OUT OF SIGHT, OUT OF MIND: LOCAL STORES AND RETAIL DAY-TRADING*

Fernando Chague†

Bruno Giovannetti‡

Guilherme Paiva§

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Abstract

Salience often covary with information. Hence, empirically showing that the salience of a stock, in itself, affects retail investors trading decisions is challenging. We document that living in a small city that has a local store of a brick-and-mortar firm more than doubles the chances of individuals picking the stock of that firm to *day-trade*. This suggests a direct relation between salience and retail investors' trading decisions: a local store in a small city i) increases the visual salience of the firm for the city residents but ii) does not provide any useful information for day-trading, which depends exclusively on high-frequency indicators. We explore the granularity of our dataset to control for indirect channels that can make retail day-trading correlate with local stores.

JEL Codes: G11, G41, R11

Keywords: retail investors decisions, limited-attention, salience, day-trade

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†Sao Paulo School of Economics - FGV, Brazil. E-mail: fernando.chague@fgv.br.

‡Corresponding author. Sao Paulo School of Economics - FGV, Brazil. Rua Itapeva, 474, 10th floor, Sao Paulo-SP, Brazil. E-mail: bruno.giovannetti@fgv.br.

§Insper, Brazil. E-mail: guilhermelp@al.insper.edu.br.

1 Introduction

Retail investors have the difficult task of choosing in which stocks to invest. With limited resources to evaluate the entire universe of stocks, they tend to focus on the ones that are salient[1] to them (Barber and Odean, 2008, and Barber, Lin, and Odean, 2019). Empirically showing that the salience of a stock, *in itself*, affects retail investors trading decisions is, however, challenging. Ideally, one would need a large experiment in which the salience of a group of stocks is exogenously changed. The issue with observational data is that usual proxies of salience (e.g., abnormal volume, abnormal returns, press coverage) often covary with the arrival of new information, that can also affect trading decisions.

In this paper, we investigate the relation between salience and retail trading by exploring a measure of salience that is uninformative for the retail trading activity that we choose to focus on. We use the existence of a local store in a small city of a brick-and-mortar exchange-listed firm[2] as our measure of (visual) salience of the firm to the retail traders who live in the city. We then focus on *day-trading*[3] to ensure that our salience measure is uninformative to the investor. Day-trading is a short-lived trading strategy that lasts minutes, hours at most. As such, day-traders rely exclusively on high-frequency indicators[4] to implement their strategies. Accordingly, a local store in a small city is unable to provide any information that can be useful specifically for day-trading.

To investigate the relation between local stores in small cities and retail day-trading, we explore two rich Brazilian datasets. The first contains the addresses of all stores from 2012 to 2017 of all brick-and-mortar firms that are listed in the stock market between 2012 and

[1] According to Bordalo, Gennaioli, and Shleifer (2022), a stimulus is salient when it attracts the decision maker's attention "bottom up," automatically and involuntarily, and this can occur because of contrast with surroundings, surprise, or prominence.

[2] A street-side business that offers products and/or services to its customers face-to-face in an office or store.

[3] Day-trading is a trading strategy that involves buying and selling the same financial asset on the same day in the same quantity. According to a 2017 article in Forbes, "day-trading is the new sexy that gets an inordinate amount of hype" (https://www.forbes.com/sites/nealegodfrey/2017/07/16/day-trading-smart-or-stupid/#411e5d8a1007).

[4] For instance, intraday price variation, order sizes, and signed measures of trading flow (Bernstein, 1995).

2017. This represents 60 firms with total market capitalization of US$ 322 billion (as of January 2015). The second dataset contains all day-trades in the Brazilian equity market of all individuals from 2012 to 2017 (a total of 8,746,980 day-trades performed by 190,655 individuals) and, crucially, the cities where these individuals live.

There were 5,570 cities in Brazil in 2017. Our baseline cutoff to define a small city is having a population of less than 100 thousand individuals (5,270 cities).[5] The main reason why we focus on local stores and day-trades in *small* cities is because we do not have in our dataset the complete addresses of individuals — we only observe the cities where they live. Hence, we cannot divide larger cities into neighborhoods and relate the day-trading activity in each neighborhood with the existence of a local store close by. However, we also believe that by focusing on small cities we have a cleaner empirical exercise: people usually circulate a lot in large cities (e.g., by living far from where they work) and, hence, they can see many stores which are not close to where they live.

Our main findings are the following. First, in an analysis within firm-month (across cities), we find that the likelihood of a firm being day-traded by individuals in a given month is 2.0 percentage points higher in a small city that has a local store of that firm compared to a small city that has no local store of that firm. Second, in an analysis within city-month (across firms), we find that the likelihood of a day-trade in a small city in a given month is 1.5 percentage point higher for a firm that has a local store in the city than for a firm that has no local store in the city. Third, in an analysis within city-firm (across months), we find that the likelihood of a given firm being day-traded in some small city is 0.9 percentage point higher in the months in which there is a local store of that firm in that city compared to the months when there is no local store. These effects are economically important: the unconditional probability of occurring some retail day-trade in a month for our average brick-and-mortar stock in our average small city is below 1% during the whole period.

[5]Use different cutoffs for robustness analyses.

To estimate these effects we account for the fact that the locations of the stores across Brazilian small cities and over time are not randomly defined by the brick-and-mortar firms. For instance, a reasonable concern is that a firm is more likely to open a local store where (and when) its unobserved regional popularity is higher. This would generate a positive relation between local stores and retail day-trading if individuals are more likely to day-trade stocks that are popular to them. However, the granularity of our dataset allows us to control for indirect channels like this. The dataset we build is at the city-firm-month level and, given these three dimensions, we can explore a number of different fixed-effects in the regressions.

First, we compare the day-trading activity by individuals in a given pair firm-month across two small cities (one with a local store of the firm, the other without) that are located in the same micro-region[6] of Brazil, keeping regional popularity constant. Second, we compare the day-trading by individuals in a given pair city-month across two different firms (one with a local store in the city, the other without), controlling for firm-microregion-month fixed-effects, which capture all omitted variables that vary in the firm-microregion-month dimension — for instance, firms regional popularity. Third, we compare the day-trading by individuals within a given pair city-firm across two different months (one with a local store of the firm in the city, the other without), also controlling for firm-microregion-month fixed-effects to account for possible dynamics in the regional popularity of the firm. Additionally, we also include city-month and firm-month fixed-effects in the regressions to control for all unobservables that vary in these dimensions.

To make things more concrete, consider the following three examples, one for each of the three specifications described in the previous paragraph. Lojas Americanas is a Brazilian retail company founded in 1940. In 2017, the firm had 1,144 local stores in Brazil. Suppose a small city A that has a Lojas Americanas store in month t and another small city B, located in the same micro-region of A, that does not have a Lojas Americanas store in

[6]Brazilian micro-regions are defined by IBGE, the Brazilian Institute of Geography and Statistics. There are 558 different micro-regions in Brazil, which are narrower in the more populated areas.

month t. We test whether, in month t, there is a greater chance of a day-trade of Lojas Americanas by individuals from city A compared to by individuals from city B. To ensure that the comparison occurs only across small cities that are in the same micro-region, the regression includes firm-microregion-month fixed-effects. Furthermore, the regression also includes city-month fixed-effects to control for all possible social-economic differences across both cities that may affect both day-trading and the existence of the local store of Lojas Americanas (e.g., per capita income, population, and unobservables).

Second, considering the same small city A, suppose that while it has a Lojas Americanas store in month t, it does not have a store from another large Brazilian retail company, say, Magazine Luiza (846 stores in 2017). To compare day-trading by individuals from city A in month t across both firms, we include firm-microregion-month and city-sector fixed-effects in the regression. The firm-microregion-month fixed-effects control for all possible differences across both firms that can vary regionally and over time, and can affect both day-trading and the existence of a local store in the city — the regional popularity of the firms, for instance. The city-sector fixed-effects ensure that we are comparing only firms with similar business characteristics.

Third, consider that we are comparing across two different months the day-trading in Lojas Americanas by individuals who live in city A. In one month there is a Lojas Americanas store in city A; in the other, there is no store. To control for a possible dynamics of the popularity of Lojas Americanas in the region of city A that could affect both retail day-trading and the store existence, we employ our stock-microregion-month fixed-effects. Moreover, to control for city A dynamics and Lojas Americanas dynamics that could also affect both retail day-trading and the store existence, we employ both city-month and firm-month fixed-effects.

Importantly, we show that when the local store has a diminished impact on the salience of the firm — the name that appears on the local storefront is different from the firm name — the effects of the local store on day-trading is either significantly smaller or null. We also perform a number of robustness exercises to show that the results remain qualitatively the same under alternative definitions and samples. We use alternative definitions for day-trade and small cities, and also look at the relation between stores and day-trading in medium and large cities.

We enhance our empirical analysis by studying the case of a specific firm in our sample, Lojas Americanas. The firm opened its first store in 1929 in Rio de Janeiro and went public in 1940, having its shares traded on the Brazilian stock market since then. During our sample period, the firm put in practice an expansion plan named "85 years in 5." The number of small cities with a local store went from 52 in 2012 to 231 in 2017. We explore this expansion to perform a difference-in-differences exercise. We look at the evolution of retail day-trading in the small cities where a store was opened and in the small cities where a store was not open. First, we show that, before the event, the probability of retail day-trading, after we control for the population, income, number of stock market investors, and location of each city, is not statistically different between the two groups of cities. We then show that, in the years after the event, the probability of a day-trade in the cities where the local store is opened begins to increase, while the probability of a day-trade in the cities where the local store is not opened remains constant.

Barber and Odean (2008) suggest that the limited attention of retail investors can cause a buying pressure on a stock when it becomes salient: when deciding which stocks to buy among all existing stocks, retail investors allocate their limited attention to the stocks that are more salient. Since retail investors in general only sell what they have in their portfolios (few stocks), selling decisions are less affected by salience. This is a very relevant hypothesis. If true, under limits to arbitrage, salience could then cause stock overpricing, at least temporarily. Barber and Loeffler (1993), Liang (1999), Da, Engelberg, and Gao (2011) and Engelberg, Sasseville, and Williams (2012) provide empirical evidence relating overpricing to salience.

Exactly as what happens with buying decisions, when individuals have to decide which stocks they are going to day-trade, they also have to choose among all stocks available in the market. Accordingly, the limited attention of day-traders should also be binding, just as highlighted by Barber and Odean (2008) for buying decisions, and salience should also play an important role for day-trading: stocks that are salient should be more day-traded. By relating the existence of local stores in small cities with retail day-trading, and considering that there is no relevant information for day-traders in these local stores (we empirically show that this is indeed the case, as expected), we deepen our understanding on the effects of salience on retail investors.

On a more general level, our empirical evidence corroborates a growing theoretical literature that emphasizes the role of salience in economic choice (Bordalo, Gennaioli, and Shleifer, 2012, Bordalo, Gennaioli, and Shleifer, 2013, and Bordalo, Gennaioli, and Shleifer, 2020). According to the survey Bordalo, Gennaioli, and Shleifer (2022), when decision makers choose, their attention is allocated to the salient attributes of the choice options. Attributes of an option are differentially salient based on i) the contrast with the attributes of the other options, ii) the surprise compared to their usual values, and iii) the prominence with which they are displayed or retrieved. In our case, individuals are choosing among stocks to be day-traded — and not among goods to be purchased, as in their models — but our salience measure for the stocks, the existence of a local store in a small city, is clearly related to prominence. Accordingly, our evidence is consistent with the general theoretical prediction that salience should affect individuals' choices.

More directly, we contribute to the empirical literature that studies the effects of salience on retail investors. Barber and Odean (2008) show that retail investors are net buyers of stocks that present high levels for variables related to salience (trading volume, absolute returns, news). Hartzmark (2015) shows that individuals are more likely to sell stocks that are extremely-ranked in their portfolios (both in terms of cumulative return since purchase and alphabetically). Kaniel and Parham (2017) show that flows to mutual funds increase when they are mentioned in Wall Street Journal "Category Kings" ranking list, compared to those funds which just missed making the list. Wang (2017) shows that ranking a stock in a more salient place can affect small investors and market variables such as volatility and volume. Choi, Haisley, Kurkoski, and Massey (2017) documents that the salience of savings rates affect 401k contributions. Frydman and Wang (2020) show that the salience of a stock's purchase price affects the disposition effect. Ozik, Sadka, and Shen (2021) show that retail trading in Robinhood platform exhibits a sharp increase among stocks with high COVID19–related media coverage. Barber, Huang, Odean, and Schwarz (2022) show that the purchase behavior of Robinhood users is highly correlated, what suggests that they engage in salience-induced trading.

We also contribute to the literature on the local bias of retail investors. It is well-documented that retail investors tilt their trading activity towards local stocks, usually defined as stocks with headquarters close to the investor — see, for instance, Huberman (2001), Ivkovic and Weisbenner (2005), and Seasholes and Zhu (2010). The reasons why local stocks are appealing to retail investors, however, are still not fully clear. This is something important to be understood since, by investing locally, individuals become subject to shocks that may affect both their earnings and their investments.

The most common explanation for the local bias is that investors may have some informational advantage by living close to the firm's headquarter. This is, however, controversial. On the one hand, Ivkovic and Weisbenner (2005), Massa and Simonov (2006), and Bodnaruk (2009) present evidence consistent with the existence of some informational advantage. On the other hand, Grinblatt and Keloharju (2001), Huberman (2001), Keloharju, Knüpfer, and Linnainmaa (2012), Goetzmann and Kumar (2008), Seasholes and Zhu (2010), and Døskeland and Hvide (2011) present evidence of the contrary, i.e., that individuals investors do not outperform in local stocks. According to the evidence provided by our paper, salience could be an important reason under the local bias. A close headquarter could simply increase the salience of the stock.

Finally, our paper also relates to the literature that studies retail day-trading. Linnainmaa (2003), Jordan and Diltz (2003), Choe and Eom (2009), Ryu (2012), Kuo and Lin (2013), Barber, Lee, Liu, and Odean (2014), Barber et al. (2019), and Chague, De-Losso, and Giovannetti (2019) show that day-trading is rather common among individuals, who in general lose.

The remainder of the paper is organized as follows. Section 2 presents our datasets and relevant descriptive statistics. Section 3 shows the main empirical results. Section 4 shows the robustness exercises. Finally, Section 5 concludes.

2 Data: day-trading, small cities, micro-regions, and local stores

We rely on data from two sources. First, the retail trading data come from the Comissão de Valores Mobiliários (CVM), the Brazilian equivalent to the SEC. The dataset is at the investor-stock-day level and contains the volume purchased, the volume sold, the quantity of shares purchased, and the quantity of shares sold by all retail investors in the Brazilian stock market from 2012 to 2017. We also observe the city of residence of each individual, which is crucial for our analyses.

Our focus in this paper in on a particular type of trading strategy: day-trading. We define an investor-stock-day observation as a day-trade if the quantity of shares purchased is equal to the quantity of shares sold — in the robustness section, we say there is a day-trade if the investor both purchases and sells shares of the same stock on the same day, not necessarily in the same quantities. There are 8,846,980 day-trades performed by 190,655 individuals in Brazil from 2012 to 2017 (across all stocks). Table 1 presents some descriptive statistics for these 190,655 individuals who performed at least one day-trade from 2012 to 2017. On average they are 40 years old (median of 37), display 198 trades (purchases, sells or day-trades) at the stock-day level (median of 70), 46 day-trades at the stock-day level (median of 4), an average purchasing volume of R$ 20,523 at the stock-day level (median of R$ 7,456), and an average purchasing volume of R$ 37,655 on a day-trading stock-day (median of R$ 12,034). Volumes on day-trading stock-day observations tend to be higher since individuals can use leverage when they are day-trading.

[Table 1 about here]

Our second data source is RAIS (Relação Anual de Informações Sociais) dataset.[7] It comes from the Brazilian Ministry of Labor and contains detailed information about all formally employed workers in Brazil. At the worker-month level, the dataset contains the worker's job description, wage, employer identification, job address, among many other information. We use this dataset to obtain the location of all stores of the 60 brick-and-mortar companies listed in the Brazilian stock market between 2012 and 2017. We also determine the dates when a new store opens in the city by looking at the date when the first worker of the store is hired; we do the same to infer store closures. In our baseline analyses, we focus on stores in small cities, which we define as the ones with less than 100 thousand people in 2017 (5,270 cities). In the robustness section, we change this definition and also consider medium and large cities. The distribution of the number of local stores of these 60 firms in each triple (small city, firm, month) is naturally concentrated in 0 (96.13%), assuming the following other values: 1 (3.49%), 2 (0.34%), and 3 or more (0.04%).[8]

The top map in Figure 1 presents the location of all 5,270 small cities in Brazil. The blue dots indicate the cities that have at least one store from a listed firm; the red dots indicate the cities without stores from listed firms. The white borders indicate the 26 Brazilian states plus the Federal District of Brasília, the Brazilian capital. The bottom map in Figure 1 presents the 558 micro-regions of Brazil, which we use throughout the paper to control for any unobserved regional characteristics. These 558 micro-regions are defined by IBGE (the Brazilian Institute of Geography and Statistics) and are smaller in more populated areas.

[7] This rich dataset has been successfully used in the labor economics literature (to mention a few, Menezes-Filho, Muendler, and Ramey, 2008, Meghir, Narita, and Robin, 2015, Ulyssea, 2018).

[8] In Brazil, the large bulk of local stores in small cities are family owned or run by small businesses that are not listed in the stock market.

The average number of small cities in a micro-region is 9.5, the median is 8, the minimum is 1, the 25th percentile is 5, the 75th percentile is 13, and the maximum is 39.

[Figure 1 about here]

We look at the individuals who live in these 5,270 Brazilian small cities and we investigate their day-trading activity in the 60 brick-and-mortar listed firms. Figure 2 presents the probability of a retail day-trade in each month between 2012 and 2017 for our average firm, among the 60 brick-and-mortar firms, in our average small city (solid line). That is, the figure shows the average in each month across all pairs city-firm of a dummy variable that is one in case we observe some day-trading on that stock by individuals living in that small city and zero otherwise. As we can see, this probability reaches a minimum of around 0.3% by the end of 2013. After this point, the probability increases and gets closer to 1.0% by the end of 2017, when the number of retail day-traders increase in the Brazilian stock market.[9] This is an important baseline number in our paper; we will estimate how the presence of a local store in a small city can affect this unconditional probability.

[Figure 2 about here]

The 60 brick-and-mortar companies listed in the Brazilian stock market have a combined market capitalization of US$ 322 billion in January 2015 (the middle of the sample), which corresponds to about 38% of the total market capitalization of the Brazilian stock exchange at the time. Table 2 shows the list of the firms, their sector, market capitalization, total number of stores in Brazil in the year with the highest value, and the number of small cities with at least one local store in each year — a missing value indicates that the firm was not listed in the Brazilian stock market in that year. The list of sectors represented is: real estate (11), services (11), retailers (9), banking and financial services (8 firms), apparel retailers

[9] Brazil has seen a significant increase in the stock market participation by retail investor in recent years (https://www.b3.com.br/pt_br/noticias/investidores.htm).

(6), education (6), malls (6), and healthcare (3). As the table shows, firms from the financial sector, the large commercial banks, are present in more small cities than any other sector.[10] Also, 11 of the 60 firms have no local store in any small city during the sample period (but did have in larger cities). One particular firm, Lojas Americanas (row 43 in the table), shows a strong expansion in small cities in the period that we will explore in the empirical analysis.

[Table 2 about here]

2.1 Local stores in small cities do not provide useful information for day-trading

Day-trading is a very short-lived trading strategy that lasts minutes, hours at most. As such, information for day-traders can only come from high-frequency indicators such as intraday price variation, order sizes, and signed measures of trading flow (see, for instance, Bernstein, 1995). Accordingly, a local store in a small city should not provide any useful information for day-trading — for long-horizon investment, in turn, there could be, in principle, some valuable information in some local stores (see, for instance, Gerken and Painter, 2022).

To empirically show that local stores in small cities do not provide useful information for day-trading, we construct a dataset with all day-trades performed by individuals who live in small cities in all the 60 brick-and-mortar firms during 2012-2017 — a total of 246,858 day-trades. We then estimate a daytrade-by-daytrade regression

$$Ret_{i,s,t} = \beta_1 Store_{i,s,t} + \gamma_{s,t} + \epsilon_{i,s,t} \tag{1}$$

where $Ret_{i,s,t}$ is the return of the day-trade performed by individual i on stock s on day t (computed as the total daily volume sold minus the total daily volume purchased divided by the total daily volume purchased), $Store_{i,s,t}$ is a dummy variable that is one if individual i

[10] These are bank branches, not ATMs

lives in a small city that has a local store of firm s on day t, and $\gamma_{s,t}$ are stock-day fixed-effects (a constant for each pair stock-day).

The stock-day fixed-effects allow us to compare, for a given stock and on a given day, the returns obtained by all individuals who live in small cities and decided to day-trade that stock on that day. Coefficient β_1 is then comparing the result obtained by the average individual who lives in a small city that has a local store of the firm with the result obtained by the average individual who lives in a small city without a store. If local stores can give valuable information for day-trading, we should find $\beta_1 > 0$. In contrast, if day-traders cannot extract useful information from local stores, we should find $\beta_1 = 0$. As expected, this is indeed what we find. The estimated coefficient is equal to -0.0001 with t-statistic of -0.28.

3 Main empirical analyses

In principle, salience can affect the trading behavior of individuals (Barber and Odean, 2008). However, empirically showing that salience, in itself, affects retail investors is challenging: salience often covary with the arrival of new information.

We now document that an individual who lives in a small city has a significantly higher probability of *day-trading* a stock of a brick-and-mortar firm that has a store in that city. This evidence is consistent with salience, in itself, affecting the trading behavior of individuals. First, a local store in a small city clearly increases the visual salience of a brick-and-mortar firm for the city residents. Second, a local store in a small city provides no information that can be used for day-trading, as documented in the previous section.

Importantly, as we now carefully discuss, we explore the granularity of our dataset to control for confounding effects that can make retail day-trading activity to be indirectly related to the existence of local stores, such as, socioeconomic variables that vary across cities and over time, and firm-specific variables that vary regionally and over time.

3.1 Day-trading and stores: within a firm-month, across different cities

We first compare day-trading activity across different cities within a given pair firm-month. Is the chance of, in a given month, individuals day-trading stocks of a given brick-and-mortar firm higher in a small city where the firm has a local store compared to another small city where the firm has no local store?

To answer this question, we construct a stock-city-month panel dataset that is balanced across i) the 60 brick-and-mortar firms from Table 2, ii) the 5,270 small cities in Brazil, and iii) all months in which the firm is listed in the Brazilian equity market between 2012 and 2017 (a total of 19,625,480 observations). We then run the following regressions

$$DT_{s,c,t} = \beta_1 Store_{s,c,t} + \gamma_{s,t} + \epsilon_{s,c,t} \tag{2}$$

$$DT_{s,c,t} = \beta_1 Store_{s,c,t} + \beta_2 Pop_{c,t} + \beta_3 Inc_{c,t} + \beta_4 Inv_{c,t} + \gamma_{s,t} + \epsilon_{s,c,t} \tag{3}$$

$$DT_{s,c,t} = \beta_1 Store_{s,c,t} + \beta_2 Pop_{c,t} + \beta_3 Inc_{c,t} + \beta_4 Inv_{c,t} + \gamma_{s,mr,t} + \epsilon_{s,c,t} \tag{4}$$

$$DT_{s,c,t} = \beta_1 Store_{s,c,t} + \gamma_{c,t} + \gamma_{s,t,mr} + \epsilon_{s,c,t} \tag{5}$$

where $DT_{s,c,t}$ is a dummy variable indicating whether we observe a day-trade on stock s in month t executed by individuals living in city c, $Store_{s,c,t}$ is a dummy variable indicating whether there is a local store from firm s in city c in month t, $\gamma_{s,t}$ are stock-month fixed-effects (a constant for each pair stock-month), $Pop_{c,t}$ is the log of the number of residents in city c in month t, $Inc_{c,t}$ is a proxy of the per capita income in city c in month t in thousands of reais,[11] $Inv_{c,t}$ is the number of individuals, divided by 100, who live in city c and who have traded (buy, sell, or day-trade) any stock in the Brazilian stock market in the 12-month period before month t,[12] $\gamma_{s,mr,t}$ are stock-microregion-month fixed-effects (a constant for each triple stock-microregion-month), and $\gamma_{c,t}$ are city-month fixed-effects (a constant for

[11] We compute this proxy from the RAIS dataset by adding all wage incomes from all formal establishments in the city.

[12] When we use this control we have to drop the year of 2012 from our regressions.

each pair city-month). In all regressions in the paper standard-errors are clustered by stock, by city and also by month.

Our baseline estimate for β_1 comes from equation (4), the one with the finest controls. However, the other specifications are also helpful to build intuition about the existing biases in the estimation of β_1. Table 3 presents the results.

[Table 3 about here]

In equation (1), by including stock-month fixed-effects, we are comparing the probability of observing a day-trade of a given firm s in a given month in a small city A, in which the firm has a local store, with this probability in a small city B in which the firm does not have a local store. The estimate of β_1 is significant at 1% and suggests that the probability is 3.8 percentage points higher in city A. This estimate should be upward biased, however. For instance, small cities that are richer have a higher chance of having more people day-trading in the stock market and, also, a higher chance of having a local store of firm s. The same concern should be valid for more populous small cities, for instance.

In equation (2), we then include three controls at the level city-month: the population, the per capita income and the number of individuals that invest in the stock market. Accordingly, we are now comparing day-trading of stock s across different small cities holding those variables fixed. The estimate of β_1 reduces to 0.025, significant at 1%.

Although cities are now comparable with respect to these important socioeconomic dimensions, we may still be comparing cities that are located in very different places. Brazil is a big country with some heterogeneities across its different regions. Hence, it may well be the case that a firm is popular in a region in the North but almost unknown in another region in the South. This unobserved variable, regional popularity, could also bias β_1 upwards; in a city located in a region where the firm is popular, there can be both more day-traders and stores.

In equation (3), we hence substitute the stock-month fixed-effects for stock-microregion-month fixed-effects (the 558 micro-regions that are shown in the bottom map of Figure 1). Now, we are comparing only small cities in a given month which are located in the same micro-region. The estimate for β_1 with this finer control is 0.023, still significant at 1%.

[Figure 1 about here]

Finally, we employ city-month fixed effects instead of controlling for $Pop_{c,t}$, $Inc_{c,t}$, and $Inv_{c,t}$. Now, the regression controls for any dynamics in each city that may affect day-trading and were not being captured by those three observables. The estimate for β_1 is now 0.020, significant at 1%.

By employing stock-microregion-month and city-month fixed effects, equation (4) controls the effect of local stores on day-trading for important alternative indirect channels. We are comparing the day-trading activity on a given stock s in a given month across two small cities that i) are close to each other (are located in the same microregion of Brasil) and ii) have the same city-month level characteristics, but one city has a local store of firm s and the other does not. We find that the probability of day-trading occurring is 2 percentage points higher in the small city with the local store. According to Section 2, the probability of occurring some day-trade for our average stock in our average small city is lower than 1%. This shows that the 2 percentage points increase is, indeed, very large.

3.2 Day-trading and stores: within a city-month, across different firms

We now compare day-trading activity across different firms within a given small city in a given month. Do individuals who live in a given small city are more likely to day-trade the stock of a brick-and-mortar firm that has a store in the city than the stock of another brick-and-mortar firm that has no store?

To investigate this, we run the following stock-city-month panel regressions,

$$DT_{s,c,t} = \beta_1 Store_{s,c,t} + \gamma_{c,t} + \epsilon_{s,c,t} \tag{6}$$

$$DT_{s,c,t} = \beta_1 Store_{s,c,t} + \beta_2 MktCap_{s,t} + \beta_3 TotalStores_{s,t} + \gamma_{c,t} + \epsilon_{s,c,t} \tag{7}$$

$$DT_{s,c,t} = \beta_1 Store_{s,c,t} + \beta_2 MktCap_{s,t} + \beta_3 TotalStores_{s,t} + \gamma_{c,sec,t} + \epsilon_{s,c,t} \tag{8}$$

$$DT_{s,c,t} = \beta_1 Store_{s,c,t} + \gamma_{s,t} + \gamma_{c,sec,t} + \epsilon_{s,c,t} \tag{9}$$

$$DT_{s,c,t} = \beta_1 Store_{s,c,t} + \gamma_{s,mr,t} + \gamma_{c,sec,t} + \epsilon_{s,c,t} \tag{10}$$

where $DT_{s,c,t}$ is the same dummy variable indicating whether we observe a day-trade on stock s in month t executed by individuals living in city c, $Store_{s,c,t}$ is the same dummy variable indicating whether there is a local store from firm s in city c in month t, $\gamma_{c,t}$ are city-month fixed-effects (a constant for each pair city-month), $MktCap_{s,t}$ is the log of the market capitalization of firm s in month t (the median value in the month), $TotalStores_{s,t}$ is the log of the total number of local stores that firm s has in Brazil in month t, $\gamma_{c,secmt}$ are city-sector-month fixed-effects (a constant for each triple city-sector-month), $\gamma_{s,t}$ are stock-month fixed-effects (a constant for each pair stock-month), and $\gamma_{s,mr,t}$ are stock-microregion-month fixed-effects (a constant for each triple stock-microregion-month).

As before, our baseline estimate for β_1 comes from the last equation, the one with the finest controls, but we discuss the other equations to highlight the potential confounding effects that we are controlling for. Table 4 presents the results.

[Table 4 about here]

In equation (5), by including city-month fixed-effects, we are comparing, in a given pair city-month, the likelihood of observing a day-trade in the stock of a firm A that has a local store in the city in that month with the likelihood of observing a day-trade in the stock of firm B that has no local store in the city in that month. The estimate of β_1 is 0.026, significant at 1%, indicating a difference in these likelihoods of 2.6 percentage points. However, comparing firms of different sizes, for instance, should bias β_1. If firm A is larger than firm B, firm A has a higher chance of being day-traded (may be more popular in the country) and, also, of having a local store in city c.

To avoid this potential bias, we include two controls at the stock-month level in equation (6), namely, the firm's market capitalization and the total number of stores the firm has in Brazil. Thus, we are now comparing day-trading in city c in month t across different firms, with and without a local store, holding those important firm-level characteristics fixed. The estimate of β_1 reduces to 0.023, but is still significant at 1%.

Although market capitalization and total number of stores are controlled for, we may still be comparing firms from very distinct sectors, for instance, a commercial bank and a drugstore. If banks are, for some reason, more popular than drugstores, this could affect the estimation. In equation (7), we then substitute the city-month fixed-effects for city-sector-month fixed-effects. We are now comparing only firms of the same size and from the same sector. With these set of controls, the estimate of β_1 becomes 0.018, significant at 1%.

Next, we substitute the controls $MktCap_{s,t}$ and $TotalStores_{s,t}$ by general stock-month fixed-effects. By doing this, we control for any stock-specific time dynamics that may be correlated with day-trading. The estimate for β_1 becomes 0.016, significant at 1%.

In the specification of equation (8), there is one important potential bias still unaddressed. Bank B may have no branch in the city because it is not popular *in that specific region*. That is, the regional popularity of the firm may be affecting both the existence of a local store and the day-trading activity in the city. In the analysis of the previous section (within a given firm, across different cities) we addressed this concern by comparing only cities that are close to each other, i.e., in the same micro-region. In this section, we substitute the stock-month fixed-effects for stock-microregion-month fixed-effects. We are now comparing, within a given city-month, the likelihood of observing a day-trade of, say, a bank A that has a local branch in the city with the likelihood of observing a day-trade of a bank B that has no local branch in the city, and both banks are comparable regarding all characteristics that can vary at the microregion-month level, for instance, their regional popularity. The estimate for β_1 becomes 0.015, still significant at 1%.

The coefficient from equation (9) is our baseline estimate in this sub-section. That is, in a given pair city-month, the probability of individuals day-trading stocks from a firm with a local store is 1.5 percentage point higher than the probability of individuals day-trading stocks from a firm without a local store. Due to the set of fixed-effects included, both firms belong to the same sector and are comparable across all characteristics that vary at the firm-microregion-month level. Again, the magnitude of the coefficient is very large compared with the unconditional probability of observing a day-trade of below 1% reported in Section 2.

3.3 Day-trading and stores: within a pair city-firm, across different months

Finally, we fix both the city and the firm. Do individuals who live in a small city day-trade more the stock of a firm in the months when the firm has a local store in the city compared to the months when there is no local store? As we can see from the last six columns in Table 2, we have many instances of stores openings and closures across the small cities during our sample periods.

To answer this question, we now run the following stock-city-month panel regressions,

$$DT_{s,c,t} = \beta_1 Store_{s,c,t} + \gamma_{c,s} + \epsilon_{s,c,t} \tag{11}$$

$$\begin{aligned} DT_{s,c,t} = {} & \beta_1 Store_{s,c,t} + \beta_2 MktCap_{s,t} + \beta_3 TotalStores_{s,t} \\ & + \beta_4 Pop_{c,t} + \beta_5 Inc_{c,t} + \beta_6 Inv_{c,t} + \gamma_{c,s} + \epsilon_{s,c,t} \end{aligned} \tag{12}$$

$$DT_{s,c,t} = \beta_1 Store_{s,c,t} + \gamma_{s,t} + \gamma_{c,t} + \gamma_{c,s} + \epsilon_{s,c,t} \tag{13}$$

$$DT_{s,c,t} = \beta_1 Store_{s,c,t} + \gamma_{s,mr,t} + \gamma_{c,t} + \gamma_{c,s} + \epsilon_{s,c,t} \tag{14}$$

where $DT_{s,c,t}$ is the same dummy variable indicating whether we observe a day-trade on stock s in month t executed by individuals living in city c, $Store_{s,c,t}$ is the same dummy variable indicating whether there is a local store from firm s in city c in month t, $\gamma_{c,s}$ are city-stock fixed-effects (a constant for each pair city-stock), $\gamma_{s,t}$ are stock-month fixed-effects (a constant for each pair stock-month), $\gamma_{c,t}$ are city-month fixed-effects (a constant for each pair city-month), $\gamma_{s,mr,t}$ are stock-microregion-month fixed-effects (a constant for each triple stock-microregion-month), and the control variables in equation (11) are the same ones already used.

Table 5 presents the results. As before, we begin with equation (10) and discuss all equations to highlight the potential alternative channels that we are controlling for.

[Table 5 about here]

In equation (10), by including city-stock fixed-effects, we are comparing, in a given city c and for a given firm s, the likelihood of observing a day-trade in a month when the firm has a local store in the city with the likelihood of observing a day-trade in a month when the firm does not have a local store in the city. The estimate of β_1 is 0.015, significant at 5%.

Equation (10) may be comparing very different months, however. First, cities change over time, what can affect both the number of local stores and the intensity of the retail day-trading. Second, firms also change over time, also affecting both their local stores and day-trading on their stocks. Accordingly, in equation (11), we include the same city-month and firm-month controls used before. The estimate of β_1 is 0.011, significant at 10%. Naturally, a more flexible way to control for city-month and firm-month characteristics is to use the city-month and the stock-month fixed-effects instead of these five control variables. This is what we do in equation (12), where the estimate of β_1 reduces to 0.009, significant at 10%.

There is, however, another potential indirect channel by which local store may correlate to day-trading within a given pair city-stock. Suppose that the way the popularity of a given firm changes over time in Brazil is heterogeneous across different regions of the country, what could affect both local stores and day-trades differently in each region. This would not be captured by the stock-month fixed effect because of the heterogeneity across regions. To account for that, in equation (13) we use stock-microregion-month fixed-effects instead of stock-month fixed-effects. The estimate of β_1, however, does not change (it is now significant at 5%).

Summing up, for a given pair city-firm, the probability of individuals day-trading the stock in a month when there is a local store is 0.9 percentage point higher than the probability of individuals day-trading the stock in a month without a store. As before, the economic magnitude is important, as the unconditional probability of observing a day-trade in the average city and average firm is below 1% during our sample.

Openings and closings

The parameter obtained from equation (13) captures the dynamic effect from both openings and closings of stores in a given pair city-firm. We now investigate whether this dynamic effect indeed comes from both openings *and* closings.

In our sample, there are 357 city-firm pairs, the "opening group", in which i) the number of local stores goes from 0 to 1 and ii) we observe at least 12 months with no store and 12 months with the store during our sample period.[13] In turn, there are 175 city-firm pairs, the "closing group", in which i) the number of local stores goes from 1 to 0 and ii) we observe at least 12 months with the store and 12 months without the store.[14]

For these two groups of city-firm pairs, we estimate how the day trade probability evolves in the months around the opening or closing event. We evaluate how the day trade probability in months $-11, -10, ..., 0, 1, ..., 12$ compares with the day trade probability in month -12 (i.e., 12 months before the opening or closing event).

The top plot in Figure 3 presents the average day trade probability across the 357 city-firm pairs in the opening group in each one of the 25 months around the opening event (12

[13]These 357 city-firm pairs come from 26 different firms and 292 different small cities.
[14]These 175 city-firm pairs come from 19 different firms and 171 different small cities.

months before, the month of the store opening, and 12 months after). For each month we present the p-value of the null hypothesis that the probability of a retail day trade in that respective month is equal to the probability of a retail trade in month -12. If the p-value is below 5%, the circle is presented in red. In the period before the store opening, we see that the probability of retail day-trading is statistically stable, except for months -4 and -1. We believe that this anticipation of the effect may be because the local store construction should begin before month 0 — which is the month when we observe workers already hired by the firm in the RAIS dataset. In turn, in the month of the store opening (month 0) and in the 12 following months, the probability of retail day trade is in general significantly higher than in month -12.

[Figure 3 about here]

The bottom plot in Figure 3 presents the average day trade probability across the 175 city-firm pairs in the closing group in each one of the 25 months around the closing event. In this case, the probability of retail day-trading is statistically stable in all months, i.e., significantly equal to month -12.

The dynamic effect coming from local stores openings (and not closings) seems consistent with the salience channel. Once a stores opens, salience is affected in a sharp, discontinuous way. On the other hand, once a stores closes, the firm should continue present in people's minds, at least during the following months.

In the next section, we further study the dynamic effect of stores openings focusing on an important expansion plan of a brick-and-mortar firm that occurred in Brazil during our sample period.

3.4 Lojas Americanas case

The retailer "Lojas Americanas" provides us with an interesting case to study. During our sample, the firm pursued an aggressive expansion plan in small cities. We explore this to see how the effect of local stores on day-trading evolves over time.

The firm opened its first store in 1929 in Rio de Janeiro and went public in 1940, having its shares traded on the Brazilian stock market since then. In 2014, the firm started an expansion plan named "85 years in 5." As we can see in Table 2, in 2012, 52 small cities had a local store of the firm. This number increases to 65 in 2013, 104 in 2014, 146 in 2015, 173 in 2016, and 231 in 2017. Figure 5 shows a map with the small cities that received a local store between 2012 and 2017.

[Figure 5 about here]

To illustrate the salience mechanism, Figure 4 shows a photo of the front of a Lojas Americanas store taken from Google Streets in the city of Nova Esperança, a small city from the State of Paraná. We also show a photo from Google Streets of the same location taken in May, 2012, when there was another store (a local furniture business) in the same location. The Americanas store is located in the main street of the city, which is usually the case since this is the place where all residents shop in these cities. Apart from its strategic location, the storefront clearly displays the firm's name in white and red, increasing the firm's salience in the city.

[Figure 4 about here]

To investigate the dynamics of the relation between local stores and retail day-trading in the case of Lojas Americanas, we proceed as follows. We select all small cities in which a local store was opened in 2014 (39 cities across 36 micro-regions). For each one of these 39 cities we compute: i) a dummy variable $DT_{c,y}$ that is one in case we observe a day-trade of Lojas Americanas by an individual from this city in year y (2013-2017) and zero otherwise, ii) $Pop_{c,y}$, the monthly average of the log of the population of the city during year y, iii) $Inc_{c,y}$, the monthly average of the per capita income in the city during year y, and iv) $Inv_{c,y}$, the monthly average during year y of the number of individuals who live in city c and traded any stock in the previous 12 months. We also compute these four variables for all small cities in Brazil that have no stores of Lojas Americanas during the complete period between 2013 and 2017 (5,039 cities). With the city-year panel and both groups of cities, we then regress $DT_{c,y}$ on $Pop_{c,y}$, $Inc_{c,y}$, $Inv_{c,y}$ and γ_{mr} (micro-region fixed-effects), to obtain $ResDT_{c,y}$, the residual of this regression.

After obtaining $ResDT_{c,y}$, we then compute its average within each year across all 39 small cities in which a local store was opened in 2014, defining it as $\overline{ResDT}_y(1)$. Separately, we also compute the average of $ResDT_{c,y}$ within each year across all 5,039 small cities that have zero local stores from Lojas Americanas during the complete period between 2013 and 2017, defining it as $\overline{ResDT}_y(0)$. By doing that, we are estimating for these two groups of cities the probability of a retail day-trade of Lojas Americanas in each year, after controlling for population, income, number of investors, and location of the city.

The top plot of Figure 6 shows how $\overline{ResDT}_y(1)$ and $\overline{ResDT}_y(0)$ evolve over time. The gray circle displaying the 95% confidence interval refers to the 39 small cities in which a local store was opened in 2014. The black circle refers to the 5,039 small cities with no local store in the whole period – given the large number of small cities in this group, the 95% confidence band is too narrow to be shown in the plot. In 2013 and 2014, the probability of retail day-trading, controlled for the population, income, number of stock market investors, and location of the city, was not statistically different between the two groups of cities. In turn, in the years after the store opening (2015, 2016, and 2017), the probability of a day-trade in a small city with the local store begins to sharply increase. In 2017, it reaches about 30%.

[Figure 6 about here]

We also run the same exercise described above using all small cities in which a local store was opened in 2015 (41 cities in 39 micro-regions) and 2016 (27 cities in 27 micro-regions). The middle and bottom plots in Figure 6 present the results. As before, in the years previous to the store opening, the probability of retail day-trading, controlled for the population, income, number of stock market investors, and location of the city, is not statistically different between the cities where the store was opened and all other cities where no store was opened. In turn, after the store opening, the probability of a day-trade occurring begins to increase.

To complete this section, we summarize in a regression the information contained in Figure 6. We estimate the city-year panel regression

$$
\begin{aligned}
DT_{c,y} &= \beta_1 Treat_c + \beta_2 After_y + \beta_3 Treat_c \times After_y \\
&\quad + \beta_4 Pop_{c,y} + \beta_5 Inc_{c,y} + \beta_5 Inv_{c,y} + \gamma_{mr} + \epsilon_{c,y} \qquad (15)
\end{aligned}
$$

Analogous to the top plot of Figure 6, we estimate regression (14) considering all 39 small cities in which a local store was opened in 2014 (for which $Treat_c = 1$) and all 5,039 small cities that have zero local stores from Lojas Americanas during the complete period between 2013 and 2017 (for which $Treat_c = 0$). The dummy variable $After_y$ is equal to one for the years 2015, 2016, and 2017, and zero before that.

Analogous to the middle plot of Figure 6, we also estimate regression (14) considering all 41 small cities in which a local store was opened in 2015 (for which $Treat_c = 1$) and all 5,039 small cities that have zero local stores from Lojas Americanas during the complete period between 2013 and 2017 (for which $Treat_c = 0$). The dummy variable $After_y$ is equal to one for the years 2016, and 2017, and zero before that.

Finally, analogous to the bottom plot of Figure 6, we also estimate regression (14) considering all 27 small cities in which a local store was opened in 2016 (for which $Treat_c = 1$) and all 5,039 small cities that have zero local stores from Lojas Americanas during the complete period between 2013 and 2017 (for which $Treat_c = 0$). The dummy variable $After_y$ is equal to one for the year 2017, and zero before that.

Table 6 presents the results. Column 1 refers to the regression with the 39 small cities in which a local store was opened in 2014. The coefficient β_3 indicates that the probability of a day-trade in 2015, 2016, or 2017 is 17.0 percentage points higher in a city that received a local store compared to a city that did not receive any local store. Column 2 refers to the regression with the 41 small cities in which a local store was opened in 2015. The coefficient β_3 indicates that the probability of a day-trade in 2016 or 2017 is 19.9 percentage points higher in a city that received a local store compared to a city that did not receive any local store. Finally, column 3 refers to the regression with the 27 small cities in which a local store was opened in 2016. The coefficient β_3 indicates that the probability of a day-trade in 2017 is 6.3 percentage points higher in a city that received a local store compared to a city that did not receive any local store.

[Table 6 about here]

3.5 Local stores that do not increase firms salience

The local stores of 19 firms from Table 2 are arguably less likely to increase the salience of their respective firms. This is because the names that appear on the storefront differ from the names under which these firms are listed in the stock market. An example is the pharmaceutical retailer Profarma (stock PFRM3). The firm has local drugstores with four different flags, "Drogasmil", "Farmalife", "Tamoio", and "Rosário", which does not resemble the firm's actual listing name, Profarma.[15] Column 5 in Table 2 describes whether each firm and their local stores have the same name.

When the storefront name does not clearly increase the firm's salience, we expect the effect on day-trading to be smaller or null. That is, the estimates of β_1 in equations (4), (9), and (13), our baseline regressions, should be smaller or even insignificant for these 19

[15]It is possible to see the four different flags here: https://grupoprofarma.com.br/en/our-flags/.

firms. Indeed, this is what Table 7 shows. Column 1 shows a smaller point estimate for β_1 equal to 0.008, although statistically significant at 1%. In columns 2 and 3, the estimates are statistically equal to zero. In turn, when we run these regressions with the other 41 firms that have the same names that appear on their storefronts, we find a positive effect. Columns 4, 5, and 6 show estimates equal to 0.022 (significant at 1%), 0.018 (significant at 1%), and 0.010 (significant at 10%), respectively.

[Table 7 about here]

4 Robustness analyses

In this section we show that the previously documented results remain qualitatively the same under some alternative definitions and samples. We use new definitions for what constitute a day-trade and small cities, focus the analyses on small cities of Sao Paulo, the richest State of Brazil, and look at the relation between stores and day-trading in big cities.

4.1 Alternative definitions for day-trade

We identified a day-trade as a day in which the investor purchased and sold the same stock in the exact same quantities. In this section, we only require the investor to have purchased and sold the same stock on the same day, not necessarily in the same quantities.

The dashed-line in Figure 2 shows the probability of a day-trade, under this less restrictive definition, in each month between 2012 and 2017 for our average stock in our average small city. As expected, the probability is now slightly higher.

We replicate equations (4), (9), and (13) using the alternative definition for day-trade to compute the dependent variable $DT_{s,c,t}$. The results presented in Table 8 are qualitatively the same as before.

[Table 8 about here]

We also verify whether individuals who already day-trade are also affected by the presence of a local store. To do that, we compute the dummy variable $DT_{s,c,t}$ considering only day-traders: individuals who have at least 10 other day-trades (in any stock, not necessarily in the 60 brick-and-mortar firms) in the previous 12 months. We replicate equations (4), (9), and (13). Again, the results presented in Table 9 remain qualitatively the same.

[Table 9 about here]

4.2 Alternative thresholds for small cities

Our baseline definition for a small city is having a population of less than 100 thousand people in 2017. In this subsection, we first increase the number of cities in our regressions by changing this threshold to 250 thousand individuals. We replicate the baseline regressions (4), (9), and (13) using all cities with less than 250 thousand individuals (5,460 cities). The results presented in Table 10 are robust to the inclusion of more cities in the sample.

[Table 10 about here]

The results are also robust to the exclusion of very small cities from the sample, i.e., 3,120 cities with less than 10 thousand individuals. We replicate regressions (4), (9), and (13) using all cities with population between 10 thousand and 100 thousand individuals (2,820 cities). Results are presented in Table 11.

[Table 11 about here]

4.3 Focusing on the State of Sao Paulo

The State of Sao Paulo is the richest state in Brazil, being responsible for about 30% of the Brazilian GDP. It has 645 cities, 569 with less than 100 thousand people (i.e., small cities according to our baseline classification). Sao Paulo is the state with the highest density of small cities with local stores (see blue dots in Figure 1). We can thus estimate equations (4), (9), and (13) focusing only in the small cities in the State of Sao Paulo.

Table 12 presents the results. Results are qualitatively the same as the ones obtained when we look at the entire country.

[Table 12 about here]

4.4 Medium and large cities

Instead of studying the relation between day-trading and local stores using small cities, we could, in principle, focus on one very large city, divide it into neighborhoods, and relate the day-trading activity in each neighborhood with the existence of a local store close by. Unfortunately, this is not possible because we do not have the exact addresses of individuals (we only observe the cities where they live).[16] In this sub-section, however, we show that if we estimate equations (4), (9), and (13) considering only cities with more than 250 thousand individuals (a total of 110 cities), we obtain results that are consistent with the ones we have been presenting.

Given that we are now looking at medium and large cities, instead of using dummy variables for the existence of a local store and for whether we observe a day-trade, we use the number of local stores of firm s in city c in month t ($NumStores_{s,c,t}$) and the number of retail day-trades in firm s in city c in month t ($NumDT_{s,c,t}$). Moreover, instead of using

[16] We would like to note that, even if we observed the exact location of individuals, we believe the small cities may still offer a cleaner empirical exercise. In a big city, people circulate a lot, usually living far from where they work. As such, then can see many stores which are not close to where they live.

micro-regions to control for regional unobserved effects we use states fixed-effects. We then run equations (4), (9), and (13) with those variables. Table 13 presents the results.

[Table 13 about here]

According to column 1 in Table 13, one additional local store of a given firm across two different cities with more than 250 thousand individuals increases in 14.79 the number of monthly day-trades of this firm in these cities, after controlling for stock-state and city-month fixed-effects (the average number of monthly day-trades in these cities is 96.32). According to column 2, a firm that has one additional local store in a given city will have 17.61 more monthly day-trades compared to another firm — after controlling for city-sector and stock-state-month fixed-effects. Finally, according to column 3, if a given firm builds an additional local store in a given city, the number of monthly day-trades should increase in 73.42 — after controlling for city-month and stock-state-month fixed-effects.

5 Conclusion

Day-trading is an extremely short-lived trading strategy that lasts, at most, few hours. As such, it is very unlikely that day-trading can benefit from any piece of information that one could gather from a local store in a small city. Therefore, if the presence of a local store in a small city increases the chances of individuals day-trading the respective firm, it should be because of the increased visual salience, as the local store naturally makes the firm known to the city residents.

We find in this paper a robust and strong positive relation between the presence of local stores in small cities from brick-and-mortar firms and the propensity for day-trading the respective stock among the individuals who live in the city. Importantly, the granularity of our dataset allows us to control for many indirect channels that could be behind this relation. We also perform a number of robustness exercises. In particular, we examine the case of firms that have a different storefront name from their listing names in the stock exchange and we find that the effects are weaker or null, as expected.

We believe that our results increase the understanding about the direct effects of salience on the behavior of retail investors. By combining a low-frequency salience measure — the existence of a local store in a small city — with a high-frequency trading activity that is becoming increasingly common among individuals — day-trading — we can plausibly isolate the potential information-related channel that often pollutes the relation between stock salience and trading decisions.

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A Figures and Tables

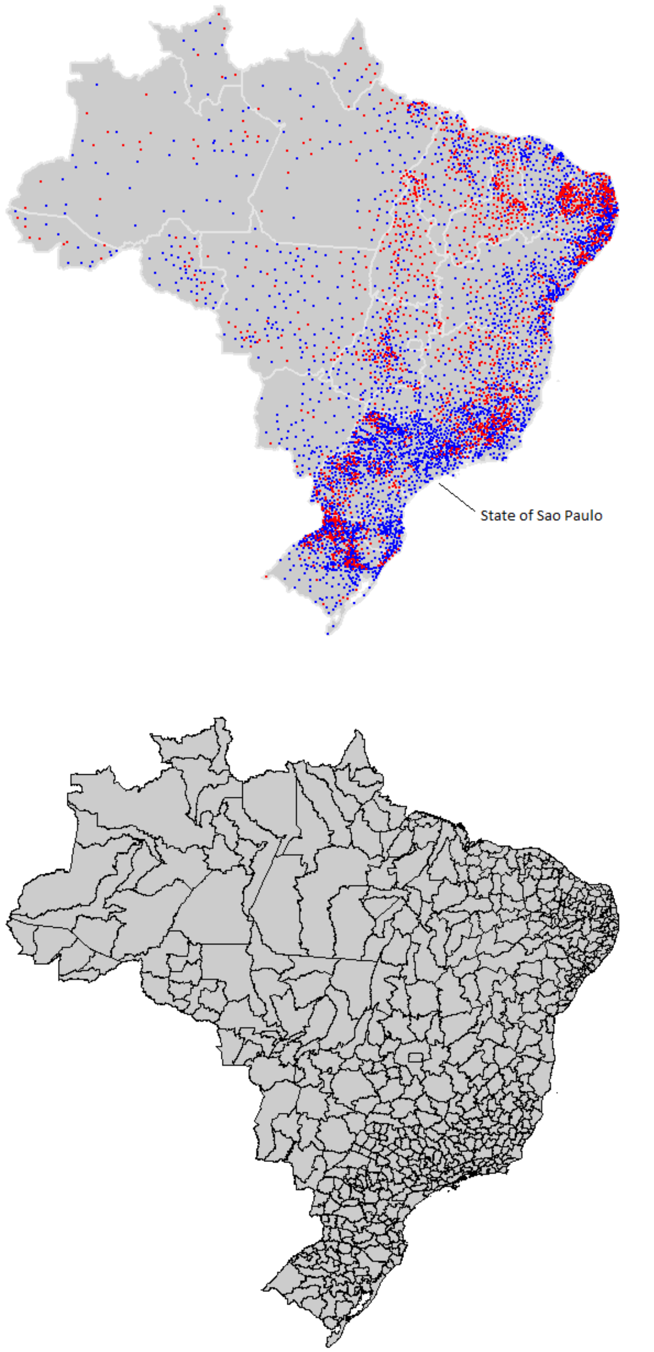

Figure 1: Small cities and microregions

The top map shows the 5,270 small cities in Brazil (the ones with less than 100 thousand people). The ones in blue have a local store of some of the 60 firms between 2012 and 2017; the ones in red have no local store of any of the 60 firms in the period; the white frontiers represent the Brazilian states. The bottom map presents the micro-regions of Brazil, which were defined by IGBE (the Brazilian Institute of Geography and Statistics) in 1990.

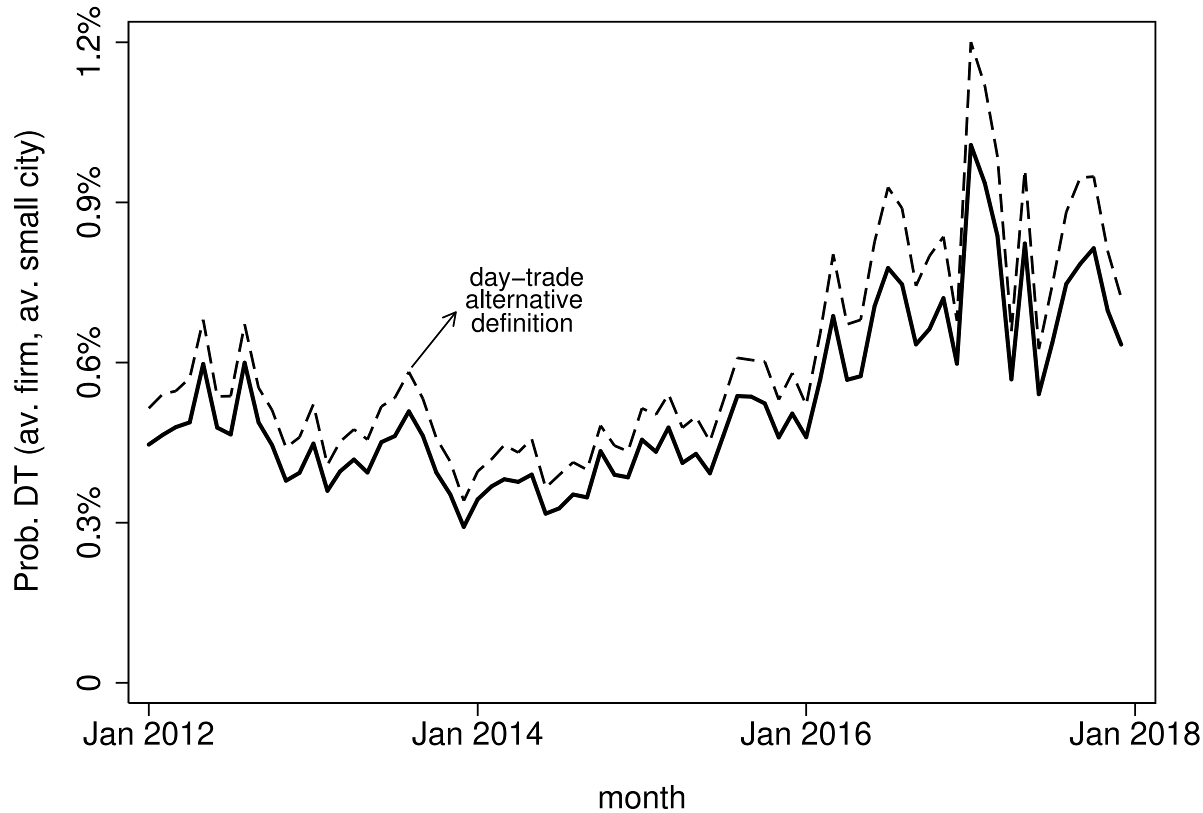

Figure 2: Probability of retail day-trading in the average small city and firm

This figure presents the probability of a retail day-trade in each month between 2012 and 2017 for our average stock in our average small city. That is, the figure shows the average in each month across all pairs city-firm of a dummy variable that is one in case we observe some day-trading on that stock by individuals living in that small city and zero otherwise. The solid line is for the baseline definition of day-trade (same positive quantity purchased and sold). The dashed line is for the alternative definition of a day-trade used in the robustness section (positive quantity purchased and positive quantity sold).

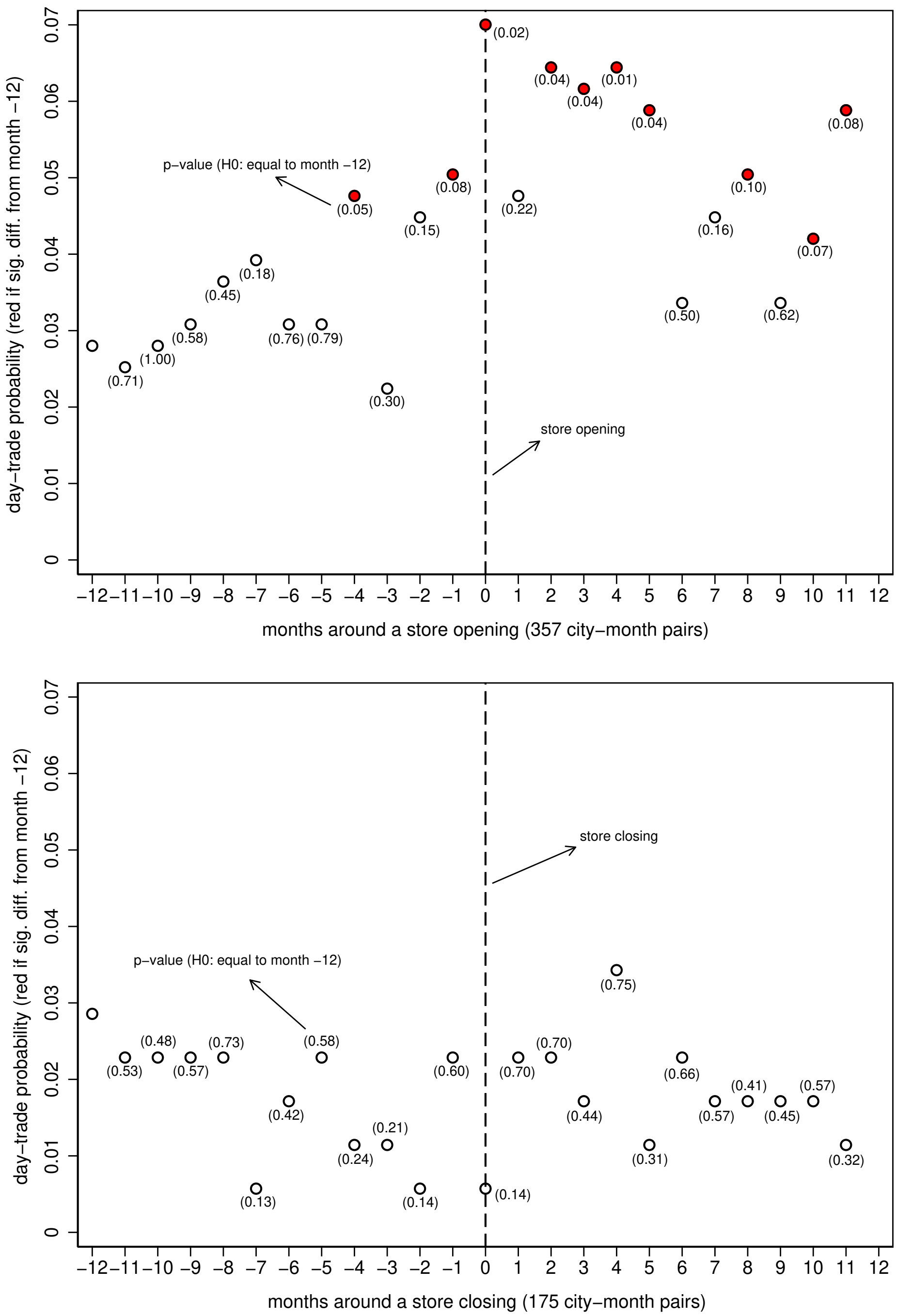

Figure 3: Store opening vs. closing in a city-firm pair

These figures show the probability of a retail day trade in 12 months before and 12 months after the opening or closing of a local store in a city-firm pair. In the top plot (openings) we focus on city-firm pairs in which the number of local stores goes from 0 to 1 and we have at least 12 months with no store and 12 months with the store (357 city-firm pairs). To construct the bottom plot (closings) we focus on city-firm pairs in which the number of local stores goes from 1 to 0 and we have at least 12 months with the store and 12 months with no store (175 city-firm pairs). Standard-errors are clustered by stock, by city and by month. For each month we present the p-value of the null hypothesis that the probability of a retail day trade in that respective month is equal to the probability of a retail trade in month -12 (i.e., 12 months before the opening or the closing event). If the p-value is below 5%, the circle is presented in red.

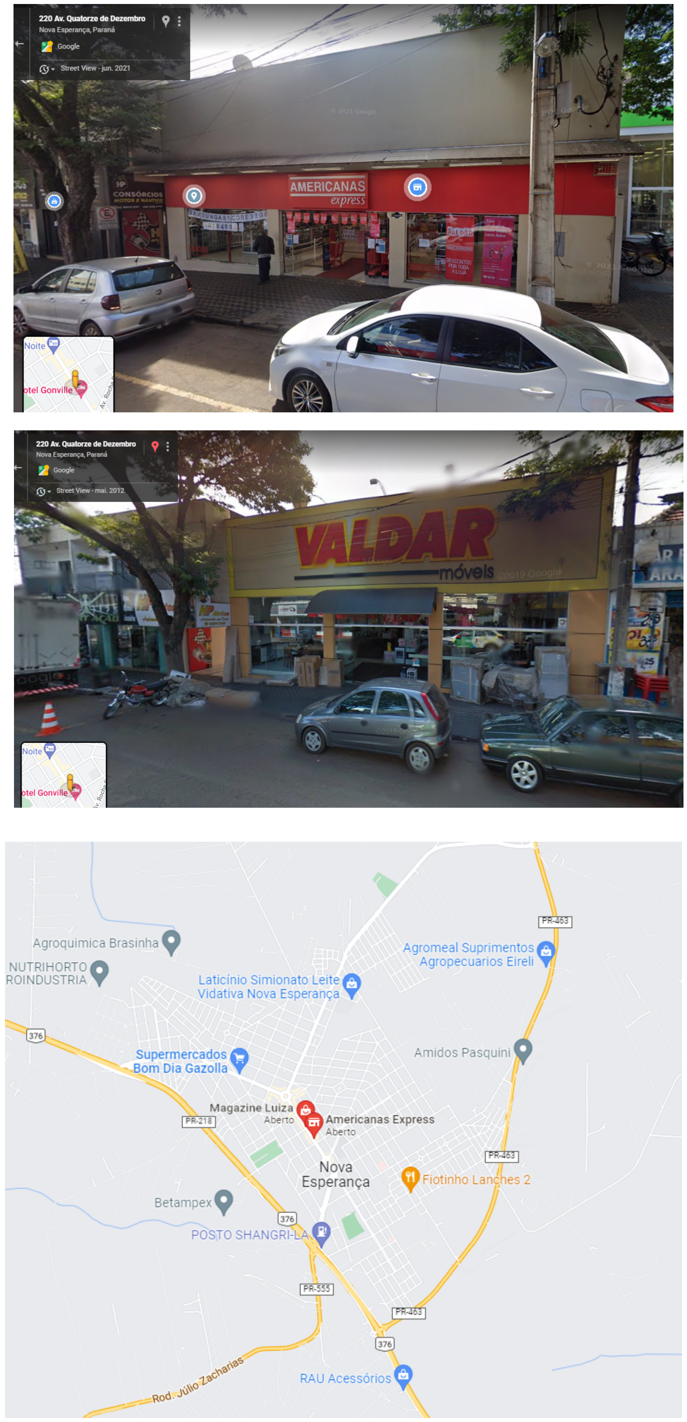

Figure 4: Store front of a Lojas Americanas local store

The top photo shows the front of a Lojas Americanas store in the city of Nova Esperança, in the state of Paraná; the photo in the middle shows the same location in May 2012 (with a store from a non-listed firm). The map shows the city of Nova Esperança.

Figure 5: Small cities that received a Lojas Americanas store between 2012 and 2017
This figure shows all 107 small cities (the ones with less than 100 thousand people) that received a local store from Lojas Americanas in the years between 2012 and 2017.

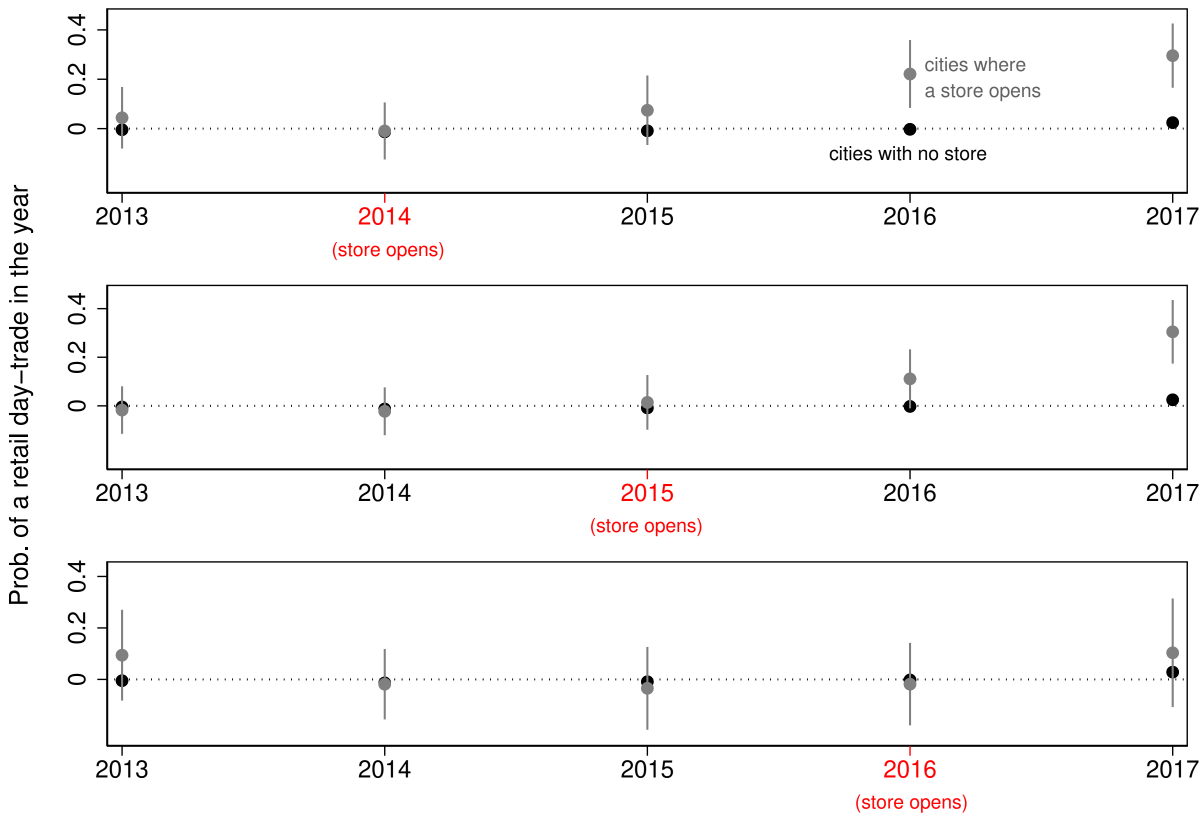

Figure 6: The dynamics of the relation between stores and day-trading

This figure illustrates the dynamics of the relation between local stores and retail day-trading in the case of Lojas Americanas. In the top plot, we select all small cities in which a local store was opened in 2014 (39 cities). For each one of these 39 cities we compute a dummy variable $DT_{c,y}$ that is one in case we observe a day-trade of Lojas Americanas by an individual from this city in year y (2013-2017) and zero otherwise, $Pop_{c,y}$, the monthly average of the log of the population of the city during year y, $Inc_{c,y}$, the monthly average of the per capita income in the city during year y, and $Inv_{c,y}$, the monthly average during year y of the number of individuals in city c that traded (buy, sell, or day-trade) any stock in the previous 12 months. We also compute these four variables for all small cities in Brazil that have zero local stores from Lojas Americanas during the complete period between 2013 and 2017 (5,039 cities). With the city-year panel with both groups of cities, we then regress $DT_{c,y}$ on $Pop_{c,y}$, $Inc_{c,y}$, $Inv_{c,y}$, and Brazilian micro-regions fixed-effects. Define $ResDT_{c,y}$ as the residual of this regression. After obtaining $ResDT_{c,y}$, we compute its average within each year across all 39 small cities in which a local store was opened in 2014, defining it as $\overline{ResDT}_y(1)$. Separately, we also compute the average of $ResDT_{c,y}$ within each year across all 5,039 small cities in Brazil that have zero local stores from Lojas Americanas during the complete period between 2013 and 2017 ($\overline{ResDT}_y(0)$). The top plot shows how $\overline{ResDT}_y(1)$ and $\overline{ResDT}_y(0)$ evolve over time. The gray circles refer to $\overline{ResDT}_y(1)$ with its 95% confidence band. The black circle refers to $\overline{ResDT}_y(0)$ (given the large number of small cities in this group, the 95% confidence band is too narrow to appear in the plot). The same exercise is performed using all small cities in which a local store was opened in 2015 and 2016 (middle and bottom plots).

Table 1: Day-traders descriptive statistics

This table presents descriptive statistics for the 190,655 individuals who made at least one day-trade between 2012 and 2017. A day-trade is defined as a stock-day observation in which the individual purchased and sold the same quantities of the stock. For each individual we compute the number of stock-day observations with some trading activity, the number of stock-day observation with a day-trading activity, the average volume among the stock-day purchases not related to day-trade, the average volume purchased in a stock-day observation with a day-trade, and his or her age in 2015.

	statistics across 190,655 individuals					
	average	pct 5	pct 25	pct50	pct 75	pct 95
number of trades (stock-day)	197.55	4	26	70	175	683
number of day-trades (stock-day)	46.40	1	1	4	19	148
average volume purchased (stock-day) in R$	20,523	1,008	3,236	7,456	17,775	69,367
average volume purchased in day-trades (stock-day) in R$	37,655	1,061	4,396	12,034	32,427	138,185
age in 2015	40.3	23	31	37	49	66

Table 2: Brick-and-mortar firms descriptive statistics

This table presents the 60 retail firms that sell goods or services to individuals through local stores and are listed in the Brazilian equity market in some year between 2012 and 2017. They are sorted by sector. All these 60 firms are included in our regressions in the months they can be traded in the stock market — missing values in last columns of the table appear in the years the firm is not listed in the equity market. Among the 60 firms, 11 firms have zero local stores in small cities during the whole period.

#	Firm name	Ticker	Sector	Stores show firm name?	Mkt. cap. in Jan 2015 (US$ million)	Total number of stores in Brazil	Number of small cities with a local store					
							2012	2013	2014	2015	2016	2017
1	Alpargatas	ALPA	apparel	n	1,935	57	11	11	11	10	9	9
2	Arezzo	ARZZ	apparel	y	1,005	66	3	3	4	5	5	5
3	Hering	HGTX	apparel	y	1,564	113	12	13	15	15	14	15
4	Guararapes	GUAR	apparel	n	2,071	321	9	6	6	5	3	3
5	Lojas Marisa	AMAR	apparel	y	973	438	1	1	1	1	1	1
6	Lojas Renner	LREN	apparel	y	4,065	524	1	1	2	3	4	7
7	Anhanguera	AEDU	education	y	1,944	190	16	16	16			
8	Kroton Educacional	KROT	education	n	6,226	190	16	16	16	16	16	16
9	Anima Educação	ANIM	education	n	541	52		3	3	4	5	6
10	Ser Educacional	SEER	education	n	930	75		0	0	0	0	0
11	Somos Educação	SEDU	education	n	1,158	163	8	7	7	7	7	7
12	Estácio Participações	YDUQ	education	y	1,955	191	2	2	2	2	2	3
13	Banco ABC Brasil	ABCB	finance	y	753	4	0	0	0	0	0	0
14	Banrisul	BRSR	finance	y	2,360	541	341	342	342	342	342	341
15	BB Seguridade	BBSE	finance	n	21,482	5,713		2,899	2,915	2,921	2,916	2,909
16	Banco Bradesco	BBDC	finance	y	54,454	6,525	2,028	2,028	2,019	2,012	1,998	1,977
17	Banco do Brasil	BBAS	finance	y	28,117	5,725	2,871	2,899	2,914	2,920	2,915	2,908
18	Banco BTG Pactual	BPAC	finance	y	7,737	24	1	1	1	1	1	1
19	Itau-Unibanco	ITUB	finance	y	65,719	5,046	942	933	932	933	931	900
20	Banco Santander BR	SANB	finance	y	23,694	2,541	527	527	526	495	494	493
21	Alliar	AALR	health	n	662	121					9	9
22	Fleury	FLRY	health	y	1,310	226	3	3	3	3	2	2
23	Hermes Pardini	PARD	health	y	1,021	134						7
24	Aliansce	ALSC	malls	n	1,102	39	0	0	0	0	0	0
25	Sonae Sierra Brasil	ALSO	malls	n	632	16	0	0	0	0	0	0
26	BR Malls Participações	BRML	malls	n	3,322	104	0	0	0	0	0	0
27	Iguatemi	IGTA	malls	y	1,654	55	0	0	0	0	0	0

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#	Firm name	Ticker	Sector	Stores show firm name?	Mkt. cap. in Jan 2015 (US$ million)	Total number of stores in Brazil	Number of small cities with a local store 2012	2013	2014	2015	2016	2017
28	Jereissati Participações	MLFT	malls	n	548	55	0	0	0	0	0	0
29	Multiplan	MULT	malls	n	3,853	117	0	0	0	0	0	0
30	Direcional	DIRR	real estate	y	488	81	0	1	1	1	2	2
31	Even	EVEN	real estate	y	498	9	1	1	1	1	1	1
32	Eztec	EZTC	real estate	y	1,196	43	0	1	1	1	1	1
33	Gafisa	GFSA	real estate	y	365	59	1	0	2	3	4	4
34	Cyrela Realt	CYRE	real estate	y	2,070	158	0	0	0	2	2	2
35	Helbor	HBOR	real estate	y	386	7	0	0	0	0	0	0
36	JHSF Participações	JHSF	real estate	y	485	37	1	1	1	1	2	2
37	MRV	MRVE	real estate	y	2,030	212	6	5	4	2	2	2
38	PDG Realt	PDGR	real estate	y	272	146	0	0	0	0	0	0
39	Rossi Residencial	RSID	real estate	y	76	166	1	1	0	0	0	0
40	Tenda	TEND	real estate	y	517	31						0
41	BR Pharma	BPHA	retail	n	283	836	81	91	95	95	91	86
42	Carrefour BR	CRFB	retail	y	11,712	592						12
43	Lojas Americanas	LAME	retail	y	6,716	1,271	52	65	104	146	173	231
44	Magazine Luiza	MGLU	retail	y	742	903	301	303	314	323	340	386
45	Pão de Açucar CBD	PCAR	retail	y	7,966	1,379	26	27	27	28	28	27
46	Profarma	PFRM	retail	n	194	203	7	7	8	8	7	7
47	Raia Drogasil	RADL	retail	y	3,551	1,640	81	81	95	95	100	104
48	Saraiva Livrarias	SLED	retail	y	67	144	1	1	1	1	1	1
49	Viavarejo	VVAR	retail	n	2,644	1,178			102	110	110	110
50	Azul S.A.	AZUL	service	y	3,614	108						21
51	BR Brokers	BBRK	service	n	184	118	2	3	2	1	0	0
52	CVC Brasil	CVCB	service	y	1,175	459			34	35	46	52
53	Gol	GOLL	service	y	1,237	93	10	10	9	10	10	10
54	IMC S/A	MEAL	service	n	453	188	13	14	14	14	14	13
55	Localiza	RENT	service	y	2,731	484	43	49	56	60	69	72
56	Lopes Brasil	LPSB	service	y	284	50	1	1	1	1	1	1
57	Movida	MOVI	service	y	696	200						6
58	Oi	OIBR	service	y	2,362	335	39	67	72	72	71	68
59	Tam S/A	TAMM	service	y	2,089	82	5					
60	Telefônica Brasil	VIVT	service	n	22,155	655	20	18	19	19	19	12

Table 3: Day-trading and local stores: within a pair firm-month, across cities

This table shows the estimates of stock-city-month panel regressions of $DT_{s,c,t}$, a dummy variable that is one if there is at least one day trade in stock s, in month t, by an individual living in city c, on $Store_{s,c,t}$, a dummy variable that is one if there is a local store from firm s, in city c, in month t. Control variables are $Pop_{c,t}$, the log of the number of residents in city c in month t, $Inc_{c,t}$, the per capita income in city c in month t in thousands of reais, and $Inv_{c,t}$, the number of individuals, divided by 100, that live in city c who presented any trade (buy, sell, or day-trade) of any stock in the Brazilian stock market in the 12-month period before month t. Standard-errors are clustered by stock, by city and by month and the t-statistics are presented in parentheses. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively.

	$DT_{s,c,t}$			
	(1)	(2)	(3)	(4)
$Store_{s,c,t}$	0.038***	0.025***	0.023***	0.020***
	(6.63)	(4.39)	(4.32)	(4.38)
$Pop_{c,t}$		0.001**	0.002**	
		(1.99)	(2.18)	
$Inc_{c,t}$		0.002	0.001	
		(1.57)	(0.83)	
$Inv_{c,t}$		0.078***	0.076***	
		(6.55)	(6.95)	
stock-month FE	✓	✓		
stock-microregion-month FE			✓	✓
city-month FE				✓
Obs	19,625,480	16,574,098	16,574,098	19,625,480
Adj-R2	0.02	0.07	0.10	0.18

Table 4: Day-trading and local stores: within a pair city-month, across firms

This table shows the estimates of stock-city-month panel regressions of $DT_{s,c,t}$, a dummy variable that is one if there is at least one day trade in stock s, in month t, by an individual living in city c, on $Store_{s,c,t}$, a dummy variable that is one if there is a local store from firm s, in city c, in month t. Control variables are $MktCap_{s,t}$, the log of the market capitalization of firm s in month t (the median value in the month) and $TotalStores_{s,t}$, the log of the total number of local stores that firm s has in Brazil in month t. Standard-errors are clustered by stock, by city and by month and the t-statistics are presented in parentheses. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively.

			$DT_{s,c,t}$		
	(1)	(2)	(3)	(4)	(5)
$Store_{s,c,t}$	0.026***	0.023***	0.018***	0.016***	0.015***
	(3.85)	(3.64	(3.22)	(3.33)	(3.39)
$MktCap_{s,t}$		0.001	0.001		
		(1.16)	(0.78)		
$TotalStores_{s,t}$		0.001*	0.001**		
		(1.68)	(2.61)		
city-month FE	✓	✓			
city-month-sector FE			✓	✓	✓
stock-month FE				✓	
stock-microregion-month FE					✓
Obs	19,625,480	19,625,480	19,625,480	19,625,480	19,625,480
Adj-R2	0.14	0.14	0.14	0.17	0.19

Table 5: Day-trading and local stores: within a city-firm, across months

This table shows the estimates of stock-city-month panel regressions of $DT_{s,c,t}$, a dummy variable that is one if there is at least one day trade in stock s, in month t, by an individual living in city c, on $Store_{s,c,t}$, a dummy variable that is one if there is a local store from firm s, in city c, in month t. Control variables are $Pop_{c,t}$, the log of the number of residents in city c in month t, $Inc_{c,t}$, the per capita income in city c in month t in thousands of reais, and $Inv_{c,t}$, the number of individuals living in city c who presented any trade (buy, sell, or day-trade) of any stock in the Brazilian stock market in the 12-month period before month t, $MktCap_{s,t}$, the log of the market capitalization of firm s in month t (the median value in the month), and $TotalStores_{s,t}$, the log of the total number of local stores that firm s has in Brazil in month t. Standard-errors are clustered by stock, by city and by month and the t-statistics are presented in parentheses. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively.

	$DT_{s,c,t}$			
	(1)	(2)	(3)	(4)
$Store_{s,c,t}$	0.015**	0.011*	0.009*	0.009**
	(2.10)	(1.78)	(1.77)	(2.03)
$Pop_{c,t}$		0.013		
		(1.61)		
$Inc_{c,t}$		0.003***		
		(3.52)		
$Inv_{c,t}$		0.064***		
		(4.82)		
$MktCap_{s,t}$		0.001		
		(1.56)		
$TotalStores_{s,t}$		0.002		
		(1.33)		
city-stock FE	✓	✓	✓	✓
stock-month FE			✓	
city-month FE			✓	✓
stock-microregion-month FE				✓
Obs	19,625,480	19,625,480	19,625,480	19,625,480
Adj-R2	0.22	0.23	0.26	0.28

Table 6: Lojas Americanas: city-year panel regression

The table shows city-year panel regressions using the expansion of Lojas Americanas. We regress $DT_{c,y}$, a dummy variable that is one in case we observe a day-trade of Lojas Americanas by an individual from city c in year y and zero otherwise, on $Pop_{c,y}$, the monthly average of the log of the population of city c during y, $Inc_{c,y}$, the monthly average of the per capita income in city c during year y, $Inc_{c,y}$, the monthly average during year y of the number of individuals in city c who traded (buy, sell, or day-trade) any stock in the 12 previous months, microregions fixed-effects, and on $Treat_c$, $After_y$, and their interaction. In column 1, we consider all 39 small cities in which a local store was opened in 2014 (for which $Treat_c = 1$) and all 5,039 small cities that have zero local stores from Lojas Americanas during the complete period between 2013 and 2017 (for which $Treat_c = 0$), and the dummy variable $After_y$ is equal to one for the years 2015, 2016, and 2017, and zero before that. In column 2, we consider all 41 small cities in which a local store was opened in 2015 (for which $Treat_c = 1$) and all 5,039 small cities that have zero local stores from Lojas Americanas during the complete period between 2013 and 2017 (for which $Treat_c = 0$), and the dummy variable $After_y$ is equal to one for the years 2016 and 2017, and zero before that. In column 3, we consider all 27 small cities in which a local store was opened in 2016 (for which $Treat_c = 1$) and all 5,039 small cities that have zero local stores from Lojas Americanas during the complete period between 2013 and 2017 (for which $Treat_c = 0$), and the dummy variable $After_y$ is equal to one for 2017 and zero before that. Standard-errors are clustered by city and by month and the t-statistics are presented in parentheses. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively.

		$DT_{c,y}$	
	(1)	(2)	(3)
$Treat_c$	0.054	0.020	0.021
	(1.23)	(0.71)	(0.39)
$After_y$	0.018	0.028*	0.042***
	(1.85)	(2.34)	(9.30)
$Treat_c \times After_y$	0.170**	0.199**	0.063**
	(3.19)	(2.76)	(4.46)
$Pop_{c,y}$	0.027	0.027	0.038*
	(2.11)	(2.02)	(2.52)
$Inc_{c,y}$	0.023*	0.014**	0.017**
	(2.19)	(3.82)	(3.93)
$Inv_{c,y}$	0.396***	0.402***	0.266**
	(8.80)	(8.04)	(3.98)
microregion FE	✓	✓	✓
Obs	25,575	25,585	25,515
Adj-R2	0.24	0.24	0.21

Table 7: When the firm name is not salient

The first three columns of this table show the estimates of equations (4), (9), and (14) when we use only the 19 firms that do not have their names in their local stores. The last three columns show the estimates of equations (4), (9), and (14) when we use only the 41 firms that have their names in their local stores. Standard-errors are clustered by stock, by city and by month and the t-statistics are presented in parentheses. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively.

	$DT_{s,c,t}$					
	store name $\neq$ firm name			store name = firm name		
	(1)	(2)	(3)	(4)	(5)	(6)
$Store_{s,c,t}$	0.008***	0.010	0.004	0.022***	0.018***	0.010*
	(5.85)	(1.43)	(0.67)	(5.15)	(3.51)	(1.71)
stock-microregion-month FE	✓	✓	✓	✓	✓	✓
city-stock FE			✓			✓
city-sector-month FE		✓			✓	
city-month FE	✓		✓	✓		✓
Obs	6,448,760	6,448,760	6,448,760	13,139,480	13,139,480	13,139,480
Adj-R2	0.18	0.12	0.27	0.19	0.19	0.28

Table 8: Day-trade alternative definition

This table shows the estimates of equations (4), (9), and (14) when we use the alternative definition of day-trade to construct $DT_{s,c,t}$. Here, we define as a day-trade an individual-stock-day observation with a positive quantity purchased and a positive quantity sold, which do not have to be the same . Standard-errors are clustered by stock, by city and by month and the t-statistics are presented in parentheses. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively.

	$DT_{s,c,t}$		
	(1)	(2)	(3)
$Store_{s,c,t}$	0.023***	0.017***	0.011**
	(4.49)	(3.45)	(2.13)
stock-microregion-month FE	✓	✓	✓
city-stock FE			✓
city-sector-month FE		✓	
city-month FE	✓		✓
Obs	19,625,480	19,625,480	19,625,480
Adj-R2	0.18	0.19	0.29

Table 9: Day-trades by individuals who already day-trade

This table shows the estimates of equations (4), (9), and (14) when we compute our dependent variable $DT_{s,c,t}$ considering only day-trades by individuals who have already made at least 10 other day-trades (any stock) in the previous 12 months. Standard-errors are clustered by stock, by city and by month and the t-statistics are presented in parentheses. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively.

	$DT_{s,c,t}$		
	(1)	(2)	(3)
$Store_{s,c,t}$	0.018***	0.014***	0.008*
	(4.30)	(3.39)	(1.78)
stock-microregion-month FE	✓	✓	✓
city-stock FE			✓
city-sector-month FE		✓	
city-month FE	✓		✓
Obs	19,625,480	19,625,480	19,625,480
Adj-R2	0.20	0.21	0.33

Table 10: Including more cities

This table shows the estimates of equations (4), (9), and (14) when we include all cities with less than 250 thousand individuals (5,460 cities) in the regressions. Standard-errors are clustered by stock, by city and by month and the t-statistics are presented in parentheses. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively.

	$DT_{s,c,t}$		
	(1)	(2)	(3)
$Store_{s,c,t}$	0.026***	0.022***	0.011**
	(4.06)	(3.20)	(2.41)
stock-microregion-month FE	✓	✓	✓
city-stock FE			✓
city-sector-month FE		✓	
city-month FE	✓		✓
Obs	20,333,040	20,333,040	20,333,040
R2	0.23	0.24	0.35

Table 11: Excluding very small cities

This table shows the estimates of equations (4), (9), and (14) when we exclude cities with less than 10 thousand individuals (3,120 cities) from the sample. Standard-errors are clustered by stock, by city and by month and the t-statistics are presented in parentheses. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively.

		$DT_{s,c,t}$	
	(1)	(2)	(3)
$Store_{s,c,t}$	0.015***	0.010**	0.010**
	(3.17)	(2.03)	(2.02)
stock-microregion-month FE	✓	✓	✓
city-stock FE			✓
city-sector-month FE		✓	
city-month FE	✓		✓
Obs	10,501,680	10,501,680	10,501,680
R2	0.20	0.20	0.29

Table 12: Focusing on the State of Sao Paulo

This table shows the estimates of equations (4), (9), and (14) when we focus only on the small cities of state of Sao Paulo. Standard-errors are clustered by stock, by city and by month and the t-statistics are presented in parentheses. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively.

	$DT_{s,c,t}$		
	(1)	(2)	(3)
$Store_{s,c,t}$	0.030***	0.021**	0.009*
	(3.10)	(2.39)	(1.67)
stock-microregion-month FE	✓	✓	✓
city-stock FE			✓
city-sector-month FE		✓	
city-month FE	✓		✓
Obs	2,118,956	2,118,956	2,118,956
R2	0.23	0.24	0.34

Table 13: Large cities

This table shows the estimates of equations (4), (9), and (14) when we consider only cities with more than 250 thousand individuals (110 cities) in the regressions. Here, instead of using dummy variables for the existence of a local store and the existence of day-trade, we use the number of local stores of firm s in city c in month t ($NumStores_{s,c,t}$) and the number of retail day-trades in firm s in city c in month t ($NumDT_{s,c,t}$). Standard-errors are clustered by stock, by city and by month and the t-statistics are presented in parentheses. *, **, and *** indicate significance levels of 0.10, 0.05, and 0.01, respectively.

	$NumDT_{s,c,t}$		
	(1)	(2)	(3)
$NumStores_{s,c,t}$	14.79***	17.61***	73.42**
	(7.21)	(8.97)	(2.14)
stock-state-month FE	✓	✓	✓
city-stock FE			✓
city-sector-month FE		✓	
city-month FE	✓		✓
Obs	409,640	409,640	409,640
R2	0.23	0.24	0.55