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Financial Constraints and Firm Adjustments During a Sales Disruption*

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Abstract

We address two main questions: (i) how do firms respond to an unanticipated shock to their cash flows, and (ii) what is the role of financial constraints in mediating such response? To answer these questions, we study the behavior of Chilean firms during the 2019 social unrest, which caused a series of disruptions to the firms' activity over several months. Exploiting quasi-experimental variation in the exposure of firms to these incidents, we find that more exposed firms experience larger declines in sales, larger employment losses, and are more likely to fall behind in their financial obligations than less affected firms. Moreover, these responses are significantly stronger for those firms more likely to be financially constrained. A back-of-the-envelope calculation suggests that constrained firms translate almost half the decline in sales into lower demand for labor and intermediate inputs, more than double the transmission of unconstrained firms.

Resumen

Este artículo trata principalmente dos preguntas: (i) ¿cómo responden las empresas a shocks a su flujo de caja, y (ii) cuál es el rol de las restricciones financieras en esa respuesta? Para responder a estas preguntas, estudiamos el comportamiento de las empresas chilenas durante el estallido social de 2019, que causó una serie de disrupciones en la actividad de las empresas durante varios meses. Explotando una variación cuasi-experimental en la exposición de las empresas a dichos incidentes, encontramos que las empresas más expuestas experimentaron caídas más grandes en ventas, mayores reducciones en el empleo, y fueron más propensas a atrasarse en sus obligaciones financieras que las empresas menos expuestas. Además, estas respuestas fueron significativamente más fuertes para aquellas empresas que con mayor probabilidad se encontraban restringidas financieramente. Un cálculo aproximado sugiere que las empresas restringidas trasladaron casi la mitad de la caída en ventas a una menor demanda por trabajo e insumos, más del doble que las empresas no restringidas.

*The views expressed are those of the author and do not necessarily represent the views of the Central Bank of Chile or its board members. All errors are our own.

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1 Introduction

Firms face uncertain cash flows. There is a vast empirical literature on the links between cash flow fluctuations and firms' decisions, resilience and survival, spanning fields from corporate finance to macroeconomics.[1] However, cash flows, and sales in particular, are also highly endogenous to other outcomes and characteristics of the firms, and potentially predictable to some extent. Thus, a clean identification of the effects of unexpected fluctuations in sales on other firms' outcomes is hard to achieve.

To address the endogeneity of sales, a strand of literature has tried to identify demand shocks. Giroud and Mueller (2017) use arguably exogenous variation in house price changes across US zip codes to identify local shocks to firms' sales during the Great Recession. Importantly to this paper, they find a significant link between the financial situation of firms at the onset of the great Recession and the subsequent adjustment in employment.

The outbreak of COVID-19, and the policies adopted to contain it, brought about significant restrictions to businesses worldwide. It also prompted a rapidly growing literature on the impact of such disruption on firms (see for instance Bloom, Fletcher, and Yeh (2021) and references therein). In a similar vein to our paper, Bartlett III and Morse (2020) analyzes the response of firms in Oakland to the pandemic shock along several real and financial dimensions.

In this paper, we focus on the Chilean social unrest of 2019. Compared to the previous examples, it provides a number of advantages for empirical research. The Chilean social unrest had an impact on firms in at least three ways: directly, through a disruption in their sales, via general equilibrium, through an aggregate decline in economic activity, and through a forward-looking channel, due to revised expectations about the future state of the economy and higher uncertainty. We argue that, as opposed to the house price shock in Giroud and Mueller (2017), but similar to the COVID-19 outbreak, it is possible to isolate an idiosyncratic component of the shock using a firm-level measure of exposure to it, lessening the concern about general equilibrium effects. Relative to the COVID-19 case, we argue that the firm-level degree of exposure to the shock is less correlated with the forward-looking channel. In

[1] E.g. Kaplan and Zingales (1997), Baum, Caglayan, Ozkan, and Talavera (2006), Baum, Caglayan, , and Talavera (2009), Chay and Suh (2009).

the COVID-19 case, a firm's idiosyncratic exposure to the shock is based on the industry it belongs to, the possibility to function remotely, and their reliance on in-person interactions with customers. A protracted pandemic would systematically worsen the prospects of the same firms that were hit harder by the initial outbreak. In the case of the Chilean social unrest, the correlation between current exposure and potential future exposure is less clear, as there is variation in exposure even within sector and among relatively similar firms, whereas medium- and long-run concerns were arguably similar for firms across the board.[2]

Overall, the Chilean social unrest of 2019 provides a unique environment to study the effects of sales disruptions at the firm level. We combine administrative data on sales, intermediate inputs and employment, information from the credit registry, geographical data on the distance of firms to subway stations, and public data on the daily status of subway stations to construct a monthly dataset on firms' exposure to the social unrest shock, economic activity and financial outcomes. Equipped with these data, we address two main questions: (i) how do firms adjust to an unanticipated shock to their cash flows, and (ii) what is the role of financial constraints in mediating their responses?

We find that firms closer to a conflict focus experience larger declines in sales, larger employment losses, and are more likely to fall behind in their financial obligations than less affected firms. Using undrawn credit line balances at the onset of the crisis as a measure of financial slack, we find that these responses are significantly stronger for those firms more likely to be financially constrained.

The rest of the paper is organized as follows. Section 2 highlights the key elements of the 2019 social unrest for our empirical design. Section 3 describes the data used in this study. Our empirical results are presented in section 4. Additional results and robustness checks are discussed in section 5. Section 6 concludes.

2 The 2019 social unrest in Chile as a sales disruption

In October, 2019, a series of students' protests against an increase in Santiago's subway fares escalated into massive, wide-spread, and often violent protests throughout the country. A

[2]In particular, the prospects of reforms and changes in economic policy as a response to the crisis, including an agreement to write a new constitution, would not affect firms based on their proximity to a conflict focus at a particular point in time.

full account of the roots and the development of this episode in Chilean history is beyond the scope of this paper.[3] In this section we address the elements most relevant to our empirical strategy.

First, the riots that took place from the 18$^{\text{th}}$ of October disrupted the sales of many firms throughout the Metropolitan Region of Santiago. Some establishments would interrupt their activities earlier than usual whenever a protest took place close to them. Those which remained open would face increased barriers for customers and suppliers trying to reach them, as either the police or the demonstrators would interrupt pedestrian and motorized traffic in the area. This disruption to the firms' operations and, ultimately, their ability to serve their customers is the focus of our study.

Second, the mass transit system of Santiago, and especially its subway lines, was the epicenter of the initial riots. Many subway stations were damaged in the first few days of large-scale protests, with incidents sprawling to most of the subway network. This is crucial to our analysis for two reasons: first, the distance to an affected subway station can be used to approximate the distance to a conflict focus. Using data from a platform in which users can report incidents as they witness them, Cartes, Asahi, and Fernández (2021) show that the frequency of incidents peaks at subway stations and decays exponentially with the distance from them.[4] Second, the magnitude of the damages to some subway stations in the first days of protests led the subway company to preemptively close down individual subway stations as soon as it became aware of large demonstrations or violent incidents near them. The company would inform about changes in the status of each station in real time via Twitter, providing us with daily data on the location of incidents throughout the Metropolitan Region. Therefore, we can construct a dynamic measure of exposure to the riots based on a firm's proximity to a closed subway station.

Third, the unrest was unanticipated by firms. The magnitude of the events on the 18$^{\text{th}}$ of October, as well as the demonstrators' demands, far exceeded that of the incidents in the week prior. Moreover, what matters for our empirical strategy is that firms could not anticipate the timing and intensity of the events at the time of choosing their location.

[3] See Caroca, Cartes, Davies, Olivari, Rica, and Vogt-Geisse (2020) for a more detailed timeline of the events and an empirical analysis of the evolution of riot intensity over time.

[4] Cartes, Asahi, and Fernández (2021) also confirm in their data the presence of significant incidents throughout the subway network in the first four days of protests.

Location decisions are highly endogenous, and a firm's proximity to a subway station is likely to be correlated to other characteristics. However, to the extent that firms could not have predicted the frequency and intensity of incidents around each specific subway station at the time of choosing their location, we can treat their exposure to conflicts ex-post as a quasi-random assignment.[5]

3 Data [6]

3.1 Firm-level information [7]

We use administrative data on firms from three different sources. Information on total revenue from sales and intermediate input purchases comes from the tax authority (SII, *Servicio de Impuestos Internos* or Chilean IRS); specifically, from the VAT form submitted monthly by Chilean firms.[8]

We combine this information with the bank credit registry maintained by the financial market supervisor (CMF, *Comisión para el Mercado Financiero*). We use two sets of records: on one hand, we have access to a set of stock variables, which includes debt, undrawn credit lines and arrears. On the other hand, we can also examine loan origination, including rates, amounts, and an indicator for new loans and renegotiations. In both cases, we use this information at the firm-month level.

Finally, we use information from the Unemployment Insurance System (UI) collected by the Chilean pension supervisor (SP, *Superintendencia de Pensiones*). The data set contains anonymized monthly employment information for approximately 5 million formal workers in 2019. The Chilean UI covers all formal dependent workers above 18 years who work

[5]Our baseline analysis excludes subway stations that were damaged in October 18th and kept closed throughout our sample period, as they don't provide time variation in the treatment. This includes the station located at the square that hosts most demonstrations in Santiago. Therefore, our results are not driven by firms located near the place where demonstrations were most likely to occur before October 2019.

[6]This study was developed within the scope of the research agenda conducted by the Central Bank of Chile (CBC) in economic and financial affairs of its competence. The CBC has access to anonymized information from various public and private entities, by virtue of collaboration agreements signed with these institutions.

[7]To secure the privacy of workers and firms, the Central Bank of Chile (CBC) mandates that the development, extraction and publication of the results should not allow the identification, directly or indirectly, of natural or legal persons. Officials of the CBC processed the disaggregated data. All the analysis was implemented by the authors and did not involve nor compromise the CBC or the institutions that share their data with the CBC.

[8]The information contained in the databases of the Chilean IRS is of a tax nature originating in self-declarations of taxpayers presented to the Service; therefore, the veracity of the data is not the responsibility of the Service. All SII's forms descriptions are available at www.sii.cl.

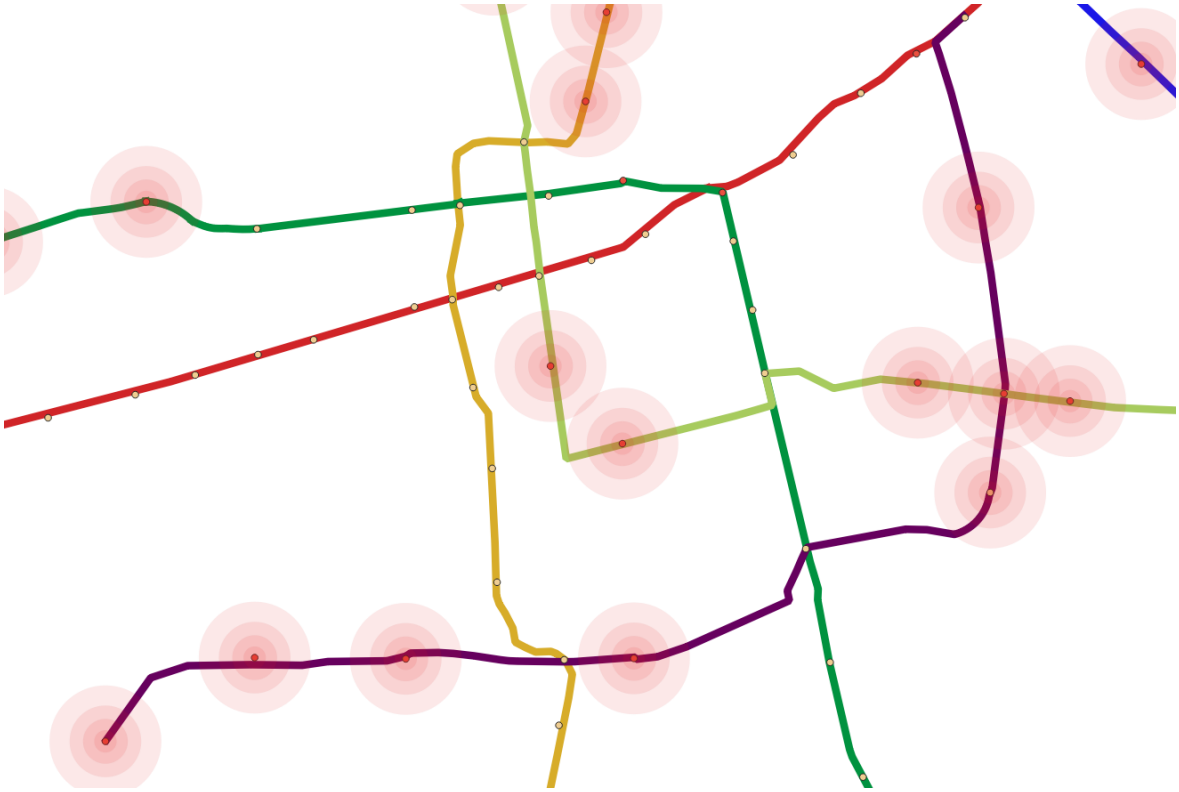
in the private sector. We obtain firm-level information on the number of employees and the total wage bill in each firm. The information available allows us to distinguish between workers with either permanent or temporary employment contracts at the monthly-frequency. Therefore, we work with 6 monthly variables from this source, aggregated at the firm level: total number of workers, number of fixed-term workers, number of permanent workers, total wage bill, wage bill of fixed-term workers and wage bill of permanent workers.

3.2 Geographic information

As discussed in section 2, the social unrest of October 2019 in Chile was characterized by multiple foci of conflict and transportation disruption throughout the Metropolitan Region. The location and timing of these incidents provides variation in exposure to the conflict at the firm-month level. In order to exploit this variation, we first geocode all the subway stations in Santiago. Then, we use the information about station shutdowns provided by the company managing the subway system (*Metro de Santiago*). This company published in *Twitter*, in real time, information about individual stations being closed (and re-opened) due to their proximity to a conflict focus. We used web-scraping to collect this information between October 2019 and January 2020. Figure 1 shows the layout of subway network at an arbitrary day: colored lines depict the subway lines, and white dots represent the stations. Red dots indicate stations that were closed on a particular day, during business hours, due to incidents taking place in their vicinity. Changes in this pattern over time provides us with time variation in the location of conflict foci.

Next, we compute the minimum distances between each firm and a conflict focus for each day, and use this information to construct our time-varying measure of exposure to the shock. Specifically, for each day and firm, we obtain the minimum distance between a closed subway station and the firm. We then average the minimum distances for each firm during a given month, obtaining a monthly measure of exposure to the shock. We attempted to construct this measure for all firms in the Metropolitan Region, (about 545,000 firms), although it was not possible to obtain a distance measure for all of them. Our final database of exposure covers 86% of all firms in the Metropolitan Region.

Figure 1: Subway network and closed stations



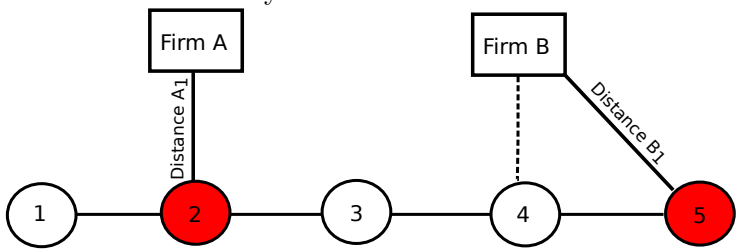
Partial layout of the subway network in Santiago de Chile. Red dots represent subway stations closed during business hours due to nearby incidents, on an arbitrary day during Chilean the social unrest of 2019.
Source: Own elaboration from public information on subway stations' location and status.

Figure 2 illustrates the identification strategy through two hypothetical firms, labeled A and B, in the vicinity of a subway line. The circles represent subway stations (labeled 1 through 5): white circles correspond to open stations, while closed stations are marked in red. Panel A shows the situation on day 1, when stations 2 and 5 are closed. The exposure to conflict of firm A in day 1 is measured by *Distance* A_1 , the distance between firm A and the nearest closed station (station 2). In turn, the exposure of firm B is measure by its distance to station 5 (*Distance* B_2). Notice that firm B is closer to station 4 than station 5, however, station 4 is open, and therefore its distance to firm B (represented by a dashed line) is not relevant to measure the exposure of firm B to the shock. Panel B shows the situation on a different day, in which all stations are open except for station 3. Hence, the measure of exposure for firms A and B on day 2 are now *Distance* A_2 and *Distance* B_2, respectively. Again, the distance to the nearest stations (dashed lines) are not relevant, as

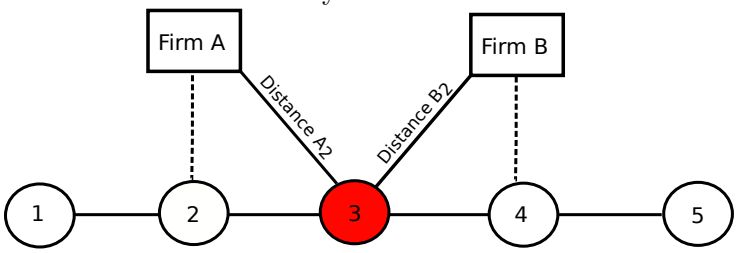
those stations remain open. Finally, to obtain our monthly measure of exposure for firms A and B, we would calculate the monthly averages $Exposure_{A,t} = \frac{1}{D_t}\sum_{d=1}^{D_t} DistanceA_{d,t}$ and $Exposure_{B,t} = \frac{1}{D_t}\sum_{d=1}^{D_t} DistanceB_{d,t}$ respectively, with D_t equal to the number of days in a given month t.

Figure 2: EXAMPLE: SUBWAY STATIONS' STATUS AND DAILY DISTANCE TO A CONFLICT FOCUS

Panel A. Day 1: stations 2 and 5 are closed



Panel B. Day 2: station 3 is closed



Firms represented by rectangles and subway stations represented by circles. Circles shaded red correspond to close stations, while white circles are closed stations. Solid lines depict the relevant distances between firms and the nearest closed station. Dashed lines show the distance between a firm and the nearest station regardless of its status.

3.3 Sample and definitions

In order to work with our monthly administrative records, we aggregate the daily records of distance to conflict information by computing the monthly average. We keep firms that have a maximum distance of 7.5 km to a closed station between October 2019 and January 2020. This removes firms in more rural *comunas* and, more generally, firms that are too far away from the subway network, since any conflict focus near them would not be accurately captured by our strategy.

We exclude corporations, which have access to other sources of funding besides the local banking system. We also remove firms in the public sector, as well as other economic sectors

that are not relevant for our analysis, such as education, agriculture, mining, and utilities. These are sectors for which the headquarters location is unlikely to coincide with the location of their economic activity, or whose revenue would not fluctuate on a daily basis according to the incidence of conflicts in the streets.

Sales and intermediate input purchases are measured by their interannual growth rates at the monthly frequency. Employment is measured by the interannaul growth rate in the number of employees. We consider total employment as well as separate measures for workers with open-ended and fixed-term contracts.[9]

To study the financial impact of the 2019 social unrest, we build indicators for firms that have at most 30-day arrears, between 31-day and 90-day arrears, and firms in default (more than 90 days behind in their payments). We also construct indicators for firms obtaining new commercial loans and firms renegotiating existing loans within a given month. In addition, we construct two variables characterizing the financial situation of firms at the onset of the social unrest: leverage and an indicator of financial constraints. We define leverage in period t as the ratio between total outstanding debt at t and total sales in the 12 months prior to t. We also construct an indicator for firms above the 75^{th} percentile of the leverage distribution. Our measure of financial constraints is an indicator of whether the firm has any undrawn credit line balances left at the beginning of the conflict.[10] Our choice in this regard is rooted in the existing literature. Campello, Gambona, Graham, and Harvey (2011) show that firms with limited access to credit lines face starker trade-offs during a crisis, while firms with better access are able to smooth the shock more easily. Campello, Graham, and Harvey (2010) document that firms with more limited access to other forms of credit are more likely to draw down their credit line balances in response to a crisis. In turn, Greenwald, Krainer, and Paul (2020) provide evidence that firms rely heavily in credit lines balances when facing adverse shocks; in fact, according to the authors, credit line drawdowns explain most of the increase in bank credit to firms in response the COVID-19 outbreak and other aggregate shocks.

[9] As a robustness check, we also used the growth rate in the total wage bill, for all employees and for each contract type, as an alternative measure of labor adjustments. The results are very similar to the ones in our main exercise, and omitted for brevity.

[10] We cannot observe used credit line balances separately from other sources of debt, nor total credit line limits. Therefore, we are unable to construct alternative measures, such as the percentage of drawn balances relative to total limits.

Table 1: Firm characteristics in September 2019

Statistics:	Mean	s.d.	P25	P50	P75	P90	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. *Full sample, pre-shock*							
Av. min. distance (km)	2.661	1.541	1.492	2.268	3.574	5.288	47,223
Sales (in 10^6 of pesos)	3,617	146,839	65	182	606	2,231	47,223
Debt (in 10^6 of pesos)	425	5,085	0	3	43	218	47,223
Available Credit line (in 10^6 of pesos)	102	4,019	1	4	13	35	47,223
Debt/sales	0.331	1.395	0.000	0.026	0.160	0.476	47,223
Av. credit line/sales	0.069	0.207	0.004	0.021	0.058	0.135	47,223
Arrears (≤ 30)	0.069	0.254	0.000	0.000	0.000	0.000	47,223
Arrears ($31-90$)	0.027	0.162	0.000	0.000	0.000	0.000	47,223
Arrears (90+)	0.017	0.128	0.000	0.000	0.000	0.000	47,223
Employment	30.617	266.289	1	3	11	35	47,223
Employment: permanent	22.951	228.485	1	3	9	26	47,223
Employment: temporary	7.667	82.592	0	0	1	6	47,223
Panel B. *Financially constrained*							
Av. min. distance (km)	2.637	1.566	1.432	2.223	3.590	5.333	6,192
Sales (in 10^6 of pesos)	2,916	22,570	38	110	419	2,830	6,192
Debt (in 10^6 of pesos)	835	6,325	1	7	60	449	6,192
Available Credit line (in 10^6 of pesos)	0	0	0	0	0	0	6,192
Debt/sales	0.870	2.434	0.008	0.089	0.407	1.983	6,192
Av. credit line/sales	0.000	0.000	0.000	0.000	0.000	0.000	6,192
Arrears (≤ 30)	0.157	0.364	0.000	0.000	0.000	1.000	6,192
Arrears ($31-90$)	0.089	0.285	0.000	0.000	0.000	0.000	6,192
Arrears (90+)	0.079	0.269	0.000	0.000	0.000	0.000	6,192
Employment	32.100	356.630	0	2	6	25	6,192
Employment: permanent	22.203	304.395	0	1	5	19	6,192
Employment: temporary	9.897	90.290	0	0	1	4	6,192
Panel C. *High leverage*							
Av. min. distance (km)	2.622	1.539	1.435	2.218	3.523	5.233	9,690
Sales (in 10^6 of pesos)	1,826	11,620	57	180	564	2,055	9,690
Debt (in 10^6 of pesos)	1,697	10,486	37	112	353	1,702	9,690
Available Credit line (in 10^6 of pesos)	95	4,618	0	5	16	42	9,690
Debt/sales	1.469	2.801	0.298	0.463	1.009	3.645	9,690
Av. credit line/sales	0.086	0.262	0.001	0.022	0.066	0.163	9,690
Arrears (≤ 30)	0.160	0.367	0.000	0.000	0.000	1.000	9,690
Arrears ($31-90$)	0.066	0.248	0.000	0.000	0.000	0.000	9,690
Arrears (90+)	0.041	0.199	0.000	0.000	0.000	0.000	9,690
Employment	19.530	152.724	0	3	9	27	9,690
Employment: permanent	14.866	122.835	0	2	8	22	9,690
Employment: temporary	4.663	74.539	0	0	1	5	9,690

Our final sample contains all firms in the Metropolitan Region for which we could successfully calculate our measure of exposure to social conflict, are less than 7.5km away from a conflict focus, and have enough information on sales to calculate the year-over-year growth rates over the relevant time frame. We consider two time intervals: the first one, from April 2019 to January 2020, is used for an event-study type of analysis, spanning pre- ans post-conflict months. The second one, between October 2019 and January 2020, is used in our regression specifications, which require the distance to conflict to be defined for every month. These regressions include firm-level controls defined in September 2019, though. It should be noted that in both exercises we use the same firms.

Table 1 shows the characteristics of firms in our sample in September 2019, before the conflict started. We display the composition of our sample in terms of leverage and financial constraints the table 2. In addition, we provide information on firms grouped under the "commerce" sector, which comprises hotels, restaurants, and retail firms. We use this group of firms in our robustness analysis, under the assumption that firms in these sectors should be more sensitive to the kind of disruption that we are studying.[11].

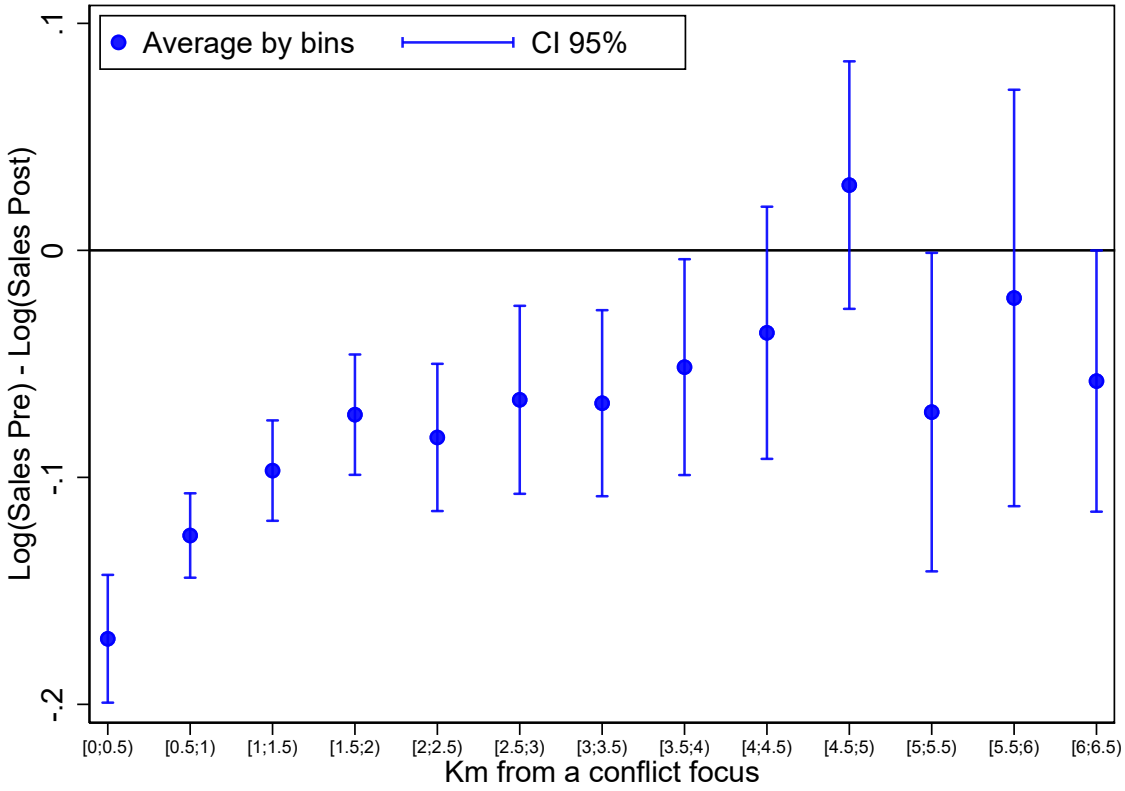
Table 2: Sample composition

Statistics:	Full sample	Commerce sector
	(1)	(2)
Total firms	47,223	16,074
Firms with financial constraints	6,192	1,960
Firms with high leverage	9,690	3,521
Firms with high leverage and financial constraints	2,144	560

Figure 3 illustrates the link between sales and exposure to the social conflict as defined above. We group firms based on their average distance to a conflict focus in October 2019, in 500-meter intervals. Each point in the figure represents the average growth rate of sales between September and November for all firms in one of these groups. A clear increasing pattern emerges from this plot: on average, firms further away from conflict foci experienced milder declines in sales during the first months of unrest. We explore this relationship, as well as the response of firms to declining sales, in the next section.

[11]Separate descriptive statistics for this group of firms are provided in table 10 in appendix A.2

Figure 3: Distance from conflict focus and the growth rate of sales



4 Empirical analysis

4.1 Event-Study

We begin our empirical analysis by performing an event study of the effect of the 2019 riots on sales and other variables of interest. We use the sample described in section 3.3. To avoid possible seasonal effects, we construct year-over-year growth rates of monthly outcome variables. For this exercise, October 2019, the month where the riots begun, is taken as the event, and the intensity of exposure to the event is measured as the average of our daily measure of exposure for the period October 2019 - January 2020.

More precisely, we estimate the equation (1)

$$\Delta Sales_{it} = \alpha_i + \beta_t + \sum_{k=-5}^{4} \gamma_k MeanDist_i \times \mathbb{1}(k = t) + \varepsilon_{it} \quad (1)$$

$$\gamma_{-1} = 0 \quad (2)$$

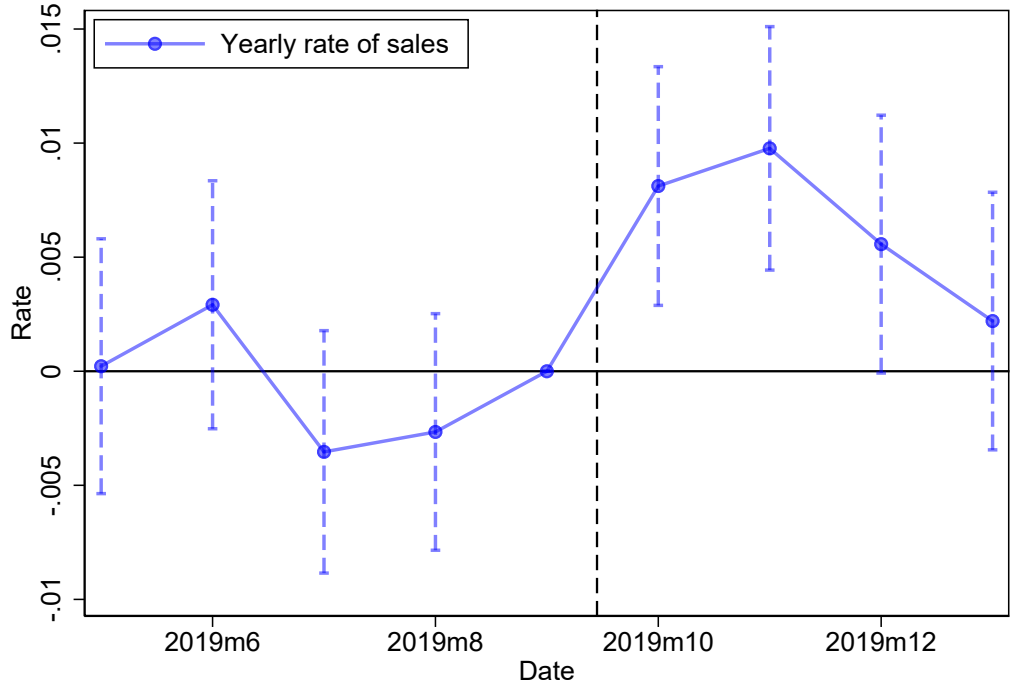
where $MeanDist_i$ is the average of the daily minimum distances to a conflict focus to which a firm i was exposed between October and January, α_i is a firm fixed effect, and β_t is a time effect. We focus our attention on the coefficients γ_k, which capture the marginal effect of being further away from a conflict focus on the growth rate of sales at different points in time. Negative values of k represent any pre-trend on the outcome variable that may have existed for firms further away from conflicts ex-post, while positive values of k capture the impact of the event at different horizons. The impact on September 2019 ($k = -1$), the last month before the conflict started, is normalized to zero. A positive γ implies that larger distances are associated with a higher growth rate of the outcome variable; that is, being closer to a conflict focus correlates with lower outcomes.

Figure 4 shows the results with sales as the outcome variable. Each point in the figure represents a γ_k, and the dashed lines represent confidence intervals of two standard errors.[12] There is a significant effect of the distance to closed subway stations on sales. It is particularly strong at the beginning of the social unrest period, declining from December 2019, and no longer statistically significant in January 2020. Moreover, we don't find evidence of any pre-trend separating the growth rate of sales for firms further away from conflicts from those nearer to them. This is perhaps unsurprising given the inclusion of firm fixed effects, but it is reassuring for the interpretation of the results after October 2019.

A limitation of the exercise presented in this section is that it considers the social unrest of 2019 as a single event that took place in October 2019, with consequences lasting potentially for several periods. However, this epsiode is better characterized as a series of riots spread over several months, with varying frequency and intensity. Hence, this preliminary analysis is contaminated by the presence of new shocks in later months. In the next section, we turn to monthly regressions where the measure of exposure for each firm varies from month to month.

[12] Standard errors are clustered at the firm level.

Figure 4: Event study - Sales



Number of firms 46221. Total sample 372.10 in thousands.

4.2 Monthly regressions

Our main analysis stems from a set of regressions of several outcomes of interest on the firms' exposure to riots. For continuous outcomes, we run the linear regression

$$\Delta Y_{it} = \alpha + \beta X_i + \tau_t + \gamma MeanDist_{it} + \delta MeanDist_{it} \times D_i + \varepsilon_{it} \quad (3)$$

where ΔY_{it} is the year-over-year growth rate of a variable of interest, such as sales, intermediate inputs, employment, debt and undrawn credit line balances. X_i is a set of firm-level controls, including a discrete measure of firm size[13], sector dummies, employment level, leverage[14], and undrawn credit line balances relative to sales. All time-varying controls are measured in September 2019, before the first incident, and therefore constant over time for a given firm in our regressions. In turn, τ_t captures month-year fixed effects, $MeanDist_{it}$ is

[13] We consider 4 sizes based on total sales in the year before the conflict.
[14] We use debt over sales as our measure of leverage

the average distance of firm i to a conflict focus during month ta, and D_i is an interaction variable to capture potential heterogeneity in the effect of the conflict on firms' outcomes. In particular, we consider interactions with a measure of financial constraints, leverage, and an indicator for firms belonging to the commerce sector. [15]

4.2.1 Frims' adjustments in response to the shock

The table (3) presents our main results regarding the response of firms to a sales disruption. Column (1) confirms the main mechanism, as hinted by figure 3: there is a positive and statistically significant effect of being further away from conflict on total sales. Columns (2) to (4) show only marginally significant, positive effects on available credit line balances and expenditures in intermediate inputs. However, columns (5) to (7) show strong link between exposure to conflict and employment losses, especially among workers under temporary contracts. Quantitatively, our point estimates imply that a firm at the 25$^{\text{th}}$ percentile of the distribution of distance to conflict (1.5 km) would experience 1 additional percentage point of total employment decline as a consequence of the shock, relative to an identical firm at the 75$^{\text{th}}$ percentile (3.6 km). For temporary contracts, the difference is amplified to 1.9 percentage points.

Table 3: Adjustmens: baseline estimation

Dep. Variable:	Sales	Debt	Credit line	Materials	Employment		
					All	Perm.	Temp.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. *Baseline estimations*							
$MinDist_{it}$	0.009***	0.001	0.003*	0.003*	0.005***	0.004***	0.009***
	(0.001)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.003)
Constant	-0.028***	-0.046***	-0.015	-0.022**	0.011	0.009	0.067**
	(0.009)	(0.016)	(0.011)	(0.010)	(0.008)	(0.008)	(0.028)
Observations	167974	117994	137843	158249	124198	121269	43004

[15] Detailed summary statistics for the variables used in our regressions can be found in table 9 in appendix .

4.2.2 Financial constraints

We turn our attention to the role of financial constraints in mediating the response of firms to the social unrest shock. Table 4 show the results of an extended specification that includes a potential measure of financial constraints: in panel A, we use the indicator based on credit line balances, as defined in section 3; in panel B, we use the high leverage dummy. Several results stand out. First, firms in the high leverage group adjust real variables (namely, employment), as much as the rest of our sample. If anything, they seem to increase their debt levels more as their exposure to a conflict focus increases, a result we do not find for the sample as a whole. We conjecture that these results, taken together, my imply that highly-levered firms are simply firms with better access to bank credit, both before and after the crisis. Panel A tells a different story for firms without available credit line balances. Such firms experience a significant decrease in their purchase of intermediate inputs (column 4), which we do not find for the whole sample, and much stronger employment losses as a consequence of their exposure to the social conflict. These results are consistent with the role of credit lines as buffers to shocks highlighted by Greenwald, Krainer, and Paul (2020). They are also consistent with the behavior of firms with limited access to credit lines in Campello, Gambona, Graham, and Harvey (2011). In their paper, credit line balances appear to be a direct substitute for cash; in times of reduced cash flows, firms with limited access to credit lines are forced into larger reductions in investment. Our estimates suggest that they may find it difficult to finance working capital as well.

The amplification effect of financial constraints are not only statistically significant, but also economically sizable. Our point estimates for the response of employment to the exposure to the shock for constrained firms is three times as large as the one found for the full sample, and the difference is even bigger for temporary employment. For a financially constrained firm, moving from the third quartile of the distance to conflict distribution to the first imply 2.1 additional percentage points of employment reduction compared to the equivalent thought experiment for an unconstrained firm. In the case of temporary employment, the constrained firm would reduce the number of workers by 4 percentage points more than the unconstrained firm. Thus, the total difference in employment reduction between identical firms with depleted credit lines located at the 25^{th} and the 75^{th} percentiles of the distance to conflict distribution is

2.9 percentage points for total employment and 5.4 percentage points for temporary contracts. Moreover, the firms closer to the conflict would experience a decline 2.9 percentage points steeper in purchases of intermediate inputs relative to those further away, an effect that is absent for unconstrained firms.

Table 4: Financial constraint and leverage

Dep. Variable:	Sales	Debt	Credit line	Materials	Employment		
					All	Perm.	Temp.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. *Firms financially constraint*							
$MinDist_{it}$	0.007***	0.001	0.002	0.001	0.004***	0.004***	0.007**
	(0.001)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.003)
FC_{it}	-0.095***	-0.217***	-0.236***	-0.104***	-0.063***	-0.056***	-0.046
	(0.013)	(0.017)	(0.069)	(0.015)	(0.012)	(0.012)	(0.032)
$FC_{it} \times MinDist_{it}$	0.014***	0.002	0.039*	0.014***	0.010***	0.006*	0.019**
	(0.004)	(0.005)	(0.021)	(0.004)	(0.004)	(0.004)	(0.009)
Constant	-0.011	0.007	-0.011	-0.004	0.020**	0.017**	0.072**
	(0.009)	(0.017)	(0.011)	(0.011)	(0.008)	(0.008)	(0.029)
Observations	167974	117994	137843	158249	124198	121269	43004
Panel B. *Firms with high leverage*							
$MinDist_{it}$	0.007***	0.005*	0.002	0.001	0.005***	0.005***	0.009***
	(0.001)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.004)
$Leverage_{it}$	-0.048***	0.190***	-0.077***	-0.060***	-0.027***	-0.030***	-0.001
	(0.009)	(0.014)	(0.013)	(0.012)	(0.008)	(0.008)	(0.024)
$Leverage_{it} \times MinDist_{it}$	0.006**	-0.009**	0.001	0.007*	-0.001	-0.003	-0.004
	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.002)	(0.008)
Constant	-0.019**	-0.096***	-0.002	-0.011	0.016*	0.014*	0.067**
	(0.009)	(0.017)	(0.011)	(0.011)	(0.008)	(0.008)	(0.029)
Observations	167974	117994	137843	158249	124198	121269	43004

4.3 Back-of-the-envelope decomposition of the response of firms to the shock

The results presented so far are informative of the effect of the intensity of exposure to the social unrest shock into several firm-level outcomes. However, they do not map directly into a measure of the transmission from a decline in sales into the behavior of firms. In this subsection, we provide a back-of-the-envelope calculation of the transmission of a shock to sales into the firm's demand for inputs. Our calculations are based on the fitted values of changes in sales, purchases of intermediate inputs and total wage bill based on the coefficients of the regressions in 3, with those outcomes as dependent variables. The outcome variables in our regressions are expressed as interannual growth rates. Therefore, we obtain the change in pesos implied by our results in two steps: first, we compute the fitted value for the growth rate of sales, purchases and the wage bill for each firm using the coefficient in table 3 and the firm's distance to conflict. Then, using the levels of the variables for the same firm one year prior, we construct the implied change in the level of the variables for that firm due to the social unrest shock. Finally, we use the change in the level of sales to normalize the change in the levels of demand for inputs, obtaining an approximate measure of the change in demand per peso of disrupted sales at the firm level. Table 12 in appendix A.4 report the central moments of the distribution of fitted values and the per-peso calculations, for the full sample as well as subsamples based on the availability of credit lines.[16] The mean of the obtained distribution is heavily influenced by outliers with either too small changes in sales or too small changes in the demand for inputs. The former are particularly troublesome, as they generate runaway values for the ratio of the change in input demand to the change in sales. Hence, we use the median values for this exercise.

Taking the result of our back-of-the-envelope calculation at face value, the median response to a $1 shock to sales in our sample is to cut the purchase if intermediate inouts by 14 cents, and to reduce the wage bill by 11 cents. Together, this figures imply a transmission of 25% of the shock into lower demand for inputs. However, only the transmission to labor demand was statistically significant in our baseline regression.

When we separate our sample into constrained and unconstrained firms using our pre-

[16] To calculate a peso-demand for labor, we used an alternative specification with the growth rate of the wage bill as the dependent variable, as opposed as the number of workers, as in our baseline specification.

ferred measure based on undrawn credit line balances, we find that unconstrained firms have a more muted response, passing through 19% of the shock to their demand for inputs, of which only the 11-cent reduction in their wage bill is statistically significant. On the other hand, constrained firms reduce their overall demand for inputs by 45 cents for every peso of sales decline. The bulk of the additional transmission from sales to input demand comes from lower purchases of intermediate inputs, an effect that is statistically significant for constrained firms.

While crude, these calculations suggest that unconstrained firms are able to absorb between 81 and 89 percent of a sales shock without cutting back their demand for inputs, while constrained firms translate almost half the shock to sales into lower demand for inputs, further transmitting the shock to their suppliers and workers.

4.3.1 Repayment behavior and loan origination

Finally, we look into the repayment behavior of firms in response to the sales disruption of 2019. Prior correlational studies for Chile have found a robust association between cash flows, and sales in particular, and firm arrears or default (e.g. Zurita (2008), Fernández and Vásquez (2019), Castro, Cerletti, Fernández, and Vásquez (2019)). We seek to test this result using our quasi-experimental setup. Furthermore. we want to assess the importance of financial constraints in shaping the transmission of a shock to sales into arrears and default. To that end, we estimate a series of nonlinear regressions to capture the effect of the exposure to a conflict focus on a number of discrete choices made by firms.[17] In particular, we estimate probit models of the form:

$$Y_{it} = \mathbb{1}\left(\beta_i X_i + \tau_t + \gamma MeanDist_{it} + \varepsilon_{it} > 0\right) \tag{4}$$

where Y_{it} is an indicator of interest: whether the firm transitions from being on time on its debt payments to being up to 30 days late, whether the firm transitions from being up to 30 days late to being up to 90 days late, whether the firm transitions from being up to 90 days late to default, whether the firm obtained a new loan, and whether the firm renegotiated an existing loan during month t. X_t is the same set of controls used in our linear regressions.

[17] As a robustness check, we provide the results of a linear probability model estimation in appendix A.3.

Table 5: PROBIT - MARGINAL EFFECTS

Dependent Variable:	Arrears			Renegotiation	New credit
	$30 <$	$31 - 90$	$90+$		
	(1)	(2)	(3)	(4)	(5)
Panel A. *Baseline*					
$MinDist_{it}$	-0.001***	-0.007***	0.002	-0.000**	-0.000*
	(0.000)	(0.003)	(0.004)	(0.000)	(0.000)
Observations	152706	7421	2663	168010	168010
Panel B. *Firms financially constrained*					
$MinDist_{it}$	-0.001**	-0.006**	0.000	-0.000**	-0.000**
	(0.000)	(0.003)	(0.004)	(0.000)	(0.000)
FC_i	0.025***	0.052***	-0.005	-0.000	-0.004**
	(0.002)	(0.020)	(0.025)	(0.001)	(0.002)
$FC_i \times MinDist_{it}$	-0.003***	-0.004	0.008	0.000	0.001
	(0.001)	(0.007)	(0.008)	(0.000)	(0.001)
Observations	152706	7421	2663	168010	168010
Panel C. *Firms with high leverage*					
$MinDist_{it}$	-0.001***	-0.011***	0.010**	-0.001***	-0.000**
	(0.000)	(0.004)	(0.005)	(0.000)	(0.000)
$Leverage_{it}$	0.034***	-0.019	0.058***	0.003***	0.014***
	(0.002)	(0.016)	(0.022)	(0.001)	(0.001)
$Leverage_{it} \times MinDist_{it}$	0.000	0.010*	-0.018**	0.001***	0.000
	(0.001)	(0.005)	(0.007)	(0.000)	(0.000)
Observations	152706	7421	2663	168010	168010
Panel D. *Only commerce sector*					
$MinDist_{it}$	-0.001***	-0.007**	-0.003	-0.000	-0.000
	(0.000)	(0.003)	(0.005)	(0.000)	(0.000)
$Commerce_{it}$	0.016***	-0.017	0.033	0.002**	0.007***
	(0.003)	(0.030)	(0.044)	(0.001)	(0.002)
$Commerce_{it} \times MinDist_{it}$	0.001*	0.001	0.013*	-0.000*	-0.000
	(0.001)	(0.005)	(0.008)	(0.000)	(0.000)
Observations	152706	7421	2663	168010	168010

Table 5 shows the marginal effects obtained from our probit estimates. Panel A shows a significant, negative effect of distance to a conflict focus on the probability of falling in arrears (columns 1 and 2). In other words, firms more exposed to a conflict focus are more likely to fall behind in their financial obligations. Given the rare occurrence of arrears and default in our sample,[18] the effect, while small, is not negligible: the marginal effect is 0.1 percentage points, relative to a baseline probability of falling in arrears of 2.9%. The effect is quantitatively larger for firms already in arrears of up to 30 days, although this is a smaller group of more financially distressed firms. The result on arrears is stronger for financially constrained firms when using our preferred measure of financial constraints. The overall marginal effect for these firms is four times as large as the baseline estimate (panel B, column 1). We don't find any additional effect of financial constrains on the probability of obtaining or renegotiating a loan in the short run. Panel C reinforces our interpretation of high-leverage firms as firms with better access to credit. They are marginally more likely to renegotiate a loan as a consequence of being further away from conflict. However, for the sub-sample of firms already in 31- to 90-day arrears, they also seem more likely to default as a response to being more exposed to the social conflict.

5 Additional results and robustness checks

In this section, we conduct some additional analysis to assess the robustness of our main results.

5.1 Placebo test of baseline estimations

The first concern we want to address is a potential lack of time variation in our measure of exposure to the shock at the firm level. Without time variation, our measure of exposure could simply be capturing the location of firms relative to subway stations, a decision that is potentially endogenous to other decisions made by the firm. If so, our results would not stem form sales disruption triggered by the social unrest of 2019, but rather from systematic, preexisting differences across firms. We argue that, in that case, our main estimates would

[18] Notice that we are measuring transition probabilities from one repayment state to the immediate worse state. Therefore, as arrears and default are scarce among our sample of firms, the number of observations falls drastically between columns (1) and (3).

not pass a placebo test: we would obtain similar results when using our measure of exposure to the conflict in a different time period. This is the exercise we report in table 6: we repeat the exercise in table 3 one year earlier. Specifically, we fixed all firm-level controls at their September 2018 values, and we look at outcomes between October 2018 and January 2019, using for each month the measure of exposure corresponding to the same month one year later. The results in table 6 are reassuring: we are unable to find any of the effects in our baseline estimation in this alternative, counterfactual setting. Hence, at the very least, our results are not spuriously driven by time-invariant or preexisting determinants of the location choices of Chilean firms.

Table 6: Placebo test: October 2018-January 2019

Dependent Variable:	Sales	Debt	Credit line	Materials	Employment		
					All	Permanent	Temporary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. *Baseline estimations*							
$MinDist_{it}$	0.0016	-0.0020	0.0004	0.0028*	0.0018	0.0011	0.0019
	(0.0014)	(0.0026)	(0.0016)	(0.0017)	(0.0013)	(0.0012)	(0.0033)
	[0.2459]	[0.4462]	[0.8104]	[0.0975]	[0.1412]	[0.3763]	[0.5626]
Constant	-0.0109	0.0087	-0.0919***	-0.0088	-0.0042	-0.0020	-0.0184
	(0.0092)	(0.0171)	(0.0108)	(0.0110)	(0.0088)	(0.0088)	(0.0300)
	[0.2352]	[0.6116]	[0.0000]	[0.4255]	[0.6306]	[0.8164]	[0.5398]
Observations	157071	109757	129789	147937	117556	114685	42434

5.2 Retail firms, hotels and restaurants

Given the nature of the shock, we conjecture that some sectors would experience a steeper decline in sales, due to their reliance on in-person interactions with customers. In particular, we focus our attention on hotels, restaurants and retail stores, which we label as the "commerce" sector. Table 7 reports the results of our baseline estimation when augmented with an interaction term between an indicator of the commerce sector and our measure of exposure to social conflict. Column (1) shows that, indeed, firms in these industries were more severely affected by their proximity to a conflict focus: compared to the rest of the sample, their sales are three times as sensitive to the exposure to the shock. Noticeably, we don't

find evidence of additional adjustments in employment or debt, and only mildly significant evidence of larger adjustments in intermediate inputs. It could be interesting to delve deeper into the apparent resilience of firms in these sectors, although such analysis is beyond the scope of this paper.

Table 7: Retail, hotels and restaurants

Dependent Variable:	Sales	Debt	Credit line	Materials	Employment		
					All	Permanent	Temporary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. *Retail, hotels and restaurants*							
$MinDist_{it}$	0.005***	-0.000	0.002	0.001	0.005***	0.005***	0.006
	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.004)
$Retail_{it}$	-0.015	0.051***	0.016	0.009	0.026***	0.040***	-0.084***
	(0.010)	(0.018)	(0.012)	(0.011)	(0.008)	(0.008)	(0.023)
$Retail_{it} \times MinDist_{it}$	0.012***	0.005	0.004	0.006**	0.000	-0.001	0.008
	(0.002)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.006)
Constant	-0.018**	-0.041**	-0.012	-0.017	0.011	0.008	0.073**
	(0.009)	(0.017)	(0.011)	(0.011)	(0.008)	(0.008)	(0.029)
Observations	167974	117994	137843	158249	124198	121269	43004

5.3 Multi-establishment firms

Finally, we address another major threat for our empirical strategy: our measure of exposure relies on the distance between incidents and the headquarters of a firm acting as a proxy for the disruption of that firm's activities. However, for firms comprising multiple establishments, this measure may be a poor approximation to their effective overall exposure to the events of 2019. Unfortunately, we do not have access to the detailed establishment location data we would need to construct an establishment-level measure of exposure to the shock. However, we can use information from another administrative record, the electronic invoice system, provided by the tax authority. These records do not provide the precise location of establishments, but include firm and *comuna* identifiers.[19] Therefore, we can identify firms with presence in more than one *comuna* as multi-establishment firms. Thus, as a robust-

[19] A *comuna* is a municipal administrative district. The Metropolitan Region of Santiago comprises 52 *comunas*.

ness check, we repeat our baseline regressions excluding all firms known to have multiple establishments.

Table 8: Without multi-establishment firms

Dependent Variable:	Sales	Debt	Credit line	Materials	Employment		
					All	Permanent	Temporary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. *Baseline estimations without multi-establishment firms*							
$MinDist_{it}$	0.008***	0.004	0.004**	0.004**	0.006***	0.005***	0.011***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)	(0.004)
Constant	-0.034***	-0.052***	-0.011	-0.019	0.015	0.014	0.065*
	(0.011)	(0.018)	(0.013)	(0.012)	(0.010)	(0.010)	(0.033)
Observations	117584	85507	96511	114248	90487	88260	33427

The results are presented in table 8. They almost identical to those in table 3. If anything, there is a more precisely estimated, positive effect of distance to conflict on undrawn credit line balances and intermediate input purchases. In other words, focusing on firms for which the legal address is more likely to capture the location of their economic activity, we find that firms more exposed to the shock tend to cut back their purchases of intermediate inputs more, and to draw down their credit line balances more.

6 Conclusions

We present new evidence on the response of firms to unexpected shocks to sales. Exploiting the unique characteristics of the Chilean social unrest of 2019, we are able to construct an arguably exogenous measure of idiosyncratic exposure to the shock that varies across firms and over time, overcoming many of the identification challenges faced by the existing literature.

Using our measure of exposure to the shock, we find that firms adjust their employment levels downwards in response to a transitory decline in sales. This response is significantly stronger for firms more likely to be financially constrained: firms without undrawn credit line balances left exhibit larger declines in employment, and also reduce their expenditures in intermediate inputs. This is consistent with previous studies, which find that credit lines

act as liquidity buffers to smooth shocks to cash flows, and that firms with limited access to credit lines are unable to find alternative means to ease these shocks in the short run. Quantitatively, we find that constrained firms pass through almost half the shock to sales into lower demand for intermediate inputs and labor, while unconstrained firms are able to absorb almost 90% of the shock without changing their demand for inputs.

We find some evidence of higher financial distress as a consequence of sales shocks: firms more directly exposed to the shock are more likely to fall behind in their financial obligations. The magnitude of the effect is small, albeit sizable relative to the baseline incidence of arrears in our sample. Once again, we find a significantly higher risk of falling in arrears for firms without available credit line balances to draw from.

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A Appendix

A.1 Definitions and summary statistics

We construct the interannual growth rates as follows:

$$\Delta y_{it} = \frac{y_{it} - y_{it-12}}{\frac{y_{it}+y_{it-12}}{2}} \tag{5}$$

1. Yearly rate of sales : interannual rate of total sales from F29.
2. Yearly rate of debt $(t+1)$: interannual rate of total debt from D10.
3. Yearly rate of av. credit line $(t+1)$: interannual rate of available credit line from D10.
4. Yearly rate of materials $(t+1)$: interannual rate of net amount of material from F29.
5. Yearly rate of employment $(t+1)$: interannual rate of employees' number from AFC.
6. Yearly rate of empl. permanent $(t+1)$: interannual rate of employees' number from AFC, but only employees with permament contract.
7. Yearly rate of empl. temporary $(t+1)$: interannual rate of employees' number from AFC, but only employees with temporary contract.
8. Leverage: we use the stock of debt at september 2019 (pre-shock measure) and the total amount of sales between september 2018 and september 2019. Then, we build the ratio $debt/totalsales$.
9. Dummy for High leverage : this dummy takes a one for firms over centile 75 of Leverage.
10. Available credit line / total sales: this variable is the ratio between the stock of available credit line in september 2019 and the total amount of sales between september 2018 and september 2019.
11. Dummy variable for financially constrainted : this dummy takes a one for all the firms which have not available credit line in september 2019.

A.2 Summary statistics

Table 9: Full sample summary statistics

Statistics:	Mean	s.d.	P25	P50	P75	P90	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. *Left hand side variables*							
Yearly rate of sales	-0.02	0.71	-0.38	0.00	0.34	0.86	167,974
Yearly rate of debt $(t+1)$	0.05	0.92	-0.41	-0.02	0.54	1.40	117,972
Yearly rate of av. credit line $(t+1)$	0.07	0.63	-0.09	0.02	0.25	0.85	137,828
Yearly rate of materials $(t+1)$	-0.01	0.86	-0.52	0.01	0.50	1.15	158,221
Yearly rate of employment $(t+1)$	0.01	0.41	-0.13	0.00	0.15	0.46	124,188
Yearly rate of empl. permanent $(t+1)$	0.03	0.40	-0.11	0.00	0.18	0.50	121,259
Yearly rate of empl. temporary $(t+1)$	-0.07	0.72	-0.67	0.00	0.40	0.91	43,001
Panel B. *Right hand side variables*							
Min. distance (km)	2.65	1.83	1.21	2.16	3.71	5.73	167,974
Leverage: debt/sales	0.31	1.30	0.00	0.03	0.16	0.45	167,974
Av. credit line/sales	0.06	0.17	0.00	0.02	0.06	0.12	167,974
Dummy for High leverage	0.20	0.40	0.00	0.00	0.00	1.00	167,974
Dummy for financially constrainted	0.12	0.33	0.00	0.00	0.00	1.00	167,974
Firm size: small $(UF_i \leq 2400)$	0.26	0.44	0.00	0.00	1.00	1.00	167,974
Firm size: medium 1 $(2400 < UF_i \leq 25000)$	0.50	0.50	0.00	1.00	1.00	1.00	167,974
Firm size: medium 2 $(25000 < UF_i \leq 100000)$	0.14	0.35	0.00	0.00	0.00	1.00	167,974
Firm size: large $(100000 < UF_i)$	0.09	0.28	0.00	0.00	0.00	0.00	167,974
Financial services	0.26	0.44	0.00	0.00	1.00	1.00	167,974
Commerce	0.35	0.48	0.00	0.00	1.00	1.00	167,974
Construction	0.09	0.28	0.00	0.00	0.00	0.00	167,974
Manufacture	0.11	0.31	0.00	0.00	0.00	1.00	167,974
Real Estate	0.03	0.17	0.00	0.00	0.00	0.00	167,974
Services	0.06	0.24	0.00	0.00	0.00	0.00	167,974
Transportation	0.10	0.30	0.00	0.00	0.00	0.00	167,974

Table 10: Sub-sample pre-shock statistics

Statistics:	Mean	s.d.	P25	P50	P75	P90	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. *Retail, hotels and restaurants*							
Av. min. distance (km)	2.475	1.487	1.337	2.016	3.260	4.899	16,074
Sales (in 10^6 of pesos)	3,998	71,654	75	217	710	2,535	16,074
Debt (in 10^6 of pesos)	316	3,488	0	6	56	245	16,074
Available Credit line (in 10^6 of pesos)	62	2,420	1	4	15	37	16,074
Debt/sales	0.199	0.713	0.001	0.042	0.182	0.436	16,074
Av. credit line/sales	0.063	0.197	0.005	0.019	0.053	0.121	16,074
Arrears (≤ 30)	0.081	0.273	0.000	0.000	0.000	0.000	16,074
Arrears ($31 - 90$)	0.031	0.175	0.000	0.000	0.000	0.000	16,074
Arrears (90+)	0.020	0.141	0.000	0.000	0.000	0.000	16,074
Employment	27.367	310.314	1	3	10	29	16,074
Employment: permanent	23.103	274.662	1	3	9	24	16,074
Employment: temporary	4.264	45.258	0	0	1	5	16,074

A.3 Repayment behavior and loan origination: Linear probability model

Table 11: Linear probability model

Dependent Variable:	Arrears			Renegotiation	New credit
	$30 <$	$31 - 90$	$90+$		
	(1)	(2)	(3)	(4)	(5)
Panel A. *Baseline*					
$MinDist_{it}$	-0.001***	-0.007***	0.004	-0.000	-0.000
	(0.000)	(0.003)	(0.004)	(0.000)	(0.000)
Constant	0.051***	0.119***	-0.010	0.017***	0.060***
	(0.003)	(0.031)	(0.037)	(0.002)	(0.003)
Observations	152706	7421	2663	168010	168010
Panel B. *Firms financially constrained*					
$MinDist_{it}$	-0.001*	-0.006**	0.002	-0.000	-0.000
	(0.000)	(0.003)	(0.005)	(0.000)	(0.000)
FC_i	0.035***	0.066***	-0.006	-0.000	-0.003*
	(0.004)	(0.024)	(0.028)	(0.001)	(0.002)
$FC_i \times MinDist_{it}$	-0.005***	-0.006	0.010	0.000	0.001
	(0.001)	(0.007)	(0.009)	(0.000)	(0.001)
Constant	0.047***	0.110***	-0.005	0.017***	0.060***
	(0.003)	(0.031)	(0.037)	(0.002)	(0.003)
Observations	152706	7421	2663	168010	168010
Panel C. *Firms with high leverage*					
$MinDist_{it}$	-0.001**	-0.012***	0.012**	-0.000***	-0.000*
	(0.000)	(0.004)	(0.006)	(0.000)	(0.000)
$Leverage_{it}$	0.049***	-0.021	0.056**	0.004***	0.017***
	(0.003)	(0.016)	(0.023)	(0.001)	(0.002)
$Leverage_{it} \times MinDist_{it}$	-0.001	0.010*	-0.017**	0.001**	0.002**
	(0.001)	(0.005)	(0.008)	(0.000)	(0.001)
Constant	0.044***	0.128***	-0.034	0.016***	0.057***
	(0.003)	(0.032)	(0.038)	(0.002)	(0.003)
Observations	152706	7421	2663	168010	168010
Panel D. *Only commerce sector*					
$MinDist_{it}$	-0.001***	-0.007**	-0.002	0.000	0.000
	(0.000)	(0.004)	(0.005)	(0.000)	(0.000)
$Commerce_{it}$	0.014***	-0.017	0.027	0.001**	0.003**
	(0.002)	(0.032)	(0.036)	(0.001)	(0.001)
$Commerce_{it} \times MinDist_{it}$	0.001	0.000	0.014*	-0.000**	-0.001
	(0.001)	(0.005)	(0.008)	(0.000)	(0.000)
Constant	0.051***	0.119***	0.005	0.016***	0.059***
	(0.003)	(0.031)	(0.038)	(0.002)	(0.003)
Observations	152706	7421	2663	168010	168010

A.4 Distribution of fitted values of the effects

Table 12: Fitted distribution of effects

Dependent Variable:	Sales	Debt	Credit line	Materials	Employment			
					All	Permanent	Temporary	Wage bill
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. *All firms*								
Average	1952635.84	1524307.28	292398.58	588249.84	0.21	0.14	0.28	199506.95
Median	77554.63	32382.68	13968.16	19396.35	0.03	0.02	0.03	25184.78
Average Δ$ per $1 of Sales	-	8.35	0.48	0.52	-	-	-	0.43
Median Δ$ per $1 of Sales	-	0.22	0.08	0.14	-	-	-	0.11
Obs.	74047	29626	34097	39820	31024	30328	10947	31024
Panel B. *Unconstrained firms*								
Average	1500488.30	1370129.30	238209.88	245194.53	0.16	0.11	0.20	151314.29
Median	59959.50	30823.93	11458.96	8409.40	0.03	0.02	0.03	20427.99
Average Δ$ per $1 of Sales	-	9.86	0.49	0.24	-	-	-	0.32
Median Δ$ per $1 of Sales	-	0.24	0.09	0.08	-	-	-	0.11
Obs.	68919	25857	33847	35165	28025	27409	9933	28025
Panel C. *Constrained firms*								
Average	6587528.26	2303053.90	246590.88	3016654.37	0.87	0.46	1.51	788737.73
Median	248049.52	40459.20	53516.94	69477.25	0.08	0.05	0.10	53600.29
Average Δ$ per $1 of Sales	-	4.40	0.67	2.03	-	-	-	1.75
Median Δ$ per $1 of Sales	-	0.18	0.24	0.33	-	-	-	0.12
Obs.	5128	3769	250	4655	2999	2919	1014	2999

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