Financial Reporting and Consumer Behavior*

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Abstract

We show that financial reporting spurs consumer behavior. Using granular GPS data, we show that foot-traffic to firms' commerce locations significantly increases in the days following their earnings announcements. Foot-traffic increases more for announcements with extreme earnings surprises, that correspond to firms' fiscal year-ends, that occur outside of Fridays, and that elicit greater internet search volume, consistent with earnings announcements spurring consumer behavior by garnering attention. Consumer activity also rises with reductions in solvency risk among firms selling durable goods, consistent with consumers responding to information about firms' longevity conveyed by their earnings. Using demographic information, we show financial reporting disproportionately affects foot-traffic in populations more likely to consume financial news. Collectively, these results suggest earnings announcements serve a marketing function by drawing attention to and providing information about firms, and that a byproduct of the financial reporting process is that it shapes consumer behavior.

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1. Introduction

This study examines the implications of the financial reporting process through the lens of consumer behavior. Using granular GPS data on the location of individual consumers, we show consumer foot-traffic to public companies' locations of commerce significantly increases following their quarterly earnings announcements. Our evidence adds directly to the growing literature on the externalities of the financial reporting process by showing that it shapes consumer behavior. In addition, it yields important insights for research on investor attention, consumer behavior, and recurring events.

We investigate the impact of financial reporting using a setting of quarterly earnings announcements. Our study is motivated by the idea that earnings announcements likely affect consumer decisions by increasing attention to the firm announcing earnings. Consumers are attention-constrained and should be more likely to visit stores of brands they recognize and can easily recall to mind, consistent with evidence in the marketing literature that media attention and advertising are associated with increased brand awareness and sales (Demers and Lewellen, 2003; Stephen and Galak, 2012; Lewis and Reiley, 2014; Hartmann and Klapper, 2018). Prior research finds that limited attention is an information friction impacting trading activity (e.g., Merton, 1987; Lee, 1992; Barber and Odean, 2008; Hirshleifer et al., 2008). The same information frictions affecting retail investors likely affect consumers, especially given the overlap in the two groups (e.g., Bernard, Cade, and Hodge, 2018; Medina, Mittal, and Pagel, 2021). Therefore, consumers may patronize businesses that are more salient due to the increased attention that accompanies earnings announcements.

An additional non-mutually exclusive channel through which earnings announcements affect consumer behavior for some firms is by informing consumers about firms' longevity (e.g., solvency). The longevity of some firms, such as firms selling durable goods, can be important to consumers (Hortaçsu et al., 2013). For example, a consumer is more likely to shop at Tesla after learning that Tesla is likely to remain in business and be able to service warranties. This channel is consistent with prior work showing one effect of advertising is to provide product information relevant to consumers (Ackerberg, 2001). Our study thus builds on prior evidence that earnings announcements trigger investor attention and provide information about firms' future prospects (e.g., Barber and Odean, 2008; Noh, So, and Verdi, 2021), both of which suggest that financial reporting is likely relevant for consumer behavior.

Much of the prior research on financial reporting focuses on its impact on investors, managers, regulators, and economically linked firms (e.g., Healy and Palepu, 2001). In contrast, research that links financial disclosures and consumer behavior is scarce, despite survey evidence that managers believe financial reporting is important to attract and retain customers (Graham, Harvey, and Rajgopal, 2005; Dichev et al., 2013). Studying the link between financial reporting and consumer behavior in large samples has traditionally posed several challenges. Most financial accounting datasets aggregate firms' sales information at low frequencies, across periods both before and after the release of financial accounting information. This lack of granularity makes it hard to study consumer behavior in event-time relative to the release date of financial reports. The lack of granularity also precludes analyses from accounting for key drivers of consumer activity unrelated to earnings announcements, such as variation driven by the time of year, day of week, and/or geographic location.

Our study overcomes these challenges by using a novel database provided by SafeGraph that tracks the GPS coordinates of a large panel of consumers' cell phones across the U.S. from January 2017 through February 2020. Our sample ends prior to the beginning of COVID-19-related mobility restrictions in the U.S. The coverage is expansive and highly granular. For example, in February of 2020, the SafeGraph database contains records covering approximately 13% of the U.S. population (see Fig. 1). The SafeGraph database does not identify personal information about the consumer but does capture their precise intra-day location. SafeGraph matches these GPS records with commercial locations and provides the daily visits to stores, which we show tracks within-firm variation in revenue (see Appendix C). These data allow us to observe changes in foot-traffic to specific stores in event-time relative to the release of firms' earnings news. Our main analyses rely on a sample of approximately 50 million observations at the store-date level, measured in the 21 days surrounding firms' earnings announcement dates.

Our first result is that foot-traffic significantly increases to firms' commerce locations following their earnings announcements. Using dynamic tests, we show that increases in foot-traffic are greatest approximately three-to-six days after announcements, whereas no similar increase exists immediately before announcements. Foot-traffic slowly dissipates from seven days after, consistent with our results primarily reflecting a transitory change driven by the announcement. In terms of economic magnitude, we find that daily store visits on average increase by approximately 2% during the 3-6 day period after the announcement relative to baseline levels, and by 1.6% during the 7-10 day period after the announcement.

A key benefit of the granularity of the data is that it allows us to leverage a host of fixed effects that help mitigate alternative explanations for our findings. For example, the use of day-of-week fixed effects accounts for variation in activity associated with weekends versus weekdays and the use of store fixed effects accounts for variation attributable to a particular location. Similarly, we use yearmonth-by-industry and yearmonth-by-county fixed effects to mitigate concerns that our results are explained by industry-wide fluctuations in demand (e.g., retail stores in the holiday season) or time-varying local economic conditions. Thus, alternative explanations for our findings would need to explain variation in consumer activity that concentrates after the announcement and that is not explained by day-of-week, macroeconomic, and/or local economic activity.

We predict and find evidence that earnings announcements play a marketing role by using variation in the extent to which each announcement increases attention. We show a greater increase in foot-traffic to commerce locations operated by firms with extreme negative or positive earnings surprises, which are more likely to garner coverage from the financial press (e.g., Niessner and So, 2018). Additionally, we find that foot-traffic to firms' commerce locations increases more when firms announce fiscal-year-end results, which tend to attract more attention than non-fiscal-year-end announcements. Similarly, our results are predictably weaker for Friday earnings announcements, even after controlling for firms' earnings surprise, consistent with consumer attention motivating their visits. We also directly estimate the announcement-related increase in attention using within-firm variation in Google search volume, and show that our results are strongest when this increase in attention is highest. These results suggest a potential channel through which consumers' store visits increase post-announcement—companies' personalized or retargeted online advertising targets those consumers who search for the firm online, thereby influencing subsequent shopping behavior (Bleier and Eisenbeiss, 2015; Johnson, Lewis, and Nubbemeyer, 2017; Sahni, Narayanan, and Kalyanam, 2019).

We also find that, for a subset of firms, variation in consumer activity tracks variation in the information conveyed in firms' earnings announcements. Post-announcement spikes in consumer foot-traffic increase with the extent to which solvency risk declines, but this phenomenon is limited to firms selling durable goods (e.g., cars or appliances). These findings suggest consumers update their expectations about firms' solvency based on the information in financial reports, and they change their shopping behavior for durable goods accordingly. Our results are consistent with evidence in Hortaçsu et al. (2013) and Bowen, DuCharme, and Shores (1995) that consumers forgo purchasing durable items from firms that risk being unable to provide complementary services such as warranties, spare parts, or maintenance.

Using demographic information about the areas in which businesses reside, we show that our main findings predictably concentrate in areas likely to consume more financial news and to have a more elastic demand for products and services. We find more pronounced results in areas with a greater representation of English-speaking and Caucasian households. To the extent these populations are more likely to consume financial news, these findings dovetail nicely with our evidence that changes in consumer behavior are driven by attention to firms' earnings announcements. Our results also concentrate in areas with a moderate level of education and income, where consumers likely have highly elastic demand for the products and services of our sample firms and therefore are more likely to respond to earnings news.

An added benefit of using demographic information is that it allows us to exploit within-announcement variation. As a result, our demographic tests are also useful in mitigating a variety of alternative explanations related to firm-level patterns that occur in event-time relative to earnings announcements. For example, these tests mitigate concerns that our results are driven by company-wide events such as sales or employee stock grants. In additional tests, using a panel of advertising data, we find that our results do not appear to be explained by variation in national advertising campaigns coinciding with earnings announcements.

We also conduct additional tests to investigate the economic importance of our findings. A key innovation of our study is the use of granular GPS data to study changes in consumer activity at the store-day level. This granularity simultaneously allows us to account for variation attributable to day-of-week, macroeconomic, and/or local economic activity. However, by focusing on GPS data, our main findings reflect foot-traffic at locations of commerce which are not necessarily related to firms' revenues. For example, consumers may increase their store visits without increasing their purchases. Relatedly, e-commerce is an increasingly important portion of companies' sales and therefore our foot-traffic measure may not generalize.[1] To address these concerns, we conduct two tests to corroborate the economic importance of our main findings.

First, we show that within-firm variation in foot-traffic aligns with variation in firms' total sales. Specifically, firms with larger upticks in post-announcement foot-traffic subsequently report higher sales in their next earnings announcement. These findings suggest that increased foot-traffic is indicative of increased consumer purchases, while also mitigating concerns that post-announcement increases in foot-traffic simply pull forward sales that

[1] The proportion of US e-commerce retail sales as a percent of total retail sales has grown from 4.2% in Q12010 to 11.4% in Q12020. Source: https://www.census.gov/retail/ecommerce/historic_releases.html.

would have taken place further into the future.

Second, we corroborate our main findings in an alternative sample of online transactions. Our use of GPS data necessitates the focus on in-person consumer activity at brick-and-mortar stores, but also raises potential concerns regarding the importance of store foot-traffic and the generalizability of our findings to e-commerce businesses. Using a panel of online transactions for retailers from Comscore, we show that consumers increase the quantity of purchases following firms' earnings announcements, suggesting that our main inferences likely shape firms' revenues and extend beyond consumers' visits to traditional storefronts.

A central contribution of our paper is to study the relation between consumer activity and the financial reporting process among publicly traded firms. Our study is motivated by prior survey evidence that shows CFOs believe financial reporting is important to assure current customers and attract prospective customers (Graham, Harvey, and Rajgopal, 2005; Dichev et al., 2013). Broadly, our study is related to research on disclosures and their effects on multiple groups of stakeholders. For example, prior research examines the effects of consumer advertising on investors (e.g., Grullon, Kanatas, and Weston, 2004; Lou, 2014; Madsen and Niessner, 2019; Focke, Ruenzi, and Ungeheuer, 2020; Liaukonytė and Žaldokas, 2021). Conversely, financial reporting can also have effects on consumers. Most prior research focuses on investors, creditors, regulators, and economic partners. By contrast, our study examines the externalities of financial reporting for consumers as another key constituency of public firms.[2] In doing so, our paper seeks to expand the boundaries of research in accounting and stimulate further research on the implications of financial reporting for consumers.

Our findings are subject to a few important caveats. First, our main findings rely on

[2]There is a growing literature examining associations between firm disclosures and various stakeholders. For example, Choi, Choi, and Malik (2021) and deHaan, Li, and Zhou (2021) show employees and job seekers use firms' financial information, Chen et al. (2021) shows depositors use banks' financial information, and Bowen, DuCharme, and Shores (1995), Chakravarthy, deHaan, and Rajgopal (2014), Gassen and Muhn (2018), and Breuer, Hombach, and Müller (2020) show firms' disclosures are shaped by their transacting stakeholders, such as suppliers and customers.

GPS data from smartphones and thus, to the extent individuals with smartphones are more likely to consume financial news, our findings may overstate the extent to which financial reporting would affect broader individuals. Second, our main tests necessarily rely on firms with physical locations of commerce and thus may overstate the extent to which financial reporting spurs consumer activity in firms without brick-and-mortar operations. However, our corroborating results using an alternative panel of online retailers suggest that our inferences do not hinge on the study of firms with physical locations of commerce.

A second contribution of our paper is to highlight feedback effects of the financial reporting process. Specifically, our results suggest that consumers increase consumption activity for firms with more attention-grabbing earnings news, indicating that managers may prefer to spotlight their earnings news as a means to increase subsequent consumer activity. Thus, our findings highlight an additional payoff for managers strategically timing their earnings announcement news (e.g., Johnson and So, 2018; Noh, So, and Verdi, 2021). Our results suggest that the financial reporting process, by shaping consumer behavior, can affect the fundamentals of the firm.

Finally, our study contributes to a growing area of research that uses granular consumer-generated data to track the supply and demand of various resources. For example, granular data, such as consumer browsing activity and satellite data tracking the number of cars in store parking lots, predict revenue and earnings surprises (Froot et al., 2017; Zhu, 2019; Katona et al., 2021). Li and Venkatachalam (2021) shows cell phone geo-location data signal oil production shocks and predict stock prices. Access to big data has not only affected stock price formation and firms themselves (e.g., Zhu, 2019), but also facilitated research on investors' local information advantage (e.g., Kang, Stice-Lawrence, and Wong, 2021). Using similar geo-location data as ours, Painter (2021) shows consumer demographics change in response to firms' political corporate statements and Gurun, Nickerson, and Solomon (2022) shows firms providing public goods, such as allowing anyone to use in-store bathrooms, experience a decline in store visits. Jin, Stubben, and Ton (2021) finds customer loyalty, also measured using geo-location data, explains variation in the persistence of revenues and earnings. Our study differs by examining the role of financial reporting in shaping consumer demand. Moreover, our study is among the first to show heterogeneous effects of financial reporting across populations based on race/income/education, suggesting that financial reporting shapes consumption by disproportionately impacting populations more likely to consume financial media.

The results of our study complement those in a concurrent working paper by Kimbrough, Wang, and Wei (2021). In that study, the authors use high-frequency data on consumer brand perceptions and find that brand perceptions improve in response to more positive earnings surprises. Like our study, their evidence suggests that earnings announcements create externalities for consumers by providing information relevant to consumers. Our study differs by using granular GPS data to directly study changes in consumers' shopping activity and to shed light on mechanisms through which earnings announcements shape consumer demand. For example, in addition to the information in announcements improving brand perceptions, we also document that earnings announcements can spur consumer behavior simply by increasing brand awareness.

The rest of our paper is organized as follows. In Section 2, we describe the GPS data in detail and describe our sample selection process. Section 3 discusses our main findings and Section 4 reports the results of supplementary tests. Section 5 concludes.

2. Data and Sample Selection

The data for this paper come from five primary sources. We obtain firm fundamentals from Compustat, price and return data from CRSP, analysts' forecasts of earnings from IBES, and institutional ownership and insider trading information from Thomson Reuters. Our main tests center on firms' quarterly earnings announcements. We measure earnings announcement dates following Dellavigna and Pollet (2009) by comparing announcement dates in IBES and Compustat and using the earlier of the two if the announcement dates differ. Finally, we merge these data with SafeGraph data on foot-traffic from consumers to stores operated by the announcing firm.

A key innovation of our study is the use of GPS coordinate data from SafeGraph, which allows us to observe foot-traffic to firms' locations of commerce at daily frequencies. SafeGraph is a data company that aggregates anonymized location data from numerous applications in order to provide insights about physical places, via the SafeGraph Community. To enhance privacy, SafeGraph excludes census block group information if fewer than five devices visited an establishment in a month from a given census block group. The data have been used in prior studies in economics and finance and studies on earnings persistence (e.g., Painter, 2021; Jin, Stubben, and Ton, 2021; Gurun, Nickerson, and Solomon, 2022). However, ours is the first to our knowledge to use the data for research on the impacts of financial reporting. The dataset for our study runs from January 2017 to February 2020.

SafeGraph aggregates anonymized data from numerous smartphone applications that rely on location services, such as those related to weather, dating, or local news. These applications run on both Apple and Android smartphones. Thus, although our inferences are limited to the consumer behavior of individuals who use smartphones, the data we observe are broadly representative of all smartphone users.[3] The data cover around 44,546,450 unique devices and represent roughly 13% of the U.S. population by February 2020.

SafeGraph collects data based on pings to cell phone towers to triangulate the precise GPS coordinates of a smartphone at various points throughout the day. The longitudinal and latitudinal data are sufficiently detailed such that SafeGraph can observe a device's location within a radius of a few meters. SafeGraph attributes GPS pings to locations of commerce if

[3]SafeGraph data is well-sampled across geographies and demographic categories. For example, the Pearson correlation coefficient between the number of devices covered by SafeGraph and the population in a given county is above 0.9. Similarly, the correlation coefficients between the number of devices covered by SafeGraph and the population for each race, education attainment, and income level are all above 0.9.

they are found within the geographical polygons which denote a structure or property's exact physical boundaries. The detailed geo-location data allow SafeGraph to determine whether an individual visits a firm's location of commerce, using the establishments' footprints, the number of pings, and the time between pings. SafeGraph also uses an algorithm to remove store employees from the computation of store visits. The data we use in our main tests track daily visits to firms' locations of commerce at the establishment-date level. For example, we can observe how many individuals visited the Best Buy in Mountain View, California (E. Charleston Rd.) on a given date and the average duration of their visits. Appendix B reports the number of stores in each state in our sample.

For some of our tests, we also merge each establishment with information about its corresponding county using data from the 2016 American Community Survey from the Census Bureau.[4] The census data allow us to infer the demographics of the likely visitors to the establishment. Continuing with the example above, the census data allow us to observe that likely consumers located near Best Buy in Mountain View are predominately affluent and college educated or above.

Fig. 1 contains two maps detailing the fraction of each state's population tracked by SafeGraph GPS coordinate data. Panel A of the figure highlights that the data are expansive, covering roughly 13% of the U.S. population by February 2020, the end of our sample period. There is some heterogeneity across states. For example, in February 2020, the SafeGraph data track smartphone locations of approximately 16% of the population of Texas and 11% of the population of California. Panel B of Fig. 1 contains a similar graph corresponding to January 2017, which marks the beginning of our sample period. The two charts collectively illustrate the growth in coverage of the SafeGraph data over time. Our analyses account for coverage growth using an event-study design and a host of fixed effects.

Our event-study design relies on tracking foot-traffic in event-time relative to public

[4] We conducted a geospatial join using latitude and longitude coordinates of firms' locations of commerce and data on geometry boundaries of census blocks.

firms' earnings announcements. To do so, we merge SafeGraph's foot-traffic data at the establishment-date level with the earnings announcement dates of the establishments' corresponding public firms. By construction, all earnings announcements in our sample pertain to public firms with brick-and-mortar store fronts. Thus, a limitation of the SafeGraph database is that it does not allow us to track changes in consumer activity at firms that primarily transact online, such as Amazon or Wayfair. However, in additional analyses using online transaction data from Comscore, we show that our results generalize to e-commerce.

We drop firms operating in industries in which consumer visits are likely not discretionary, such as utilities, finance, agriculture, healthcare, and pharmaceuticals. We also drop visits to subsidiaries with different names from the firm announcing earnings. For example, Anthropologie is a subsidiary of Urban Outfitters, but we do not consider Anthropologie when examining Urban Outfitters' announcements because consumers may not recognize this brand as a subsidiary of the firm announcing earnings. After requiring data for control variables from Compustat, CRSP, IBES, and Thomson Reuters, our final sample includes 2,485 earnings announcements for 222 unique firms. The foot-traffic data for the calendar days [−10,+10] around each announcement include 47,882,818 store-day observations from 223,943 unique establishments.

Fig. 2 provides a breakdown of our sample firms based on their two-digit NAICS classification. Unsurprisingly, the vast majority of firms in our sample (∼84%) are firms in retail or accommodation/food services. The next largest group consists of wholesalers. We intentionally keep firms in the remaining industries for parsimony and generalizability, but our results do not appear sensitive to retaining these firms. One of the advantages of the geo-location data on store visits is its broad coverage of stores. For instance, it covers several different granular categories within the retail industry (e.g., fashion, furniture, appliances, movie theaters, restaurants, coffee shops, and car dealerships). In addition, the brands of stores in our sample are easily recognized by the consumer as associated with the firm announcing earnings.

In contrast, alternative datasets on product-level (e.g., UPC-level) purchases are primarily concentrated in the food and beverage industry. This industry is dominated by large parent firms with several products under brands harder for a consumer to associate with the parent (e.g., Tropicana orange juice and Frito's chips would both be linked to PepsiCo's earnings, but many consumers may not make this connection). Although our data are better suited for measuring the impacts of financial reporting on consumer behavior, a caveat is that we are unable to observe the intent to shop for announcing firms' brands at non-announcing firms' stores. For example, we do not capture visits to Target to purchase PepsiCo products around PepsiCo's earnings announcements.

Table 1 contains summary statistics for the main variables in our analyses. The average store in our sample has 17.7 tracked visits per day, although there is considerable cross-sectional variation with a standard deviation above 30. Table 1 also reports firm-quarter-level variables and demographic variables pertaining to the county in which a given commerce location resides.

3. Main Results

3.1. Store Visits around Earnings Announcements

Our main tests examine whether consumer visits to stores change in the days after the earnings announcement using a short-window event study. We estimate the following regression using the sample of all store-days in the [−10,+10] calendar-day window around the earnings announcement:

$$\begin{aligned} ln(1 + Daily\ Store\ Visits)_{s,d} = &\beta_1 Post_{s,d} + \theta Controls_{i,d} + \Sigma\beta_k Day\text{-}of\text{-}Week_k \\ &+ \Sigma\beta_s Store_s + \Sigma\beta_t Year\text{-}month_t + \epsilon_{s,d}, \end{aligned} \tag{1}$$

where $ln(1 + Daily\ Store\ Visits)_{s,d}$ is the log of 1 plus the number of visits to store s on date d relative to the earnings announcement, measured using SafeGraph foot-traffic data. $Post_{s,d}$ is an indicator variable equal to one if the day falls in the period after the earnings announcement (i.e., days 0 to +10 after the earnings announcement) and 0 otherwise. $Controls_{i,d}$ is a vector of control variables measured for each firm i's announcement, including a loss indicator, leverage, the book-to-market ratio, capital expenditures, ROA, the market value of the firm, a fourth quarter indicator, the firm's recent buy-and-hold abnormal return, illiquidity, volatility, institutional ownership, insider trades, and analyst coverage. All variables are defined in Appendix A. The coefficient of interest in Eq. 1 is β_1. If consumers increase their visits to stores after earnings announcements, we predict $\beta_1 > 0$.

We estimate several versions of Eq. 1 to account for different forms of potential confounds. We estimate the first version using day-of-week, store, and year-month fixed effects. Our inclusion of day-of-week fixed effects accounts for variation in activity associated with a particular day of the week, including variation in weekend versus weekday visits. The inclusion of store fixed effects controls for time-invariant characteristics of stores that may affect foot-traffic such as the size or the accessibility of the store. The inclusion of year-month fixed effects accounts for any time-varying macroeconomic factors (e.g., consumer foot-traffic is higher when the economy is doing well and many firms have reported strong earnings). The use of year-month fixed effects also addresses variation in visits attributable to the expansion of SafeGraph data coverage over time.

The second version of Eq. 1 we estimate replaces the year-month fixed effects with county × year-month fixed effects, which account for time-varying, location-specific activity (e.g., economic growth tied to a particular county affects store visits in that county). The third version we estimate replaces year-month fixed effects in Eq. 1 with 6-digit NAICS industry × year-month fixed effects, which account for time-varying, industry-specific factors (e.g., industry-specific growth). Finally, we estimate a version of Eq. 1 with both county × year-month and industry × year-month fixed effects. In all specifications, standard errors are clustered by firm and year-month, which allows for correlation across year-months within a given firm and correlation across firms within a given year-month.

Table 2 contains our first main findings. Across all specifications, we show that foot-traffic significantly increases following firms' quarterly earnings announcement dates. The results suggest that, relative to the 10 days before firms' earnings announcement dates, consumers increase their visits to stores operated by the announcing firm in the 11 days following the announcement. The coefficient on *Post* is 0.011 across all four columns of Table 2, which suggests that daily store visits on average increase by approximately 1.1% in the 10-day period after the announcement.[5] Subsequent tests find predictable cross-sectional variation in the magnitude of this effect.

As a case study to help visualize the nature of our findings, Fig. 3 plots Ralph Lauren's average store visits in event-time relative to its earnings announcement date on July 31, 2018. To control for day-of-week effects, the store visits for each day are demeaned by Ralph Lauren's average store visits on a given day of the week during our sample period. The chart highlights a sizeable increase in abnormal day-of-week adjusted foot-traffic to Ralph Lauren stores concentrated in the third through sixth day following its earnings announcement, during which Ralph Lauren reported a positive earnings surprise relative to the analyst consensus forecast. The graph shows that abnormal foot-traffic more than doubles during this time frame and partly dissipates shortly after, consistent with the announcement eliciting a transitory increase in consumer activity.

In Table 3, we build upon Fig. 3 and decompose the post-announcement increase in store visits to study dynamics on consumer behavior in event-time relative to firms' earnings announcement dates. We do so by replacing the $Post_{s,d}$ indicator with a series of indicator variables for several windows around the earnings announcement: $Day\ [EA-4, EA-3]_{s,d}$,

[5] As the dependent variable is log-transformed, we compute the percentage increase in visits as $exp(0.011) - 1 = 0.011$. We also note that, despite the large sample, t-statistics in our specifications should not be huge, due to the variation explained by the inclusion of several fixed effects, which reduce degrees of freedom, and double-clustering of standard errors by firm and year-month.

$Day\ [EA{-}2, EA{-}1]_{s,d}$, $Day\ [EA, EA{+}2]_{s,d}$, $Day\ [EA{+}3, EA{+}4]_{s,d}$, $Day\ [EA{+}5, EA{+}6]_{s,d}$, and $Day\ [EA+7, EA+10]_{s,d}$. The excluded group is store-days in the [−10,−5] days prior to the earnings announcement. Thus, the coefficient on each indicator variable for the date relative to the earnings announcement measures the change in store visits relative to the store-days in the excluded group. We again estimate four versions of this dynamic regression using the same fixed-effect structures as in Table 2.

Table 3 and Fig. 4 present the results of these dynamic tests. Compared to days [−10,−5] prior to the announcement, foot-traffic increases significantly three-to-six days after the announcement. This somewhat delayed response is likely due to people waiting until they have consumption needs or time to shop (e.g., weekends) and aligns with advertising's lagged effect on sales documented in the marketing literature (Bass and Clarke, 1972; Benjamin, Jolly, and Maitland, 1960). For example, Hartmann and Klapper (2018) finds the sales effects of Super Bowl advertisements are concentrated in weeks with subsequent sporting events. We find that heightened foot-traffic slowly dissipates starting seven days after the announcement, consistent with our results reflecting a transitory increase in consumer attention driven by the announcement. By contrast, store visits in the few days prior to the announcement and in the [0,+2] window after the announcement do not appear to be significantly higher than store visits on days [−10, −5]. In terms of economic magnitude, the results in Table 3 and Fig. 4 indicate that daily store visits on average increase by approximately 2% during the 3-6 day period after the announcement relative to baseline levels, and by 1.6% during the 7-10 day period after the announcement.

Our evidence that consumer activity increases after firms' announcements but not before mitigates explanations for our findings unrelated to the release of earnings information. Moreover, the robustness of our findings across fixed-effect structures mitigates explanations related to time-varying macroeconomic and/or regional factors such as wealth shocks. Finally, because SafeGraph intentionally excludes employee foot-traffic from their data, our findings are unlikely attributable to changes in firm policies or demand driven by employee compensation plans. Taken together, the results in Tables 2 and 3 are consistent with earnings announcements spurring a transitory increase in consumers' visits to stores beginning a few days after the announcement, which partially dissipates soon after.

3.2. Cross-sectional Tests

In this section, we exploit cross-sectional variation in the characteristics of firms' earnings announcements to examine the roles of consumer attention and earnings information in explaining our main results. We first examine whether within-firm changes in store visits around firms' earnings announcements vary with the amount of attention their announcements are likely to generate.

In our first test examining the role of attention, we predict that consumer activity spikes in response to extreme positive and negative surprises. This prediction builds on the evidence in Lee (1992), Barber and Odean (2008), and Hirshleifer et al. (2008) that investor attention spikes for firms with extreme news, suggesting that earnings announcements can also play a marketing role by spurring consumers to patronize businesses that are more salient post-announcement. We test this prediction by re-estimating Eq. 1 after interacting the post-announcement indicator variable, $Post_{s,d}$, with indicators for the quintile rank of each firm's analyst-based earnings surprise, $SURP_{i,q}$. High and low quintile ranks of $SURP_{i,q}$ capture extreme earnings news. We measure $SURP_{i,q}$ as the difference between the current announcement's EPS before extraordinary items and the analyst consensus forecast for the same amount, scaled by the standard deviation of analyst forecasts in the 90 days prior to the announcement. We include the same vector of control variables used in our main tests and the 1st Quintile of SURP main effect is subsumed.

Consistent with our predictions, the coefficients on the interaction terms in Table 4 show that the post-announcement increase in visits is concentrated in announcements with extreme surprises. Specifically, we find a significant increase in visits for extreme positive earnings surprises in the top two quintiles and extreme negative surprises in the bottom quintile.[6] Because extreme news is more likely picked up in financial headlines (e.g., Niessner and So, 2018; Noh, So, and Verdi, 2021), these results suggest earnings announcements drive consumer activity by garnering attention.

To corroborate the role of attention in spurring consumer behavior, we also examine whether our results are more pronounced when consumers likely devote more attention to the announcement. We employ several proxies for consumer attention: an indicator for the fourth quarter (i.e., announcements associated with the fiscal year end), a Friday indicator, and Google search volume. While the fiscal year end announcements and Friday announcements are associated with more attention and less attention, respectively, Google search volume is a direct measure of public attention to the announcement. We investigate the role of attention by interacting our post-announcement indicator with Fourth Quarter Ind., Friday Ind., or Abnormal Google Search Index $[EA-1,\, EA+1]$.

Table 5, Panel A presents results showing that the effect is more pronounced for announcements associated with the fourth fiscal quarter. The coefficient on Post × Fourth Quarter Ind. is positive and significant in all columns. The results suggest that announcements associated with the fiscal year end, which garner more consumer attention, are followed by more pronounced increases in foot-traffic. Table 5, Panel B presents results showing that the effect is less pronounced for announcements on Fridays. The coefficient on Post × Friday Ind. is negative and significant in all columns.[7]

Table 6 presents results showing that the effect is more pronounced for announcements with greater attention, as proxied by abnormal Google search volume in days [−1,+1] around the announcement. We use the daily search index provided by Google Trends, which is normalized to reflect within-firm variation and takes a value between 1 and 100. Our sample

[6] The cutoff between the 1st and 2nd Quintiles of SURP is −0.58, and the cutoff between the 3rd and 4th Quintiles is 1.53. The 5th Quintile of SURP has a lower bound of 5.

[7] A test of the sum of the coefficients on $Post_{s,d}$ and $Post_{s,d} \times Friday\ Ind._{i,q}$ finds that the sum is insignificantly different from 0 (p-values ranging from 0.29 to 0.34 across the four columns of the table).

for this cross-sectional test is slightly reduced, as Google provides missing values for firms with insufficient search data to compute firm-specific indices. Results in Table 6 find a positive and significant coefficient on Post × ln(Abnormal Google Search Index $[EA{-}1, EA{+}1]$) in all columns. Thus, increased consumer attention, as proxied by retail investors' internet searches for the firm, explains variation in the extent to which consumers increase their store visits after earnings announcements.[8] This result hints at the potential role of personalized or retargeted advertising in changing consumer foot-traffic following earnings announcements. Consistent with a growing literature in marketing documenting the effectiveness of this type of advertising, consumers who search for the announcing firm online may be more exposed to personalized or retargeted advertising which can ultimately change their shopping behavior (Bleier and Eisenbeiss, 2015; Johnson, Lewis, and Nubbemeyer, 2017; Sahni, Narayanan, and Kalyanam, 2019).

We next examine whether changes in the perceived longevity of firms, measured using the Altman Z-score, have an effect on post-announcement increases in store visits. Prior literature suggests that consumers, especially those who purchase durable goods, care about the long-term viability of firms, because they benefit from the continuing availability of parts and services (e.g., Bowen, DuCharme, and Shores, 1995; Hortaçsu et al., 2013). Consistent with this evidence, Chakravarthy, deHaan, and Rajgopal (2014) finds firms selling durable goods specifically target customers when rebuilding their reputations after a restatement, and Kimbrough, Wang, and Wei (2021) finds consumers' brand perceptions improve when firms selling durable goods announce positive earnings.

We re-estimate Eq. 1 after interacting *Post* with a fiscal-quarter-matched change in the Altman Z-score. For parsimony, we present results for just the specification with the most stringent fixed effect structure, which includes day-of-week, store, county × year-month, and industry × year-month fixed effects. In Table 7, we find that the increase in store visits after

[8]We embrace the possibility that a firm's retail investors are more likely to be their potential consumers (e.g., Bernard, Cade, and Hodge, 2018).

the announcement is more pronounced when the announcement reveals a decrease in the announcing firm's bankruptcy risk. Consistent with information about firm longevity being of consequence only for certain types of firms, Table 7 finds that this effect is concentrated in firms selling durable goods.[9] This evidence is consistent with consumers changing their shopping behavior when the content of earnings news reveals reduced bankruptcy risk for firms selling durable goods (e.g., cars, appliances) but not for other types of firms (e.g., restaurants).

Our final set of cross-sectional tests leverages demographic characteristics to explain variation in the patterns of consumer behavior we document. We merge establishment-level data with information about each establishment's corresponding county using data from the 2016 American Community Survey from the Census Bureau. We infer the demographic information about the likely visitors to the establishment based on the population of the geographic area in which a given establishment resides. We predict that the increase in store visits following earnings announcements is more pronounced for counties comprised of consumers who are more likely to pay attention to financial media (e.g., English speaking consumers, consumers with some education) and whose demand for products and services is more elastic (e.g., consumers with a moderate level of education and income). We estimate a version of Eq. 1 where we interact *Post* with county-level characteristics, such as the % of the population that is English Speaking, the % that is Caucasian, and the % that has various levels of education or income. The main effect on each of characteristics is subsumed by the store fixed effects.

Table 8, Panel A presents results showing that the effect is more pronounced for stores in counties with a greater proportion of English speakers. To the extent these populations are more likely to consume financial news, these findings dovetail nicely with our evidence that changes in consumer behavior are driven by attention to firms' earnings announcements. We

[9]Following Bowen, DuCharme, and Shores (1995), firms with SIC codes 150-179, 245, 250-259, 283, 301, and 324-399 are categorized as selling durable goods. They primarily include car dealerships and furniture stores from our sample and do not include retailers selling multiple brands like Best Buy and Lowe's.

also explore whether our results vary with the racial makeup of the county in which the store resides. Column 2 of Table 8, Panel A finds that the increase in store visits is more pronounced for stores in counties with a greater proportion of Caucasian people.

Table 8, Panel B presents results showing that the effect is more pronounced for stores in counties with a moderate level of education. Column 1 finds that the increase in visits is less pronounced in counties where a greater proportion of the population has less than or equal to a 9th grade level of education. In contrast, column 2 finds that the increase in visits is more pronounced in counties where a greater proportion of the population has between a 9th grade and Bachelor's level of education. Column 3 finds that the effect is less pronounced when a greater proportion of the population has a level of education greater than a Bachelor's degree. These results suggest that variation in the role of earnings announcements in shaping consumer behavior is nonlinear in consumers' education levels.

Table 8, Panel B also finds that the effect is more pronounced for stores in counties with a moderate level of income. Column 4 finds an insignificant coefficient on Post $\times$ % of Income$<=$45K. Column 5 finds that the increase in visits is more pronounced in counties where a greater proportion of the population has an annual family income between \$45,000 and \$125,000. Column 6 finds that the effect is less pronounced when a greater proportion of the population has an annual family income greater than \$125,000. These income amounts are in 2016 USD. According to the Census Bureau, \$45,000 and \$125,000 approximately correspond to the upper limits of the third to fourth quintiles, respectively, of 2016 US household income. To the extent consumers with a moderate level of education and income are more likely to both consume financial media and have relatively elastic demand for normal (non-luxury) goods and services, these are the counties where we should expect the role of earnings announcements in shaping consumer behavior to be most pronounced.

In addition to highlighting how financial reporting interacts with demographics to shape consumer behavior, the results in Table 8 also help rule out several alternative explanations related to firm-level patterns that occur in event-time relative to earnings announcements. For example, unobserved variation in sales or promotions that occur in event-time relative to the earnings announcement would need to also explain relatively pronounced effects in these population groups in order to fully explain our results.

4. Supplementary Tests

In this section, we report results from two sets of tests designed to mitigate concerns about our main inferences.

4.1. Measuring Economic Activity

Our results thus far show that foot-traffic to a store increases in the days following the earnings announcement of the public firm operating the store. The increase in store visits varies with cross-sectional characteristics associated with the extent to which the announcement garners attention and, for some firms, the information in the announcement. We also find variation in the effect attributable to demographic characteristics. However, a limitation of the results thus far is that our focus is on GPS data, which may not track variation in firms' revenues. For example, increased foot-traffic does not necessarily translate to increased sales, and increased foot-traffic may be largely unimportant for firms operating primarily online and relying on consumers' online purchases.

To address this limitation, we conduct two tests to show that our main findings represent real economic activity. Our first supplementary test investigates whether the post-announcement increase in foot-traffic is associated with greater contemporaneous sales (i.e., sales reported at the next earnings announcement). If the post-announcement uptick in visits reflects real economic activity, then we should expect to observe that increased post-announcement store visits, both in raw terms and relative to the pre-announcement benchmark, are associated with greater subsequent sales.

To test whether the foot-traffic results in increased quarterly sales, we follow Froot et al. (2017) and estimate the following regression, where the unit of analysis is the firm-quarter:

$$\begin{aligned} SUR_{i,q} =& \beta_1 ln(1 + average\ daily\ visits\ post\ EA)_{i,q-1} + \beta_2 SUR_{i,q-1} + \beta_3 SUR_{i,q-4} \\ &+ \Sigma\beta_i Firm_i + \Sigma\beta_{j,t} Industry \times Year\text{-}month_{j,t} + \epsilon_{i,q} \end{aligned} \tag{2}$$

where $ln(1 + average\ daily\ visits\ post\ EA)_{i,q-1}$ is the log of 1 plus average daily visits to all stores operated by firm i in the week after firm i's previous earnings announcement, measured using SafeGraph foot-traffic data. $SUR_{i,q}$ is the Standardized Unexpected Revenue for the current quarter. We include controls for one-quarter-prior and four-quarters-prior SUR to account for the trends and seasonality in firms' revenues. We also include firm and industry $\times$ year-month fixed effects. The inclusion of firm fixed effects isolates within-firm variation in sales. The inclusion of industry $\times$ year-month fixed effects accounts for time-varying, industry-specific factors (e.g., industry-specific growth).

In Table 9, we show post-announcement increases in foot-traffic correspond to increases in firms' revenues. The positive coefficient on $ln(1 + average\ daily\ visits\ post\ EA)_{i,q-1}$ suggests increases in post-announcement foot-traffic align with variation in firms' total sales in the current quarter (i.e., the quarter during which the foot-traffic is measured). These results also mitigate concerns that post-announcement foot-traffic merely represents a shifting of sales from further in the future to the few days after the announcement.

We also estimate a version of Eq. 2 which replaces the variable of interest with the *change* in foot-traffic from pre- to post-announcement, $\Delta ln(1 + average\ daily\ visits)_{i,q-1}$ from pre-EA to post-EA. Column 2 of Table 9 finds a positive and marginally significant coefficient on the variable of interest. Measuring the post-earnings announcement store visits as a change in store visits mitigates concerns that firm-specific factors unrelated to the earnings announcement but coinciding with the time period during which the announcement occurs explain the association between store visits and contemporaneous total sales. Thus, alternative explanations for these findings would need to explain both variation in total sales for the quarter and changes in foot-traffic between the short window pre-announcement and the short window post-announcement.

Our second supplementary test corroborates our main findings using transaction-level data from an alternative data source, Comscore. The use of this alternative sample allows us to test whether earnings announcements play a role in shaping economic activity using fine-grained transaction data. Furthermore, the sample enables to examine whether our main results generalize to the e-commerce space, which comprises an increasingly important portion of consumer purchases.

Comscore provides information about online transactions for approximately 150 of the largest e-commerce retailers in the U.S. The data provider relies on an opt-in panel of U.S. internet users' internet browsing activity and identifies consumer purchase transactions using a page-scraping agent. We obtain transaction-level data for 2006 to 2019. We clean the Comscore data by ensuring each machine (e.g., a user purchasing products from a computer) in the sample makes more than one purchase a year. Then we aggregate the transactions to the domain-day level, where each transaction represents a user in Comscore's panel who has completed the checkout procedure at that domain. In addition, we keep only those domains with at least 300 unique machines making purchases. After matching website domains to Compustat, CRSP, IBES, and Thomson Reuters, we drop firms operating in industries in which consumer visits are likely not discretionary. We drop the firms in the same industries as those dropped in our main sample for analysis. Our final sample is comprised of 135,240 unique firm-days in the $[-10,+10]$ window around earnings announcements.

We test whether online transactions increase in the days after firms' earnings announcements using a short-window event study. We estimate the following regression using the sample of all firm-days in the $[-10,+10]$ calendar days around the announcement date:

$$ln(1 + Daily\ Online\ Transactions)_{i,d} = \beta_1 Post_{i,d} + \theta Controls_{i,d} + \Sigma\beta_k Day\text{-}of\text{-}Week_k + \Sigma\beta_i Firm_i + \Sigma\beta_q Year\text{-}quarter_q + \epsilon_{i,d} \quad (3)$$

where $ln(1 + Daily\ Online\ Transactions)_{i,d}$ is the log of 1 plus the number of transactions at any domain operated by firm i on date d relative to the earnings announcement, measured using Comscore online transaction data. $Post_{i,d}$ is an indicator variable equal to one if the day falls in the period after the earnings announcement (i.e., days 0 to +10 after the earnings announcement) and 0 otherwise. $Controls_{i,d}$ is the same vector of control variables used in our estimation of Eq. 1. The coefficient of interest in Eq. 3 is β_1. If consumers increase their online purchase activity after earnings announcements, we predict $\beta_1 > 0$.

The first version of Eq. 3 we estimate includes day-of-week, firm, and year-quarter fixed effects. Our inclusion of day-of-week fixed effects accounts for variation in activity associated with a particular day of the week, including variation in weekend versus weekday transactions. The inclusion of firm fixed effects focuses exclusively on within-firm variation in transactions. The inclusion of year-quarter fixed effects accounts for any time-varying macroeconomic factors and addresses variation in transactions attributable to the changes in the size of Comscore's panel over time.[10] The second version of Eq. 3 we estimate replaces the year-quarter fixed effects with industry × year-quarter fixed effects, which account for time-varying, industry-specific factors (e.g., industry-specific growth). Standard errors are clustered by firm and year-quarter, which allows for correlation across year-quarters within a given firm and correlation across firms within a given year-quarter.

The results in Table 10 indicate that, relative to the 10 days before firms' earnings announcement dates, consumers increase their online transactions at domains operated by

[10] Unlike our main sample for analysis using SafeGraph foot-traffic data, our Comscore dataset is much less comprehensive, so we use year-quarter rather than year-month fixed effects to ensure each year-quarter has several firms announcing earnings.

the announcing firm in the 10 days after the announcement. The coefficient on *Post* is positive and significant in column 1 and positive and marginally significant in column 2. The coefficient of 0.009 suggests that daily online transactions on average increase by approximately 0.9% in the 10-day period after the announcement. Taken together, the results in Tables 9 and 10 show that our main results documenting increased store visits likely shape firms' revenues and extend to their e-commerce businesses as well.

4.2. Role of Advertising

A key contribution of our study is in providing evidence that earnings announcements serve a marketing role that influences consumer behavior. Our results suggest that the act of announcing earnings spurs consumer behavior by making the announcing firm more salient in the public eye, which points to an externality of the financial reporting process.

A potential alternative explanation for our findings is that the post-announcement spike in consumer activity is driven by variation in advertising (e.g., national marketing campaigns) that coincides with the announcement. Increases in advertising campaigns around earnings announcements would be problematic for our inferences because we expect that some types of advertising drive consumer demand independent of earnings announcements. Note that this alternative explanation is different from a potential channel for our results—that consumers increase their attention and search activity as a consequence of the earnings announcement, and it is this increase in search activity that drives personalized or retargeted advertising. The alternative explanation we investigate relates to marketing campaigns that are not driven by consumers' increase in search activity. To mitigate this alternative explanation for our findings, we leverage an extensive panel of advertising data obtained from Nielsen Ad Intel spanning 2017 through 2019. Ad Intel collects granular information about the date, medium, and intensity of advertising exposure.

To capture variation in firms' national TV advertising, we examine several measures available in event-time. For each firm-day, we measure TV advertising exposure calculated as the total expenditures for TV ads, total run time of TV ads, and the number of unique national TV ads. We merge these measures with our main sample of earnings announcements.

A key takeaway is that we find no evidence that the amount of national TV advertising significantly changes around firms' earnings announcements, indicating that firms do not appear to align advertising campaigns with announcement dates. Thus, the results of these tests mitigate concerns that non-retargeted advertising is a correlated omitted variable that causes the increases in consumer activity that we document in our main analyses. Similarly, we find that controlling for the extent of advertising in the few days before the measurement of store visits does not remove the variation in post-announcement consumer behavior. These results are presented in Appendix D. By mitigating concerns about our results being driven by this type of advertising, these tests reinforce our inference that earnings announcements serve a marketing function that shapes consumer behavior.

5. Conclusion

We examine the implications of the financial reporting process through the lens of consumer behavior. We show consumer foot-traffic to public companies' locations of commerce significantly increases following their quarterly earnings announcements. Our findings point to consumer behavior being driven by increased consumer attention. In addition, for firms where consumers care more likely about the longevity of the firm, consumers also adjust their behavior to align with information about firms' prospects revealed during earnings announcements. Our results also suggest that financial reporting has a disproportionate impact on some populations based on their income, race, and education. Taken together, the results of our study highlight that the externalities of financial reporting extend beyond the stakeholders typically studied in prior research. In particular, financial reporting shapes consumer demand and has feedback effects on the fundamentals of the firm.

Appendix A. Variable Definitions

Variable	Definition
Δ *Altman Z-score*	fiscal-quarter-matched change in the Altman Z-score (Altman (1968)). The Altman Z-score is calculated as 3.3*EBIT + 0.999*Sales + 0.6*Market Value of Equity/Total Liabilities + 1.2*(Current Assets - Current Liabilities) + 1.4*Retained Earnings. EBIT, Sales, Current Assets - Current Liabilities, and Retained Earnings are all scaled by Total Assets. Market Value of Equity is measured as of the end of the calendar year.
Δ *ln(1+average daily visits) from pre-EA to post-EA*	change in the log of 1 plus average daily store visits from the week before to the week after the firm's previous earnings announcement.
% of 45K<Income<=125K	percent of population with annual family income greater than $45,000 and up to $125,000 in the county in which the store is located, based on the 2016 American Community Survey from the Census Bureau.
% of 9th grade<Edu<=Bachelor's	percent of people with educational attainment greater than 9th grade and up to Bachelor's degree among population 25 years and over in the county in which the store is located, based on the 2016 American Community Survey from the Census Bureau.
% of Caucasian	percent of population that is Caucasian in the county in which the store is located, based on the 2016 American Community Survey from the Census Bureau.
% of Edu<=9th grade	percent of people with educational attainment up to 9th grade among population 25 years and over in the county in which the store is located, based on the 2016 American Community Survey from the Census Bureau.
% of Edu>Bachelor's	percent of people with educational attainment greater than Bachelor's degree among population 25 years and over in the county in which the store is located, based on the 2016 American Community Survey from the Census Bureau.
% of English Speaking	percent of English speaking population in the county in which the store is located, based on the 2016 American Community Survey from the Census Bureau.
% of Income<=45K	percent of population with annual family income up to $45,000 in the county in which the store is located, based on the 2016 American Community Survey from the Census Bureau.
% of Income>125K	percent of population with annual family income greater than $125,000 in the county in which the store is located, based on the 2016 American Community Survey from the Census Bureau.
Analyst Coverage	log of one plus the number of analysts following the firm during the current fiscal quarter, obtained from I/B/E/S.
BTM	book value of equity scaled by market value of equity at the end of the previous fiscal quarter.
Capex	capital expenditures scaled by total assets at the end of the previous fiscal quarter.
Fourth Quarter Indicator	takes the value of 1 if the firm-quarter is the fourth fiscal quarter, and 0 otherwise.
Friday Indicator	takes the value of 1 if the firm announces earnings on a Friday, and 0 otherwise.
Abnormal Google Search Index	the average of the Google ticker search index in the 3 days centered on the firm's earnings announcement date, based on data from Google Trends. The Google search index is normalized for each firm and takes a value between 1 and 100.

Appendix A (cont'd). Variable Definitions

Variable	Definitions
Insider Trading	total insider trades (i.e., sales + purchases) of the CEO and CFO over the 3-month period before the earnings announcement date scaled by shares outstanding at the beginning of the previous fiscal quarter. Insider trading data are obtained from Thomson Reuters.
Insti. Ownership	percent of institutional investors at the most recent fiscal quarter-end, obtained from Thomson Reuters.
K-th Quintile of SURP	takes the value of 1 if the firm-quarter is in the K-th quintile of $SURP$, and 0 otherwise.
Leverage	total liabilities scaled by total assets at the end of the previous fiscal quarter.
ln(1+3-day Moving Avg. Ad Count)	log of 1 plus a 3-day rolling average of unique national TV ads run on the firm-day measured using Ad Intel data.
ln(1+3-day Moving Avg. Ad Expend.)	log of 1 plus a 3-day rolling average of total expenditures (in USD) for national TV ads on the firm-day measured using Ad Intel data.
ln(1+3-day Moving Avg. Ad Run Time)	log of 1 plus a 3-day rolling average of total run time (in seconds) of national TV ads on the firm-day measured using Ad Intel data.
ln(1+average daily visits post-EA)	log of 1 plus average daily store visits during the first week after the firm's previous earnings announcement.
ln(1+Daily Ad Count)	log of 1 plus unique national TV ads run on the firm-day measured using Ad Intel data.
ln(1+Daily Ad Expend.)	log of 1 plus total expenditures (in USD) for national TV ads on the firm-day measured using Ad Intel data.
ln(1+Daily Ad Run Time)	log of 1 plus total run time (in seconds) of national TV ads on the firm-day measured using Ad Intel data.
ln(1+Daily Online Transactions)	log of 1 plus the count of online transactions, measured at the firm-day level using Comscore transactions for all domains operated by the firm.
ln(1+Daily Store Visits)	log of 1 plus store visits measured at the store-day level using SafeGraph data.
ln(MVE)	log of price per share × number of shares outstanding at the end of the current fiscal quarter. MVE is measured in millions of USD.
Loss Indicator	takes the value of 1 if the firm reports a loss in the previous fiscal quarter, and 0 otherwise.
Post	takes the value of 1 if the day is on the earnings announcement or within 10 days after the earnings announcement, and 0 if it is within 10 days before the announcement.
QBHR	buy-and-hold abnormal return over the 3-month period before the earnings announcement date.
ROA	net income scaled by average total assets in the previous fiscal quarter.
Stock Illiquidity	average of daily bid-ask spreads, measured as $100 \times \frac{ask-bid}{(ask+bid)/2}$ over the 3-month period before the earnings announcement date.
Stock Volatility	standard deviation of daily stock returns over the 3-month period before the earnings announcement date.
SUR	standardized unexpected revenue measured as $\frac{(rev_{i,q}-rev_{i,q\text{-}4})-\mu_{i,t}}{\sigma_{i,t}}$ where $\mu_{i,t}$ is the average and $\sigma_{i,t}$ is the standard deviation of $(rev_{i,q} - rev_{i,q\text{-}4})$ over the preceding 8 quarters and $rev_{i,q}$ is the quarterly revenue, following Froot et al. (2017).
SURP	difference between the current EPS before extraordinary items and analysts' consensus scaled by the standard deviation of analyst forecasts in the 90 days prior to the earnings announcement. Analysts' consensus is measured as the median of analysts' forecasts in the 90 days prior to the earnings announcements.

Appendix B. Stores by State

This table reports the count of stores located in each state for our sample.

State	Store Count	State	Store Count
AK	279	MT	763
AL	4,672	NC	8,639
AR	2,958	ND	460
AZ	4,092	NE	1,362
CA	20,793	NH	902
CO	3,851	NJ	4,809
CT	1,974	NM	1,595
DC	393	NV	2,012
DE	729	NY	8,882
FL	14,202	OH	9,273
GA	8,836	OK	3,488
GU	7	OR	2,301
HI	463	PA	8,425
IA	2,469	PR	337
ID	1,166	RI	557
IL	8,399	SC	4,660
IN	5,310	SD	595
KS	2,469	TN	6,103
KY	3,843	TX	23,503
LA	4,224	UT	1,809
MA	3,325	VA	6,632
MD	3,853	VI	2
ME	794	VT	383
MI	6,518	WA	4,147
MN	2,915	WI	3,553
MO	5,366	WV	1,640
MS	2,794	WY	417
Total Store Count			223,943

Appendix C. Validation of the Foot-Traffic Measure

This table reports estimates from firm-quarter-level regressions of standardized unexpected revenue (SUR) on quarterly daily visits following Froot et al. (2017): $SUR_{i,q} = \beta_1 ln(1 + quarterly\ visits)_{i,q} + \beta_2 SUR_{i,q-1} + \beta_3 SUR_{i,q-4} + \Sigma\beta_i Firm_i + \Sigma\beta_{j,t} Industry \times Year\text{-}month_{j,t} + \epsilon_{i,q}$. See Appendix A for variable definitions. All continuous variables are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report t-statistics in parentheses based on two-way cluster robust standard errors, clustered by firm and calendar year-month. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

	SUR	
	(1)	(2)
ln(1+quarterly average of daily visits)	0.538*	
	(1.789)	
ln(1+total quarterly visits)		0.411**
		(2.107)
Lagged_SUR	0.509***	0.509***
	(8.362)	(8.376)
Four-quarter-lagged_SUR	-0.064**	-0.064**
	(-2.560)	(-2.561)
N	2,073	2,073
R-sq	0.713	0.713
S.E. clustering	Firm and year-month (two-way)	
Firm FE	Y	Y
Industry × year-month FE	Y	Y

Appendix D. Advertising around Earnings Announcements

Panel A reports estimates from the following firm-day-level regression using the sample of all firm-days in the [−10,+10] calendar-day window around the earnings announcement: $ln(1 + Daily\ Advertising)_{i,d} = \beta_1 Day\ [EA-1, EA+1]_{i,d} + \theta Controls_{i,d} + \Sigma\beta_k Day\text{-}of\text{-}Week_k + \Sigma\beta_i Firm_i + \Sigma\beta_t Year\text{-}month_t + \epsilon_{i,d}$. $Day\ [EA-1, EA+1]_{i,d}$ takes the value of 1 if the firm-day is between −1 and +1 days relative to the announcement, and 0 otherwise. Panel B reports estimates from the following store-day-level regression using the sample of all store-days in the [−10,+10] calendar-day window around the earnings announcement: $ln(1 + Daily\ Store\ Visits)_{s,d} = \beta Post_{s,d} + \gamma ln(1 + 3\text{-}day\ Advertising) + \theta Controls_{i,d} + \Sigma\beta_k Day\text{-}of\text{-}Week_k + \Sigma\beta_s Stores_s + \Sigma\beta_t Year\text{-}month_t + \epsilon_{s,d}$. $Post_{s,d}$ takes the value of 1 if the store-day is on the day of or after the operating firm's earnings announcement, and 0 otherwise. *3-day Advertising* is a rolling average of daily advertising over the last 3 days. See Appendix A for variable definitions. All continuous variables are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report t-statistics in parentheses based on two-way cluster robust standard errors, clustered by firm and calendar year-month. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

Panel A: Change in Advertising around Announcements

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(1+Daily Ad Count)		ln(1+Daily Ad Expend.)		ln(1+Daily Ad Run Time)	
Day [EA-1, EA+1]	0.014	0.013	0.025	0.020	0.024	0.021
	(0.449)	(0.305)	(0.398)	(0.317)	(0.530)	(0.444)
N	37,289	37,289	37,289	37,289	37,289	37,289
R-sq	0.694	0.691	0.655	0.652	0.658	0.655
S.E. clustering	Firm and year-month (two-way)					
Controls	Y	Y	Y	Y	Y	Y
Day-of-Week FE	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	N	Y	N	Y	N
Year-quarter FE	N	Y	N	Y	N	Y

Panel B: Controlling for Advertising

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(1+Daily Store Visits)					
Post	0.010**	0.011**	0.010**	0.011**	0.010**	0.011**
	(2.092)	(2.165)	(2.089)	(2.172)	(2.094)	(2.199)
ln(1+3-day Moving Avg. Ad Count)	-0.001	0.007***				
	(-0.196)	(4.027)				
ln(1+3-day Moving Avg. Ad Expend.)			-0.001	0.002***		
			(-0.517)	(3.948)		
ln(1+3-day Moving Avg. Ad Run Time)					-0.001	0.004***
					(-0.394)	(4.309)
N	43,214,572	43,214,558	43,214,572	43,214,558	43,214,572	43,214,558
R-sq	0.811	0.822	0.811	0.822	0.811	0.822
S.E. clustering	Firm and year-month (two-way)					
Controls	Y	Y	Y	Y	Y	Y
Day-of-Week FE	Y	Y	Y	Y	Y	Y
Store FE	Y	Y	Y	Y	Y	Y
Year-month FE	Y	N	Y	N	Y	N
County × year-month FE	N	Y	N	Y	N	Y
Industry × year-month FE	N	Y	N	Y	N	Y

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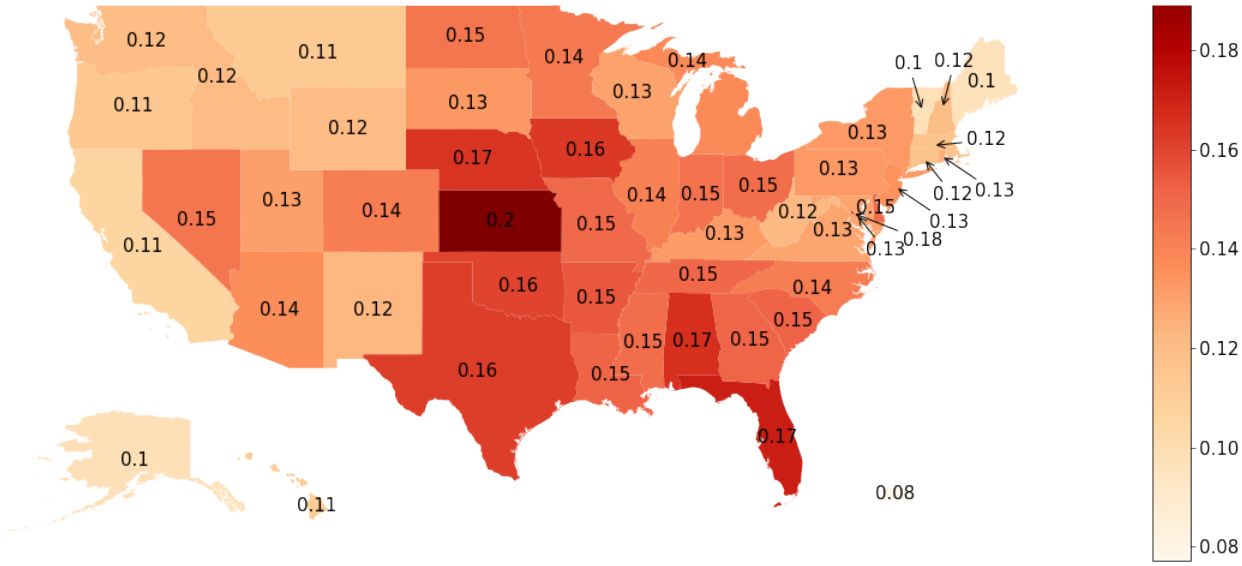
Fig. 1. SafeGraph Data Coverage

Panels A and B show the coverage ratios of the SafeGraph data by state (i.e., number of people covered by SafeGraph divided by total population in the state) for the last month (February 2020) and first month (January 2017) of our sample period, respectively.

Panel A: February 2020



Panel B: January 2017

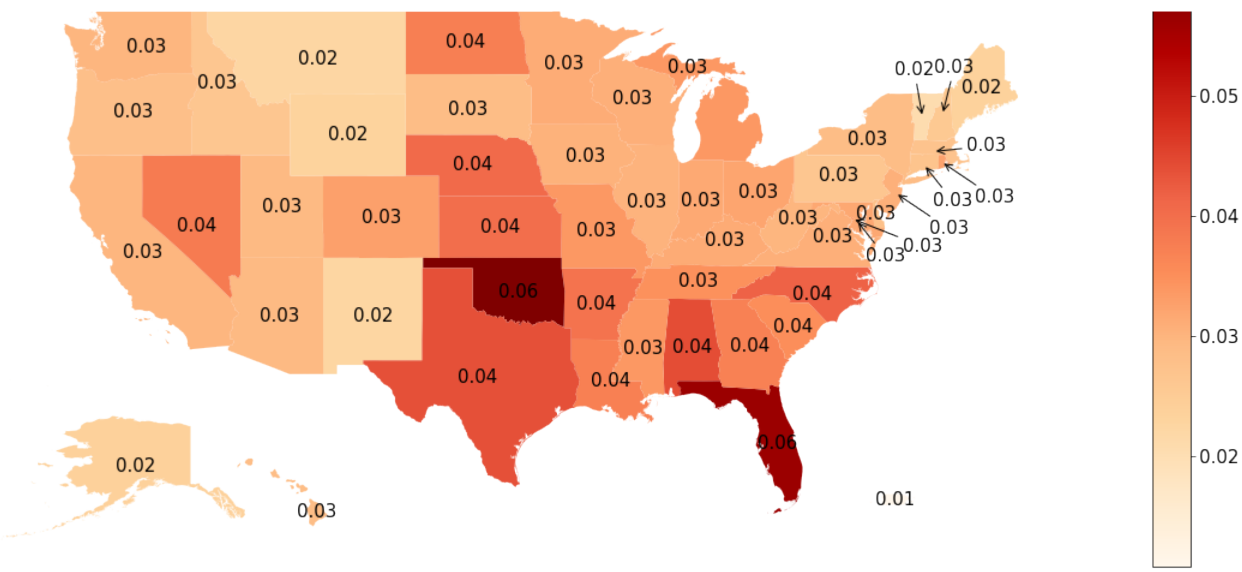


Fig. 2. Industry Composition

The pie chart below shows the industry composition of our sample firms disaggregated into 2-digit NAICS codes.

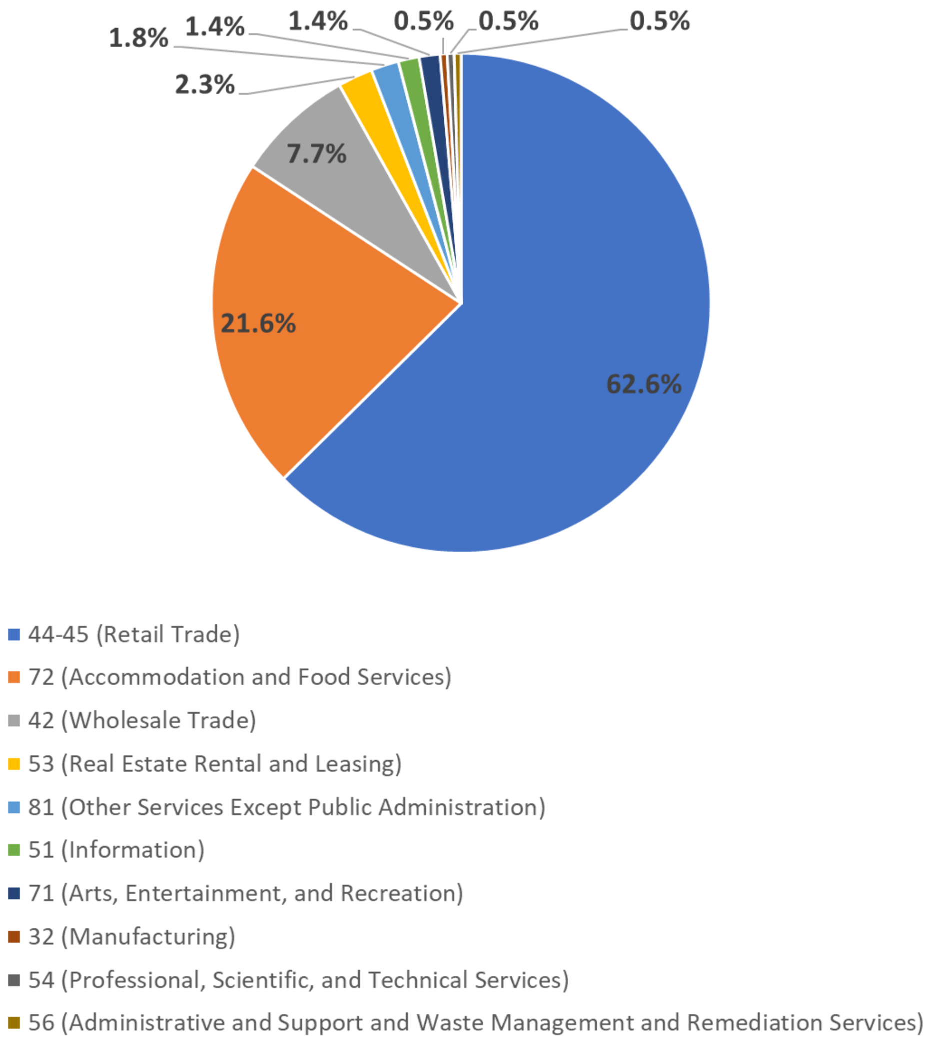


Fig. 3. Ralph Lauren's Announcement on July 31, 2018

This figure plots Ralph Lauren's average store visits around its earnings announcement on July 31, 2018. To control for day-of-week effects, the average store visit for each day is demeaned by Ralph Lauren's average store visit on a given day of the week during our sample period.

Fig. 4. Dynamic Regression

This figure plots coefficients β_1, β_2, β_3, β_4, β_5, and β_6 and their 90% confidence intervals estimated from the following store-day-level regression: $ln(1 + Daily\ Store\ Visits)_{s,d} = \beta_1 Day\ [EA-4, EA-3]_{s,d} + \beta_2 Day\ [EA-2, EA-1]_{s,d} + \beta_3 Day\ [EA, EA+2]_{s,d} + \beta_4 Day\ [EA+3, EA+4]_{s,d} + \beta_5 Day\ [EA+5, EA+6]_{s,d} + \beta_6 Day\ [EA+7, EA+10]_{s,d} + \theta Controls_{i,d} + \Sigma\beta_k Day\text{-}of\text{-}Week_k + \Sigma\beta_s Store_s + \Sigma\beta_t Year\text{-}month_t + \epsilon_{s,d}$. $Day\ [EA+a, EA+b]_{s,d}$ takes the value of 1 if the store-day is between a and b days relative to the announcement, and 0 otherwise. Results of this regression are tabulated in Table 3. See Appendix A for variable definitions. All continuous variables are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report t-statistics in parentheses based on two-way cluster robust standard errors, clustered by firm and calendar year-month.

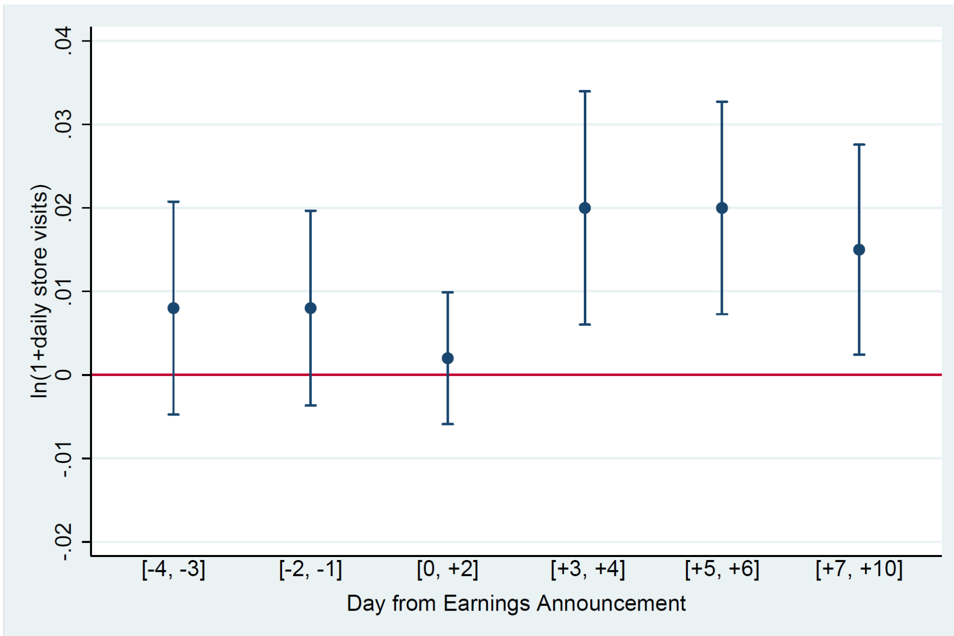


Table 1. Summary statistics

The sample consists of store-days operated by publicly traded firms around their announcements from January 2017 to February 2020. See Appendix A for variable definitions. To facilitate interpretation, we present summary statistics for raw, unlogged variable values. All continuous variables, except for the buy-and-hold abnormal stock returns, are winsorized at the 1% and 99% levels to limit the influence of outliers.

Variable	Mean	Median	S.D.	Min	Max	*N*
Store-Day Variable						
Daily Store Visits	17.7	9	31.5	0	636	47,882,818
Firm-Day Variable						
Daily Online Transactions	6.27	1	32.4	1	1474	135,240
Firm-Quarter Variables						
SURP	1.3	1.0	3.0	-6.3	9.5	2,050
Loss Indicator	0.2	0	0.4	0	1	2,485
Leverage	0.69	0.63	0.37	0.30	4.07	2,485
BTM	0.45	0.36	0.48	-0.30	2.71	2,485
Capex	0.03	0.02	0.03	0.002	0.13	2,485
ROA	0.01	0.01	0.03	-0.08	0.11	2,485
MVE (in millions)	17,610	2,477	48,074	57	347,498	2,485
Fourth Quarter Indicator	0.3	0	0.4	0	1	2,485
QBHR	0.02	0.01	0.19	-0.40	0.42	2,485
Stock Illiquidity	0.17	0.05	0.31	0.01	1.37	2,485
Stock Volatility	0.02	0.02	0.01	0.01	0.06	2,485
Insti. Ownership	0.74	0.83	0.29	0.00	1.00	2,485
Insider Trading	0.14	0	0.45	0	2.26	2,485
Analyst Coverage	11.6	10	8.7	0	31	2,485
Friday Indicator	0.08	0	0.28	0	1	2,485
Abnormal Google Search Index	52.1	53.1	22.8	14.1	97.9	1,330
Altman Z-Score	4.2	3.6	2.6	0.98	12.9	2,350
SUR	4.44	1.95	7.91	-10.89	46.11	1,933
Census Variables (by county)						
% of English Speaking	0.98	0.99	0.04	0.76	1.0	3,082
% of Caucasian	0.83	0.90	0.16	0.30	0.97	3,082
% of Edu <= 9th grade	0.08	0.06	0.05	0.02	0.23	3,082
% of 9th grade < Edu <= Bachelor's	0.85	0.86	0.05	0.65	0.91	3,082
% of Edu > Bachelor's	0.07	0.06	0.04	0.03	0.29	3,082
% of Income <= 45K	0.38	0.38	0.10	0.14	0.56	3,082
% of 45K < Income <= 125K	0.49	0.50	0.06	0.34	0.59	3,082
% of Income > 125K	0.13	0.11	0.07	0.05	0.47	3,082

Table 2. Changes in Store Visits after Earnings Announcements

This table reports estimates from the following store-day-level regression using the sample of all store-days in the [−10,+10] calendar-day window around the earnings announcement: $ln(1 + Daily\ Store\ Visits)_{s,d} = \beta Post_{s,d} + \theta Controls_{i,d} + \Sigma\beta_k Day\text{-}of\text{-}Week_k + \Sigma\beta_s Store_s + \Sigma\beta_t Year\text{-}month_t + \epsilon_{s,d}$. $Post_{s,d}$ takes the value of 1 if the store-day is on the day of or after the operating firm's earnings announcement, and 0 otherwise. See Appendix A for variable definitions. All continuous variables are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report t-statistics in parentheses based on two-way cluster robust standard errors, clustered by firm and calendar year-month. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

	ln(1+Daily Store Visits)			
	(1)	(2)	(3)	(4)
Post	0.011**	0.011**	0.011**	0.011**
	(2.211)	(2.223)	(2.251)	(2.270)
Loss Indicator	-0.027*	-0.023*	-0.028**	-0.024*
	(-1.905)	(-1.906)	(-2.135)	(-1.957)
Leverage	-0.010	0.047	-0.017	-0.004
	(-0.232)	(1.407)	(-0.436)	(-0.097)
BTM	0.014	0.023	0.053*	0.048*
	(0.404)	(0.704)	(1.858)	(1.702)
Capex	0.005	-0.101	-0.139	-0.078
	(0.018)	(-0.392)	(-0.490)	(-0.335)
ROA	-0.645**	-0.579**	-0.280	-0.329**
	(-2.661)	(-2.673)	(-1.467)	(-2.135)
ln(MVE)	0.109***	0.093***	0.061***	0.055***
	(4.152)	(4.363)	(2.890)	(2.780)
Fourth Quarter Indicator	-0.017	-0.021	0.012	0.011
	(-1.148)	(-1.628)	(0.786)	(0.794)
QBHR	0.004	-0.010	0.014	0.004
	(0.116)	(-0.328)	(0.325)	(0.101)
Stock Illiquidity	0.043	0.004	-0.009	-0.024
	(1.319)	(0.166)	(-0.175)	(-0.551)
Stock Volatility	-0.090	-0.183	0.398	0.044
	(-0.111)	(-0.231)	(0.455)	(0.049)
Insti. Ownership	-0.001	0.000	-0.047	-0.035
	(-0.024)	(0.014)	(-1.652)	(-1.216)
Insider Trading	0.007	0.004	0.015	0.013
	(0.775)	(0.510)	(0.868)	(0.796)
Analyst Coverage	-0.018	-0.010	0.001	0.002
	(-0.816)	(-0.464)	(0.067)	(0.082)
N	47,882,818	47,882,805	47,882,818	47,882,805
R-sq	0.811	0.817	0.815	0.821
S.E. clustering	Firm and year-month (two-way)			
Day-of-Week FE	Y	Y	Y	Y
Store FE	Y	Y	Y	Y
Year-month FE	Y	N	N	N
County × year-month FE	N	Y	N	Y
Industry × year-month FE	N	N	Y	Y

Table 3. Dynamic Changes in Store Visits after Earnings Announcements

This table reports estimates from the following store-day-level regression using the sample of all store-days in the [−10,+10] calendar-day window around the earnings announcement: $ln(1 + Daily\ Store\ Visits)_{s,d} = \beta_1 Day\ [EA-4, EA-3]_{s,d} + \beta_2 Day\ [EA-2, EA-1]_{s,d} + \beta_3 Day\ [EA, EA+2]_{s,d} + \beta_4 Day\ [EA+3, EA+4]_{s,d} + \beta_5 Day\ [EA+5, EA+6]_{s,d} + \beta_6 Day\ [EA+7, EA+10]_{s,d} + \theta Controls_{i,d} + \Sigma\beta_k Day\text{-}of\text{-}Week_k + \Sigma\beta_s Store_s + \Sigma\beta_t Year\text{-}month_t + \epsilon_{s,d}$. $Day\ [EA+a, EA+b]_{s,d}$ takes the value of 1 if the store-day is between a and b days relative to the announcement, and 0 otherwise. See Appendix A for variable definitions. All continuous variables are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report t-statistics in parentheses based on two-way cluster robust standard errors, clustered by firm and calendar year-month. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

	ln(1+Daily Store Visits)			
	(1)	(2)	(3)	(4)
Day [EA-4, EA-3]	0.008	0.008	0.008	0.008
	(1.032)	(1.048)	(1.094)	(1.088)
Day [EA-2, EA-1]	0.008	0.009	0.009	0.009
	(1.128)	(1.157)	(1.238)	(1.249)
Day [EA, EA+2]	0.002	0.002	0.002	0.002
	(0.417)	(0.458)	(0.462)	(0.512)
Day [EA+3, EA+4]	0.020**	0.020**	0.020**	0.020**
	(2.353)	(2.371)	(2.409)	(2.425)
Day [EA+5, EA+6]	0.020***	0.020***	0.020***	0.020***
	(2.585)	(2.623)	(2.726)	(2.728)
Day [EA+7, EA+10]	0.015**	0.015*	0.016**	0.016**
	(1.961)	(1.960)	(2.018)	(2.036)
N	47,882,818	47,882,805	47,882,818	47,882,805
R-sq	0.811	0.817	0.815	0.821
S.E. clustering	Firm and year-month (two-way)			
Controls	Y	Y	Y	Y
Day-of-Week FE	Y	Y	Y	Y
Store FE	Y	Y	Y	Y
Year-month FE	Y	N	N	N
County × year-month FE	N	Y	N	Y
Industry × year-month FE	N	N	Y	Y

Table 4. Role of Earnings Surprises

This table reports estimates from the following store-day-level regression: $ln(1 + Daily\ Store\ Visits)_{s,d} = \sum_{k=2}^{5} \beta Kth\ Quintile\ of\ SURP_{i,q} + \sum_{k=1}^{5} \beta Post_{s,d} \times Kth\ Quintile\ of\ SURP_{i,q} + \theta Controls_{i,d} + \Sigma\beta_k Day\text{-}of\text{-}Week_k + \Sigma\beta_s Stores_s + \Sigma\beta_t Year\text{-}month_t + \epsilon_{s,d}$. $Post_{s,d}$ takes the value of 1 if the store-day is on the day of or after the operating firm's earnings announcement, and 0 otherwise. See Appendix A for other variable definitions. All continuous variables are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report t-statistics in parentheses based on two-way cluster robust standard errors, clustered by firm and calendar year-month. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

	ln(1+Daily Store Visits)			
	(1)	(2)	(3)	(4)
2nd Quintile of SURP	0.004	0.004	0.002	0.006
	(0.285)	(0.319)	(0.109)	(0.392)
3rd Quintile of SURP	-0.009	-0.007	-0.002	0.006
	(-0.567)	(-0.460)	(-0.121)	(0.429)
4th Quintile of SURP	-0.012	-0.010	-0.012	-0.009
	(-0.798)	(-0.736)	(-0.842)	(-0.698)
5th Quintile of SURP	-0.013	-0.015	-0.013	-0.013
	(-0.806)	(-1.045)	(-0.834)	(-1.064)
Post × 1st Quintile of SURP	0.011**	0.011**	0.010**	0.010**
	(2.106)	(2.112)	(2.069)	(2.083)
Post × 2nd Quintile of SURP	0.003	0.003	0.003	0.003
	(0.577)	(0.580)	(0.621)	(0.633)
Post × 3rd Quintile of SURP	0.010	0.010	0.010	0.010
	(1.193)	(1.191)	(1.186)	(1.186)
Post × 4th Quintile of SURP	0.012**	0.013**	0.014**	0.014**
	(2.026)	(2.056)	(2.198)	(2.212)
Post × 5th Quintile of SURP	0.019**	0.019**	0.019**	0.019**
	(2.557)	(2.557)	(2.530)	(2.536)
N	44,549,572	44,549,563	44,549,572	44,549,563
R-sq	0.814	0.821	0.818	0.825
S.E. clustering	Firm and year-month (two-way)			
Controls	Y	Y	Y	Y
Day-of-Week FE	Y	Y	Y	Y
Store FE	Y	Y	Y	Y
Year-month FE	Y	N	N	N
County × year-month FE	N	Y	N	Y
Industry × year-month FE	N	N	Y	Y

Table 5. Interactions with Fourth Fiscal Quarter and Friday Indicators

Panel A reports estimates from the following store-day-level regression using the sample of all store-days in the [−10,+10] calendar-day window around the earnings announcement: $ln(1 + Daily\ Store\ Visits)_{s,d} = \beta_1 Post_{s,d} + \beta_2 Fourth\ Quarter\ Ind._{i,q} + \beta_3 Post_{s,d} \times Fourth\ Quarter\ Ind._{i,q} + \theta Controls_{i,d} + \Sigma\beta_k Day\text{-}of\text{-}Week_k + \Sigma\beta_s Store_s + \Sigma\beta_t Year\text{-}month_t + \epsilon_{s,d}$. Panel B reports estimates from the following store-day-level regression: $ln(1 + Daily\ Store\ Visits)_{s,d} = \beta_1 Post_{s,d} + \beta_2 Friday\ Ind._{i,q} + \beta_3 Post_{s,d} \times Friday\ Ind._{i,q} + \theta Controls_{i,d} + \Sigma\beta_k Day\text{-}of\text{-}Week_k + \Sigma\beta_s Store_s + \Sigma\beta_t Year\text{-}month_t + \epsilon_{s,d}$. $Post_{s,d}$ takes the value of 1 if the store-day is on the day of or after the operating firm's earnings announcement, and 0 otherwise. See Appendix A for other variable definitions. All continuous variables are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report t-statistics in parentheses based on two-way cluster robust standard errors, clustered by firm and calendar year-month. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

Panel A: Fourth Fiscal Quarter Effect

	ln(1+Daily Store Visits)			
	(1)	(2)	(3)	(4)
Post	0.005	0.005	0.006	0.006
	(0.925)	(0.933)	(1.000)	(1.015)
Fourth Quarter Ind.	-0.028*	-0.033**	0.001	0.000
	(-1.859)	(-2.334)	(0.061)	(0.006)
Post × Fourth Quarter Ind.	0.022**	0.022**	0.021**	0.021**
	(2.516)	(2.466)	(2.465)	(2.449)
N	47,882,818	47,882,805	47,882,818	47,882,805
R-sq	0.811	0.817	0.815	0.821
S.E. clustering	Firm and year-month (two-way)			
Controls	Y	Y	Y	Y
Day-of-Week FE	Y	Y	Y	Y
Store FE	Y	Y	Y	Y
Year-month FE	Y	N	N	N
County × year-month FE	N	Y	N	Y
Industry × year-month FE	N	N	Y	Y

Panel B: Friday Effect

	ln(1+Daily Store Visits)			
	(1)	(2)	(3)	(4)
Post	0.013***	0.013***	0.012***	0.012***
	(2.820)	(2.887)	(2.650)	(2.712)
Friday Ind.	0.029	0.034	0.023	0.053
	(1.037)	(1.052)	(0.620)	(1.403)
Post × Friday Ind.	-0.022***	-0.023***	-0.021**	-0.021**
	(-2.950)	(-3.035)	(-2.328)	(-2.372)
N	47,882,818	47,882,805	44,549,572	44,549,563
R-sq	0.811	0.821	0.814	0.825
S.E. clustering	Firm and year-month (two-way)			
Controls	Y	Y	Y	Y
Controls for SURP and Post × SURP	N	N	Y	Y
Day-of-Week FE	Y	Y	Y	Y
Store FE	Y	Y	Y	Y
Year-month FE	Y	N	Y	N
County × year-month FE	N	Y	N	Y
Industry × year-month FE	N	Y	N	Y

Table 6. Interactions with Abnormal Google Search Volume

This table reports estimates from the following store-day-level regression using the sample of all store-days in the $[-10,+10]$ calendar-day window around the earnings announcement: $ln(1 + Daily\ Store\ Visits)_{s,d} = \beta_1 Post_{s,d} + \beta_2 ln(Abnormal\ Google\ Search\ Index)_{i,q} + \beta_3 Post_{s,d} \times ln(Abnormal\ Google\ Search\ Index)_{i,q} + \theta Controls_{i,d} + \Sigma\beta_k Day\text{-}of\text{-}Week_k + \Sigma\beta_s Store_s + \Sigma\beta_t Year\text{-}month_t + \epsilon_{s,d}$. $Post_{s,d}$ takes the value of 1 if the store-day is on the day of or after the operating firm's earnings announcement, and 0 otherwise. See Appendix A for other variable definitions. All continuous variables are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report t-statistics in parentheses based on two-way cluster robust standard errors, clustered by firm and calendar year-month. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

	ln(1+Daily Store Visits)			
	(1)	(2)	(3)	(4)
Post	-0.042**	-0.042**	-0.041**	-0.041**
	(-2.035)	(-2.032)	(-1.965)	(-1.970)
ln(Abnormal Google Search Index [EA-1, EA+1])	0.021	0.019	0.007	0.000
	(1.103)	(1.111)	(0.480)	(0.020)
Post × ln(Abnormal Google Search Index [EA-1, EA+1])	0.014***	0.014***	0.014***	0.014***
	(2.866)	(2.879)	(2.779)	(2.790)
N	32,367,818	32,367,806	32,367,818	32,367,806
R-sq	0.828	0.835	0.832	0.838
S.E. clustering	Firm and year-month (two-way)			
Controls	Y	Y	Y	Y
Day-of-Week FE	Y	Y	Y	Y
Store FE	Y	Y	Y	Y
Year-month FE	Y	N	N	N
County × year-month FE	N	Y	N	Y
Industry × year-month FE	N	N	Y	Y

Table 7. Role of Information on Solvency

This table reports estimates from the following store-day-level regressions using the sample of all store-days in the [−10,+10] calendar-day window around the earnings announcement for all firms, firms selling durable goods, and firms not selling durable goods, respectively: $ln(1 + Daily\ Store\ Visits)_{s,d} = \beta_1 Post_{s,d} + \beta_2 \Delta Altman\ Z\text{-}score_{i,q} + \beta_3 Post_{s,d} \times \Delta Altman\ Z\text{-}score_{i,q} + \theta Controls_{i,d} + \Sigma\beta_k Day\text{-}of\text{-}Week_k + \Sigma\beta_s Store_s + \Sigma\beta_t Year\text{-}month_t + \epsilon_{s,d}$. $Post_{s,d}$ takes the value of 1 if the store-day is on the day of or after the operating firm's earnings announcement, and 0 otherwise. Following Bowen, DuCharme, and Shores (1995), firms with SIC codes 150-179, 245, 250-259, 283, 301, and 324-399 are categorized as selling durable goods. See Appendix A for other variable definitions. All continuous variables are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report t-statistics in parentheses based on two-way cluster robust standard errors, clustered by firm and calendar year-month. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

	ln(1+Daily Store Visits)		
	All Firms	Firms Selling Durable Goods	Firms Not Selling Durable Goods
	(1)	(2)	(3)
Post	0.010*	-0.003	0.010*
	(1.890)	(-0.348)	(1.794)
Δ Altman Z-score	0.022***	-0.011	0.021***
	(3.180)	(-1.076)	(3.022)
Post × Δ Altman Z-score	-0.001	0.028**	-0.001
	(-0.537)	(2.352)	(-0.449)
N	44,813,556	1,434,317	43,379,226
R-sq	0.823	0.746	0.826
S.E. clustering	Firm and year-month (two-way)		
Controls	Y	Y	Y
Day-of-Week FE	Y	Y	Y
Store FE	Y	Y	Y
Year-month FE	N	N	N
County × year-month FE	Y	Y	Y
Industry × year-month FE	Y	Y	Y

Table 8. Role of Visitor Demographics

This table reports estimates from the following store-day-level regression using the sample of all store-days in the [−10,+10] calendar-day window around the earnings announcement: $ln(1 + Daily\ Store\ Visits)_{s,d} = \beta_1 Post_{s,d} + \beta_2 Post_{s,d} \times Demographic\ of\ Store\ Location_s + \theta Controls_{i,d} + \Sigma\beta_k Day\text{-}of\text{-}Week_k + \Sigma\beta_s Stores_s + \Sigma\beta_t Year\text{-}month_t + \epsilon_{s,d}$. $Post_{s,d}$ takes the value of 1 if the store-day is on the day of or after the operating firm's earnings announcement, and 0 otherwise. See Appendix A for other variable definitions. All continuous variables are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report t-statistics in parentheses based on two-way cluster robust standard errors, clustered by firm and calendar year-month. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

Panel A: Race and Language

	ln(1+Daily Store Visits)	
	(1)	(2)
Post	-0.098***	-0.003
	(-3.789)	(-0.381)
Post × % of English Speaking	0.114***	
	(4.259)	
Post × % of Caucasian		0.019**
		(2.076)
N	47,881,490	47,881,490
R-sq	0.821	0.821
S.E. clustering	Firm and year-month (two-way)	
Controls	Y	Y
Day-of-Week FE	Y	Y
Store FE	Y	Y
Year-month FE	N	N
County × year-month FE	Y	Y
Industry × year-month FE	Y	Y

Panel B: Education and Income

	ln(1+Daily Store Visits)					
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.015***	-0.063***	0.018***	0.006	-0.020	0.018***
	(2.875)	(-2.858)	(3.477)	(0.879)	(-1.542)	(3.575)
Post × % of Edu<=9th grade	-0.056**					
	(-2.336)					
Post × % of 9th<Edu<=Bachelor's		0.091***				
		(3.461)				
Post × % of Edu>Bachelor's			-0.066***			
			(-2.711)			
Post × % of Income<=45K				0.015		
				(1.286)		
Post × % of 45K<Income<=125K					0.066**	
					(2.493)	
Post × % of Income>125K						-0.034***
						(-2.874)
N	47,881,490	47,881,490	47,881,490	47,881,490	47,881,490	47,881,490
R-sq	0.821	0.821	0.821	0.821	0.821	0.821
S.E. clustering	Firm and year-month (two-way)					
Controls	Y	Y	Y	Y	Y	Y
Day-of-Week FE	Y	Y	Y	Y	Y	Y
Store FE	Y	Y	Y	Y	Y	Y
Year-month FE	N	N	N	N	N	N
County × year-month FE	Y	Y	Y	Y	Y	Y
Industry × year-month FE	Y	Y	Y	Y	Y	Y

Table 9. Impact of Post-Announcement Visits

This table reports estimates from firm-quarter-level regressions of standardized unexpected revenue (SUR) on near-announcement store visits following Froot et al. (2017). Column (1) reports estimates from the following firm-quarter-level regression: $SUR_{i,q} = \beta_1 ln(1 + \textit{average daily visits post EA})_{i,q-1} + \beta_2 SUR_{i,q-1} + \beta_3 SUR_{i,q-4} + \Sigma\beta_i Firm_i + \Sigma\beta_{j,t} Industry \times Year\text{-}month_{j,t} + \epsilon_{i,q}$. Column (2) reports estimates from $SUR_{i,q} = \beta_1 \Delta ln(1 + \textit{average daily visits})_{i,q-1} + \beta_2 SUR_{i,q-1} + \beta_3 SUR_{i,q-4} + \Sigma\beta_i Firm_i + \Sigma\beta_{j,t} Industry \times Year\text{-}month_{j,t} + \epsilon_{i,q}$. $SUR_{i,q}$ is measured for the current firm-quarter q. $ln(1 + \textit{average daily visits post EA})_{i,q-1}$ and $\Delta ln(1 + \textit{average daily visits})_{i,q-1}$ capture store visits around the earnings announcement for the previous firm-quarter q-1. See Appendix A for variable definitions. All continuous variables are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report t-statistics in parentheses based on two-way cluster robust standard errors, clustered by firm and calendar year-month. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

	SUR	
	(1)	(2)
ln(1 + average daily visits post-EA)	0.491**	
	(1.993)	
Δ ln(1 + average daily visits) from pre-EA to post-EA		0.311*
		(1.931)
Lagged SUR	0.488***	0.487***
	(8.315)	(8.211)
Four-quarter-lagged SUR	-0.059**	-0.058**
	(-2.084)	(-2.051)
N	1,933	1,933
R-sq	0.705	0.704
S.E. clustering	Firm and year-month (two-way)	
Firm FE	Y	Y
Industry × year-month FE	Y	Y

Table 10. Changes in Online Transactions after Earnings Announcements

This table reports estimates from the following firm-day-level regression using the sample of all firm-days in the [−10,+10] calendar-day window around the earnings announcement: $ln(1 + Daily\ Online\ Transactions)_{i,d} = \beta_1 Post_{i,d} + \theta Controls_{i,d} + \Sigma\beta_k Day\text{-}of\text{-}Week_k + \Sigma\beta_i Firm_i + \Sigma\beta_q Year\text{-}quarter_q + \epsilon_{i,d}$. $Post_{i,d}$ takes the value of 1 if the firm-day is on the day of or after the firm's earnings announcement, and 0 otherwise. See Appendix A for variable definitions. All continuous variables are winsorized at the 1% and 99% levels to limit the influence of outliers. We estimate and report t-statistics in parentheses based on two-way cluster robust standard errors, clustered by firm and calendar year-quarter. *, **, *** indicate statistical significance at less than 10%, 5%, and 1%, respectively.

	ln(1+Daily Online Transactions)	
	(1)	(2)
Post	0.009**	0.009*
	(1.982)	(1.727)
N	135,240	135,240
R-sq	0.544	0.692
S.E. clustering	Firm and year-qtr (two-way)	
Controls	Y	Y
Day-of-Week FE	Y	Y
Firm FE	Y	Y
Year-qtr FE	Y	N
Industry × year-qtr FE	N	Y