phoenix	Arizona Republic	2012-09-01
Chicago	Chicago Tribune	2012-11-01
Houston	Houston Chronicle	2012-11-19
Miami	Miami Herald	2012-12-19
San Francisco	San Francisco Chronicle	2013-04-01
Denver	Denver Post	2013-12-02

Panel B: Observations by Event

City	Observations	Firms	Treated (Local) Firms	Control Firms
Boston	3,753	820	176	644
Los Angeles	2,529	545	207	338
Phoenix	476	107	31	76
Chicago	1,950	414	163	251
Houston	1,993	425	174	251
Miami	1,328	293	42	251
San Francisco	3,078	653	275	378
Denver	4,264	912	77	835

Panel C: Observations by Classification

	Covered Firms	Non-Covered Firms	Inconsistent Firms
Quarterly Observations	3,021	6,970	7,571
Firm-Event Observations	532	1,352	1,443

Table 2
Descriptive Statistics

This table reports descriptive statistics for the full sample of quarterly observations, and multiple subsamples. Panel A reports descriptive statistics for the full stacked dataset. Panel B reports descriptive statistics for the subsample of *Covered Firms*. Panel C presents tests of differences in mean firm attributes between treated and control firms for the sample of *Covered Firms* during the pre-period.

Panel A: Full Sample Descriptive Statistics

Statistic	N	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)
Local News Articles	19,371	0.164	0.380	0.000	0.000	0.000
Abnormal Volume	19,371	0.599	0.528	0.244	0.576	0.940
Abnormal Retail Volume	19,371	0.712	0.705	0.240	0.662	1.133
Abnormal Large Volume	19,371	1.245	2.252	0.148	0.983	2.191
Abnormal Bid-Ask Spread	19,371	0.013	0.112	−0.004	0.005	0.023
Abnormal Depth	19,371	0.092	0.278	−0.074	0.085	0.252
IPE	13,582	3.660	1.689	3.125	4.074	4.643
IPE-Unadjusted	13,582	4.127	2.182	3.186	4.176	5.095
Abnormal Local Downloads	19,371	0.539	0.937	−0.069	0.560	1.209
Abnormal Local Retail Downloads	19,371	0.249	0.702	−0.134	0.000	0.693
Abnormal Non-Local Downloads	19,371	1.018	0.874	0.470	1.070	1.613
SUE	19,371	0.0002	0.092	−0.001	0.0004	0.002
|SUE|	19,371	0.020	0.090	0.001	0.002	0.006
Negative Surprise	19,371	0.337	0.473	0	0	1
Size	19,371	7.137	1.629	5.903	7.041	8.203
MTB	19,371	3.502	4.993	1.361	2.102	3.540
Institutional Ownership	19,371	0.685	0.312	0.510	0.794	0.918
National Media	19,371	1.742	1.736	0.000	2.197	3.219
Analysts	19,371	1.463	0.811	0.693	1.386	2.079
Return Volatility	19,371	0.111	0.057	0.070	0.099	0.137
Employees	19,371	1.250	1.179	0.303	0.873	1.939
Shareholders	19,371	1.031	1.227	0.122	0.432	1.656

Panel B: *Covered Firms* Descriptive Statistics

Statistic	N	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)
Local News Articles	3,021	0.395	0.521	0.000	0.000	0.693
Abnormal Volume	3,021	0.650	0.473	0.332	0.617	0.955
Abnormal Retail Volume	3,021	0.738	0.614	0.326	0.691	1.129
Abnormal Large Volume	3,021	1.198	1.878	0.391	0.940	1.722
Abnormal Bid-Ask Spread	3,021	0.010	0.069	−0.002	0.003	0.012
Abnormal Depth	3,021	0.077	0.269	−0.079	0.072	0.237
IPE	2,172	3.908	1.505	3.403	4.252	4.749
IPE-Unadjusted	2,172	4.363	1.969	3.462	4.359	5.225
Abnormal Local Downloads	3,021	0.694	0.952	0.000	0.714	1.386
Abnormal Local Retail Downloads	3,021	0.359	0.770	−0.134	0.000	0.934
Abnormal Non-Local Downloads	3,021	1.217	0.846	0.707	1.270	1.784
SUE	3,021	0.003	0.065	−0.0001	0.001	0.002
|SUE|	3,021	0.012	0.064	0.0004	0.001	0.003
Negative Surprise	3,021	0.258	0.437	0	0	1
Size	3,021	8.041	1.801	6.609	8.077	9.452
MTB	3,021	3.906	5.138	1.461	2.326	4.398
Institutional Ownership	3,021	0.763	0.264	0.682	0.850	0.925
National Media	3,021	2.783	1.875	0.000	3.332	4.234
Analysts	3,021	1.840	0.798	1.386	1.946	2.398
Return Volatility	3,021	0.106	0.054	0.068	0.094	0.131
Employees	3,021	1.883	1.445	0.604	1.622	3.016
Shareholders	3,021	1.384	1.421	0.181	0.842	2.217

Panel C: *Covered Firm* Pre-Period Comparisons

Variable	Treat Mean	Control Mean	Difference	t-stat	p-value
Local News Articles	0.462	0.446	0.016	0.510	0.610
Abnormal Volume	0.659	0.642	0.017	0.579	0.563
Abnormal Retail Volume	0.726	0.694	0.032	0.841	0.401
Abnormal Large Volume	0.351	0.317	0.035	0.289	0.773
Abnormal Bid-Ask Spread	0.003	0.006	−0.002	−0.443	0.658
Abnormal Depth	0.091	0.061	0.030	1.896	0.058
IPE	3.974	3.869	0.105	1.122	0.262
IPE-Unadjusted	4.448	4.300	0.147	1.144	0.253
Abnormal Local Downloads	0.669	0.586	0.083	1.468	0.143
Abnormal Local Retail Downloads	0.348	0.312	0.035	0.798	0.425
Abnormal Non-Local Downloads	1.278	1.194	0.084	1.641	0.101
SUE (Decile)	5.546	5.737	−0.190	−1.277	0.202
|SUE| (Decile)	4.740	4.742	−0.002	−0.011	0.991
Negative Surprise	0.273	0.237	0.036	1.380	0.168
Size	7.908	8.007	−0.099	−0.946	0.345
MTB	3.164	4.239	−1.075	−4.359	0.00001
Institutional Ownership	0.754	0.760	−0.006	−0.398	0.691
National Media	2.838	2.868	−0.030	−0.271	0.786
Analysts	1.767	1.861	−0.094	−1.985	0.048
Return Volatility	0.102	0.111	−0.009	−3.203	0.001
Employees	1.916	1.832	0.084	0.994	0.320
Shareholders	1.493	1.357	0.136	1.620	0.106

Table 3
Trading Volume

This table reports the results of *Eq. (1)* for *Abnormal Volume* in Columns (1) and (2), *Abnormal Retail Volume* in Columns (3) and (4), and *Abnormal Large Volume* in Columns (5) and (6) for the sample of *Covered Firms*. *Post* is an indicator equal to one in calendar quarters after paywall adoption. *Treat* is an indicator equal to one for local firms. All variables are defined in Appendix I. Firm-Event and Year-Quarter-Event fixed effects are included. t-statistics in parentheses are based on standard errors clustered by city-quarter. ***, **, and * denote significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

	Dependent variable:					
	Abnormal Volume		Abnormal Retail Volume		Abnormal Large Volume	
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Treat	−0.050*	−0.047*	−0.160***	−0.151***	−0.159*	−0.117
	(−1.831)	(−1.890)	(−4.310)	(−4.140)	(−1.887)	(−1.356)
SUE		0.008		0.021**		0.039
		(0.825)		(1.975)		(1.093)
|SUE|		0.013**		0.018**		0.005
		(2.177)		(2.076)		(0.173)
Negative Surprise		0.133***		0.171***		0.366**
		(3.118)		(2.784)		(2.240)
Size		0.013		−0.017		−0.433***
		(0.236)		(−0.266)		(−2.586)
MTB		−0.0003		0.0003		−0.007
		(−0.093)		(0.089)		(−0.602)
Institutional Ownership		0.008		0.066		−0.047
		(0.115)		(0.876)		(−0.096)
National Media		0.049***		0.067***		0.141***
		(4.562)		(5.217)		(3.056)
Analysts		−0.043*		−0.021		−0.180
		(−1.718)		(−0.624)		(−1.332)
Return Volatility		−0.939***		−1.164***		−2.034
		(−3.091)		(−3.368)		(−1.518)
Employees		0.106		0.233**		0.452**
		(1.317)		(2.139)		(2.023)
Shareholders		0.0003		0.026		−0.080
		(0.008)		(0.647)		(−0.447)
Firm-Event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter-Event FEs	Yes	Yes	Yes	Yes	Yes	Yes

Observations	3,021	3,021	3,021	3,021	3,021	3,021
Adjusted R^2	0.279	0.311	0.295	0.326	0.165	0.179

Table 4
Liquidity

This table reports the results of *Eq. (1)* for *Abnormal Bid-Ask Spread* in Columns (1) and (2), *Abnormal Depth* in Columns (3) and (4) for the sample of *Covered Firms*. *Post* is an indicator equal to one in calendar quarters after paywall adoption. *Treat* is an indicator equal to one for local firms. All variables are defined in Appendix I. Firm-Event and Year-Quarter-Event fixed effects are included. t-statistics in parentheses are based on standard errors clustered by city-quarter. ***, **, and * denote significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

	Dependent variable:			
	Abnormal Bid-Ask Spread		Abnormal Depth	
	(1)	(2)	(3)	(4)
Post*Treat	0.015***	0.016***	−0.044**	−0.043***
	(2.919)	(2.769)	(−2.565)	(−2.693)
SUE		0.001		−0.002
		(0.542)		(−0.389)
|SUE|		0.001		0.004
		(0.903)		(0.706)
Negative Surprise		0.002		0.024
		(0.432)		(0.794)
Size		0.004		−0.010
		(0.372)		(−0.433)
MTB		−0.001		0.0002
		(−1.393)		(0.056)
Institutional Ownership		−0.00003		0.034
		(−0.002)		(0.840)
National Media		0.002*		0.014***
		(1.780)		(2.714)
Analysts		0.003		−0.010
		(0.779)		(−0.444)
Return Volatility		−0.043		0.305
		(−0.707)		(1.108)
Employees		−0.011		0.064
		(−1.199)		(0.976)
Shareholders		0.019**		0.015
		(2.266)		(0.661)
Firm-Event FEs	Yes	Yes	Yes	Yes
Year-Quarter-Event FEs	Yes	Yes	Yes	Yes
Observations	3,021	3,021	3,021	3,021
Adjusted R^2	0.133	0.136	0.180	0.186

Table 5
Speed of Price Discovery

This table reports the results of *Eq. (1)* for measures of speed of price discovery for the sample of *Covered Firms*. Columns (1) and (2) report regression results for *IPE*, and Columns (3) and (4) report regression results for *IPE-Unadjusted*. The sample for all columns only includes quarterly observations with 5-day cumulative abnormal buy-and-hold return greater than 2 percent. *Post* is an indicator equal to one in calendar quarters after paywall adoption. *Treat* is an indicator equal to one for local firms. All variables are defined in Appendix I. Firm-Event and Year-Quarter-Event fixed effects are included. t-statistics in parentheses are based on standard errors clustered by city-quarter. ***, **, and * denote significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

	Dependent variable:			
	IPE		IPE-Unadjusted	
	(1)	(2)	(3)	(4)
Post*Treat	−0.385***	−0.389***	−0.332*	−0.358**
	(−2.808)	(−2.766)	(−1.920)	(−2.022)
SUE		0.034		−0.023
		(0.930)		(−0.380)
|SUE|		0.035		0.112***
		(1.072)		(2.838)
Negative Surprise		0.223		−0.029
		(1.059)		(−0.087)
Size		0.065		−0.110
		(0.309)		(−0.393)
MTB		−0.008		0.026
		(−1.368)		(1.202)
Institutional Ownership		0.549		−0.440
		(1.461)		(−1.281)
National Media		0.010		0.012
		(0.244)		(0.161)
Analysts		0.130		0.277*
		(1.387)		(1.882)
Return Volatility		−0.299		0.100
		(−0.243)		(0.057)
Employees		0.412		0.358
		(1.139)		(0.801)
Shareholders		−0.262		−0.249
		(−0.973)		(−0.920)
Firm-Event FEs	Yes	Yes	Yes	Yes
Year-Quarter-Event FEs	Yes	Yes	Yes	Yes
Observations	2,172	2,172	2,172	2,172
Adjusted R^2	0.026	0.029	0.018	0.029

Table 6
Abnormal 8-K Downloads and *IPE* Sub-Sample Analysis

This table examines the possible drivers of the reduction in *IPE* documented in Table 5. Panel A reports the results of *Eq. (1)* for *Abnormal Local Downloads* in Columns (1) and (2) and *Abnormal Non-Local Downloads* in Columns (3) and (4) for the sample of *Covered Firms*. Panel B reports the results of applying *Eq. (1)* to *IPE* for various sub-samples of *Covered Firms*. In columns (1) and (2) *Covered Firms* are split into *Low SUE* and *High SUE* firms by the median of firm's mean value of *SUE* over the event window. In columns (3) and (4), *Covered Firms* are split into *High Fraction Negative SUE* and *Low Fraction Negative SUE* sub-samples by the median fraction of earnings announcements in the event window for which the firm misses analyst expectations (*NegSUE* == 1). *Post* is an indicator equal to one in calendar quarters after paywall adoption. *Treat* is an indicator equal to one for local firms. All variables are defined in Appendix I. Firm-Event and Year-Quarter-Event fixed effects are included. t-statistics in parentheses are based on standard errors clustered by city-quarter. ***, **, and * denote significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

	Dependent variable:					
	Abnormal Local Downloads		Abnormal Local Retail Downloads		Abnormal Non-Local Downloads	
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Treat	−0.220***	−0.234***	−0.132**	−0.154**	−0.007	−0.017
	(−3.231)	(−3.356)	(−2.058)	(−2.278)	(−0.129)	(−0.326)
SUE		−0.005		0.004		−0.005
		(−0.344)		(0.281)		(−0.331)
|SUE|		−0.016		−0.028**		0.005
		(−1.291)		(−2.199)		(0.455)
Negative Surprise		−0.021		0.056		−0.011
		(−0.201)		(0.659)		(−0.116)
Size		−0.131		0.046		0.132
		(−1.445)		(0.498)		(1.627)
MTB		0.025***		0.022***		0.007
		(5.929)		(4.896)		(1.408)
Institutional Ownership		0.113		−0.312**		−0.067
		(0.865)		(−2.345)		(−0.494)
National Media		0.025		0.006		0.024
		(1.381)		(0.345)		(1.563)
Analysts		−0.074		−0.006		−0.056
		(−1.549)		(−0.114)		(−1.422)
Return Volatility		1.089*		0.603		0.018
		(1.734)		(1.263)		(0.032)
Employees		0.222		0.335**		0.164
		(1.196)		(2.128)		(1.136)

Shareholders		−0.076		−0.088		0.141*
		(−0.916)		(−1.304)		(1.748)
Firm-Event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter-Event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,021	3,021	3,021	3,021	3,021	3,021
Adjusted R^2	0.246	0.255	0.171	0.185	0.405	0.408

Panel B: *IPE* By Earnings News

	Dependent variable: IPE			
	Low SUE	High SUE	High Fraction Negative SUE	Low Fraction Negative SUE
	(1)	(2)	(3)	(4)
Post*Treat	−0.769***	0.163	−0.489**	−0.397
	(−3.792)	(0.429)	(−2.569)	(−1.177)
Chi-Squared	4.117		0.033	
p-value	0.042**		0.855	
Firm-Event FEs	Yes	Yes	Yes	Yes
Year-Quarter-Event FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1,132	1,040	1,443	729
Adjusted R^2	0.047	−0.005	0.025	0.040

Table 7
Local Media Coverage and Capital Market Outcomes

This table reports OLS regressions of measures of trading volume, liquidity, and speed of price discovery on *Local Coverage*, an indicator equal to one if a firm's local newspaper issued an article about the firm's earnings announcement in the [-1, +1] window surrounding the earnings announcement date in a given quarter and zero otherwise, and controls. The dependent variable is *Abnormal Volume* in Column (1), *Abnormal Retail Volume* in Column (2), *Abnormal Large Volume* in Column (3), *Abnormal Bid-Ask Spread* in Column (4), and *IPE* in Column (5). The sample includes both *Covered Firms* and *Non-Covered Firms*. Firm and year-quarter fixed effects are included. t-statistics in parentheses are based on standard errors clustered by year-quarter. ***, **, and * denote significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

	Dependent variable:				
	Abnormal Volume	Abnormal Retail Volume	Abnormal Large Volume	Abnormal Depth	IPE
	(1)	(2)	(3)	(4)	(5)
Local Coverage	0.039**	0.060***	0.059	0.018**	0.156*
	(2.395)	(2.768)	(1.080)	(2.080)	(1.952)
SUE	0.003	0.007	0.016	0.001	0.003
	(0.927)	(1.570)	(0.992)	(0.674)	(0.161)
|SUE|	0.017***	0.023***	0.042***	0.005***	0.049***
	(7.047)	(6.475)	(3.585)	(3.384)	(3.541)
Negative Surprise	−0.001	−0.019	−0.054	−0.003	−0.060
	(−0.076)	(−1.096)	(−0.959)	(−0.413)	(−0.634)
Size	0.053***	0.064***	0.075	0.010	0.012
	(2.915)	(2.646)	(1.002)	(0.957)	(0.100)
MTB	0.036**	0.007	−0.267***	0.005	0.081
	(2.015)	(0.291)	(−3.496)	(0.483)	(0.951)
Institutional Ownership	0.0001	−0.001	−0.005	0.001	0.008
	(0.074)	(−0.248)	(−0.728)	(0.522)	(0.578)
National Media	0.013	0.038	0.083	0.011	−0.021
	(0.379)	(0.778)	(0.545)	(0.527)	(−0.098)
Analysts	0.046***	0.058***	0.114***	0.012***	0.094***
	(10.757)	(10.638)	(6.150)	(5.265)	(4.872)
Return Volatility	−0.014	−0.032**	−0.096*	0.007	0.056
	(−1.513)	(−2.199)	(−1.925)	(1.056)	(0.986)
Employees	−0.897***	−1.437***	−1.169	0.035	−0.294
	(−5.609)	(−7.037)	(−1.523)	(0.345)	(−0.398)
Shareholders	0.034	0.037	−0.055	−0.039	0.156

	(0.923)	(0.737)	(−0.358)	(−1.573)	(0.802)
Firm FEs	Yes	Yes	Yes	Yes	Yes
Year-Quarter FEs	Yes	Yes	Yes	Yes	Yes
Observations	12,652	12,652	12,652	12,652	8,819
Adjusted R^2	0.308	0.310	0.176	0.144	0.063

Table 8
Cross-Sectional Analyses

This table reports the results of cross-sectional analyses. Across Panels A-C, Columns (1)-(8) report the results of applying *Eq. (1)* to *Abnormal Retail Volume*, *Abnormal Bid-Ask Spread*, *Abnormal Depth*, and *IPE* to various sub-samples of *Covered Firms*. In Panel A, the sample of *Covered Firms* is split into High and Low Size firms based on pre-period median market capitalization. In Panel B, the sample of *Covered Firms* is split into High and Low News firms based on the pre-period median fraction of earnings announcements with national media coverage. In Panel C, the sample of *Covered Firms* is split into High and Low IO firms based on the median pre-period institutional ownership. *Post* is an indicator equal to one in calendar quarters after paywall adoption. *Treat* is an indicator equal to one for local firms. All variables are defined in Appendix I. Firm-Event and Year-Quarter-Event fixed effects are included. t-statistics in parentheses are based on standard errors clustered by city-quarter. Chi-squared values and corresponding p-values are reported for testing the difference in magnitude of the $Post{*}Treat$ coefficient across sub-samples. ***, **, and * denote significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

Panel A: Size

	Dependent variable:							
	Abnormal Retail Volume		Abnormal Bid-Ask Spread		Abnormal Depth		IPE	
	Low Size	High Size	Low Size	High Size	Low Size	High Size	Low Size	High Size
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Treat	−0.156**	−0.029	0.027***	0.006	−0.060*	−0.007	−0.658***	−0.173
	(−2.324)	(−0.522)	(3.865)	(1.166)	(−1.807)	(−0.315)	(−2.722)	(−0.931)
Chi-Squared	3.418		1.413		1.657		2.001	
p-value	0.064*		0.235		0.198		0.157	
Firm-Event FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter-Event FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,496	1,501	1,496	1,501	1,496	1,501	1,196	1,109
Adjusted R^2	0.302	0.400	0.148	−0.178	0.155	0.226	−0.005	0.100

Panel B: National Media Coverage

	Dependent variable:							
	Abnormal Retail Volume		Abnormal Bid-Ask Spread		Abnormal Depth		IPE	
	Low News	High News	Low News	High News	Low News	High News	Low News	High News
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Treat	−0.229***	−0.042	0.029***	0.005	−0.066**	−0.027	−0.972***	0.112
	(−4.728)	(−0.722)	(3.514)	(0.894)	(−2.388)	(−1.298)	(−4.720)	(0.615)
Chi-Squared	3.473		0.745		3.584		8.910	
p-value	0.062*		0.388		0.058*		0.003***	
Firm-Event FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter-Event FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,327	1,670	1,327	1,670	1,327	1,670	1,022	1,283
Adjusted R^2	0.307	0.381	0.151	−0.125	0.132	0.246	0.002	0.086

Panel C: Institutional Ownership

	Dependent variable:							
	Abnormal Retail Volume		Abnormal Bid-Ask Spread		Abnormal Depth		IPE	
	Low IO	High IO	Low IO	High IO	Low IO	High IO	Low IO	High IO
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Treat	−0.193***	−0.067	0.015*	0.018**	−0.081***	−0.021	−0.824***	−0.020
	(−4.036)	(−1.163)	(1.774)	(2.257)	(−2.725)	(−1.050)	(−3.345)	(−0.095)
Chi-Squared	0.083		1.86		1.654		3.225	
p-value	0.774		0.173		0.198		0.073*	
Firm-Event FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter-Event FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,494	1,527	1,494	1,527	1,494	1,527	1,066	1,106
Adjusted R^2	0.358	0.300	0.169	0.081	0.209	0.182	0.017	0.055

Table 9
Changes in Coverage

This table reports the results of *Eq. (1)* for measures of local media coverage and national media coverage for the sample of *Covered Firms*. In Panel A, the dependent variable is *I(Coverage)*, an indicator equal to one for earnings announcements with local media coverage, in Column (1), *Local News Articles*, the log-adjusted count of local news articles written about an earnings announcement by the firm's local newspaper, in Column (2), and *Tone*, the average tone of earnings announcement articles written by the firm's local newspaper, in Column (3). In Panel B, the dependent variable is *Newswires*, the log adjusted count of Dow-Jones Newswires written about the firm's earnings announcement, in Column (1), and *National Media*, the log-adjusted count of national media articles written about the firm's earnings announcement, in Column (2). *Post* is an indicator equal to one in calendar quarters after paywall adoption. *Treat* is an indicator equal to one for local firms. All variables are defined in Appendix I. Firm-Event and Year-Quarter-Event fixed effects are included. t-statistics in parentheses are based on standard errors clustered by city-quarter. ***, **, and * denote significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

Panel A: Changes in Local Coverage

	Dependent variable:		
	I(Coverage)	Local News Articles	Tone
	(1)	(2)	(3)
Post*Treat	0.007	0.016	0.037
	(0.081)	(0.177)	(0.814)
SUE	0.0001	0.0001	0.010
	(0.016)	(0.005)	(0.897)
|SUE|	0.015**	0.007	0.020**
	(2.094)	(0.982)	(2.153)
Negative Surprise	0.031	0.031	−0.119*
	(0.768)	(0.526)	(−1.932)
Size	−0.006	0.002	0.217***
	(−0.127)	(0.038)	(2.696)
MTB	−0.001	−0.004*	−0.001
	(−0.437)	(−1.733)	(−0.740)
Institutional Ownership	−0.001	−0.015	−0.148
	(−0.011)	(−0.217)	(−1.095)
National Media	0.020**	0.010	0.021*
	(1.990)	(1.265)	(1.831)
Analysts	0.007	−0.031	0.054
	(0.299)	(−1.394)	(1.247)
Return Volatility	0.526*	0.065	−0.278
	(1.669)	(0.242)	(−0.586)
Employees	0.030	−0.011	−0.059
	(0.282)	(−0.081)	(−0.518)
Shareholders	0.091**	0.090***	−0.072
	(2.560)	(2.695)	(−0.517)
Firm-Event FEs	Yes	Yes	Yes
Year-Quarter-Event FEs	Yes	Yes	Yes
Observations	3,021	3,021	1,261
Adjusted R^2	0.387	0.385	0.076

Panel B: Changes in National Coverage

	Dependent variable:	
	Newswires	National Media
	(1)	(2)
Post*Treat	−0.010	−0.016
	(−0.081)	(−0.149)
SUE	−0.048*	−0.041*
	(−1.956)	(−1.863)
|SUE|	0.054**	0.049**
	(2.157)	(2.016)
Negative Surprise	−0.158	−0.110
	(−1.140)	(−0.870)
Size	0.426***	0.516***
	(3.824)	(5.224)
MTB	−0.012	−0.009
	(−1.023)	(−0.874)
Institutional Ownership	0.325	0.200
	(1.132)	(0.648)
Analysts	−0.047	−0.069
	(−0.523)	(−0.799)
Return Volatility	1.069	0.341
	(0.963)	(0.390)
Employees	−0.170	0.006
	(−0.596)	(0.019)
Shareholders	−0.305**	−0.181
	(−2.019)	(−1.391)
Firm-Event FEs	Yes	Yes
Year-Quarter-Event FEs	Yes	Yes
Observations	3,021	3,021
Adjusted R^2	0.458	0.596

Table 10
Parallel Trends

This table reports analysis of parallel trends. Across all columns, *Eq. (1)* is modified to replace $Post * Treat$ with interactions of *Treat* with indicators for each quarter relative to paywall adoption. Quarter $Q - 4$ is removed and used as a baseline. Across Columns (1)-(5), the modified version of *Eq. (1)* is applied to *Abnormal Retail Volume*, *Abnormal Bid-Ask Spread*, *Abnormal Depth*, *IPE*, and *Abnormal Local Downloads* for the sample of *Covered Firms*. *Treat* is an indicator equal to one for local firms. All variables are defined in Appendix I. Firm-Event and Year-Quarter-Event fixed effects are included. t-statistics in parentheses are based on standard errors clustered by city-quarter. ***, **, and * denote significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

	Dependent variable:				
	Abnormal Retail Volume	Abnormal Bid-Ask Spread	Abnormal Depth	IPE	Abnormal Local Downloads
	(1)	(2)	(3)	(4)	(5)
Q-3*Treat	−0.074	0.005	−0.010	-0.226	−0.086
	(−1.543)	(0.657)	(−0.472)	(−1.373)	(−0.722)
Q-2*Treat	−0.014	0.011	0.004	-0.243	−0.164
	(−0.284)	(1.634)	(0.189)	(−0.988)	(−1.594)
Q+2*Treat	−0.133**	0.034***	−0.070***	−0.672***	−0.408***
	(−2.481)	(3.956)	(−3.806)	(−3.001)	(−3.754)
Q+3*Treat	−0.112**	0.014*	−0.018	−0.489**	−0.394***
	(−2.174)	(1.929)	(−0.822)	(−2.123)	(−4.678)
Q+4*Treat	−0.304***	0.036***	−0.045**	−0.601***	−0.159*
	(−6.109)	(3.227)	(−2.409)	(−3.075)	(−1.772)
Firm-Event FEs	Yes	Yes	Yes	Yes	Yes
Year-Quarter-Event FEs	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	3,021	3,021	3,021	2,172	3,021
Adjusted R^2	0.327	0.111	0.185	0.038	0.257

Table 11
Placebo Test and Triple-Difference

This table reports the results of a placebo test using *Non-Covered Firms* and an alternative difference-in-difference-in-difference specification. In Panel A, *Eq. (1)* is applied to *Abnormal Retail Volume*, *Abnormal Bid-Ask Spread*, *Abnormal Depth*, and *IPE* for the sample of *Non-Covered Firms*. In Panel B, *Eq. (2)* is applied to *Abnormal Retail Volume*, *Abnormal Bid-Ask Spread*, *Abnormal Depth*, and *IPE* for the combined sample of *Covered Firms* and *Non-Covered Firms*. In both panels, across all columns, control variables are included, but not tabulated for brevity. *Post* is an indicator equal to one in calendar quarters after paywall adoption. *Treat* is an indicator equal to one for local firms. *Covered* is an indicator equal to one for *Covered Firms* and zero for *Non-Covered Firms*. All variables are defined in Appendix I. Firm-Event and Year-Quarter-Event fixed effects are included. t-statistics in parentheses are based on standard errors clustered by city-quarter. ***, **, and * denote significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

Panel A: Non-Covered Firms Placebo Test

	Dependent variable:				
	Abnormal Retail Volume	Abnormal Bid-Ask Spread	Abnormal Depth	IPE	Abnormal Local Downloads
	(1)	(2)	(3)	(4)	(5)
Post*Treat	0.002	0.007	0.021	0.079	−0.011
	(0.052)	(1.153)	(1.194)	(0.571)	(−0.213)
Firm-Event FEs	Yes	Yes	Yes	Yes	Yes
Year-Quarter-Event FEs	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	6,970	6,970	6,970	4,757	6,970
Adjusted R^2	0.317	0.164	0.120	0.062	0.175

Panel B: Difference-in-Difference-in-Difference Design

	Dependent variable:				
	Abnormal Retail Volume	Abnormal Bid-Ask Spread	Abnormal Depth	IPE	Abnormal Local Downloads
	(1)	(2)	(3)	(4)	(5)
Post*Treat	−0.030	−0.002	0.021	0.021	−0.056
	(−0.779)	(−0.205)	(1.371)	(0.174)	(−1.199)
Post*Covered	0.082**	−0.008	0.022	0.081	0.090
	(2.104)	(−0.875)	(1.100)	(0.690)	(1.505)
Post*Treat*Covered	−0.129**	0.104**	−0.065**	−0.319*	−0.164*
	(−2.252)	(2.354)	(−2.208)	(−1.745)	(−1.646)
Firm-Event FEs	Yes	Yes	Yes	Yes	Yes
Year-Quarter-Event FEs	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	9,991	9,991	9,991	7,507	9,991
Adjusted R^2	0.306	0.195	0.138	0.072	0.206

Table 12
Design Changes

This table reports the results of applying *Eq. (1)* to the main variables of interest under alternative sample construction procedures. In Panel A, *Eq. (1)* is applied to the main variables of interest using the stacked sample with previously treated firms removed from the sample of control firms. In Panel B, textitEq. (1) is applied to the main variables of interest using the stacked sample with quarters Q-1 and Q+1 included. In both panels, across all columns, control variables are included, but not tabulated for brevity. *Post* is an indicator equal to one in calendar quarters after paywall adoption. *Treat* is an indicator equal to one for local firms. All variables are defined in Appendix I. Firm-Event and Year-Quarter-Event fixed effects are included. t-statistics in parentheses are based on standard errors clustered by city-quarter. ***, **, and * denote significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

Panel A: Alternative Strict Control Group

	Dependent variable:				
	Abnormal Retail Volume	Abnormal Bid-Ask Spread	Abnormal Depth	IPE	Abnormal Local Downloads
	(1)	(2)	(3)	(4)	(5)
Post*Treat	−0.182***	0.021***	−0.028*	−0.590***	−0.265***
	(−4.590)	(3.209)	(−1.840)	(−3.496)	(−2.884)
Firm-Event FEs	Yes	Yes	Yes	Yes	Yes
Year-Quarter-Event FEs	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	2,223	2,223	2,223	1,721	2,223
Adjusted R^2	0.313	0.130	0.160	0.004	0.263

Panel B: Alternative Window

	Dependent variable:				
	Abnormal Retail Volume	Abnormal Bid-Ask Spread	Abnormal Depth	IPE	Abnormal Local Downloads
	(1)	(2)	(3)	(4)	(5)
Post*Treat	−0.123***	0.010**	−0.033**	−0.253*	−0.175***
	(−3.107)	(2.353)	(−2.238)	(−1.722)	(−2.893)
Firm-Event FEs	Yes	Yes	Yes	Yes	Yes
Year-Quarter-Event FEs	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	4,037	4,037	4,037	3,098	4,037
Adjusted R^2	0.329	0.168	0.208	0.061	0.271