

# Retail Customer Reactions to Private Equity Acquisitions

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January 2021

## Abstract

We study short-term changes in customer visits to retail outlets around the announcement of private equity (PE) acquisitions, using aggregated and anonymized mobile phone data covering approximately ten percent of all mobile devices in the United States. Given the monthly frequency of the data, we can separate the announcement reaction of customers from the effect on operational improvements, which can only take place after the completion of the acquisition. There is a significant reduction in customer visits in retail outlets immediately following a PE acquisition. The decrease in visits is larger for larger PE firms and those previously involved in more lawsuits. The customer reaction also varies depending on income level, stock market participation, and self-employment rate, as well as religious and political orientation of the outlet location. It also depends on local competition.

*JEL classification:* D12, G14, G24, G34

*Keywords:* private equity, buyout, customer reaction, reputation

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# 1 Introduction

A large number of studies suggest that private equity (PE) ownership tends to have a positive effect on firms' operations (e.g. Acharya, Gottschalg, Hahn, and Kehoe, 2013; Boucly, Sraer, and Thesmar, 2011). Despite this, PE investors often spark negative public commentary and press coverage, as well as frequent attacks by politicians. In January 2019, Elizabeth Warren, a U.S. senator, published a piece of draft legislation with the title "Stop Wall Street Looting Act", aimed at PE funds.<sup>1</sup> The chairman of Germany's center-left Social Democratic Party, Franz Müntefering, spoke of irresponsible swarms of locusts, which devoured everything in their path, leaving companies to go broke during the Germany's federal election campaign of 2005. The criticism is not confined to the political left. In March 2019, the Republican Senator Marco Rubio released a report titled "American Investment in the 21st Century", attacking the control of the economy by financial investors.<sup>2</sup>

Retail PE buyouts have attracted public scrutiny recently after bankruptcies of several large PE-backed retail chains, such as Toys "R" Us, Payless ShoeSource, or Sports Authority. Business Insider wrote a report titled "The retail apocalypse is being fueled by private equity firms adding to debt loads"<sup>3</sup>. Bloomberg published an article "Americas Retail Apocalypse Is Really Just Beginning"<sup>4</sup>. In an article "Private equity's role in retail has killed 1.3 million jobs, study says,"<sup>5</sup> The Washington Post refers to a study "Pirate Equity: How Wall Street Firms are Pillaging American Retail," which was written by several initiatives, such as Private Equity Stakeholder Project, The Center for Popular Democracy, United for Respect, Americans for Financial Reform, and Hedge Clippers<sup>6</sup>. This study claims that, between

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<sup>1</sup>See, e.g., Financial Times: *Elizabeth Warren is right to worry about private equity looting* (<https://www.ft.com/content/f2bc6d4e-afbd-11e9-8030-530adfa879c2>), July 28, 2019.

<sup>2</sup>See Marco Rubio's webpage (<https://www.rubio.senate.gov/public/index.cfm/2019/5/rubio-releases-report-on-domestic-investment>), May 15, 2019.

<sup>3</sup>See Business Insider (<https://www.businessinsider.com/brick-and-mortar-retail-private-equity-debt-financing-lbo-2017-8?international=true&r=US&IR=T>), August 2, 2017

<sup>4</sup>see Bloomberg (<https://www.bloomberg.com/graphics/2017-retail-debt/>), November 8, 2017

<sup>5</sup>see Washington Post (<https://www.washingtonpost.com/business/2019/07/24/private-equitys-role-retail-has-decimated-million-jobs-study-says/>), July 24, 2019

<sup>6</sup>see Private Equity Stakeholder project's website (<https://pestakeholder.org/report/pirate-equity-how-wall-street-firms-are-pillaging-american-retail/>, July 25, 2019)

2012 and March 2019, 10 out of the 14 largest retail chain bankruptcies were at PE-acquired chains. The study further argues that over the past ten years, almost 600 thousand people working at retail companies under PE ownership have lost their jobs. Furthermore, additional 728 thousand jobs were destroyed in other companies (mainly suppliers) due to retail chain bankruptcies and store closures. These developments caused also reductions in the local tax base of municipalities and states.

While most academic research does not support such drastic negative connotations, it seems plausible that they may affect (or simply reflect) the public opinion on PE investors as reckless investors that make gains from buying and selling (or even destroying) struggling companies at the expense of other stakeholders, primarily the employees. Hence, an announcement that a ‘pirate’ fund or a ‘locust’ is acquiring a retail business may not be a neutral event for the customers of the target firm, which may translate their supposedly negative perceptions into their shopping behavior.

In this study, we examine the impact of PE buyouts on customer visits to retail outlets. If customers have a negative view on private equity firms, they may vote with their feet and reduce their visits to the outlets when they hear that a PE firm is acquiring this outlet. Studies using low-frequency data cannot distinguish such reputational effect from the impact of operational control (such as advertising, refurbishing of outlets, improvements in cleanliness, etc.). We address this measurement challenge by studying changes in monthly customer visits to retail outlets around the buyout announcement, using aggregated and anonymized mobile phone data from SafeGraph. These data cover approximately 10% of all mobile devices in the United States. For the purposes of our analysis, we collect data on announcements of retail acquisitions between April 1, 2018 and December 31, 2019 from Preqin, which we match to our SafeGraph location data. We end up with 10,364 outlets that belong to target buyouts, which we compare to a control group of 234,642 outlets from the same zip code and NAICS industry code. This allows us to observe short-term changes in customer visits following the announcement.

We find that announcements of buyouts are, on average, associated with a six percent reduction in the number of customer visits to target outlets in the four months following the announcement, relative to other similar outlets in the same zip code. This effect is both statistically significant and economically large. We also observe a similar drop in the number of unique visitors. It appears that the initial decrease in visits gets reversed in the following months, eventually surpassing control firms by month seven after the announcement.

Next, we examine the role of PE firm reputation in customer reactions. To the extent there is a reputational effect from the acquisition announcement, this is likely to be more negative if the PE firm has a worse reputation. We measure PE firm reputation by the intensity of lawsuits in the period five or ten years prior to the deal. Similarly, the size of the PE firms might matter for how negatively they are viewed by the customers. We find that buyouts by larger PE firms and ones with more involvement in lawsuits in the years preceding the deal experience larger reductions in customer visits. This is consistent with the effect being at least partly driven by the negative reputation of the acquirer.

We then study how the reputational effect varies with the demographic characteristics of the customer base. It seems possible that views on private equity firms might be more positive among wealthier people, or those active in financial markets or engaging in entrepreneurial activity. To test these predictions, we construct zip-code-level proxies of household income, stock market participation, and self-employment rate, using SOI tax data. We find that the reaction to PE acquisitions is significantly less negative in areas with higher average income levels, higher stock market participation, and higher self-employment rate. We also study the extent to which customer reactions differ along political and religious lines. We find that outlets in Republican counties experience more negative reactions. The same is true of more protestant counties, while more catholic counties exhibit less negative reactions.

Finally, similar to von Meyerinck, Pursiainen, and Schmid (2021), we study the role of competition. If many similar outlets are located nearby, customers' switching costs are likely to be lower. Hence, they might be more likely to switch away from PE-acquired outlets if they

disapprove of the acquisition. We find that more competition, as measured by the number and density of competitor outlets as well as lower distance to the nearest competitor, are associated with a larger reduction in visits. Similarly, a higher market share by the target within the zipcode is associated with less negative reaction.

This project contributes to various strands of literature. We provide novel insights on customer perception of PE acquisitions and its impact on firm performance. Existing studies suggest that buyouts and venture capital investments generally lead to improvements in operational efficiency, as measured by total factor productivity (Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda, 2014; Chemmanur, Krishnan, and Nandy, 2011), profitability and revenue growth (Acharya et al., 2013; Boucly et al., 2011), innovative activity (Bertoni and Tykvová, 2015; Lerner, Sorensen, and Strömberg, 2011), health inspection records (Bernstein and Sheen, 2016), or toxic pollution (Bellon, 2020). All these studies focus on what happens after the PE buyer gains control of the target. None of them observes the short-term effect of the acquisition announcement as we do. Another difference between these studies and our study is that these studies have focused on what the buyout target does, while we focus on what its customers do. Hence, we add a unique new perspective to this literature. In perhaps the closest study to ours to date, Fracassi, Previtro, and Sheen (2020) use scanner data on sales of consumer products to show that buyouts of consumer product manufacturers are followed by significant sales growth relative to peers, and that this is driven by the launch of new products and geographic expansion. However, their study focuses only on post-completion operational effects. We show that the identity of the acquirer can have an effect on the target performance immediately after the acquisition announcement, and thus even absent any operational changes. As such, their analysis can be viewed as complementary to our study.

We also add to our knowledge about the role of reputation for companies. Apart from studies that consider this topic in general (Knittel and Stango, 2014; Chaney and Philipich, 2002; Nelson, Price, and Rountree, 2008), a few papers focus on PE reputation. Chem-

manur et al. (2011) shows that companies backed by highly reputable venture capitalists are associated with greater TFP increases than companies backed by less reputable venture capitalists. Demiroglu and James (2010); Huang, Ritter, and Zhang (2016) find evidence that PE reputation matters for LBO debt financing conditions. Nahata (2008) shows that companies backed by more reputable venture capitalists achieve better exits. There is also experimental evidence suggesting that seller reputation matters for online sales (Resnick, Zeckhauser, Swanson, and Lockwood, 2006). To our knowledge, there is no existing study on how customers of buyout targets react to PE reputation.

Moreover, the study generates new findings on the link between customer reactions on corporate events and demographic characteristics. Several studies link religiosity and financial outcomes. Overall, they support the view that religiosity in general is positively related to risk aversion (e.g. Hilary and WaiHui, 2009). To the extent that PE investments may be viewed as risky, more religious people may have a more negative view of buyouts than less religious people, which may be reflected in their different reactions as customers. The reaction may differ between different religious denominations, but the direction is unclear as the literature that distinguishes between specific religious denominations and risk taking comes to different results. Barsky, Juster, Kimball, and Shapiro (1997); Stulz and Williamson (2003); Benjamin, Choi, and Fisher (2016) find that Protestants are more risk averse than Catholics, while other studies, such as Renneboog and Spaenjers (2012); Baxamusa and Jalal (2016) find the opposite. We contribute to this literature by examining the level of local religiosity in general, but also distinguishing between the Protestants and Catholics.

The results of this study also generate valuable information for private equity practitioners. There are no systematic studies of how retail customers react to PE buyouts. Our results may have implications for PE funds considering acquisitions of retail businesses, both in terms of their financial impact, as well as deriving optimal communication strategies for such deals.

## 2 Theoretical background and relevant literature

### 2.1 Private equity impact on target firms

Davis et al. (2014) find that buyouts lead to modest net job losses but large increases in gross job creation and destruction. Buyouts also bring TFP gains at target firms, mainly through accelerated exit of less productive establishments and greater entry of highly productive ones. Davis, Haltiwanger, Handley, Lipsius, Lerner, and Miranda (2019) find that the employment effects differ between public and private target firms, with employment reductions in public targets and increases in private targets. They find significant increases in labor productivity following buyouts. Similarly, Chemmanur et al. (2011) use Longitudinal Research Database (LRD) of the U.S. Census Bureau to show that the overall efficiency of venture-capital-backed firms is higher than that of non-venture-backed firms.

Acharya et al. (2013) use deal-level data and find that the abnormal performance of deals is positive on average, after controlling for leverage and sector returns. Higher abnormal performance is related to improvement in sales and operating margin during the private phase, relative to that for quoted peers. Boucly et al. (2011) find that following a leveraged buyout, targets become more profitable, grow much faster than their peer group, issue additional debt, and increase capital expenditures. Bertoni and Tykvová (2015) find that venture capital investors can help boost innovative activity by target companies. Lerner et al. (2011) find similar results for leveraged buyouts.

Recent studies suggest that the effects of buyouts can differ substantially by industry. Eaton, Howell, and Yannelis (2020) show that in higher education, buyouts of schools lead to higher tuition and per-student debt. They also find that private equity-owned schools better capture government aid. After buyouts, education inputs, graduation rates, loan repayment rates, and earnings among graduates decrease. Bernstein and Sheen (2016) measure operational performance in restaurant chain buyouts using health inspection records. They find that store-level operational practices improve after private equity buyout, as restaurants

become cleaner, safer, and better maintained. This effect is stronger in chain-owned stores than in franchised locations. (Bellon, 2020) finds that private equity ownership leads to a 70% reduction in the baseline rate of toxic pollution in the oil and gas industry.

Gandhi, Song, and Upadrashta (2020) study PE acquisitions of US nursing homes and find that their effects differ depending on the competitive environment. In highly competitive markets, PE owners increase staffing by \$72,501 worth of care annually, while in less competitive markets they reduce staffing by an average of \$18,604. They further show that PE-owned nursing homes respond more strongly to policies intended to spur competition. Gupta, Howell, Yannelis, and Gupta (2020) find evidence of declines in patient health and compliance with care standards following PE acquisitions of nursing homes. These declines appear to reflect cuts to front-line nursing staff, one component of efficiency improvements that also include higher bed utilization.

In perhaps the closest study to ours to date, Fracassi et al. (2020) use scanner data on sales of consumer products to show that private equity buyouts of consumer product manufacturers are followed by significant sales growth relative to peers, and that this is driven by the launch of new products and geographic expansion. However, their study is done at the firm-level and focuses only on post-completion operational effects. As such, their analysis can be viewed as complementary to our study.

All these studies focus on what happens after the private equity buyers gains control of the target. None of them observes the effect of the acquisition announcement. Hence, we add a unique new perspective to this literature.

## **2.2 Reputation**

There is some evidence that the reputation of a private equity firm matters. Chemmanur et al. (2011) distinguish between high vs. low reputation venture capitalists and find that, while the TFP of firms prior to receiving financing is lower for firms backed by high-reputation venture capitalists, the increase in TFP subsequent to financing is significantly

greater for these firms, consistent with high-reputation venture capitalists having greater monitoring ability. Huang et al. (2016) argue that PE firms reputational concerns dominate their wealth expropriation incentives and help their portfolio companies reduce the cost of debt. Demiroglu and James (2010) find evidence that PE group reputation matters for obtaining higher amounts of leverage and better financing terms. Nahata (2008) finds that companies backed by more reputable venture capitalists are more likely to exit successfully, access public markets faster, and have higher asset productivity at IPOs.

More broadly, there are a handful of studies of the importance of reputation for companies. For example, Knittel and Stango (2014) find that, after the onset of the Tiger Woods scandal, the full portfolio of sponsors lost more than 2% of market value, with losses concentrated among the core three sponsors. There is also (mixed) evidence of the impact of corporate scandals on firm reputation. Chaney and Philipich (2002) find that Arthur Andersen's other clients experienced a negative stock price reaction around information of Andersen's practices related to the Enron scandal. Nelson et al. (2008) argue that this finding was driven by industry composition of Andersen's clients, and that there is little support for a reputation effect.

To our knowledge, there is no existing study of the customer reactions to PE acquisitions. The studies using measures of PE fund reputation focus on financing deals and likelihood of eventual deal success. Hence, our proposed study will make a substantial contribution to the existing literature on the effects of reputation in private equity. It will also add to the broader literature on the role of reputation in firm performance.

## **3 Data and methodology**

### **3.1 Data on customer visits**

To measure customer visits to retail outlets, we use aggregated mobile phone data from SafeGraph, a company producing anonymized mobile phone location statistics. The data

include monthly number of visits by individual visitors at each outlet. SafeGraph observes 18.75 million devices, approximately 5.6% of the U.S. population and about 10% of mobile devices. According to SafeGraph’s analysis of user characteristics, SafeGraph posits that its sample is representative of the U.S. population based on its own study of income characteristics, age, and demographics of its users. The data are widely used in studies of social distancing during the COVID-19 pandemic, and more recently also increasingly to measure consumer responses to firm actions (see, e.g., Painter, 2020; Gurun, Nickerson, and Solomon, 2020). SafeGraph data are available on a monthly basis from January 2018 onwards.

### **3.2 Buyout sample and control group**

We extract a sample of all buyout acquisitions in retail from Preqin. To have enough data for comparison before and after the buyout, we limit our sample to deals announced between April 1, 2018, and December 31, 2019. The starting date is constrained because of the availability of SafeGraph data. The ending date is limited because from March 2020 on, visits to retail outlets have been heavily affected by the COVID-19 pandemic. To combine the Preqin buyout targets to SafeGraph location data, we perform a name-based matching. This leaves us with a sample of 82 private equity acquisitions where we have the matching customer visit data. The target firms in these deals have a total of 10,364 US outlets in the data, with the number of outlets per firm ranging from one to 3,527.

To obtain a control group for comparison, we retain all outlets that are located in the same zip code and same 6-digit NAICS industry code as the target of any of the sample deals. This leaves us with a total sample of 234,642 outlets. Our sample consists of more than 1.4 million outlet-month observations, of which 5.6% are attributable to buyout targets, and the remainder to control firms. Table 1 shows summary statistics for our sample.

### 3.3 Methodology

To examine customer reactions around private equity deal announcements, we compare the number of customer visits for PE-backed outlets before and after the announcement of buyout deals to comparable non-PE-backed outlets. We perform a difference-in-differences regression analysis of the following form:

$$\ln(Visits)_{i,t} = \beta_1 \times Target_{i,j} \times Post_{j,t} + \beta_2 \times X_{i,j,t} + \epsilon_{i,t} \quad (1)$$

where  $i$  denotes an outlet,  $j$  denotes a buyout, and  $t$  denotes the calendar month.  $Visits$  is the monthly number of visits in outlet  $i$ .  $Target$  is a dummy taking the value one if the firm is a buyout target.  $Post$  is a binary variable that equals one for all months following the buyout announcement and zero for all months preceding the buyout announcement.  $X$  is a vector of controls that includes outlet fixed effects, controlling for cross-sectional differences between different outlets, and zip code x NAICS x month fixed effects, controlling for any location related trends over time.

## 4 Customer visits around the PE acquisition announcement

### 4.1 Baseline results

While in our regressions we estimate the average change between the period prior to the buyout and following the buyout and between the target and control firms, Figure 1 plots the monthly coefficients for target firms, relative to acquisition date. Immediately following the deal announcement, there is a clear reduction in customer visits by 5 to 7 percent, possibly driven by the reputational effect of the announcement. Customer visits bottom out four months after the announcement, which is likely to be close to the typical completion

date of a buyout deal, and increase significantly following that.

The results of our baseline regressions are shown in Table 2, Panel A. We run four specifications that differ in the fixed effects included. Our variable of interest is the double interaction term. It is highly statistically significant and negative in all four specifications, suggesting that there is about 6% reduction in customer visits in the 4 months following the announcement, relative to other outlets in the same NAICS industry and zipcode. The magnitude of the effect is similar across all four specifications. In further analysis, we rely on the last specification that includes outlet fixed effects and zip code x NAICS x month fixed effects.

Panel B of Table 2 repeats the regressions from Panel A with an alternative dependent variable. Instead of counting the number of visits, we focus on the number of individual visitors. The results do not change much. All of them are highly statistically significant and negative and of a similar magnitude as in Panel A.

## **4.2 Effect of PE firm size and reputation**

In the next step, we want to understand whether and how the negative effect varies with PE size and reputation. We expect that the magnitude of the customer reaction will vary depending on PE characteristics and be stronger if the public perception of the PE firm is more “negative”. This will be the case for large PE firms and for PE firms that were involved in numerous lawsuits in the past. To measure the PE firm size, we rely on the amount of funds raised in the past 10 years, which we obtain from Preqin. To account for the PE firm involvement in lawsuits, we rely on the number of lawsuits which we get from the Westlaw’s database. As ? point out, this database provides two main advantages over other databases. Compared to Lexis, which contains only judicially resolved cases, Westlaw’s database covers unresolved cases too (for example cases that were voluntarily dismissed or settled). Compared to PACER, which focuses of federal cases, Westlaw’s database contains cases from various court levels. We create three alternative variables to account for the

involvement in lawsuits. Our first two variables count the number of all lawsuits in the period 5 years, resp. 10 years prior to the deal announcement, in which the PE firm was involved as a defendant. Our third variable considers only those lawsuits that were filed in the state of the buyout target. We argue that those should be the most relevant to the local customers. The shortcoming of the third variable is its low variation as the majority of the PE firms has not been involved in any lawsuits in the particular state within the period of ten years prior to the buyout announcement. We therefore use this metric only in the 10-year horizon and not in the 5-year horizon.

To capture the effect of PE size and reputation, we apply a triple interaction term. Table 3 exhibits the results. The first column suggests that, in line with our expectations, customer visits decrease more for outlets that are subject to buyouts from larger PE firms. The number of lawsuits in past five years in column 2 is negatively associated with the customer visits. Both effects hold when we include the size and the reputation variables jointly in column 3. All triple interaction effects are highly statistically significant. Columns 4 and 5 repeat the specifications from columns 2 and 3, but use lawsuits in the past ten years instead of five years. The coefficients are again negative and statistically significant. Finally, we focus on local lawsuits in columns 6 and 7 and find, again, a negative coefficient, which, however, when included jointly with the PE size, is only marginally significant.

The results from this section support the view that customers reduce their visits to retail outlets after a buyout more if the PE firm is larger and if it has been involved as a defendant in a higher number of lawsuits.

### **4.3 Local characteristics**

We are first interested whether customers in relatively poorer areas are more concerned about PE buyouts of local retail outlets than customers in relatively richer areas. If so, the drop in the outlet visits after the buyout announcements should be greater in regions with lower wealth. We rely on data from Census Bureau and consider four different proxies

to capture local wealth: income per capita, average household income, the level of stock participation, and self-employment. We apply again a difference-in-differences analysis with triple interaction terms and show the results in Table 4. In the first four specifications, we include the wealth-related variables one after another. All four triple interaction terms are highly statistically significant and positive, suggesting that higher wealth levels mitigate the negative customer reaction after a buyout announcement. When we include all four variables jointly in column 5, the household income turns insignificant, but the other three variables stay highly statistically significant and positive.

As next, we examine the link between customer reaction to buyout announcements and the local political orientation and the local level of religiosity. Table 5 reveals, in the first column, that customers in Republican-dominated regions tend to decrease their visits more than customers in Democratic-oriented regions, suggesting, surprisingly, that customers in Republican-dominated regions have a greater aversion towards private equity deals. Religiosity per se does not have any effect (see column 2), but when we distinguish between the two prevailing denominations (Protestant and Catholic), we observe significant differences. While in Protestant-dominated areas customers reduce their visits to the buyout-owned outlets after the deal announcement more strongly than in other areas (column 3), the opposite holds for the Catholic-dominated areas (column 4). When we consider these variables together with the political orientation in column 5, we find that the political orientation dominates and the religiosity variables lose their significance. Finally, we show that the negative effect of Republican-leaning is not the result of differences in local economic developments. It persists and remains highly statistically significant even when we add the local level of GDP per capita in column 6.

Finally, we consider how customer behavior is linked to the local competition among relevant outlets. Our prior is that customers will reduce their visits to PE-owned outlets more strongly, if similar outlets are located nearby and if the target outlet has a weak position in the local market. In this case, customers can easily switch to other outlets. To measure

local competition, we rely on four alternative variables. The first variable counts the number of competing outlets from the same industry that are located in the same zip code. Besides the absolute number of outlets in the industry and area, we employ a relative measure as our second variable, namely the number of outlets over the local population. Third, we consider the individual market share of the target outlet. Our fourth and last variable in this group is the distance to the next outlet from the same industry. The results in Table 6, in which we include our variables one after another, support our prior. The coefficients on all triple interaction terms have the expected signs and all of them are highly statistically significant. The results in columns 1 and 2 suggest that customers reduce their visits more if more similar outlets are located in the same area. Column 3 shows that outlets with a stronger position in the local market have more loyal customers so that they tend to lose a lower number of visitors after a buyout announcement. Column 4 reveals that the number of switching customers increases as the distance to the next comparable outlet decreases.

## 5 Conclusion

While academic literature suggests that private equity buyouts are often associated with operational improvements for the target firm, the popular image of private equity is often negative. This has likely been exacerbated by some prestigious retail chains having gone bankrupt after a PE buyout, and politicians of all sides often vilifying PE investors. Our results suggest that announcements of PE acquisitions are followed by a short-term reduction in retail customer visits to the target firm's outlets. This is consistent with customers voting with their feet. It is also unlikely to be driven by operational factors, as the closing of an acquisition typically takes place only some months after the announcement, while the decrease in visits takes place immediately following the announcement.

We also find that PE firms that might be expected to have a more negative reputation among the public are also the ones experiencing more negative announcement reactions. In

contrast, outlets in areas where people might be expected to be less negative toward private equity experience less negative reactions. These findings suggest that customers' reluctance to deal with PE-owned businesses varies depending on the firm reputation as well as the customers' preferences.

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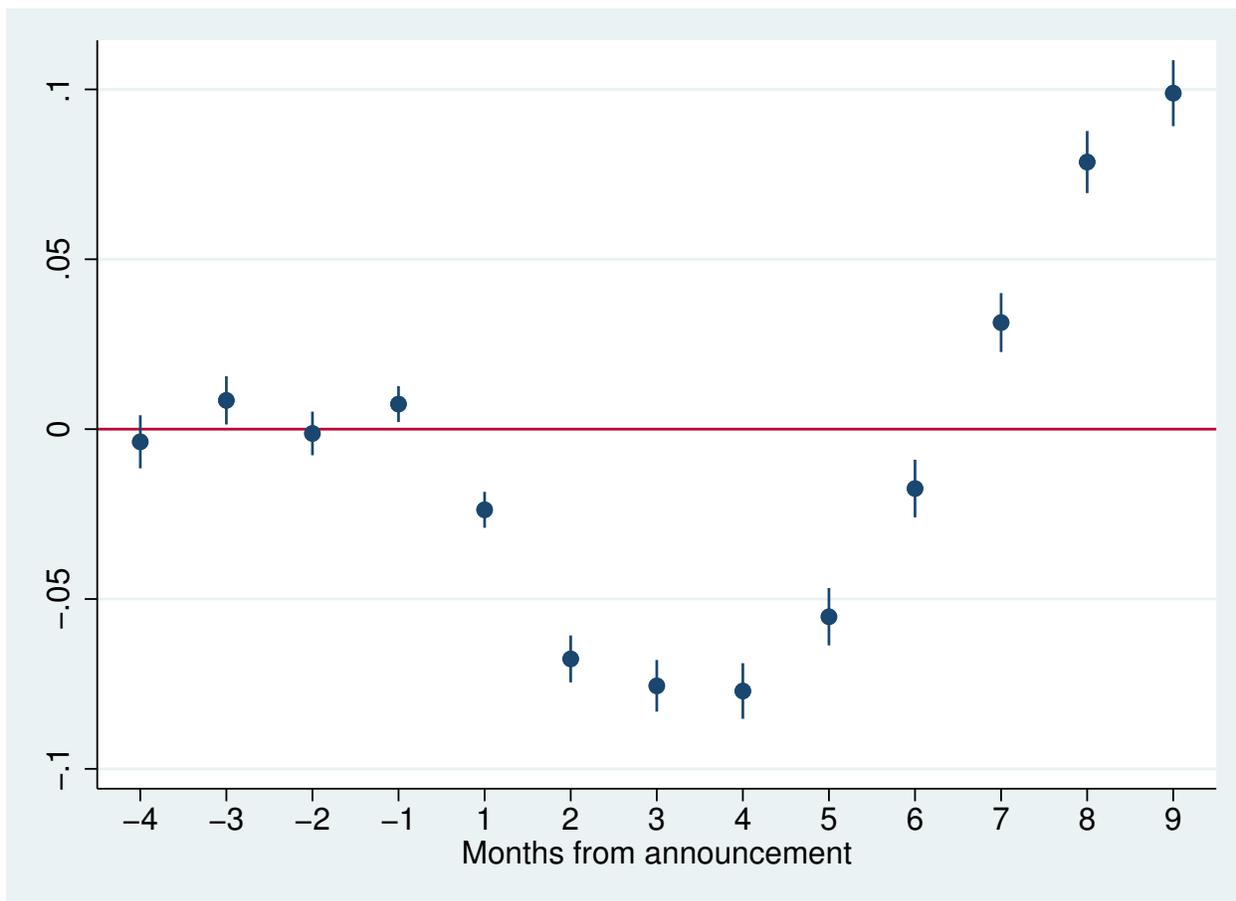
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**Figure 1: Monthly visits (month relative to acquisition announcement)**

Monthly coefficients relative to the time of acquisition announcement by a PE fund. The dependent variable is  $\ln(\text{Visits})$ , the natural logarithm of the total number of visits in the outlet during the month. Heteroscedasticity-consistent standard errors are clustered by zip code. The regression controls for store fixed effects to account for any cross-sectional differences between outlets and zip code x NAICS code x month fixed effects for monthly variation in each sector in each zip code.



**Table 1**  
**Summary statistics**

Summary statistics for the outlet-month observations in the sample. The sample includes all outlets of retail firms acquired by PE funds in the sample period, i.e. deals announced between March 2018 and December 2019, as well as all other firms operating in the same 6-digit NAICS industry in the same zip code as any of the target outlets. *Visits* is the monthly number of visits in the outlet. *Visitors* is the monthly number of unique visitors in the outlet. *Target* is a dummy taking the value one if the outlet belongs to a target firm.

	Mean	Std	p25	p50	p75
<b>Store visit</b>					
Visits	368.742	563.137	102.000	232.000	456.000
Visitors	230.261	328.821	60.000	143.000	298.000
<b>Outlet</b>					
Target	0.059	0.236	0.000	0.000	0.000
Dist. to competition	0.032	1.108	0.000	0.000	0.000
<b>Zipcode</b>					
Avg. HH income	90.873	72.658	53.139	69.877	100.276
Stock participation	0.204	0.117	0.115	0.180	0.274
Self empl. rate	0.181	0.043	0.151	0.174	0.205
Comp. outlets / population	0.002	0.020	0.001	0.001	0.001
Target market share (outlets)	0.059	0.073	0.024	0.042	0.067
<b>County</b>					
% Religious	0.497	0.118	0.404	0.486	0.569
% Protestant	0.294	0.165	0.148	0.261	0.413
% Catholic	0.156	0.119	0.062	0.132	0.219
Republican	0.450	0.498	0.000	0.000	1.000
PI per capita ('000)	56.160	20.779	43.796	51.349	62.485
<b>PE buyer</b>					
N PE buyers	1.296	0.457	1.000	1.000	2.000
Funds 10y (USDbn)	9.814	12.596	1.838	9.712	10.500
Lawsuits (5y)	4.465	3.750	2.000	4.000	7.000
Lawsuits (10y)	13.175	13.125	5.000	7.000	19.000
Lawsuits (state) (10y)	0.705	2.005	0.000	0.000	0.000
N	1,447,940				

**Table 2**  
**Customer visits following PE acquisition announcement**

The dependent variable in Panel A is  $\ln(\text{Visits})$ , the natural logarithm of the total number of visits in the outlet during the month. The dependent variable in Panel B is  $\ln(\text{Visitors})$ , the natural logarithm of the total number of unique visitors in the outlet during the month. Heteroscedasticity-consistent standard errors, clustered by zip code, are shown in parentheses.

**Panel A:  $\ln(\text{Visits})$**

	(1)	(2)	(3)	(4)
Post x Target	-0.0658*** (0.0043)	-0.0530*** (0.0035)	-0.0627*** (0.0034)	-0.0630*** (0.0034)
Post	0.0828*** (0.0021)	0.0009 (0.0020)		
Target	0.5842*** (0.0123)			
Deal x Outlet FE	No	Yes	Yes	Yes
Month FE	No	Yes	No	No
NAICS FE	No	Yes	No	No
Zip code FE	No	Yes	No	No
Deal x NAICS x Month FE	No	No	Yes	No
Deal x Zip code x Month FE	No	No	Yes	No
Deal x Zip x NAICS x Month FE	No	No	No	Yes
N	1,447,940	1,447,479	1,444,782	1,444,197
$R^2$	0.011	0.960	0.965	0.965

**Panel B:  $\ln(\text{Visitors})$**

	(1)	(2)	(3)	(4)
Post x Target	-0.0615*** (0.0040)	-0.0514*** (0.0033)	-0.0607*** (0.0030)	-0.0609*** (0.0030)
Post	0.0685*** (0.0021)	0.0020 (0.0019)		
Target	0.5905*** (0.0119)			
Deal x Outlet FE	No	Yes	Yes	Yes
Month FE	No	Yes	No	No
NAICS FE	No	Yes	No	No
Zip code FE	No	Yes	No	No
Deal x NAICS x Month FE	No	No	Yes	No
Deal x Zip code x Month FE	No	No	Yes	No
Deal x Zip x NAICS x Month FE	No	No	No	Yes
N	1,447,940	1,447,479	1,444,782	1,444,197
$R^2$	0.012	0.967	0.972	0.972

**Table 3**  
**PE firm size and involvement in lawsuits**

The dependent variable is  $\ln(Visits)$ , the natural logarithm of the total number of visits in the outlet during the month. Heteroscedasticity-consistent standard errors, clustered by zip code, are shown in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post x Target	0.2186*** (0.0226)	-0.0169** (0.0076)	0.2152*** (0.0226)	0.0770*** (0.0101)	0.1314*** (0.0233)	-0.0560*** (0.0038)	0.2122*** (0.0227)
Post x Target x $\ln(\text{Funds } 10\text{y})$	-0.0326*** (0.0026)		-0.0291*** (0.0026)		-0.0056* (0.0032)		-0.0315*** (0.0026)
Post x Target x $\ln(\text{Lawsuits } 5\text{y})$		-0.0327*** (0.0046)	-0.0186*** (0.0047)				
Post x Target x $\ln(\text{Lawsuits } 10\text{y})$				-0.0600*** (0.0039)	-0.0614*** (0.0049)		
Post x Target x $\ln(\text{Lawsuits } 10\text{y (state)})$						-0.0250*** (0.0055)	-0.0106* (0.0056)
Deal x Outlet FE	Yes						
Deal x Zip x NAICS x Month FE	Yes						
N	1,407,749	1,444,197	1,407,749	1,444,197	1,407,749	1,444,197	1,407,749
$R^2$	0.965	0.965	0.965	0.965	0.965	0.965	0.965

**Table 4**  
**Local wealth**

The dependent variable is  $\ln(Visits)$ , the natural logarithm of the total number of visits in the outlet during the month. Heteroscedasticity-consistent standard errors, clustered by zip code, are shown in parentheses.

	(1)	(2)	(3)	(4)	(5)
Post x Target	-1.3360*** (0.1256)	-0.3322*** (0.0270)	-0.1155*** (0.0064)	-0.1438*** (0.0140)	-0.8948*** (0.1553)
Post x Target x $\ln(\text{PI pc.})$	0.1172*** (0.0116)				0.0715*** (0.0148)
Post x Target x $\ln(\text{HH income})$		0.0629*** (0.0062)			-0.0051 (0.0169)
Post x Target x Stock p.			0.2726*** (0.0278)		0.1671** (0.0734)
Post x Target x Self emp.				0.4564*** (0.0762)	0.2491*** (0.0822)
Deal x Outlet FE	Yes	Yes	Yes	Yes	Yes
Deal x Zip x NAICS x Month FE	Yes	Yes	Yes	Yes	Yes
N	1,421,998	1,440,204	1,440,204	1,440,204	1,420,528
$R^2$	0.965	0.965	0.965	0.965	0.965

**Table 5**  
**Local political orientation and religiosity**

The dependent variable is  $\ln(\text{Visits})$ , the natural logarithm of the total number of visits in the outlet during the month. Heteroscedasticity-consistent standard errors, clustered by zip code, are shown in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
Post x Target	-0.0315*** (0.0049)	-0.0431*** (0.0151)	-0.0328*** (0.0072)	-0.0870*** (0.0054)	-0.0432*** (0.0134)	-1.0123*** (0.1499)
Post x Target x Republican	-0.0610*** (0.0067)				-0.0543*** (0.0078)	-0.0398*** (0.0081)
Post x Target x Religious		-0.0398 (0.0285)				
Post x Target x Protestant			-0.0981*** (0.0199)		-0.0031 (0.0268)	0.0205 (0.0275)
Post x Target x Catholic				0.1533*** (0.0277)	0.0580 (0.0372)	0.0136 (0.0382)
Post x Target x $\ln(\text{PI pc.})$						0.0885*** (0.0136)
Deal x Outlet FE	Yes	Yes	Yes	Yes	Yes	Yes
Deal x Zip x NAICS x Month FE	Yes	Yes	Yes	Yes	Yes	Yes
N	1,444,197	1,441,674	1,441,674	1,441,674	1,441,674	1,421,998
$R^2$	0.965	0.965	0.965	0.965	0.965	0.965

**Table 6**  
**Local competition**

The dependent variable is  $\ln(Visits)$ , the natural logarithm of the total number of visits in the outlet during the month. Heteroscedasticity-consistent standard errors, clustered by zip code, are shown in parentheses.

	(1)	(2)	(3)	(4)
Post x Target	-0.0204* (0.0113)	-0.0631*** (0.0034)	-0.0799*** (0.0047)	-0.0788*** (0.0043)
Post x Target x $\ln(1+\text{Comp. outlets})$	-0.0153*** (0.0037)			
Post x Target x Comp. outlets/pop.		-0.0836*** (0.0117)		
Post x Target x Share outlets			0.1627*** (0.0345)	
Post x Target x $\ln(1+\text{Dist. to comp.})$				0.0546*** (0.0102)
Deal x Outlet FE	Yes	Yes	Yes	Yes
Deal x Zip x NAICS x Month FE	Yes	Yes	Yes	Yes
N	1,444,197	1,441,567	1,444,197	1,444,181
$R^2$	0.965	0.965	0.965	0.965