Competition and the Reputational Costs of Litigation

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Abstract

We study the role of competition in customers' reactions to litigation against firms, using anonymized mobile phone location data. A class action lawsuit filing is followed by a 4% average reduction in customer visits to target firms' outlets in the following months. The effect strongly depends on competition. Outlets facing more competition experience significantly larger negative effects. Closer competition matters more, both in terms of geographic and industry proximity. Announcement returns and quarterly accounting revenues around lawsuit filings also strongly depend on competition. Our results suggest that competition is an important component in customers' ability to discipline firms for misbehavior.

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I. Introduction

Lawsuits against firms tend to be associated with a negative stock price reaction for the target firm.[1] Dyck, Morse, and Zingales (2024), among many others, suggest that part of this loss of value is attributable to reputational costs – i.e., the customers of the firm being less inclined to do business with it. Moreover, Karpoff and Lott (1993) argue that the reputational cost constitutes most of the cost incurred by firms accused of fraud. This means that a substantial part of the mechanism through which lawsuits discipline firms is customers' decision to punish firms. While this seems intuitive, the empirical measurement of this disciplining mechanism is difficult, with most evidence based on indirect measures like share price reactions or anecdotal survey data.[2,3]

One important aspect that has not been studied is the fact that customers' ability to vote with their feet depends on the availability of alternatives. In other words, it requires competition. While there are numerous studies suggesting that more competition is associated with lower prices (Dafny, Duggan, and Ramanarayanan (2012), Borenstein and Rose (1994), Kim and Singal (1994), Brown and Goolsbee (2002)), better quality (Matsa (2011)), and reduced governance problems (Giroud and Mueller (2010, 2011)), there is, to the best of our knowledge, no research on the role of competition in enforcing discipline through the legal system. If reputational costs represent a large part of the

[1] See, e.g., Bhagat, Brickley, and Coles (1994), Bizjak and Coles (1995), Bhagat, Bizjak, and Coles (1998) and Gande and Lewis (2009).

[2] For example, a recent CompareCards survey finds that 38% of all Americans feel that at least one firm is behaving wrongly and are willing to express their disapproval by withholding their dollars.

[3] Johnson, Xie, and Yi (2014) find evidence of reputational damage in business volumes with large customers.

potential punishment from litigation, a lack of competition could render the legal system ineffective in disciplining firms.

In this paper, we study the role of competition in the reputational costs associated with class action lawsuits. We conjecture that competition is a crucial part of the efficacy of the legal system to discipline firms for wrongdoing. Prior studies suggest that legal penalties from litigation are small relative to the total value loss to firms (e.g., Bizjak and Coles (1995)). This means that much of the potential cost inflicted on firms must come from reputational losses that result in a loss of business. However, such reputational damage is likely to result in financial losses by reduced business volumes only if customers have alternative providers that they can switch to.

We perform three sets of analyses. First, we analyze changes in monthly customer visits at the outlet level around lawsuit filings. Second, we study stock price reactions to the filings of class action lawsuits. Third, we analyze changes in quarterly accounting revenues around lawsuit filings. The outlet analysis allows a clean identification of reputational effects at a high frequency across a large sample of outlets. Importantly, it also enables constructing detailed measures of the different dimensions of competition at the outlet level.[4] On the other hand, the availability of data for the period from 2018 onward limits the sample of lawsuits underlying this analysis. The analysis of abnormal returns around the filings of lawsuits allows us to compare our results with prior literature (e.g., Gande and Lewis (2009)), while the analysis of quarterly revenues around lawsuit filings shows that our outlet-level results generalize to a comprehensive sample of class

[4]Moreover, it does not suffer from the confounding effects that complicate interpretations of abnormal returns or accounting measures. See, for instance, Karpoff (2012) for a discussion on the measurement of reputational losses.

action lawsuits starting in 1996.

To measure monthly customer visits to each outlet, we use aggregated and anonymized mobile phone data from SafeGraph.[5] The granularity of the data helps us overcome many of the data challenges of studies using accounting data. We collect data on 29 class action lawsuits announced between March 2018 and February 2020 and construct a panel dataset of 25,155 outlets belonging to the target firms. As a control group, we use all retail outlets within the same North American Industry Classification System (NAICS) industry and the same ZIP code as the target outlets. This allows a clean identification of the effect of the lawsuits on target retail customer volumes, controlling for any changes in local business conditions. To measure competition, we calculate Herfindahl-Hirschman indices (HHI) based on the number of stores of competing firms within the target firm's NAICS industry and ZIP code. As an alternative measure for competition, we calculate the simple number of alternative outlets in the same industry and location.

Our results provide strong evidence of reputational costs associated with class action lawsuits. The filing of a class action lawsuit is accompanied by a reduction in monthly customer visits of approximately 4%. The reduction in unique visitors is of a similar magnitude. We also find that competition plays a crucial role in the effect of lawsuits on customer behavior, with significantly larger negative effects in outlets facing more competition. The effects of competition are economically large. A one-standard-deviation increase in competition, as measured by the HHI calculated within the same ZIP code and NAICS industry, results in an additional four percentage points reduction in customer visits. Similarly, we find that a larger number of alternative firms at

[5]SafeGraph data cover approximately 10% of all mobile phone devices in the United States.

the same location is associated with a significantly larger reduction in customer visits, while in extreme cases with only one competing firm, there is no observable reduction in customer visits. These results are consistent with our prediction that competition facilitates retail customers' disciplining of firms.

Next, we study different dimensions of competition. The levels of competition within the same six-digit and four-digit NAICS industries both matter, but competition in the same two-digit NAICS industries does not have any incremental explanatory power. This suggests that competition matters primarily between firms whose products are close substitutes. We find similar results for competition at different geographic aggregation levels. Competition measured at the ZIP code level has the strongest effect on the reduction in customer visits following a lawsuit. Adding county-level competition does not have incremental explanatory power, but state-level competition seems to matter as well. This suggests that there are both a very local and a broad, state-level, component of market power.

A concern with our interpretation of results could be that lawsuits reveal information unrelated to reputational costs, for instance concerning product or service quality. In this case, declines in customer visits and revenues might not reflect customers penalizing firms for moral considerations, but rather customers staying away because of concerns about product and service quality. However, it is important to note that our setting based on securities class action lawsuits limits the scope for such confounding channels as this type of lawsuit is usually not directly related to the products or services provided by a company.[6] Still, we conduct a series of cross-sectional tests to address such

[6]For example, in one of the largest lawsuits in our sample, Papa John's, a large restaurant chain, is alleged

concerns. The negative effect of lawsuits is larger in areas with more religious populations, areas with stronger social norms, measured by the social capital index of Lin and Pursiainen (2022), and in more Republican-voting counties. These demographic differences are likely to be proxies for customers' willingness to punish firms for wrongdoing and to vote with their feet. They thus suggest that the customer reactions we observe are at least partly driven by moral considerations and not by concerns about product or service quality, as these would likely affect all locations similarly. The same is true of possible broader concerns about the integrity of the firm.[7]

We then focus on the stock price response to class action lawsuits. In a comprehensive sample of more than 3,000 class action lawsuits filed since 1996, we find that the filing of a class action lawsuit is associated with large negative stock returns. The average cumulative abnormal return (CAR) is almost -10% over a window of ten days prior to one day after the filing date, consistent with prior literature (e.g., Gande and Lewis (2009)). Supporting our outlet visit results, we find that firms facing more competition experience significantly more negative abnormal returns around the filings of class action lawsuits. This finding is robust to using various measures of competition. It is also statistically and economically significant. A one-standard-deviation increase in competition, as measured by the HHI calculated within the same NAICS industry, results in one percentage point lower CAR.

to have made "materially false and/or misleading statements as well as failed to disclose material adverse facts." These failures to disclose relate to alleged sexual harassment and other inappropriate workplace conduct by Papa John's executives. The lawsuit states that this "conduct would foreseeably have a negative impact on Papa John's business and operations, and expose Papa John's to reputational harm, heightened regulatory scrutiny, and legal liability."

[7]For example, Cline, Walkling, and Yore (2018) find that managers' personal indiscretions can affect their firms' business.

To confirm that our results on customer visits are not confined to the sample of class action lawsuits that overlaps with the SafeGraph data, we also perform an analysis of quarterly accounting revenues using a sample of around 3,400 class action lawsuits filed since 1996. For every firm that is a target of a lawsuit, we use a control group consisting of all other firms in the same six-digit NAICS industry. Relative to the control firms, targets of class action lawsuits experience a 7-8% reduction in revenues in the year following the filing of the lawsuit. As with outlet visits and abnormal returns around lawsuit filings, the reductions in revenues are significantly larger at firms in more competitive industries. A one-standard-deviation increase in competition, as measured by the HHI calculated within the same six-digit NAICS industry, is associated with one-and-a-half percentage points in additional revenue losses.

Our results suggest that retail customers reduce visits to the outlets of firms targeted in class action lawsuits. To do so, they need to be aware of either the lawsuit or the misconduct that led to it. Using data from RavenPack, we find that media attention increases substantially after the filing of a lawsuit, while the media sentiment becomes more negative. We also study the relationship between changes in media coverage of the target firms and the reduction in customer visits or revenue. Results from these analyses show that more negative changes in media sentiment around the lawsuit filing are associated with more negative reactions, as measured by monthly outlet visits and quarterly firm-level revenues.

Taken together, we uncover strong and consistent evidence that the ability of customers to discipline firms for misconduct depends on the competitive environment.

Using different methodologies and across different samples, we find the same patterns of a negative average effect of the filing of class action lawsuits, and this effect depends on the level of competition.

Our results are related to the broader debate on the purpose of the corporation and the role of the stakeholders of the firm. In the spirit of Milton Friedman, Bhagat and Hubbard (2022) argue that "the modern corporation should maximize shareholder value, while conforming to the law of land" and suggest steps to align shareholder wealth maximization with stakeholder interests. This also ties into the burgeoning debate on ESG. Edmans (2023) notes that "E and S is primarily about stakeholders, whereas G often ensures that managers act in shareholders' interest." Our results suggest that customers are important for enforcing the "law of the land". This is consistent with the discussion of corporate reputation by Karpoff (2012). But for customers to be able to perform this role, there needs to be competition. Competition is thus important for governance not only to align management and shareholder incentives (e.g., Giroud and Mueller (2010)), but also to ensure that firms follow the law.

Our findings provide new insights into the importance of competition for the proper functioning of the legal system. As a large part of the effective punishment from lawsuits comes in the form of reputational damage (e.g., Karpoff and Lott (1993), Karpoff and Lott (1999), Alexander (1999)), our results suggest that lack of competition may render the legal system ineffective in deterring corporate misconduct. This might suggest that legal punitive damages need to increase with an increase in market power to maintain the intended deterrent effect. Concerns over market power have increased as industries have

grown more concentrated and profitable, both in the U.S. (Covarrubias, Gutiérrez, and Philippon (2019), Grullon, Larkin, and Michaely (2019)) and globally (De Loecker and Eeckhout (2018), Bae, Bailey, and Kang (2021)).[8] Concerns about the abuse of market power have also led to numerous antitrust investigations (e.g. Azar, Schmalz, and Tecu (2018), Shapiro (2019)).[9]

More broadly, we contribute to the literature on the effects of competition. Product market competition can reduce managerial slack (Hart (1983)), substituting good corporate governance (Giroud and Mueller (2010), Giroud and Mueller (2011)). Aghion, Bloom, Blundell, Griffith, and Howitt (2005) show that competition affects innovation. Raith (2003), Gaspar and Massa (2006), and Irvine and Pontiff (2009) find that higher competition is correlated with higher risk, while Hou and Robinson (2006) show that firms in more concentrated industries earn lower returns. Valta (2012) reports that competition increases the cost of debt. Jung and Subramanian (2017) develop a structural industry equilibrium model and show that CEO talent matters more in more competitive markets. Chod and Lyandres (2011) show that the strategic benefit from being public is larger in more competitive markets. Nickell (1996) finds that competition is positively associated with productivity growth. Ashenfelter and Hannan (1986) show that competition limits firms' ability to discriminate employees based on gender. Pursiainen, Sun, and Xiang (2023) find that product market competition may weaken ESG performance.

Our results on the differential effects of competition measured at various levels may

[8]Kahle and Stulz (2017) report that in the U.S., there were 25% more public corporations in 1975 than there were in 2015. In 1975, the top 109 firms earned 50 percent of the total earnings of all U.S. public firms; in 2015, half of the total earnings were generated by just 30 firms.

[9]See also "Common ownership of shares faces regulatory scrutiny", *Financial Times*, January 22, 2019; "Justice Department Hits Google With Antitrust Lawsuit", *Wall Street Journal*, October 20, 2020.

have important implications in other contexts. We show that competition measured at the micro-level appears significantly more important than competition measured at a broader level, both in terms of geography and industry definitions. This suggests that research on competition benefits from studying settings in which market structures can be observed at granular levels. Our study is also closely related to the literature on shareholders' ability to influence firms through exit (see, e.g., Admati and Pfleiderer (2009), Bharath, Jayaraman, and Nagar (2013).[10] Broccardo, Hart, and Zingales (2022) show theoretically that the exit options of investors and consumers – two important stakeholder groups – share similarities as they represent an effective means of pressuring firms. Homanen (2022) finds evidence of depositors disciplining banks by withdrawing their deposits.

We also make several contributions to the literature on the reputational effects of class action lawsuits and corporate misconduct more broadly. First, our outlet-level analysis provides the most direct evidence to date of customers' decision to discipline firms following a lawsuit. Prior studies focus either on returns (e.g., Bizjak and Coles (1995), Gande and Lewis (2009)) or accounting performance (e.g., Murphy, Shrieves, and Tibbs (2009), Johnson et al. (2014)) around lawsuit filings. Hence, they suffer from potential alternative explanations, such as increases in bankruptcy costs, which makes it difficult to measure the impact of customer response in a clean fashion. We are able to measure customer visits directly at the outlet level, thus avoiding these problems. Second, our finding that the competitive environment of the target firm in a lawsuit is a key determinant of the reputational losses is novel and has important implications for identifying reputational effects in other contexts as well.

[10]See Edmans (2014) for an overview of this literature.

II. Data and Methodology

A. Class Action Lawsuits

We begin our sample construction by retrieving all securities class action lawsuits from the Securities Class Action Clearinghouse (SCAC) database maintained by Stanford Law School. The database covers all securities class actions filed in federal court after the Private Securities Litigation Reform Act of 1995 came into effect. Our initial sample contains 5,883 lawsuits filed between 1996 and late 2020. From this sample, we drop lawsuits involving financial firms and lawsuits involving firms headquartered outside of the U.S. For the different empirical analyses that we conduct in this study, we merge this sample with different datasets.

B. Retail Customer Response to Class Action Lawsuits

We examine the customer response to class action lawsuit filings using aggregated mobile phone data from SafeGraph. SafeGraph tracks 18.75 million devices, representing approximately 5.6% of the U.S. population and about 10% of all U.S. mobile phone devices. Based on SafeGraph's analysis of its user base, it posits that its customers are representative of the general U.S. population with regard to income, age, and other demographics. The data are used in studies of social distancing during the SARS-CoV-2 pandemic (Bizjak, Kalpathy, Mihov, and Ren (2022), Charoenwong, Kwan, and Pursiainen (2020), Weill, Stigler, Deschenes, and Springborn (2020)), and more recently also to

measure consumer responses to firms' actions (see, e.g., Painter (2021), Gurun, Nickerson, and Solomon (2023), Pursiainen and Tykvová (2024)). The data include the monthly number of visits and visitors at the outlet level. The coverage of SafeGraph's outlet-level data starts in January 2018.

We identify outlets of firms involved in class action lawsuits by manually searching the SafeGraph database for outlets of firms involved in a class action lawsuit in the SCAC database. We restrict the sample to lawsuits filed on or after March 1, 2018. This enables us to maintain at least two monthly pre-treatment observations for all lawsuits. Moreover, we disregard lawsuits filed after February 29, 2020. This avoids an impact of the SARS-CoV-2 pandemic on our results.[11] Finally, we retain only outlets of firms involved in class action lawsuits for which we can match control outlets. Overall, this results in a treatment sample comprising 25,155 distinct outlets associated with 29 class action lawsuits. We construct the control sample by selecting all monthly observations of outlets in the same ZIP code and six-digit NAICS industry as the treated outlets. This yields a matched control sample comprising 123,129 distinct outlets. Internet Appendix Table IA.1 reports the number of outlets for each class action lawsuit. Figure IA.1 shows the spatial distribution of outlets.

We compute several measures of competition and concentration at the outlet level. As our main measure, we calculate the HHI based on the number of outlets each firm has in the filing year, ZIP code, and six-digit NAICS industry as the treated outlet.[12] We also vary the calculation of the HHI along two dimensions to study different dimensions of

[11] The first death attributed to the SARS-CoV-2 pandemic was reported in the U.S. on February 29, 2020.
[12] We rely on NAICS rather than SIC codes because SafeGraph only provides NAICS industry codes.

competition. To analyze different levels of competition at the industry level, we compute the HHI within the filing year and ZIP code but at the four-digit NAICS industry or two-digit NAICS industry. To analyze different levels of geographic competition, we estimate the HHI within the filing year and six-digit NAICS industry but at the county or state level. We also calculate the total number of firms with stores in the filing year, six-digit NAICS industry, and ZIP code as an alternative measure of competition. Finally, we construct a dummy that is equal to one for outlets with only one competing brand being active in the same six-digit NAICS industry and ZIP code, which corresponds to the weakest form of local competition possible in our setting.

C. Stock Market Response to Class Action Lawsuits

We analyze changes in shareholder value around filings of class action lawsuits using stock price data from the Center for Research in Security Prices (CRSP). We exclude lawsuits whose outcomes are unknown at the time of writing ("ongoing") and lawsuits that are remanded to another court. We estimate changes in shareholder value around the filing date by employing standard event study methodology. For each class action lawsuit, we compute CARs as the sum of daily abnormal returns from ten trading days before the event date to one trading day after the lawsuit filing date. This event window follows prior literature, in particular, Gande and Lewis (2009). Daily abnormal returns are calculated as the observed return less a predicted return. The latter is the predicted return of a market model regression in which daily returns are regressed on daily value-weighted index returns provided by CRSP over a 250-day estimation window that ends eleven trading days prior

to the event date.[13] We require non-missing stock return observations in the event window and at least 90 non-missing return observations in the estimation window to run the market model regression. This results in a sample of 3,074 class action lawsuits. We winsorize all abnormal return measures at the 1st and 99th percentiles.

As in the outlet analysis, we use different measures for a firm's competitive environment. First, as a measure of industry concentration, we compute the HHI for a firm's six-digit NAICS industry in the fiscal quarter that precedes the filing date based on positive quarterly revenue. Second, we count the number of distinct competing firms in the NAICS industry with positive revenue. Third, we set a dummy equal to one for firms with the lowest possible industry concentration, i.e., for firms with no competing firm in their NAICS industry.

D. Revenue Response to Class Action Lawsuits

We estimate the response of the operating performance to class action lawsuit filings by studying quarterly revenues from Compustat. Again, we utilize the entire sample of class action lawsuits as provided by the SCAC. As in the analysis of stock returns, we drop lawsuits whose outcomes are unknown at the time of writing ("ongoing") and lawsuits that are remanded to another court. This results in a sample comprising 2,661 distinct treated firms being the target of 3,438 securities class action lawsuits.

Consistent with the construction of the matched control sample in the outlet-level analysis, we match each quarterly lawsuit-firm observation with all quarterly observations

[13] Our results are very similar if we use CRSP's equally-weighted market return as a market return proxy, if we use a Fama and French (1993) three or Carhart (1997) four-factor model, or if we use a market-adjusted model instead of a market model.

of firms active in the same six-digit NAICS industry.[14] This yields a matched control sample comprising 10,186 distinct firms. The measures of competition that we employ in this analysis are the same as those used in the analysis of shareholder value described above.

E. Media Response to Class Action Lawsuits

To study media attention to the target firms around class action lawsuits, we use data from RavenPack. The coverage of the data starts in January 2000. We match these data to the target firms and construct a corresponding sample of news coverage for each of our samples used in the analyses: a monthly sample including the target firms covered by our SafeGraph sample of customer visits, a daily sample covering the target firms in our stock returns analysis (using CRSP data), and a quarterly sample covering the target firms in our firm-level revenue sample (using Compustat data). These samples allow us to analyze media attention around lawsuits included in each of our analyses.

The RavenPack data include two main variables, Aggregate Event Volume (AES) that measures the volume of media coverage, and Aggregate Event Sentiment (AES) that measures the sentiment of media coverage. We use both of these measures to study the dynamics of media attention around class action lawsuits.

[14] In contrast to the outlet-level analysis, we only match based on six-digit NAICS industries and not based on ZIP codes because the sample of potential control firms is not sufficiently populated to match on both six-digit NAICS industries and ZIP codes.

III. Competition and Retail Customer Response to Class Action Lawsuits

A. Descriptive Statistics

Table 1 reports descriptive statistics for the monthly outlet visits panel. On average, we observe 400 visits to each outlet in our sample per calendar month.[15] An outlet in our sample on average has around 13 other competing firms (defined based on brand identifiers in SafeGraph) in the same six-digit NAICS industry, ZIP code, and month. The average HHI for the outlets in our sample is 0.14. As measured by different HHIs, industries become less concentrated if measured at less granular industry definitions (mean of 0.10 and 0.07 for the four-digit and two-digit NAICS industries, respectively) or at larger geographic units (mean of 0.11 and 0.10 at the county and state level, respectively).

[TABLE 1 HERE]

B. Results

To analyze how retail customers respond to class action lawsuits, we study monthly customer visits to outlets belonging to a firm that is the target of a class action lawsuit. As a control group, we use outlets in the same industry and ZIP code. We first perform a

[15] Note that this is the number of visits tracked by SafeGraph, not the total number of visits.

regression analysis specified as:

$$(1) \qquad ln(Visits)_{i,s,t} = \beta_t \times Treatment_{i,s} \times Event\ month_{i,t} + \alpha_{i,s} + \alpha_{s,j,k,t} + \epsilon_{i,s,t},$$

where i indexes an outlet, s a lawsuit, t a month, j an industry, and k a ZIP code. *Visits* is the monthly number of visits of an outlet. *Treatment* is a dummy indicating whether an outlet belongs to a firm that is the target of a lawsuit. *Event month* are dummies indicating months relative to the filing month. We include interacted lawsuit and outlet fixed effects as well as interacted lawsuit, industry, ZIP code, and month fixed effects. These fixed effects ensure that we compare within outlet changes in visits to treated outlets with changes in visits to control outlets in the same month that are active in the same industry and location. Therefore, these fixed effects control for a range of characteristics, including the time trend, industry shocks coinciding with the filing of the lawsuit, and time-varying ZIP code characteristics. Moreover, they absorb the stand-alone Treatment dummy as well as the stand-alone month dummies.

[FIGURE 1 HERE]

Figure 1 shows the estimated monthly regression coefficients for the interacted treatment-month dummies, relative to the month preceding the filing month of a lawsuit (omitted in the regression). The black line indicates results for the full sample, and the grey line for a constant sample. In both samples, customer visits of firms targeted in class action lawsuits tend to start falling already before the announcement, although this drop is not statistically significant. This is not surprising as we cannot directly observe instances

of misconduct, but use filings of a class action lawsuit as a proxy for misconduct. In some cases, information related to firms' wrongdoing may be transmitted into the market before a lawsuit is filed.[16]

To facilitate further cross-sectional analysis of heterogeneity in the reputational effect across treated stores, we define an indicator variable that equals one for the filing month and the five months after (*Post*) and perform an analysis of the interaction term of this dummy with the Treatment dummy. This analysis is specified as:

$$ln(Visits)_{i,s,t} = \beta_1 \times Treatment_{i,s} \times Post_{i,t} + \beta_2 \times Post_{i,t} + \alpha_{i,s} + \epsilon_{i,s,t}. \quad (2)$$

The results are reported in Table 2. In Column 1, the coefficient on the interaction term shows that outlets belonging to a firm that is the target in a class action lawsuit experience a reduction in visits of -5.4% in the filing month and the five months following the filing month of the lawsuit. With -6.6% and -3.8%, estimates remain economically meaningful and statistically significant if we add month fixed effects, as in Column 2, as well as interacted lawsuit, ZIP code, six-digit NAICS industry, and month fixed effects, as in Column 3. Moreover, the positive and significant coefficient on the dummy variable *Post*, which is equal to one for the filing month and the five months after, is consistent with positive spillovers to competitors. In Column 2, which includes month fixed effects, we find that the sample of matched control outlets experiences an increase in shop visits of about 1.8%, while treated outlets suffer a decline in visits of -6.6% relative to control outlets, or

[16] In Internet Appendix Section IA.V.B, we conduct an additional analysis where we change the timing of treatment to the beginning of the class action period. As expected, there are no significant pre-trends in this analysis, while the results are consistent with those from our main analysis.

-4.8% overall. Taken together, these results provide strong evidence of reputational costs of corporate misbehavior.

[TABLE 2 HERE]

To analyze the impact of local competition on store visits, we augment the regression from Column 3 of Table 2 with a triple interaction term, obtained by multiplying the existing interaction term between the Treatment and the Post dummy with different measures of local competition. These additional interaction terms allow us to gauge the incremental impact of customers' local ability to substitute visits of outlets belonging to the same firm targeted in the same class action lawsuit with visits to outlets of a competing firm.

[TABLE 3 HERE]

The results are reported in Table 3. In Column 1, we use a very granular form of the HHI, estimated at the six-digit NAICS industry and ZIP code level, as a proxy for local concentration. We continue to find a negative reputational effect following filings of class action lawsuits on store visits, as indicated by the significantly negative coefficient on the interaction term between the Treatment dummy and the Post dummy. Consistent with the conjecture that lower competition mutes the disciplining effect of class action lawsuits, the coefficient on the triple interaction term is positive and highly significant. Economically, this result has sizeable implications: A one-standard-deviation increase in local competition, as measured by the HHI, results in an additional four percentage points reduction in customer visits compared to stores in the same month that belong to the same

firm targeted in the same lawsuit. This result implies that customers' ability to punish firms for wrongdoing critically hinges on the availability of outlets of firms that compete locally.

Next, we study whether different levels of competition matter differently. To do so, we vary the industry granularity at which we measure competition to the four-digit and two-digit NAICS industries. Results in Columns 2 and 3 show that less competition within the same four-digit and two-digit NAICS industries both reduce the ability of customers to substitute visits of outlets of firms accused of wrongdoing with visits of outlets of competing firms. However, when competition measures at all three levels of industry granularity are included jointly as explanatory variables, as in Column 4, we obtain coefficients that are monotonically decreasing the less granular the industry measures are, with competition at the two-digit NAICS industry level having no incremental explanatory power beyond the two more granular measures of industry competition. This implies that the ability of customers to substitute visits to outlets of firms accused of wrongdoing is facilitated if the offerings of competing outlets are more similar.

We also vary the geographic dimension of competition. To this end, we compute the HHI based on six-digit NAICS industries at the county level and state level. The results show that higher competition at the ZIP code level (Column 1), county level (Column 5), and state level (Column 6) intensifies the reduction in store visits following filings of class action lawsuits. However, when all three measures of competition are included jointly, as in Column 7, we find that competition measured at the most granular (ZIP code) level has the strongest effect on the reduction in customer visits, followed by competition measured

at the least granular (state) level. In contrast, adding county-level competition does not have incremental explanatory power. Hence, there is both a local (i.e., ZIP code-level) and a broader (i.e., state-level) component of market power.

Next, we analyze alternative measures of competition at the ZIP code level. In Column 8, we find that a larger number of alternative brands at the same location is associated with a significantly larger reduction in customer visits.[17] In Column 9, we show that in a market with only one competing brand, the most concentrated market possible in our setting, the baseline reputational effect of a decline in outlet visits of 5.6% is completely offset (the coefficient on the triple interaction term is 6.8%).[18] Taken together, these results using alternative measures of competition at the ZIP code level underscore that competition is a necessary condition for the ability of customers to discipline firms.[19]

[TABLE 4 HERE]

As discussed in more detail in Section VI.A, the media coverage of target firms increases significantly following the filing of a class action lawsuit. In Panel A of Table 4, we study the change in customer visits depending on the change in media coverage

[17]The coefficient on the triple interaction term between the Treatment dummy, the Post dummy, and the number of competing brands remains negative and significant if we take the logarithm of the number of competing brands.

[18]The reason why we cannot estimate the effect of having no competitor – in reality, the most extreme case of (monopolistic) market power – is due to the high-dimensional fixed effects that we employ. In particular, we match control outlets to treated outlets based on month, six-digit NAICS industry, and ZIP code, so including Lawsuit × ZIP code × six-digit NAICS industry × Month FEs leaves no variation in the treatment variable within a lawsuit, ZIP code, six-digit NAICS industry, and month. The fact that we cannot estimate the effect of having no competitor in the same lawsuit, ZIP code, six-digit NAICS industry, and month is also the reason why we find a positive and significant coefficient on the two-way interaction between the Treatment dummy and the Post dummy in Column 8. Technically, this coefficient captures the baseline effect of having no competitor, but since this effect cannot be estimated econometrically, the coefficient estimate is misspecified.

[19]In Internet Appendix Table IA.4, we show that these results are robust to using the number of unique visitors as the outcome variable, instead of the number of visits.

following the filing of the lawsuit. We find no significant relationship between the change in news volume and the reduction in visits, but a more negative shift in news sentiment is associated with significantly larger reductions in customer visits. This is intuitive, as a general increase in media coverage may have a marketing effect that simply generates more publicity for the firm, while more negative media coverage is likely to generate a more negative perception by prospective customers.

A benefit of using monthly outlet data is that we can observe changes in customer visits at a high frequency, mitigating potential concerns that exposure to competition might change over time. As we measure competition at the industry-location level, the exposure to competition, as captured by these measures, is the same for target and control outlets, also mitigating potential concerns of competition affecting the likelihood of lawsuits (which we study in more detail in Internet Appendix Section IA.III). In general, this setting reduces the scope for omitted variables to affect our findings, as there would need to be an omitted variable that is strongly correlated with competition and that, conditional on a lawsuit happening, would cause larger reductions in customer visits to target outlets and the corresponding target revenues. It is not obvious what such an omitted variable could be. The most obvious explanation is that the differences in customer reactions are directly related to the availability of alternatives, i.e., competition.

One alternative explanation for our findings is that customers do not stay away from stores of companies facing a class action lawsuit for moral reasons, but because of a general concern about product and service quality of these firms. Indeed, a company indicted for corporate wrongdoing may be suspected of dishonest business practices even

though the wrongdoing addressed by the class action lawsuits in our sample are generally unrelated to product and service quality. To distinguish between these two alternative interpretations, we test whether the negative effect of lawsuits is larger in areas with a higher share of the population known to have higher moral standards and a higher propensity to hold individuals and firms accountable for deviations from these high moral standards. In contrast, concerns about product and service quality and, more generally, concerns about the integrity of the firm are expected to affect all locations similarly.

We test this empirically by interacting the Treatment dummy and the Post dummy with county demographic characteristics that are likely to be correlated with customers' willingness to punish firms for wrongdoing, controlling for the impact of competition. As a first proxy for customers' willingness to punish firms for wrongdoing, we study religiousness in an outlet's county. Prior literature shows that religion affects various economic outcomes, including risk aversion (Noussair, Trautmann, van de Kuilen, and Vellekoop (2013)), stock market participation (Hong, Kubik, and Stein (2004)), the propensity to gamble (Kumar, Page, and Spalt (2011)), and firm behavior (Hilary and Hui (2009)). Results in Column 1 of Panel B of Table 4 suggest that the adverse reputational effect of corporate misbehavior is larger in areas with a more religious population. This finding implies that religiousness leads customers to hold firms accountable to higher moral standards.

As another proxy for customers' willingness to punish firms for wrongdoing, we study counties' social norms, as measured by the social capital index of Lin and Pursiainen (2022).[20] The results are reported in Column 2. We find that stores located in counties

[20]The index is based on principal component analysis of counties' association density, regulated charitable organization density, and voter turnout rate, following the methodology of Rupasingha, Goetz, and Freshwater (2006) and similar to the index used, for instance, in Hasan, Hoi, Wu, and Zhang (2017) and Hoi, Wu,

with stronger social norms experience a stronger reduction in customer visits. This finding is in line with existing research arguing that high social capital imposes behavioral norms on managers (Hoi et al. (2019), Hoi et al. (2019)) as well as communities more generally (e.g., Buonanno, Montolio, and Vanin (2009)), driven by a higher willingness to punish those who do not live by the norms of the community (e.g., Bowles and Gintis (2002)).

As a final proxy for customers' willingness to punish firms for wrongdoing, we consider counties' political orientation, as measured by the Republican vote share in the 2016 Presidential election. Results in Column 3 show that stores located in counties that lean towards the Republican party are more likely to be punished for wrongdoing. This result is consistent with the notion that the Republican ideology favors individual accountability and market discipline to punish corporate wrongdoing, while Democrats believe in government intervention to manage corporate crime (e.g., Hutton, Jiang, and Kumar (2015)).

Taken together, the results in this section provide outlet-level evidence that customers punish firms for corporate wrongdoing. This effect is strongly dependent on competition. Outlets facing more competition experience significantly larger declines in customer visits. Hence, the ability of customers to punish firms for their wrongdoing critically hinges on the availability of alternatives for customers. The effect of competition differs across geographic and industry proximity, with more narrowly defined competition mattering more. Moreover, the effect varies across demographic characteristics of counties in which the outlets are located and is more pronounced in counties with stronger social norms and values, as proxied with religion, social capital, and political orientation. This

and Zhang (2019).

suggests that the customer reactions we observe are at least partly driven by moral considerations and not by concerns about product or service quality, as these would likely affect all locations similarly.

IV. Competition and Stock Market Response to Class Action Lawsuits

A. Descriptive Statistics

Panel A of Table 5 reports descriptive statistics for the announcement return sample. The sample of class action lawsuits underlying this analysis comprises 3,074 cases. A target in a class action lawsuit on average competes with around 100 other firms within the same six-digit NAICS industry in the quarter before the filing of the lawsuit. The average revenue-based HHI calculated at the six-digit NAICS industry level for the firms in this sample is 0.3. Twelve-day CARs, computed from ten days before to one day after the filing of a class action lawsuit, are highly negative, with a mean of -9.8% and a median of -3.9%. Prior studies document similar, if not even more negative, stock price responses to filings of class action lawsuits.[21] Gande and Lewis (2009), for instance, find an average response of -14.5% over the same twelve-day window using a sample of lawsuits filed between 1996 and 2003.

[21] We follow Gande and Lewis (2009) and use an asymmetric twelve-day event window that covers ten trading days before the filing date, the filing date, and the trading day after the filing date. Internet Appendix Figure IA.2 displays daily abnormal returns from 15 trading days before to ten trading days after the filing date of the class action lawsuits.

[TABLE 5 HERE]

B. Results

To test whether the stock market response to filings of class action lawsuits are related to the competitive environment of firms targeted by the lawsuits, we regress the twelve-day CARs on various measures of competition. For each competition measure, we first estimate a univariate regression and then rerun the regression augmented with a set of covariates capturing lawsuit and firm characteristics as well as year fixed effects.[22]

The results are reported in Panel B of Table 5. As our first measure of competition, we use a revenue-based HHI computed at the six-digit NAICS level. Results in Column 1 show that firms active in a less competitive market have significantly higher (i.e., less negative) abnormal returns around the filing of a class action lawsuit. Specifically, an increase in the HHI by one standard deviation results in one percentage point lower cumulative abnormal returns. Results reported in Column 2 suggest that adding control variables leaves the coefficient and significance level almost unchanged. As a second measure of competition, we use the number of other firms active in the same six-digit NAICS industry. The results are reported in Columns 3 and 4 and show that the more competition a targeted firm faces, the more negative the market reacts to the filing of a class action lawsuit.[23] The third measure of competition is a dummy variable that is equal to one for firms without any competitor in the six-digit NAICS industry, implying monopolistic market power. Results in Columns 5 and 6 show a positive and significant

[22]Table IA.2 presents the number of filings by calendar year and month.

[23]The coefficient on the number of competing firms remains negative and significant if we do a log-transformation of this variable.

coefficient on the dummy variable indicating no competition. The coefficient estimates suggest that returns around filings of class action lawsuits are 4.8% or 4.6% higher (i.e., less negative, as the constant is -10.0%) for firms without a competitor.

The control variables added to the specifications reported in the even columns obtain loadings as expected from prior research. The dummy variable indicating whether a class action lawsuit turns out to the be settled obtains a negative and highly significant coefficient. The coefficient estimate suggests that settled cases, on average, have -7.0% lower returns around the filing date as do cases that will be dismissed. This suggests that investors are able to gauge the merit of lawsuits at the filing date (Griffin, Grundfest, and Perino (2004), (Kempf and Spalt, 2022)). Firms with higher market-to-book ratios, a proxy for growth opportunities, and firms with less property plant and equipment as a fraction of total assets, a proxy for the asset intensity, also have significantly more negative abnormal returns. This is consistent with a growing literature showing that firms with valuable innovation output are particularly vulnerable to class action litigation (Lin, Liu, and Manso (2021), (Kempf and Spalt, 2022)).

The results in this section show that competition in firms' product markets is a key determinant of the stock market response to filings of class action lawsuits. Hence, the results in this section support our previous results from the outlet-level analysis, suggesting that competition is an important component in customers' ability to discipline firms for misbehavior. Moreover, these results imply that the stock market reaction reflects the benefits of a concentrated market structure in impeding customers' ability to discipline a firm when a class action lawsuit is filed. This suggests that investors consider the effects of

concentration when assessing the value impact of lawsuits.

V. Competition and Revenue Response to Class Action Lawsuits

A. Descriptive Statistics

Panel A of Table 6 reports descriptive statistics for the quarterly firm panel. The sample of class action lawsuits underlying this analysis comprises 3,438 cases filed since 1996.[24] On average, a firm has quarterly revenues of 249 million USD and competes with around 203 other firms within its six-digit NAICS industry. The average revenue-based HHI calculated at the six-digit NAICS industry level for the firms in this sample is 0.16.

[TABLE 6 HERE]

B. Results

To study the revenue changes around class action lawsuits, we use a similar specification as in our outlet analysis but at the firm level. We define an indicator variable that equals one for the filing quarter and the three quarters after (*Post*) and include an interaction term of

[24]The sample of class action lawsuits used in this analysis is larger than the one used for the announcement return analysis because of stock market availability from the CRSP. Moreover, at the time of writing, CRSP return data is only available until December 31, 2019, while Compustat data is available through 2020.

this dummy with the Treatment dummy. This analysis is specified as:

$$(3) \qquad ln(Revenue)_{i,s,t} = \beta_1 \times Treatment_{i,s} \times Post_{i,t} + \beta_2 \times Post_{i,t} + \alpha_{i,s} + \epsilon_{i,s,t}.$$

The results are reported in Panel B of Table 6. In Column 1, the coefficient on the interaction term shows that firms targeted by a class action lawsuit experience a reduction in revenues of -7.8% in the filing quarter and the three quarters following the filing of the lawsuit. In Column 2, we add interacted industry and quarter fixed effects. In Column 3, we add interacted lawsuit, industry, and quarter fixed effects and in Column 4, we augment our model with interacted lawsuit, industry, quarter, and state fixed effects. The interacted fixed effects in the last specification, i.e., in Column 4, ensure that we compare changes in revenues of treated firms with changes in revenues of firms in the same industry and quarter that are headquartered in the same state. Therefore, these fixed effects control for a range of characteristics, including the time trend, industry shocks that coincide with the filing, and time-varying headquarter-state characteristics. Taken together, these results on accounting revenues provide strong evidence of reputational costs of class action lawsuits and suggest that the results documented in the outlet-level analysis are not confined to the sample used in that analysis.

Next, we test whether the drop in revenues around filings of class action lawsuits depends on competition. To this end, we augment our analysis with a triple interaction term, which interacts the existing interaction term between the Treatment dummy and the Post dummy with three different measures of competition similar to those used in Section IV above. These additional interaction terms allow us to gauge the incremental impact of

customers' ability to substitute the products of a firm targeted in a class action lawsuit with products of competing firms.

The results are reported in Panel C of Table 6. The coefficient on the triple interaction term shows the expected sign and is significant across all three specifications. As in the outlet-level analysis, the economic magnitude of these results is sizeable. For instance, an increase in competition, as measured by the HHI calculated within the same six-digit NAICS industry, by one standard deviation is associated with a one-and-a-half-percentage-point loss of revenue. Hence, these findings further suggest that competition is an important component in customers' ability to discipline firms for misbehavior.

In Panel A of Table 7, we perform a regression analysis of the change in firm-level revenues depending on the change in media coverage of the target firm, similar to our outlet-level analysis in Panel A of Table 4. The results are similar to our results on outlet visits. The volume of media coverage itself is not significantly associated with differences in the reduction in revenue, but more negative changes in sentiment are associated with significantly larger reductions in revenue.

[TABLE 7 HERE]

As our revenue sample includes a large number of lawsuits, we can also analyze the revenue response to a class action lawsuit conditional on the characteristics of the lawsuit. To this end, we construct a number of lawsuit characteristics that are likely to proxy for the severity of the lawsuit. These include the stock price reaction to the lawsuit filing, the duration of the violation that resulted in the lawsuit, an indicator dummy of whether the

case was settled or dismissed, and the duration of the process until the resolution of the lawsuit (either settlement or dismissal).

This analysis is presented in Panel B of Table 7. As one would expect, the stock price reaction around the filing date is correlated with the subsequent revenue reaction (Column 1). Lawsuits perceived more negatively by the stock market are followed by larger reductions in revenue. Similarly, lawsuits where the duration of the violation is longer are associated with larger revenue reductions (Column 2), consistent with these cases being more severe. Finally, lawsuits that are settled (i.e., not dismissed) are associated with larger reductions in revenue (Column 3). This makes sense, as this indicates that these cases presumably have more merit and hence are likely associated with more severe reputational damage to the target firm. To further study the effect of the duration of the legal process, we split the sample into settled and dismissed cases. Settled cases are likely the ones with more obvious merit. Hence, a shorter duration here is likely to signal more obvious culpability and hence a more severe case. On the contrary, for dismissed cases, a longer process suggests that it was ex-ante less obvious that the lawsuit would ultimately be dismissed. Our results are consistent with these predictions. For settled cases (Column 4), shorter duration is associated with larger reductions in revenue, while the opposite holds true for dismissed cases (Column 5).

Taken together, the results in this section show that our results on customer visits are not confined to the sample of class action lawsuits that overlaps with the SafeGraph data but generalize to a comprehensive sample of about 3,400 class action lawsuits filed between 1996 and 2020. Hence, our results obtained using different methodologies and

across different samples provide strong and consistent evidence of the importance of competition on the ability of customers to discipline firms for misbehavior.

VI. Additional Analyses

A. Media Response to Class Action Lawsuits

Our results suggest that retail customers reduce their visits to the outlets of firms targeted in class action lawsuits. A similar pattern is seen in quarterly revenues, which decline for the target firm immediately following a lawsuit. In order for customers to react, they need to be aware of either the lawsuit or the misconduct that led to the lawsuit. One obvious channel through which retail customers may obtain information is the media. To test whether media attention on firms increases around class action lawsuits, we use RavenPack data on news volume and sentiment for each of our three settings (monthly customer visit sample, daily stock return sample, and quarterly revenue sample).

[FIGURE 2 HERE]

First, we perform a within-firm regression analysis of the media coverage around the filing of a class action lawsuit for each of the three samples. These analyses are shown in Figure 2. Panel A plots the monthly regression coefficients for news volume (AEV) and sentiment (AES) for the firms in the customer visits sample. Panel B shows the same analysis on a daily basis for the stock returns sample, and Panel C on a quarterly basis for the revenue sample. The patterns of media coverage are consistent across all three samples.

Media coverage increases substantially immediately following the filing of a class action lawsuit, while sentiment becomes more negative. This is consistent with increased and often negative attention to the firms targeted by these lawsuits. From Panel A, it seems that the media attention reverts to close-to-normal levels approximately three months after the filing. This is broadly consistent with Panel C, which shows a large shift for the quarter of the lawsuit filing, followed by a reversal to pre-lawsuit levels in the next one or two quarters.

B. Further Analyses and Robustness Checks

In the Internet Appendix, we perform a number of additional analyses and robustness checks. These are briefly summarized in this section and include:

(i) *Additional sample information and summary statistics.* In Internet Appendix Section IA.I, we provide additional information about the sample and summary statistics. Section IA.I.A provides a detailed list of class action lawsuits in our monthly customer visits sample. Section IA.I.B shows the geographic distribution of this sample. Section IA.I.C provides a general summary of class action lawsuits over time.

(ii) *Additional figures.* In Internet Appendix Section IA.II, we provide additional figures supporting our analysis. Section IA.II.A includes a figure showing daily abnormal returns around a class action lawsuit, while Section IA.II.B includes an analysis plotting quarterly regression coefficients for firms targeted in class action lawsuits, complementing our analysis in Sections IV and V, respectively.

(iii) *Competition and likelihood of class action lawsuits.* In Internet Appendix Section

IA.III, we perform an analysis of the likelihood of a firm being the target of a class action lawsuit, dependent on the level of competition. While there is some correlation between competition and the likelihood of being a target in a class action lawsuit, this relationship appears relatively weak and largely statistically insignificant when controlling for state fixed effects, which are likely relevant for the propensity of lawsuits, or firm fixed effects, effectively focusing on within-firm variation over time.

(iv) *Retail trader response to class action lawsuits.* In Internet Appendix Section IA.IV, we study retail investors' reactions to class action lawsuits, using data from Robinhood. We find a statistically significant reduction in the number of retail accounts holding stocks of firms targeted around the filing date compared to firms in the same industry, with approximately 10% reduction in retail holders during the 15 days following the filing. The downward trend continues over several months and does not revert.

(v) *Number of unique visitors as a measure of customer activity.* In Internet Appendix Section IA.V.A, we repeat the same analysis as in Table 2, but use the number of unique visitors as the outcome variable. The results are very similar to our baseline results reported in Table 2.

(vi) *Beginning of class action period as event date.* In Internet Appendix Section IA.V.B, we conduct an additional analysis where we change the timing of treatment to the beginning of the class action period. This approximates the beginning of the actual misconduct by the firm, instead of the filing of the lawsuit. As expected, there are no significant pre-trends in this analysis, while the results are consistent with those from our main analysis.

(vii) *Competition and profit and cash flow response to class action lawsuits.* In Internet Appendix Section IA.V.C, we expand our accounting-based analysis to also study other measures than revenue, including gross profit, EBIT (operating profit), and free cash flow. Similar to the results on revenue, all these measures of profit and cash flow decrease significantly for the target firms following the filing of a class action lawsuit, and this decrease is significantly stronger for firms in more competitive industries.

(viii) *Longer-term outcomes.* In Internet Appendix Section IA.V.D, we study the persistence of the changes in customer visits and revenues by extending the respective samples for a longer period after the lawsuit filing. Customer visits appear to return to levels similar to those before the lawsuit approximately six months after the filing, while there is no similar recovery in firm-level revenues in the quarterly Compustat sample. To the extent these samples are consistent with each other, one possible explanation is that firms that experience a decrease in customer visits react by lowering prices. This could result in customer volumes recovering, while revenues stabilize at a lower level.

(ix) *Validation of visits as a proxy for revenue.* In Internet Appendix Section IA.V.E, we show that firm-level revenues are strongly correlated with firm-level customer visits, validating our measure of customer visits as a proxy for revenue.

(x) *Analysis of treatment vs. control outlets.* In Internet Appendix Section IA.VI, we perform an analysis of the dynamics of customer visits separately for treatment outlets (i.e., outlets of firms targeted in class actions lawsuits) versus matched control outlets. Following a lawsuit filing, treated outlets exhibit a significant reduction in customer visits, while control outlets exhibit an increase in customer visits, albeit

more modest in terms of magnitude. This pattern is consistent with at least some customers shifting from target outlets to competitors.

(xi) *Outlet closures.* In Internet Appendix Section IA.VII, we include an analysis of the likelihood of outlet closure following a class action lawsuit. We find that target firm outlets more exposed to competition are significantly more likely to be closed following a class action lawsuit.

VII. Conclusion

The basis of a fair and functioning legal system is that everyone is subject to the same laws and risks similar penalties when breaking those laws. Our findings highlight a worrying aspect of the current system, where a substantial part of the damages suffered by firms from litigation come in the form of reputational losses. We show that such losses are meaningful only when there is adequate competition. This means that firms with substantial market power may not be disciplined in the same way as firms with less market power. This is even more worrying given the evidence that markets and industries have generally grown more concentrated and less competitive over the last few decades. This might suggest that legal punitive damages need to increase with an increase in market power to maintain the intended deterrent effect. Our results on the differential effects of competition measured at different levels and granularities may also have important implications in other contexts. We show that competition measured at the micro level appears significantly more important than competition measured at a broader level, both in terms of geography and industry definitions.

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Appendix A: Variable Definitions

This table reports variable definitions of all variables used in the paper as well as their data sources. Database mnemonics are in italics (if available).

Variable	Definition	Source
Visits	Number of visits of an outlet in a calendar month.	SafeGraph
Visitors	Number of visitors of an outlet in a calendar month.	SafeGraph
Closed	Dummy variable set equal to one if an outlet disappears from the data, zero otherwise.	SafeGraph
HHI ZIP NAICS6	Herfindahl-Hirschman index, calculated as the sum of squared market shares of all firms in the filing year, ZIP code, and six-digit NAICS industry. A firm's market share is computed using the fraction of outlets.	SafeGraph
HHI ZIP NAICS4	Herfindahl-Hirschman index, calculated as the sum of squared market shares of all firms in the filing year, ZIP code, and four-digit NAICS industry. A firm's market share is computed using the fraction of outlets.	SafeGraph
HHI ZIP NAICS2	Herfindahl-Hirschman index, calculated as the sum of squared market shares of all firms in the filing year, ZIP code, and two-digit NAICS industry. A firm's market share is computed using the fraction of outlets.	SafeGraph
HHI County NAICS6	Herfindahl-Hirschman index, calculated as the sum of squared market shares of all firms in the filing year, county, and six-digit NAICS industry. A firm's market share is computed using the fraction of outlets.	SafeGraph
HHI State NAICS6	Herfindahl-Hirschman index, calculated as the sum of squared market shares of all firms in the filing year, state, and six-digit NAICS industry. A firm's market share is computed using the fraction of outlets.	SafeGraph
# other brands	Number of other firms with an outlet active in the same six-digit NAICS industry, ZIP code, and filing year.	SafeGraph
One competing brand	Dummy variable set equal to one if # other brands equals one, zero otherwise.	SafeGraph
AEV	Aggregate Event Volume	Ravenpack
Change AEV (%)	Change in Aggregate Event Volume. In the monthly analysis, the volume change is the percentage change from the quarterly average before the filing (months -3, -2, and -1) to the quarterly average of the filing (months 0, 1, and 2). In the quarterly analysis, the volume change is the percentage change from the quarter before the filing to the filing quarter.	Ravenpack
AES	Aggregate Event Sentiment	Ravenpack
Change AES (%)	Change in Aggregate Event Sentiment. In the monthly analysis, the sentiment change is the percentage change from the quarterly average before the filing (months -3, -2, and -1) to the quarterly average of the filing (months 0, 1, and 2). In the quarterly analysis, the sentiment change is the percentage change from the quarter before the filing to the filing quarter.	Ravenpack
Religious (%)	Adherents per capita in a county (*totrate*).	2010 U.S. Religion Census

Social capital	County-level principal component analysis of association density, regulated charitable organization density, and voter turnout rate.	Lin and Pursiainen (2022)
Republican (%)	Fraction of votes obtained by Republic Party in 2016 Presidential Election in a county.	MIT Election Data and Science Lab
CAR(-10,1)	Cumulative abnormal return, estimated as the sum of daily (unwinsorized) abnormal returns from ten trading days before the event date to one trading day after the event date where the filing date of a security class action lawsuit marks the event date. Daily abnormal returns are calculated as the observed return (*ret*) minus a predicted return. The predicted return is estimated using a market model regression where daily returns (*ret*) are regressed on daily value-weighted index returns (*vwret*) over a 250-day estimation window that ends eleven trading days prior to the event date. At least 90 daily observations with non-missing stock return data are required. Winsorized at the 1^{st} and 99^{th} percentiles.	CRSP
HHI	Herfindahl-Hirschman index, calculated as the sum of squared market shares of all firms in the same fiscal quarter and six-digit NAICS industry. A firm's market share is computed using revenues (*revtq*).	Compustat quarterly
# other firms	Number of other firms in the same fiscal quarter in the same six-digit NAICS industry.	Compustat quarterly
No competing firm	Dummy variable set equal to one if # other firms equals zero, zero otherwise.	Compustat quarterly
One competing firm	Dummy variable set equal to one if # other firms equals one, zero otherwise.	Compustat quarterly
Settled	Dummy variable set equal to one if a class action lawsuit is settled, zero otherwise.	Stanford's Securities Class Action Clearinghouse
Market cap.	Market capitalization ($prc \times (shrout \times 1{,}000)$) eleven trading days prior to the filing of the class action lawsuit.	CRSP
Leverage	Long-term debt (*revtq*) and debt in current liabilities (*dlcq*) scaled by total assets (*atq*).	Compustat quarterly
ROE	Net income (*niq*) scaled by book value equity (*ceqq*).	Compustat quarterly
MTB	Market capitalization ($prccq \times cshoq$) scaled by book value equity (*ceqq*).	Compustat quarterly
PPE/AT	Propertly plant and equipment (*ppentq*) scaled by total assets (*atq*).	Compustat quarterly
Foreign revenue (%)	Sum of foreign revenues across all segments (*salexg*) scaled by revenues in a financial year (*revt*).	Compustat segments, Compustat annual
Idiosyncratic vola	Root mean squared error of market model regression.	CRSP
ILLIQ	Amihud's illiquidity measure estimated during the estimation window.	CRSP
Revenue	Revenue (*revtq*) in a quarter.	Compustat quarterly
Gross profit	Revenue (*revtq*) less costs of goods sold (*cogsq*) in a quarter.	Compustat quarterly
EBIT	Revenue (*revtq*) less costs of goods sold (*cogsq*) less selling, general & administrative expenses (*xsgaq*) in a quarter.	Compustat quarterly
Free cash flow	Revenue (*revtq*) less costs of goods sold (*cogsq*) less selling, general & administrative expenses (*xsgaq*) plus depreciation (*dpq*) less change in capex (*capxy*) in a quarter.	Compustat quarterly
Duration violation	End date of the class action period less start date of the class action period.	Stanford's Securities Class Action Clearinghouse

Duration resolved	Case status date less filing date of the class action lawsuit.	Stanford's Securities Class Action Clearinghouse
CALS filing	Dummy variable set equal to one if there is a filing of a class action lawsuit in a quarter, zero otherwise.	Stanford's Securities Class Action Clearinghouse
Robinhood accounts	Number of different accounts holding the stock.	Robintrack.net

Table 1
Descriptive Statistics – Monthly Outlet Sample

This table presents descriptive statistics for the outlet-month panel. The sample of treated outlets comprises 25,155 outlets associated with a firm involved in a class action lawsuit. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits targeting firms headquartered outside of the U.S., lawsuits filed before March 1, 2018, lawsuits filed after February 29, 2020, and lawsuits for which we cannot match outlets in SafeGraph. We also drop outlets for which we cannot match control outlets. For each treated outlet, we retain eleven monthly observations around the filing month of a lawsuit, five months before and five months after. The control sample is constructed by selecting all monthly observations of outlets in the same ZIP code and six-digit NAICS industry as the treated outlets. Detailed descriptions of all variables used throughout the study are provided in the Appendix.

	Mean	Std	p25	p50	p75
Store outcomes					
Visits	405.247	707.815	103.000	260.000	494.000
Visitors	273.594	372.261	72.000	184.000	350.000
Closed × 100	0.108	3.291	0.000	0.000	0.000
Treatment	0.137	0.344	0.000	0.000	0.000
Competition					
HHI ZIP NAICS6	0.141	0.131	0.055	0.077	0.194
HHI ZIP NAICS4	0.104	0.116	0.032	0.050	0.133
HHI ZIP NAICS2	0.068	0.083	0.025	0.037	0.063
HHI County NAICS6	0.112	0.111	0.045	0.059	0.148
HHI State NAICS6	0.105	0.114	0.049	0.058	0.061
# other brands	13.453	9.158	4.000	14.000	20.000
One competing brand	0.106	0.308	0.000	0.000	0.000
Media coverage					
Change AEV (%)	2.439	3.244	-0.427	0.475	4.203
Change AES (%)	0.045	0.286	-0.247	0.222	0.222
Outlet characteristics					
Religious (%)	0.551	0.135	0.451	0.545	0.642
Social capital	-0.695	0.651	-1.153	-0.656	-0.255
Republican (%)	0.477	0.163	0.366	0.469	0.596
N	2,099,633				

Table 2
Retail Customer Response to Class Action Lawsuits

This table presents results of ordinary least squares regressions using the logarithm of the number of monthly outlet visits as the dependent variable. *Treatment* is a dummy variable set equal to one if an outlet belongs to a firm that is the target in a class action lawsuit, zero otherwise. *Post* is a dummy variable set equal to one for the month of the filing of the class action lawsuit and all months thereafter. The sample of treated outlets comprises 25,155 outlets associated with a firm involved in a class action lawsuit. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits targeting firms headquartered outside of the U.S., lawsuits filed before March 1, 2018, lawsuits filed after February 29, 2020, and lawsuits for which we cannot match outlets in SafeGraph. We also drop outlets for which we cannot match control outlets. For each treated outlet, we retain eleven monthly observations around the filing month of a lawsuit, five months before and five months after. The control sample is constructed by selecting all monthly observations of outlets in the same ZIP code and six-digit NAICS industry as the treated outlets. Detailed descriptions of all variables used throughout the study are provided in the Appendix. Standard errors, reported in parentheses, are clustered at the firm-month level. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

	ln(Visits)		
	1	2	3
Treatment × Post	-0.0536***	-0.0662***	-0.0377***
	(0.0153)	(0.0104)	(0.0109)
Post	0.1114***	0.0180***	
	(0.0041)	(0.0048)	
Lawsuit × Outlet FE	Yes	Yes	Yes
Month FE	No	Yes	No
Lawsuit × ZIP × NAICS × Month FE	No	No	Yes
N	2,099,633	2,099,633	2,099,633
R^2	0.968	0.970	0.977

Table 3
Competition and Retail Customer Response to Class Action Lawsuits

This table presents results of ordinary least squares regressions using the logarithm of the monthly number of outlet visits as the dependent variable. *Treatment* is a dummy variable set equal to one if an outlet belongs to a firm that is the target in a class action lawsuit, zero otherwise. *Post* is a dummy variable set equal to one for the month of the filing of the class action lawsuit and all months thereafter. The sample of treated outlets comprises 25,155 outlets associated with a firm involved in a class action lawsuit. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits targeting firms headquartered outside of the U.S., lawsuits filed before March 1, 2018, lawsuits filed after February 29, 2020, and lawsuits for which we cannot match outlets in SafeGraph. We also drop outlets for which we cannot match control outlets. For each treated outlet, we retain eleven monthly observations around the filing month of a lawsuit, five months before and five months after. The control sample is constructed by selecting all monthly observations of outlets in the same ZIP code and six-digit NAICS industry as the treated outlets. Detailed descriptions of all variables used throughout the study are provided in the Appendix. Standard errors, reported in parentheses, are clustered at the firm-month level. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

	ln(Visits)								
	1	2	3	4	5	6	7	8	9
Treatment × Post	-0.1036***	-0.0926***	-0.0636***	-0.1047***	-0.0935***	-0.0856***	-0.1047***	0.0084*	-0.0560***
	(0.0214)	(0.0185)	(0.0147)	(0.0215)	(0.0194)	(0.0172)	(0.0217)	(0.0050)	(0.0134)
Treatment × Post × HHI ZIP NAICS6	0.2864***			0.1724***			0.1684***		
	(0.0542)			(0.0565)			(0.0426)		
Treatment × Post × HHI ZIP NAICS4		0.3073***		0.1374***					
		(0.0527)		(0.0375)					
Treatment × Post × HHI ZIP NAICS2			0.2466***	0.0265					
			(0.0459)	(0.0309)					
Treatment × Post × HHI County NAICS6					0.3092***		0.0443		
					(0.0575)		(0.0373)		
Treatment × Post × HHI State NAICS6						0.2793***	0.1182***		
						(0.0475)	(0.0322)		
Treatment × Post × # other brands								-0.0061***	
								(0.0011)	
Treatment × Post × One competing brand									0.0683***
									(0.0130)
Lawsuit × Outlet FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lawsuit × ZIP × NAICS × Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,099,633	2,099,633	2,099,633	2,099,633	2,099,633	2,099,633	2,099,633	2,099,633	2,099,633
R^2	0.977	0.977	0.977	0.977	0.977	0.977	0.977	0.977	0.977

Table 4
Media Coverage, Outlet Characteristics, and Retail Customer Response

This table presents results of ordinary least squares regressions using the logarithm of the number of monthly outlet visits as the dependent variable. *Treatment* is a dummy variable set equal to one if an outlet belongs to a firm that is the target in a class action lawsuit, zero otherwise. *Post* is a dummy variable set equal to one for the month of the filing of the class action lawsuit and all months thereafter. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits targeting firms headquartered outside of the U.S., lawsuits filed before March 1, 2018, lawsuits filed after February 29, 2020, and lawsuits for which we cannot match outlets in SafeGraph. We also drop outlets for which we cannot match control outlets. For each treated outlet, we retain eleven monthly observations around the filing month of a lawsuit, five months before and five months after. The control sample is constructed by selecting all monthly observations of outlets in the same ZIP code and six-digit NAICS industry as the treated outlets. Detailed descriptions of all variables used throughout the study are provided in the Appendix. Standard errors, reported in parentheses, are clustered at the firm-month level. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Media Coverage

	ln(Visits)		
	1	2	3
Treatment × Post	-0.1213***	-0.1097***	-0.1157***
	(0.0289)	(0.0205)	(0.0264)
Treatment × Post × HHI ZIP NAICS6	0.3328***	0.3098***	0.3226***
	(0.0690)	(0.0520)	(0.0636)
Treatment × Post × Change AEV (%)	0.0052		0.0021
	(0.0042)		(0.0036)
Treatment × Post × Change AES (%)		0.0728***	0.0669***
		(0.0239)	(0.0175)
Lawsuit × Outlet FE	Yes	Yes	Yes
Lawsuit × ZIP × NAICS × Month FE	Yes	Yes	Yes
N	2,001,680	2,001,680	2,001,680
R^2	0.977	0.977	0.977

Panel B: Outlet Characteristics

	ln(Visits)		
	1	2	3
Treatment × Post	-0.0670***	-0.1080***	-0.0560***
	(0.0203)	(0.0219)	(0.0171)
Treatment × Post × HHI ZIP NAICS6	0.2804***	0.2872***	0.2656***
	(0.0532)	(0.0546)	(0.0513)
Treatment × Post × Religious (%)	-0.0648***		
	(0.0130)		
Treatment × Post × Social capital		-0.0063**	
		(0.0030)	
Treatment × Post × Republican (%)			-0.0924***
			(0.0152)
Lawsuit × Outlet FE	Yes	Yes	Yes
Lawsuit × ZIP × NAICS × Month FE	Yes	Yes	Yes
N	2,062,975	2,089,237	2,089,795
R^2	0.977	0.976	0.977

Table 5
Competition and Stock Market Response to Class Action Lawsuits

This table presents results of analyses of the effect of competition on the stock market response to filings of class action lawsuits. Panel A reports descriptive statistics. Panel B reports results of regressions of the cumulative abnormal return (CAR) around the class action filing date on measures of competition. The sample comprises 3,074 class action lawsuits. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits targeting firms headquartered outside of the U.S., lawsuits involving firms headquartered outside of the U.S., lawsuits whose outcome are unknown ("ongoing"), and lawsuits that are remanded to another court. We also retain only lawsuits for which we can compute cumulative abnormal returns around the filing date, which requires non-missing stock return observations in the 12-day event window and at least 90 non-missing return observations in the estimation window. Detailed descriptions of all variables used throughout the study are provided in the Appendix. Robust standard errors are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Descriptive Statistics – Daily Firm Sample

	Mean	Std	p25	p50	p75
Announcement return					
CAR(-10,1)	-9.795	25.382	-19.614	-3.862	2.843
Competition					
HHI	0.305	0.244	0.130	0.212	0.392
# other firms	101.796	136.463	9.000	33.000	154.000
No competing firm	0.033	0.177	0.000	0.000	0.000
Lawsuit and firm characteristics					
Settled	0.480	0.500	0.000	0.000	1.000
Market cap. (USDbn)	5.713	23.629	0.156	0.565	2.094
Leverage	0.226	0.248	0.004	0.158	0.370
ROE	-0.104	0.516	-0.106	-0.004	0.031
MTB	3.510	6.296	1.154	2.228	4.219
PPE/AT	0.197	0.203	0.056	0.120	0.256
Foreign revenue (%)	0.014	0.063	0.000	0.000	0.000
Idiosyncratic vola	0.043	0.024	0.025	0.037	0.056
ILLIQ	0.140	0.594	0.001	0.004	0.031
N	3,074				

Panel B: Competition and Stock Market Response to Class Action Lawsuits

	CAR(-10,1)					
	1	2	3	4	5	6
HHI	3.7660**	3.9643**				
	(1.7748)	(1.8111)				
# other firms			-0.0087**	-0.0093**		
			(0.0041)	(0.0044)		
No competing firm					4.7828***	4.6250***
					(1.6633)	(1.7191)
Settled		-7.0281***		-7.0340***		-6.9853***
		(1.1105)		(1.1112)		(1.1131)
ln(Market cap.)		1.7127***		1.7564***		1.7380***
		(0.4154)		(0.4182)		(0.4160)
Leverage		-1.7364		-2.0530		-1.6351
		(2.1889)		(2.2028)		(2.1966)
ROE		-1.2460		-1.3187		-1.2098
		(1.1384)		(1.1404)		(1.1344)
MTB		-0.5247***		-0.5201***		-0.5276***
		(0.0880)		(0.0877)		(0.0878)
PPE/AT		7.4783***		6.4768***		7.6420***
		(2.1872)		(2.2238)		(2.2016)
Foreign revenue (%)		4.5177		5.1139		4.7433
		(8.8687)		(8.8850)		(8.8889)
Idiosyncratic vola		80.3879***		89.2791***		77.1025***
		(29.9528)		(31.1499)		(29.7822)
ILLIQ		0.9842		0.9476		1.0607
		(1.1445)		(1.1397)		(1.1402)
Constant	-10.9448***		-8.9061***		-9.9509***	
	(0.7545)		(0.5529)		(0.4700)	
Year FE	No	Yes	No	Yes	No	Yes
N	3,074	2,878	3,074	2,878	3,074	2,878
R^2	0.001	0.120	0.002	0.120	0.001	0.119

Table 6
Competition and the Revenue Response to Class Action Lawsuits

This table presents results of analyses of the effect of competition on the revenue response to filings of class action lawsuits. Panel A reports descriptive statistics. Panel B reports results on the effect of the revenue response to filings of class action lawsuits. Panel C reports results on the effect of competition on the revenue response to filings of class action lawsuits. *Treatment* is a dummy variable set equal to one if a firm is the target in a class action lawsuit, zero otherwise. *Post* is a dummy variable set equal to one for the quarter of the filing of the class action lawsuit and all months thereafter. The treatment sample comprises firms targeted by 3,438 class action lawsuits. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits involving firms headquartered outside of the U.S., lawsuits whose outcome are unknown ("ongoing"), and lawsuits that are remanded to another court. For each treated firm, we retain eight quarterly observations around the filing quarter of a lawsuit, four quarters before and three quarters after. The control sample is constructed by selecting all quarterly observations of firms in the same six-digit NAICS industry as the treated firms. Detailed descriptions of all variables used throughout the study are provided in the Appendix. Standard errors, reported in parentheses, are clustered at the class action lawsuit-firm level. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Descriptive Statistics – Quarterly Firm Sample

	Mean	Std	p25	p50	p75
Firm characteristics					
Revenue (USDm)	249.207	1799.703	3.223	13.465	59.349
Gross profit (USDm)	135.202	919.760	0.725	6.379	31.676
EBIT (USDm)	68.404	514.752	-1.766	0.427	8.084
Free cash flow (USDm)	84.055	567.508	-1.221	0.935	10.785
Treatment	0.014	0.116	0.000	0.000	0.000
Competition					
HHI	0.165	0.118	0.089	0.134	0.194
# other firms	203.447	138.530	99.000	155.000	342.000
One competing firm	0.001	0.035	0.000	0.000	0.000
Media coverage					
Change AEV (%)	1.867	4.585	-0.222	0.500	1.922
Change AES (%)	-0.195	0.590	-0.577	-0.300	0.023
Lawsuit characteristics					
CAR (-10,1)	-10.885	28.418	-23.732	-4.644	5.096
Duration violation (days)	346.660	353.251	111.000	257.000	450.000
Duration resolved (days)	1272.989	1015.882	518.000	926.000	1858.000
Settled	0.500	0.500	0.000	1.000	1.000
N	1,657,900				

Panel B: Revenue Response to Class Action Lawsuits

	ln(Revenue)			
	1	2	3	4
Treatment × Post	-0.0779***	-0.0706***	-0.0708***	-0.0755***
	(0.0073)	(0.0072)	(0.0072)	(0.0104)
Post	0.0794***	0.0025***		
	(0.0009)	(0.0010)		
Lawsuit × Firm FE	Yes	Yes	Yes	Yes
NAICS × Quarter FE	No	Yes	No	No
Lawsuit × NAICS × Quarter FE	No	No	Yes	No
Lawsuit × NAICS × Quarter × State FE	No	No	No	Yes
N	1,657,900	1,657,900	1,657,900	1,654,492
R^2	0.979	0.980	0.980	0.984

Panel C: Competition and Revenue Response to Class Action Lawsuits

	ln(Revenue)		
	1 HHI	2 # other firms	3 One comp. firm
Treatment × Post × Metric	0.1266***	-0.0002**	0.0902**
	(0.0315)	(0.0001)	(0.0358)
Treatment × Post	-0.1044***	-0.0486***	-0.0801***
	(0.0128)	(0.0086)	(0.0076)
Treatment × Metric	0.3205***	0.0005	-0.0574
	(0.1212)	(0.0005)	(0.0419)
Post × Metric	-0.0983***	0.0001***	-0.0335
	(0.0072)	(0.0000)	(0.0243)
Metric	-0.6425***	0.0004***	0.0190
	(0.0205)	(0.0000)	(0.0258)
Post	0.1002***	0.0592***	0.0794***
	(0.0015)	(0.0015)	(0.0009)
Lawsuit × Firm FE	Yes	Yes	Yes
N	1,657,900	1,657,900	1,657,900
R^2	0.979	0.979	0.979

Table 7
Media Coverage, Lawsuit Characteristics, and Revenue Response

This table presents results of ordinary least squares regressions using the logarithm of quarterly revenues as the dependent variable. *Treatment* is a dummy variable set equal to one if a firm is the target in a class action lawsuit, zero otherwise. *Post* is a dummy variable set equal to one for the quarter of the filing of the class action lawsuit and all months thereafter. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits involving firms headquartered outside of the U.S., lawsuits whose outcome are unknown ("ongoing"), and lawsuits that are remanded to another court. For each treated firm, we retain eight quarterly observations around the filing quarter of a lawsuit, four quarters before and three quarters after. The control sample is constructed by selecting all quarterly observations of firms in the same six-digit NAICS industry as the treated firms. Detailed descriptions of all variables used throughout the study are provided in the Appendix. Standard errors, reported in parentheses, are clustered at the class action lawsuit-firm level. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Media Coverage

	ln(Revenue)		
	1	2	3
Treatment × Post	-0.0521***	-0.0472***	-0.0468***
	(0.0091)	(0.0091)	(0.0096)
Post	0.0587***	0.0587***	0.0556***
	(0.0012)	(0.0012)	(0.0012)
Treatment × Post × Change AEV (%)	-0.0012		-0.0005
	(0.0023)		(0.0023)
Post × Change AEV (%)	0.0026***		0.0021***
	(0.0003)		(0.0003)
Treatment × Post × Change AES (%)		0.0320**	0.0318**
		(0.0151)	(0.0152)
Post × Change AES (%)		-0.0218***	-0.0195***
		(0.0020)	(0.0020)
Lawsuit × Firm FE	Yes	Yes	Yes
N	881,682	881,682	881,682
R^2	0.981	0.981	0.981

Panel B: Lawsuit Characteristics

	Full sample			Settled	Dismissed
	1	2	3	4	5
Treatment × Post	-0.0761***	-0.0202	-0.0680***	-0.4432***	0.0807*
	(0.0089)	(0.0312)	(0.0088)	(0.1468)	(0.0475)
Post	0.0678***	0.1410***	0.0992***	0.7897***	0.0867***
	(0.0010)	(0.0043)	(0.0013)	(0.0144)	(0.0100)
Treatment × Post × CAR(-10,1)	0.0006*				
	(0.0004)				
Post × CAR(-10,1)	-0.0009***				
	(0.0000)				
Treatment × Post × ln(Duration violation)		-0.0106*			
		(0.0055)			
Post × ln(Duration violation)		-0.0114***			
		(0.0008)			
Treatment × Post × Settled			-0.0264*		
			(0.0149)		
Post × Settled			-0.0368***		
			(0.0018)		
Treatment × Post × ln(Duration resolved)				0.0472**	-0.0258***
				(0.0205)	(0.0082)
Post × ln(Duration resolved)				-0.0995***	0.0027*
				(0.0020)	(0.0016)
Lawsuit × Firm FE	Yes	Yes	Yes	Yes	Yes
N	1,441,532	1,656,817	1,657,900	829,400	611,862
R^2	0.979	0.979	0.979	0.977	0.981

Figure 1: Retail Customer Response to Class Action Lawsuits

This figure plots 90% confidence intervals and estimates from coefficients of the following regression:

$$ln(Visits)_{i,s,t} = \beta_t \times Treatment_{i,s} \times Event\ month_{i,t} + \alpha_{i,s} + \alpha_{s,j,k,t} + \epsilon_{i,s,t},$$

where i indexes an outlet, s a lawsuit, t a month, j an industry, and k a ZIP code. *Visits* is the monthly number of visits at the outlet. *Treatment* is a dummy indicating whether the outlet belongs to the target of the lawsuit. *Event month* are dummies indicating months relative to the filing month. $\alpha_{i,s}$ are interacted lawsuit and outlet fixed effects. $\alpha_{s,j,k,t}$ are interacted lawsuit, six-digit NAICS industry, ZIP code, and month fixed effects. The sample of treated outlets comprises 25,155 outlets associated with a firm involved in a class action lawsuit. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms, firms headquartered outside of the U.S., lawsuits filed before March 1, 2018 or after February 29, 2020, and lawsuits for which we cannot match outlets in SafeGraph. For each treated outlet, we retain eleven monthly observations around the filing month of a lawsuit, five months before and five months after. The control sample is constructed by selecting all monthly observations of outlets in the same ZIP code and six-digit NAICS industry as the treated outlets. The constant sample only comprises firms for which data in each month are available. Detailed descriptions of all variables used throughout the study are provided in the Appendix. Standard errors are clustered at the firm-month level.

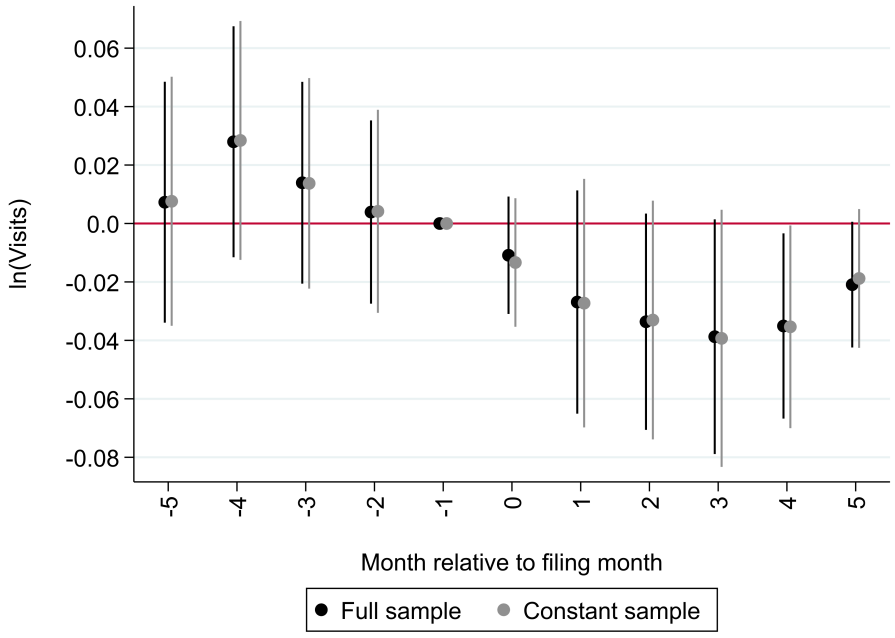


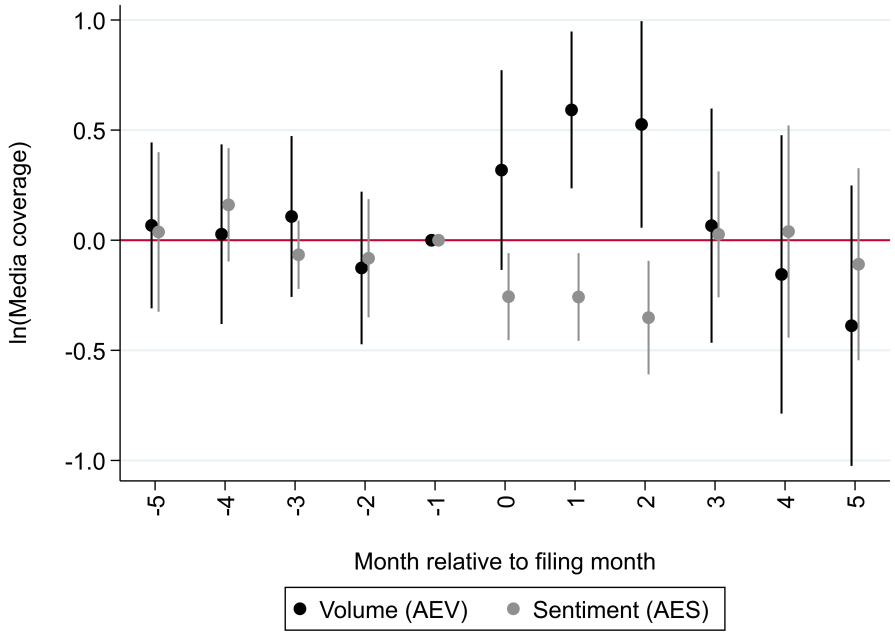
Figure 2: Media Response to Class Action Lawsuits

This figure plots 90% confidence intervals and estimates from coefficients of the following regression:

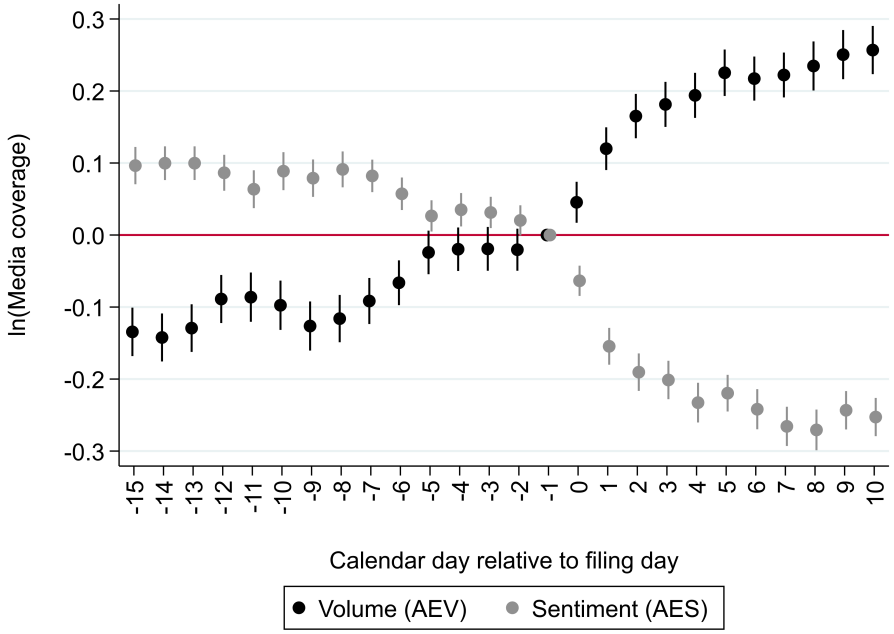
$$ln(Media\ coverage)_{i,s,t} = \beta_t \times Event\ time_{i,s,t} + \alpha_{i,s} + \epsilon_{i,s,t},$$

where i indexes a firm, s a lawsuit, and t a time intervals. *Media coverage* is either the Aggregate Event Volume (AEV) or the Aggregate Event Sentiment (AES) from the Ravenpack database. *Event time* are dummies indicating months (Panel A), calendar days (Panel B), or quarters (Panel C) relative to the filing of a class action lawsuit. $\alpha_{i,s}$ are interacted lawsuit and firm fixed effects. In Panel A, the sample comprises 273 lawsuit-months and 28 lawsuits from the SafeGraph sample (see Table 1). In Panel B, the sample comprises 39,786 lawsuit-days and 2,374 lawsuits from the CRSP sample (see Table 5). In Panel C, the sample comprises 32,039 lawsuit-quarters and 2,715 lawsuits from the Compustat sample (see Table 6). Standard errors are clustered at the lawsuit level.

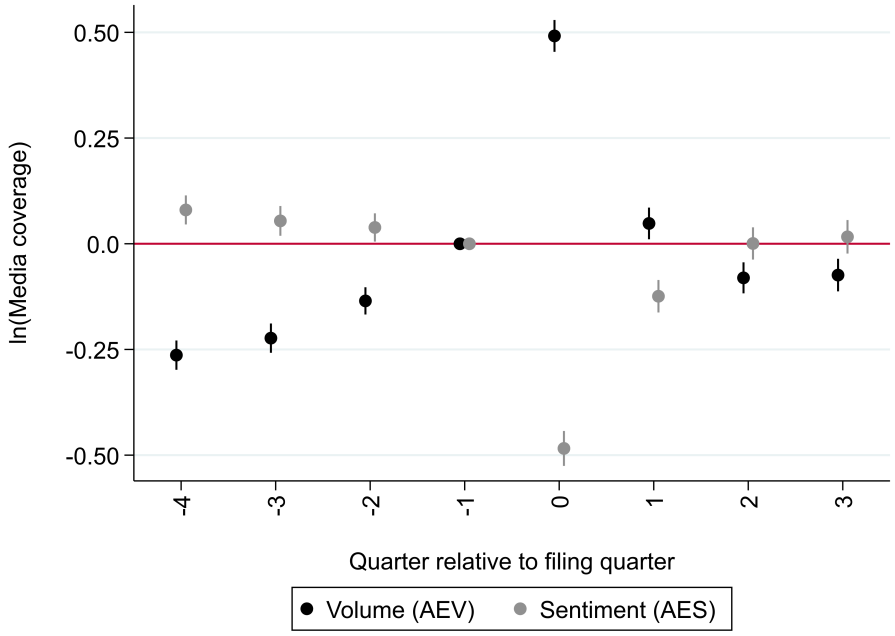
Panel A: Monthly Firm Sample



Panel B: Daily Firm Sample



Panel C: Quarterly Firm Sample



Internet Appendix for "Competition and the Reputational Costs of Litigation"

Felix von Meyerinck, Vesa Pursiainen, and Markus Schmid

IA.I Additional sample information and summary statistics

IA.I.A List of class action lawsuits in outlet sample

Table IA.1
List of class action lawsuits in outlet sample

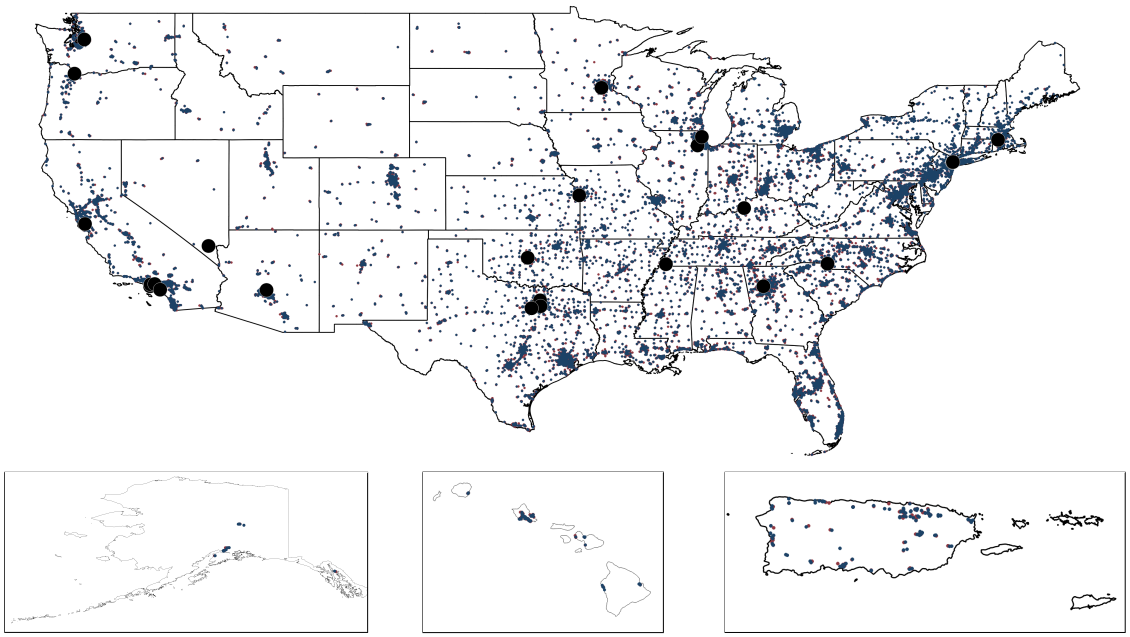
This table presents a list of the class action lawsuits in the outlet-month panel as well as the number of treatment and control outlets for each class action lawsuit.The sample of treated outlets comprises 25,155 outlets associated with a firm involved in a class action lawsuit. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits targeting firms headquartered outside of the U.S., lawsuits filed before March 1, 2018, lawsuits filed after February 29, 2020, and lawsuits for which we cannot match outlets in SafeGraph. We also drop outlets for which we cannot match control outlets. The control sample is constructed by selecting all outlets in the same ZIP code and six-digit NAICS industry as the treated outlets, resulting in a control comprising 123,129 outlets.

Filing date	Company	Treated outlets	Control outlets
02mar2018	Sprouts Farmers Market Inc.	334	1,212
02mar2018	Ulta Beauty, Inc.	881	1,830
08mar2018	La Quinta Holdings Inc.	888	7,869
09mar2018	Foot Locker, Inc.	235	817
23aug2018	Jamba, Inc.	653	4,013
24aug2018	SUPERVALU, Inc.	17	33
30aug2018	Papa John's International, Inc.	2,974	53,372
04sep2018	Skechers USA, Inc.	369	1,946
28sep2018	Sprint Corporation	2,657	7,117
17oct2018	CafePress Inc.	2	11
29oct2018	Sonic Corp	3,469	44,046
02nov2018	Walgreens Boots Alliance, Inc.	2,422	2,612
05nov2018	Costco Wholesale Corporation	461	1,773
12dec2018	Bojangles', Inc.	663	4,435
27dec2018	YogaWorks, Inc.	12	12
25feb2019	CVS Health Corporation	2,602	2,451
04mar2019	Weight Watchers Int., Inc.	65	62
01apr2019	AT&T Inc.	3,625	8,291
16apr2019	Apple Inc.	81	141
22apr2019	Sprint Corporation	2,625	7,036
30apr2019	Papa Murphys Holdings, Inc.	1,299	21,610
20may2019	Smart & Final Stores, Inc.	240	813
26jun2019	FedEx Corporation	605	580
16jul2019	Barnes & Noble, Inc.	56	61
15aug2019	CVS Health Corporation	2,601	2,445
07nov2019	Tandy Leather Factory, Inc.	4	9
03jan2020	Tiffany & Co.	36	153
11feb2020	The Habit Restaurants, Inc.	271	6,193
28feb2020	Aarons, Inc.	155	176

IA.I.B Geographic distribution of outlet sample

Figure IA.1: Spatial distribution of outlets

This figure shows the distribution of the locations of the treated outlets (red) and control outlets (blue) across the U.S. The sample of treated outlets comprises 25,155 outlets associated with a firm involved in a class action lawsuit. The sample of treated outlets comprises 25,155 outlets associated with a firm involved in a class action lawsuit. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits targeting firms headquartered outside of the U.S., lawsuits filed before March 1, 2018, lawsuits filed after February 29, 2020, and lawsuits for which we cannot match outlets in SafeGraph. We also drop outlets for which we cannot match control outlets. The control sample is constructed by selecting all outlets in the same ZIP code and six-digit NAICS industry as the treated outlets, resulting in a control comprising 123,129 outlets. Black dots mark the locations of the headquarters of the firms involved in the class action lawsuits.



IA.I.C Number of class action lawsuits over time

Table IA.2
Filings of class action lawsuits over time

This table presents filings of class action lawsuits in the announcement return sample by calendar year and month. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits involving firms headquartered outside of the U.S., lawsuits whose outcomes are unknown ("ongoing"), and lawsuits that are remanded to another court. We also retain only lawsuits for which we can compute cumulative abnormal returns around the filing date, which requires non-missing stock return observations in the 12-day event window and at least 90 non-missing return observations in the estimation window. The final sample comprises 3,074 class action lawsuits.

	Month												
Year	1	2	3	4	5	6	7	8	9	10	11	12	Sum
1996	3	4	4	11	3	5	8	11	2	9	5	2	67
1997	9	6	7	14	10	7	10	6	10	11	9	19	118
1998	13	12	20	11	9	10	11	16	9	18	17	9	155
1999	25	11	23	18	10	11	5	3	11	13	12	8	150
2000	7	14	12	9	9	11	11	12	10	19	11	5	130
2001	7	9	18	21	16	39	76	24	7	29	77	36	359
2002	8	14	7	18	13	7	17	17	6	13	13	9	142
2003	7	12	14	11	11	11	12	4	7	5	11	11	116
2004	5	12	10	9	9	15	15	19	12	9	12	7	134
2005	12	7	13	7	12	14	6	11	6	6	10	7	111
2006	3	9	7	8	7	10	8	6	8	6	3	3	78
2007	4	3	8	10	5	7	5	13	7	6	9	5	82
2008	9	2	8	8	3	3	4	5	11	8	10	7	78
2009	1	4	6	2	3	1	11	10	3	7	9	5	62
2010	3	1	7	4	7	6	4	11	13	9	12	7	84
2011	6	8	10	7	6	5	11	11	2	7	11	4	88
2012	10	10	5	7	9	8	6	7	5	3	4	6	80
2013	4	7	14	4	11	12	5	10	6	8	13	7	101
2014	9	5	11	5	11	3	9	6	8	9	8	2	86
2015	7	8	10	11	12	7	12	14	10	8	17	7	123
2016	11	11	13	8	11	12	12	18	15	10	16	14	151
2017	19	22	26	11	19	26	17	27	15	16	16	11	225
2018	17	16	18	20	7	12	22	16	17	12	19	18	194
2019	13	7	14	15	12	17	17	13	18	12	10	12	160

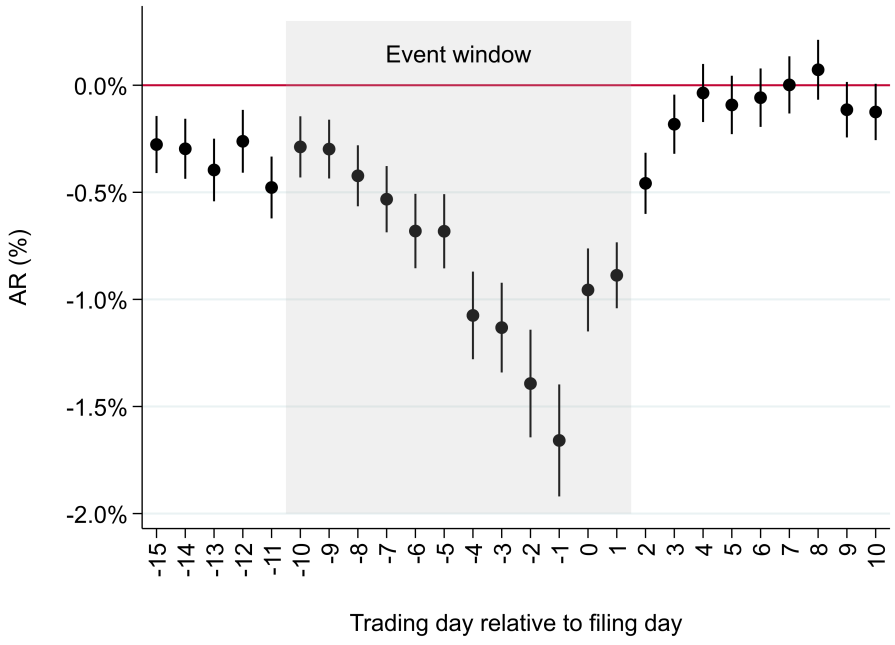
IA.II Additional figures

IA.II.A Stock market response to class action lawsuits

To provide further justification for the event window, Figure **??** displays means and 90% confidence intervals for daily abnormal returns from 15 trading days before to ten trading days after the filing date of the class action lawsuits. The figure shows that average daily abnormal returns are significantly negative for most trading days during this 26-day long window. The most negative abnormal return (-1.6%) can be observed on the day before the filing date. Economically meaningful negative abnormal returns also occur two to four days before the filing date, suggesting that the disclosure of material adverse information that potentially serves as the event that triggers the filing of a lawsuit is incorporated into stock prices already before the filing. These patterns are consistent with findings in Gande and Lewis (2009).

Figure IA.2: Stock market response to class action lawsuits

This figure plots means of daily abnormal returns (ARs) as well as 90% confidence intervals of t-tests against zero for 26 trading days around the filing date of a class action lawsuit. Daily ARs are calculated as the observed return (*ret*) minus a predicted return. The predicted return is estimated using a market model regression where daily returns (*ret*) are regressed on daily value-weighted index returns (*vwret*) over a 250-day estimation window that ends eleven trading days prior to the event date. The shaded area highlights the 12-day asymmetric event window used to compute cumulative abnormal returns. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits involving firms headquartered outside the U.S., lawsuits whose outcomes are unknown ("ongoing"), and lawsuits that are remanded to another court. We also retain only lawsuits for which we can compute cumulative abnormal returns around the filing date, which requires non-missing stock return observations in the 12-day event window and at least 90 non-missing return observations in the estimation window.



IA.II.B Revenue response to class action lawsuits

To analyze customers' response to class action lawsuits, we estimate the change in accounting revenues of firms targeted by a class action lawsuit from four quarters before to three quarters after the filing quarter (eight quarters in total) and compare it to changes in revenues of firms in the same quarter. Similar to the analysis in Section III, we first conduct a quarterly regression analysis specified as:

$$(1) \qquad ln(Revenue)_{i,s,t} = \beta_t \times Treatment_{i,s} \times Event\ quarter_{i,t} + \alpha_{i,s} + \alpha_{s,t,j} + \epsilon_{i,s,t},$$

where i indexes firms, s lawsuits, t quarters, and j industries. *Revenue* is quarterly revenue. *Treatment* is a dummy indicating whether the firm is the target of a lawsuit. *Event quarter* are dummies indicating quarters relative to the filing quarter. We include interacted lawsuit and firm fixed effects as well as interacted lawsuit, quarter, and industry fixed effects, which absorb all time-invariant firm characteristics within a lawsuit, including the Treatment dummy.

Figure **??** shows the estimated quarterly regression coefficients for the interacted treatment-quarter dummies, relative to the quarter preceding the filing of the lawsuit (omitted in the regression). Similar to the outlet-level results reported in Section III, revenues for firms targeted in class action lawsuits starts falling before the announcement. The revenue decline at the firm level seems to be somewhat larger in magnitude and lasts longer than the declined in store visits estimated at the outlet level. This may be due to the larger sample or due to other factors like store closures or divestments, that are not captured by outlet

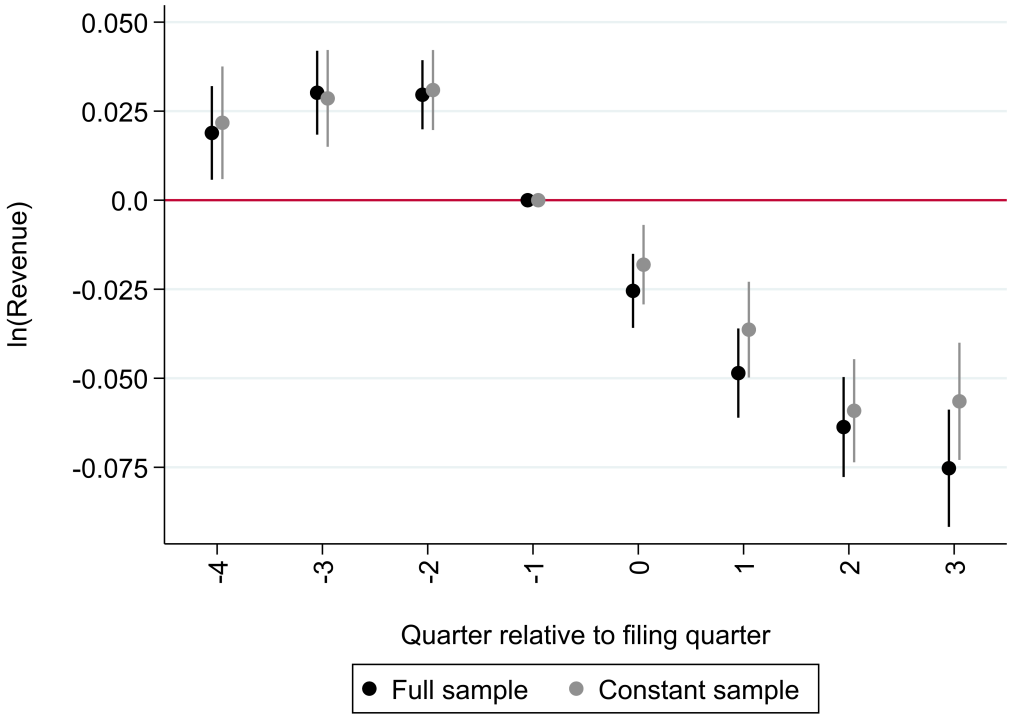
visits, having an additional negative effect on firm-level revenue.

Figure IA.3: Revenue response to class action lawsuits

This figure plots 90% confidence intervals and estimates from coefficients of the following regression:

$$ln(Revenue)_{i,s,t} = \beta_t \times Treatment_{i,s} \times Event\ quarter_{i,t} + \alpha_{i,s} + \alpha_{s,t,j} + \epsilon_{i,s,t},$$

where i indexes a firm, s a lawsuit, t a quarter, and j industries. *Revenue* is quarterly revenue. *Treatment* is a dummy indicating whether the firm is the target of the lawsuit. *Event quarter* indicate quarters relative to the filing quarter. $\alpha_{i,s}$ are interacted lawsuit and firm fixed effects. $\alpha_{s,t,j}$ are interacted lawsuit, six-digit NAICS industry, and month fixed effects. The treatment sample comprises firms targeted by 3,438 class action lawsuits. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits involving firms headquartered outside the U.S., lawsuits whose outcome are unknown ("ongoing"), and lawsuits that are remanded to another court. We also drop firms for which we cannot match control firms. For each treated firm, we retain eight monthly observations around the filing month of a lawsuit, three months before and four months after. The control sample is constructed by selecting all quarterly observations of firms in the same six-digit NAICS industry as the treated firms. The constant sample only comprises outlets of firms for which data in each quarter are available. Detailed descriptions of all variables used throughout the study are provided in the Appendix. Standard errors are clustered at the class action lawsuit-firm level.



IA.III Competition and the likelihood of class action lawsuits

In this section, we perform an analysis of the likelihood of a firm being the target of a class action lawsuit, dependent on the level of competition it faces. We construct a firm-year panel using data from Compustat and perform a regression analysis where the dependent variable is a dummy indicating if the firm is the target of a class action lawsuit in that year.

This analysis is presented in Table **??**. While the there is some correlation between competition and the likelihood of being a target in a class action lawsuit, this relationship appears relatively weak and largely statistically insignificant when controlling for state fixed effects, which are likely relevant for the propensity of lawsuits, or firm fixed effects, effectively focusing on within-firm variation over time.

We also note that our analysis generally focuses on short-term within-firm changes in customer visits or revenue, while the measures of competition are quite stable over time. This means that any of our results are unlikely to be substantially affected by competition affecting the likelihood of lawsuits.

Table IA.3
Determinants of filings of class action lawsuits

Panel A presents summary statistics for our firm-year sample. Panel B reports results of ordinary least squares regression using *CALS filing* as the dependent variable, a dummy variable taking the value of one if there is a filing of a class action lawsuit against a firm in a quarter, zero otherwise. Control variables include *ln(Market cap.)*, *Leverage*, *ROE*, *MTB*, and *PPE / AT*. Standard errors are double-clustered at the industry and quarter level.

Panel A: Descriptive statistics

	Mean	Std	p25	p50	p75
CALS filing	0.007	0.086	0.000	0.000	0.000
HHI	0.358	0.270	0.152	0.269	0.500
# other firms	45.412	71.244	4.000	14.000	60.000
No competing firm	0.059	0.235	0.000	0.000	0.000
N	361,755				

Panel B: Determinants of filings of class action lawsuits

	CALS filing × 100								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
HHI	-0.2360**			-0.1604			0.0919		
	(0.1137)			(0.0971)			(0.2118)		
# other firms		0.0030***			0.0025***			0.0039**	
		(0.0003)			(0.0003)			(0.0017)	
No competing firm			-0.0956			-0.0450			0.0662
			(0.0722)			(0.0681)			(0.1138)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	Yes	Yes	Yes	No	No	No
Firm FE	No	No	No	No	No	No	Yes	Yes	Yes
N	361,751	361,751	361,751	361,751	361,751	361,751	361,751	361,751	361,751
R^2	0.004	0.004	0.004	0.007	0.007	0.007	0.047	0.047	0.047

IA.IV Retail trader response to class action lawsuits

Another complementary channel through which retail customers might learn of lawsuits or the events leading up to them are stock investments. Retail investors have additional incentives to monitor the companies they own or consider investing in. **?** find that retail investors' investment decisions are influenced by their product market choices. Hence, it is likely that many investors in a retail company stock are also its customers. If retail investors learn of a lawsuit or the misconduct that caused it, they may react both as consumers as well as investors. Consistently, the results of **?** suggest that retail investors recognize and respond to corporate misconduct. Furthermore, retail investors may propagate the information through their social connections, amplifying the consumer reaction.

To study retail investors' reactions to class action lawsuits, we use retail trading data from Robinhood. Robinhood is a large brokerage platform that targets small and cost-conscious retail investors with its no-brokerage-fee business model. It earns its revenues primarily through margin fees, cash balance interest, and payment-for-order flow. For the period from May 2018 to August 2020, Robinhood offered an Application Programming Interface (API) that provided the number of accounts holding each stock. These data were systematically collected by a website called Robintrack.net, which offers a dataset comprising the number of accounts holding each stock on an hourly basis. This dataset has been used in a number of studies of retail investor behavior (e.g., **???**). We use these data to construct a daily panel dataset of retail accounts holding the stock of each firm that is the target of a lawsuit in our outlet sample. We construct the control sample by matching all firms in the same

six-digit NAICS industry as the firms targeted by the lawsuits. We then perform a regression analysis in which we compare the change in the number of retail accounts holding stocks of firms targeted by the lawsuits around the filing date with the change in the number of retail accounts holding stocks of control firms.

Panel A of Figure **??** plots the daily regression coefficients, relative to two trading days before the filing of the class action lawsuit. We find a statistically significant reduction in the number of retail accounts holding stocks of firms targeted around the filing date compared to firms in the same industry, with approximately 10% reduction in retail holders during the 15 days following the filing. Panel B of Figure **??** plots monthly regression coefficients for five months before to five months after the filing month of the lawsuit. These coefficient estimates imply that the downward trend continues over several months and does not revert, with the number of retail accounts holding the stocks of firms targeted in a lawsuit dropping to around 26% four months after the filing month. Taken together, the findings presented in this section suggests that it is reasonable to assume that retail customers are aware of the lawsuits, as media coverage increases substantially and retail traders respond by divesting stocks.

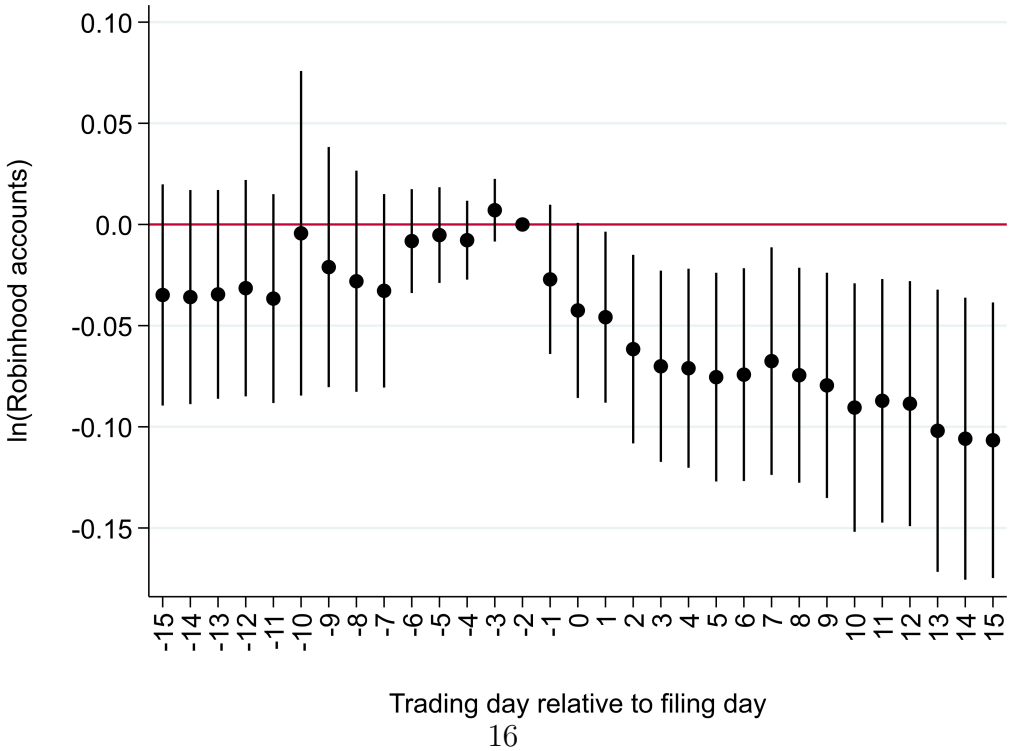
Figure IA.4: Retail trader response to class action lawsuits

This figure plots 90% confidence intervals and estimates from coefficients of the following regression:

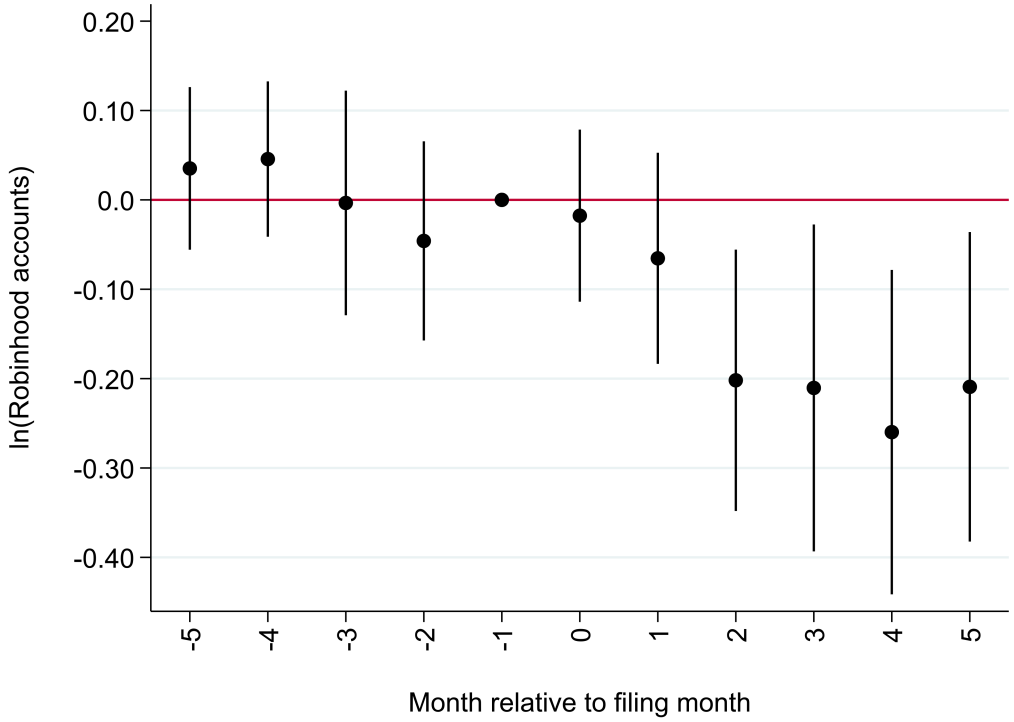
$$ln(Robinhood\ accounts)_{i,s,t} = \beta_t \times Treatment_{i,s} \times Time_{i,t} + \alpha_{i,s} + \alpha_{s,t,j} + \epsilon_{i,s,t},$$

where i indexes a firm, s a lawsuit, t time, and j an industry. *Robinhood accounts* is the number of Robinhood accounts that a hold a given stock. *Treatment* is a dummy indicating whether a firm is the target of a lawsuit. *Time* are day (month) dummies indicating trading days (calendar months) relative to the filing day (month). $\alpha_{i,s}$ are interacted lawsuit and firm fixed effects. $\alpha_{s,t,j}$ are interacted lawsuit, six-digit NAICS industry, and time fixed effects. The sample of treated firms comprises 17 firms that are targeted by a class action lawsuit. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits targeting firms headquartered outside of the U.S., lawsuits filed before May 2, 2018, lawsuits filed after February 29, 2020, lawsuits for which we cannot match outlets in SafeGraph, and lawsuits and for which we cannot match the number of Robinhood account holders. We also drop firms for which we cannot match control outlets as described in Section **??**. Panel A plots estimates of a regression using 31 daily observations around the filing day of a lawsuit, 15 trading days before to 15 trading days after the filing date. Panel B plots estimates of a regression using eleven monthly observations around the filing quarter of a lawsuit, 5 months before to 5 months after the filing month. The control sample is constructed by selecting observations of 119 firms in the Robinhood data that are active in the same six-digit NAICS industry as the treated firms. Detailed descriptions of all variables used throughout the study are provided in the Appendix. Standard errors are clustered at the class action lawsuit-firm level.

Panel A: Daily account holders



Panel B: Monthly account holders



IA.V Additional robustness checks

IA.V.A Number of unique visitors as a measure of customer activity

Table IA.4
Number of unique visitors as a measure of customer activity

This table presents results of ordinary least squares regressions using the logarithm of the number of monthly outlet visitors as the dependent variable. *Treatment* is a dummy variable set equal to one if an outlet belongs to a firm that is the target in a class action lawsuit, zero otherwise. *Post* is a dummy variable set equal to one for the month of the filing of the class action lawsuit and all months thereafter. The sample of treated outlets comprises 25,155 outlets associated with a firm involved in a class action lawsuit. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits targeting firms headquartered outside of the U.S., lawsuits filed before March 1, 2018, lawsuits filed after February 29, 2020, and lawsuits for which we cannot match outlets in SafeGraph. For each treated outlet, we retain eleven monthly observations around the filing month of a lawsuit, five months before and five months after. The control sample is constructed by selecting all monthly observations of outlets in the same ZIP code and six-digit NAICS industry as the treated outlets. Detailed descriptions of all variables used throughout the study are provided in the Appendix. Standard errors, reported in parentheses, are clustered at the firm-month level. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

	ln(Visitors)			
	(1)	(2)	(3)	(4)
Treatment × Post	-0.0359***	-0.0959***	0.0056	-0.0533***
	(0.0099)	(0.0197)	(0.0044)	(0.0122)
Treatment × Post × HHI ZIP NAICS6		0.2609***		
		(0.0499)		
Treatment × Post × # other brands			-0.0055***	
			(0.0011)	
Treatment × Post × One competing brand				0.0649***
				(0.0117)
Lawsuit × Outlet FE	Yes	Yes	Yes	Yes
Lawsuit × ZIP × NAICS × Month FE	Yes	Yes	Yes	Yes
N	2,099,633	2,099,633	2,099,633	2,099,633
R^2	0.980	0.980	0.980	0.980

IA.V.B Beginning of class action period as event date

As discussed in the main text, the treatment timing in class action lawsuits is, by definition, somewhat noisy. To address this issue, in Figure **??**, we change the timing of treatment to the beginning of the class action period. This approximates the beginning of the actual misconduct by the firm, instead of the filing of the lawsuit. We see that i) there is no longer a negative pre-trend ahead of the treatment, and ii) the estimated decrease in customer visits is larger. These observations suggest that, at least in some cases, this may better capture the timing of the customer reaction to the misconduct. At the same time, it reduces the sample size as some lawsuits move outside the period for which we have data. For this reason, and to maintain comparability with prior studies of class action lawsuits, we use the filing date as our main treatment timing. However, in Table **??**, we also perform the main analyses using the class action period beginning date instead.

We then repeat our main analysis of the effects of competition on customer response. The results, shown in Table **??**, are all qualitatively similar to our main results. However, Column 1 shows that the average negative reaction, 9.7%, is somewhat larger using this treatment timing than the filing date. Columns 2 to 4 show that the interactions with various measures of competition look similar in this analysis as in our main results. The negative reaction is significantly larger when the outlet faces more competition. These results show that our conclusions are not sensitive to the definition of the treatment timing, although the magnitude of the estimates is affected by it.

Figure IA.5: Retail customer response to class action lawsuits

This figure plots 90% confidence intervals and estimates from coefficients of the following regression:

$$ln(Visits)_{i,s,t} = \beta_t \times Treatment_{i,s} \times Event\ month_{i,t} + \alpha_{i,s} + \alpha_{s,j,k,t} + \epsilon_{i,s,t},$$

where i indexes an outlet, s a lawsuit, t a month, j an industry, and k a ZIP code. *Visits* is the monthly number of visits at the outlet. *Treatment* is a dummy indicating whether the outlet belongs to the target of the lawsuit. *Event month* are dummies indicating months relative to the beginning of the class action period. $\alpha_{i,s}$ are interacted lawsuit and outlet fixed effects. $\alpha_{s,j,k,t}$ are interacted lawsuit, six-digit NAICS industry, ZIP code, and month fixed effects. The sample of treated outlets comprises 10,980 outlets associated with a firm involved in a class action lawsuit. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits targeting firms headquartered outside of the U.S., lawsuits whose class action period begins before March 1, 2018, lawsuits whose class action period begins after February 29, 2020, and lawsuits for which we cannot match outlets in SafeGraph. We also drop outlets for which we cannot match control outlets. For each treated outlet, we retain eleven monthly observations around the filing month of a lawsuit, five months before and five months after. The control sample is constructed by selecting all monthly observations of outlets in the same ZIP code and six-digit NAICS industry as the treated outlets. The constant sample only comprises firms for which data in each month are available. Detailed descriptions of all variables used throughout the study are provided in the Appendix. Standard errors are clustered at the firm-month level.

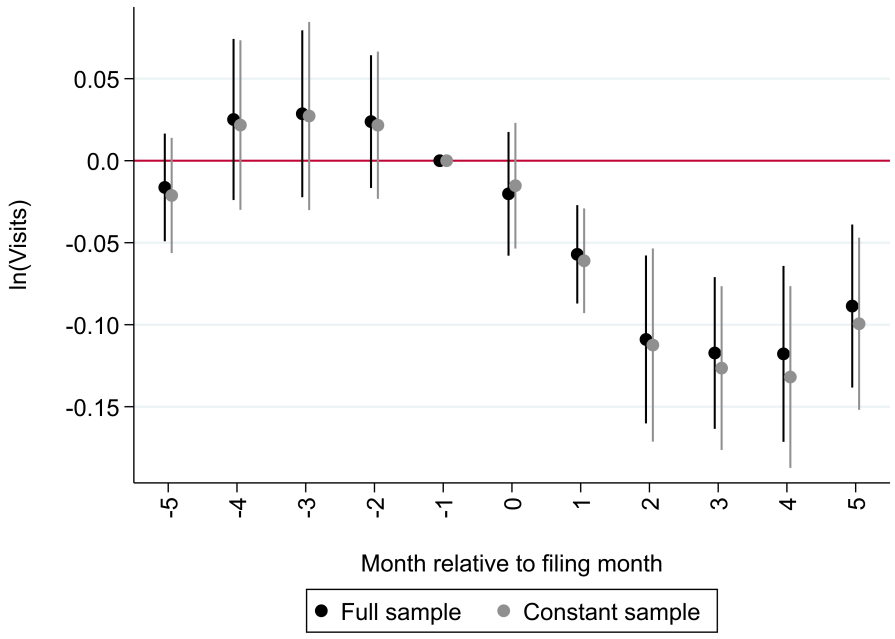


Table IA.5
Retail customer response around beginning of class action period

This table presents results of ordinary least squares regressions using the logarithm of the monthly number of outlet visits as the dependent variable. *Treatment* is a dummy variable set equal to one if an outlet belongs to a firm that is the target in a class action lawsuit, zero otherwise. *Post* is a dummy variable set equal to one for the month of the beginning of the class action period and all months thereafter. The sample of treated outlets comprises 10,980 outlets associated with a firm involved in a class action lawsuit. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits targeting firms headquartered outside of the U.S., lawsuits whose class action period begins before March 1, 2018, lawsuits whose class action period begins after February 29, 2020, and lawsuits for which we cannot match outlets in SafeGraph. For each treated outlet, we retain eleven monthly observations around the month of the beginning of the class action period, five months before and five months after. The control sample is constructed by selecting all monthly observations of outlets in the same ZIP code and six-digit NAICS industry as the treated outlets. Detailed descriptions of all variables used throughout the study are provided in the Appendix. Standard errors, reported in parentheses, are clustered at the firm-month level. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

	ln(Visits)			
	(1)	(2)	(3)	(4)
Treatment × Post	-0.0968***	-0.1541***	-0.0470***	-0.1038***
	(0.0167)	(0.0296)	(0.0089)	(0.0179)
Treatment × Post × HHI ZIP NAICS6		0.3053***		
		(0.0828)		
Treatment × Post × # other brands			-0.0055***	
			(0.0014)	
Treatment × Post × One competing brand				0.0624***
				(0.0161)
Lawsuit × Outlet FE	Yes	Yes	Yes	Yes
Lawsuit × ZIP × NAICS × Month FE	Yes	Yes	Yes	Yes
N	1,121,748	1,121,748	1,121,748	1,121,748
R^2	0.977	0.977	0.977	0.977

IA.V.C Competition and profit and CF response to class action lawsuits

Table IA.6
Competition and profit and CF response to class action lawsuits

This table presents results of analyses of the effect of competition on the profit and cash flow response to filings of class action lawsuits. *Treatment* is a dummy variable set equal to one if a firm is the target in a class action lawsuit, zero otherwise. *Post* is a dummy variable set equal to one for the quarter of the filing of the class action lawsuit and all months thereafter. The treatment sample comprises firms targeted by 3,438 class action lawsuits. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits involving firms headquartered outside of the U.S., lawsuits whose outcome are unknown ("ongoing"), and lawsuits that are remanded to another court. For each treated firm, we retain eight quarterly observations around the filing quarter of a lawsuit, four quarters before and three quarters after. The control sample is constructed by selecting all quarterly observations of firms in the same six-digit NAICS industry as the treated firms. We transform the dependent variable using the inverse hyperbolic sine. Detailed descriptions of all variables used throughout the study are provided in the Appendix. Standard errors, reported in parentheses, are clustered at the class action lawsuit-firm level. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

	ihs(Gross profit)		ihs(EBIT)		ihs(Free cash flow)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Post	-0.1668***	-0.2348***	-0.3812***	-0.4962***	-0.1921***	-0.3157***
	(0.0237)	(0.0439)	(0.0378)	(0.0696)	(0.0427)	(0.0822)
Post	0.0898***	0.1051***	0.0481***	0.0254***	0.1044***	0.1137***
	(0.0021)	(0.0037)	(0.0033)	(0.0056)	(0.0040)	(0.0071)
Treatment × Post × HHI		0.2771**		0.3334*		0.4340*
		(0.1127)		(0.1922)		(0.2344)
Post × HHI		-0.0793***		0.1527***		-0.0430
		(0.0173)		(0.0265)		(0.0316)
HHI		-0.3090***		-0.3678***		-0.2279***
		(0.0469)		(0.0712)		(0.0824)
Treatment × HHI		-0.4472		-0.9528*		-0.6380
		(0.3435)		(0.5097)		(0.5205)
Lawsuit × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
N	1,641,905	1,641,905	1,328,940	1,328,940	1,078,377	1,078,377
R^2	0.922	0.922	0.856	0.856	0.838	0.838

IA.V.D Longer-term outcomes

Figure IA.6: Longer-term outcomes – monthly customer visits

This figure plots 90% confidence intervals and estimates for coefficients of the following regression:

$$ln(Visits)_{i,s,t} = \beta_t \times Treatment_{i,s} \times Event\ month_{i,t} + \alpha_{i,s} + \alpha_{s,j,k,t} + \epsilon_{i,s,t},$$

where i indexes an outlet, s a lawsuit, t a month, j an industry, and k a ZIP code. *Visits* is the monthly number of visits at the outlet. *Treatment* is a dummy indicating whether the outlet belongs to the target of the lawsuit. *Event month* are dummies indicating months relative to the filing month. $\alpha_{i,s}$ are interacted lawsuit and outlet fixed effects. $\alpha_{s,j,k,t}$ are interacted lawsuit, six-digit NAICS industry, ZIP code, and month fixed effects. The sample of treated outlets comprises 25,155 outlets associated with a firm involved in a class action lawsuit. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits targeting firms headquartered outside of the U.S., lawsuits filed before March 1, 2018, lawsuits filed after February 29, 2020, and lawsuits for which we cannot match outlets in SafeGraph. We also drop outlets for which we cannot match control outlets. For each treated outlet, we retain thirteen monthly observations around the filing month of a lawsuit, five months before and seven months after. The control sample is constructed by selecting all monthly observations of outlets in the same ZIP code and six-digit NAICS industry as the treated outlets. Standard errors are clustered at the firm-month level.

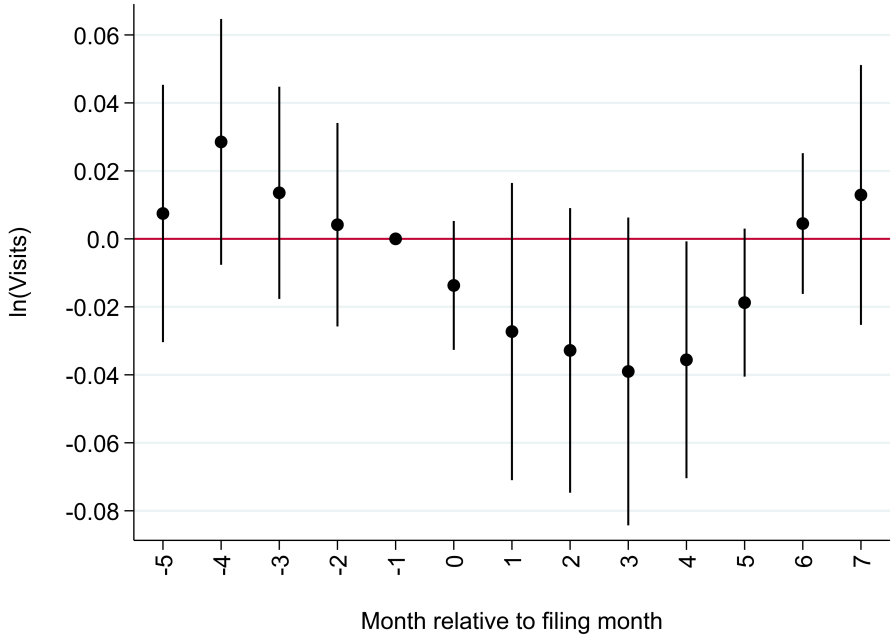
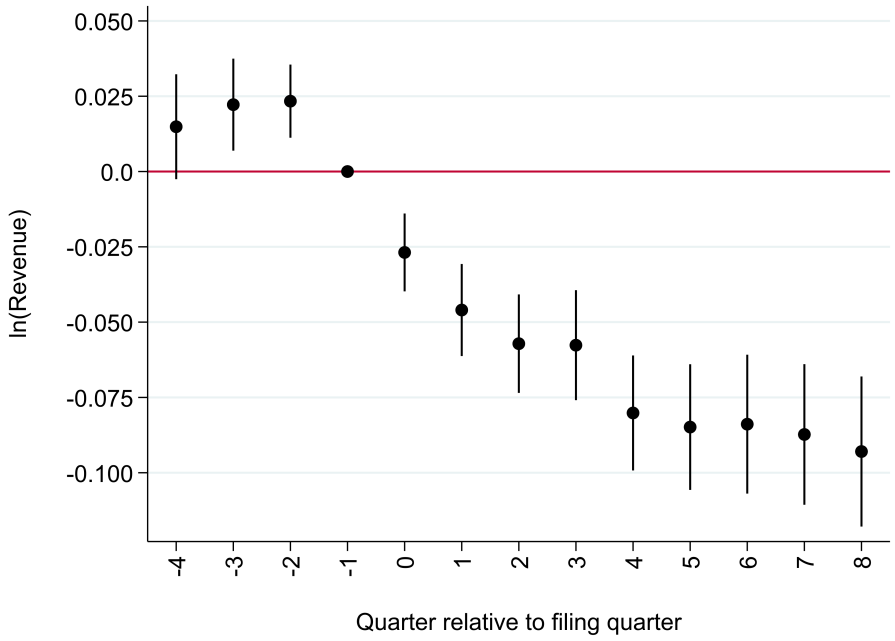


Figure IA.7: Longer-term outcomes – quarterly revenues

This figure plots 90% confidence intervals and estimates for coefficients of the following regression:

$$ln(Revenue)_{i,s,t} = \beta_t \times Treatment_{i,s} \times Event\ quarter_{i,t} + \alpha_{i,s} + \alpha_{s,t,j} + \epsilon_{i,s,t},$$

where i indexes a firm, s a lawsuit, t a quarter, and j industries. *Revenue* is quarterly revenue. *Treatment* is a dummy indicating whether the firm is the target of the lawsuit. *Event quarter* indicate quarters relative to the filing quarter. $\alpha_{i,s}$ are interacted lawsuit and firm fixed effects. $\alpha_{s,t,j}$ are interacted lawsuit, six-digit NAICS industry, and month fixed effects. The treatment sample comprises firms targeted by 3,438 class action lawsuits. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits involving firms headquartered outside the U.S., lawsuits whose outcome are unknown ("ongoing"), and lawsuits that are remanded to another court. We also drop firms for which we cannot match control firms. For each treated firm, we retain thirteen quarterly observations around the filing month of a lawsuit, four months before and eight months after. The control sample is constructed by selecting all quarterly observations of firms in the same six-digit NAICS industry as the treated firms. Detailed descriptions of all variables used throughout the study are provided in the Appendix. Standard errors are clustered at the class action lawsuit-firm level. Detailed descriptions of all variables used throughout the study are provided in the Appendix.



IA.V.E Validation of visits as a proxy for revenue

Table IA.7
Quarterly revenues and quarterly outlet visits

This table presents results of ordinary least squares regressions using the logarithm of quarterly revenues as the dependent variable. *Visits* (*Visitors*) is the sum of the number of visits (visitors) across all stores per financial quarter. The sample comprises all nine financial quarters covered by the outlet visitor data and all firms in the outlet sample. The sample of treated outlets comprises 25,155 outlets associated with a firm involved in a class action lawsuit. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits targeting firms headquartered outside of the U.S., lawsuits filed before March 1, 2018, lawsuits filed after February 29, 2020, and lawsuits for which we cannot match outlets in SafeGraph. Standard errors, reported in parentheses, are clustered at the firm level. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

	ln(Revenue)	
	(1)	(2)
ln(Visits)	0.5848***	
	(0.1667)	
ln(Visitors)		0.6302***
		(0.1452)
Firm FE	Yes	Yes
N	223	223
R^2	0.997	0.998

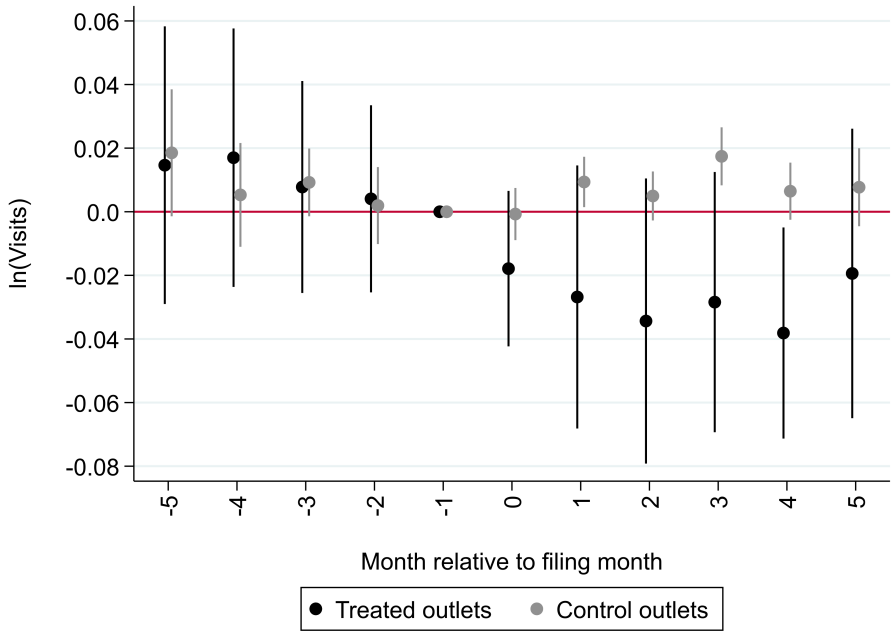
IA.VI Analysis of treatment vs control outlets

Figure IA.8: Retail customer response to class action lawsuits, treatment vs control outlets

This figure plots 90% confidence intervals and estimates from coefficients of the following regression:

$$ln(Visits)_{i,s,t} = \beta_t \times Event\ month_{i,t} + \alpha_{i,s} + \alpha_t + \epsilon_{i,s,t},$$

where i indexes an outlet, s a lawsuit, and t a month. *Visits* is the monthly number of visits at the outlet level. *Event month* are dummies indicating months relative to the filing month. $\alpha_{i,s}$ are interacted lawsuit and outlet fixed effects. α_t are calendar month fixed effects. The sample of treated outlets comprises 25,155 outlets associated with a firm involved in a class action lawsuit. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits targeting firms headquartered outside of the U.S., lawsuits filed before March 1, 2018, lawsuits filed after February 29, 2020, and lawsuits for which we cannot match outlets in SafeGraph. We also drop outlets for which we cannot match control outlets. For each treated outlet, we retain eleven monthly observations around the filing month of a lawsuit, five months before and five months after. The control sample is constructed by selecting all monthly observations of outlets in the same ZIP code and six-digit NAICS industry as the treated outlets. Detailed descriptions of all variables used throughout the study are provided in the Appendix. Standard errors are clustered at the firm-month level.



IA.VII Outlet closures

Table IA.8
Outlet closures

This table presents results of ordinary least squares regressions using a dummy variable indicating outlet closure as the dependent variable. *Treatment* is a dummy variable set equal to one if an outlet belongs to a firm that is the target in a class action lawsuit, zero otherwise. *Post* is a dummy variable set equal to one for the month of the filing of the class action lawsuit and all months thereafter. The sample of treated outlets comprises 25,155 outlets associated with a firm involved in a class action lawsuit. From the initial sample of lawsuits obtained from the Securities Class Action Clearinghouse database, we drop lawsuits involving financial firms (two-digit NAICS industry 52), lawsuits targeting firms headquartered outside of the U.S., lawsuits filed before March 1, 2018, lawsuits filed after February 29, 2020, and lawsuits for which we cannot match outlets in SafeGraph. For each treated outlet, we retain eleven monthly observations around the filing month of a lawsuit, five months before and five months after. The control sample is constructed by selecting all monthly observations of outlets in the same ZIP code and six-digit NAICS industry as the treated outlets. Detailed descriptions of all variables used throughout the study are provided in the Appendix. Standard errors, reported in parentheses, are clustered at the firm-month level. ***, **, * denote statistical significance at the 1%, 5%, and 10% level, respectively.

	Closed × 100					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Post	0.1178***	0.0766***	-0.0182	0.0302**	-0.0367*	0.0121
	(0.0312)	(0.0274)	(0.0120)	(0.0139)	(0.0198)	(0.0133)
Post	0.0607***	-0.0170*				
	(0.0071)	(0.0103)				
Treatment × Post × HHI ZIP NAICS6				-0.2105***		
				(0.0605)		
Treatment × Post × # other brands					0.0024**	
					(0.0011)	
Treatment × Post × One competing brand						-0.1132***
						(0.0296)
Lawsuit × Outlet FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	No	Yes	No	No	No	No
Lawsuit × ZIP × NAICS × Month FE	No	No	Yes	Yes	Yes	Yes
N	2,099,633	2,099,633	2,099,633	2,099,633	2,099,633	2,099,633
R^2	0.686	0.686	0.782	0.782	0.782	0.782

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