BANCA D'ITALIA
EUROSISTEMA

Temi di discussione

(Working Papers)

The grocery trolley race in times of Covid-19: evidence from Italy

by Emanuela Ciapanna and Gabriele Rovigatti

June 2021

Number 1341

BANCA D'ITALIA
EUROSISTEMA

Temi di discussione

(Working Papers)

The grocery trolley race in times of Covid-19: evidence from Italy

by Emanuela Ciapanna and Gabriele Rovigatti

Number 1341 - June 2021

ISSN 1594-7939 (print)
ISSN 2281-3950 (online)

Printed by the Printing and Publishing Division of the Bank of Italy

THE GROCERY TROLLEY RACE IN TIMES OF COVID-19. EVIDENCE FROM ITALY

by Emanuela Ciapanna* and Gabriele Rovigatti*

Abstract

We study the sales dynamics of grocery chain stores during the first wave of the COVID-19 pandemic in Italy. We document a sustained growth in revenues for storable products, such as food staples and household supplies, beginning right before restrictions on mobility were introduced, and lasting throughout the whole lockdown period. We also examine the revenue surge by disentangling the role of different types of stores. We find that the increase has been driven by the dynamics of smaller outlets, located in urban areas and closer to the city centre, while hypermarkets experienced a drop during the lockdown period, probably relating to their more peripheral position. We also exploit both the remarkable granularity of scanner data and the staggered implementation of restrictions across Italian regions to causally identify the short-term effects of mobility constraints on outlets' sales. According to our estimates, large grocery stores in areas subject to lockdown measures earned revenues around 10 per cent lower than their control group did.

JEL Classification: D12, D18, I30.
Keywords: Covid-19, grocery retail trade, consumption hoarding, mobility restrictions contagion risk.
DOI: 10.32057/0.TD.2021.1341

Contents

* Bank of Italy, Directorate General for Economics, Statistics and Research

1 Introduction[1]

Italy has been among the first European countries to experience a widespread diffusion of the Covid-19, as well as one of the most hit countries in terms of deaths during the first wave. Since early March 2020, in order to contain the pressure on the overloaded healthcare system, several measures have been put in place by the Government under the guiding principle of social distancing to limit individuals' exposure to the virus in the workplace and in public spaces. Productive activities deemed 'non-essential' and suspended by the Prime Minister's Decree (DPCM) of 22 March 2020 until May 4^{th} represented about one third of total value added, with percentages of up to around two thirds for the accommodation and catering services component and almost 100 per cent for recreational activities. The impact of the pandemics on the various economic sectors, also due to unprecedented fall in demand, has been uneven. The immediate effects have been particularly severe in manufacturing, transports, catering, accommodation, recreation and culture, personal services, and in large swathes of retail trade. In March and April, the latter experienced a drop in sales of 23 percent compared to the corresponding period of 2019, driven by the severe decrease in the non-food compartment (-45 percent). Instead, the food sub-sector outperformed, registering an overall increase of 5 percent in sales year on year (figure 1).

The present analysis focuses on the effects of the Government restrictions to mobility, imposed in Italy between the end of February and the beginning of May 2020, on sales of fast moving consumer goods (FMCGs) in grocery non specialized stores, namely hypermarkets, supermarkets, superettes and discounts.[2] We document the dynamics of revenues in the period immediately before and during the lockdown, distinguishing between different types of stores, products and by geographical breakdowns. Our findings point to a sustained growth in sales in the two-month period March-April 2020 of more than 16 percent with respect to the corresponding period in 2019, a considerable increase, comparable in magnitude to Christmas' sales peaks in "normal" times. The revenue surge during the restrictions is ascribable to both quantity and price increase,

[1] We would like to thank Federico Cingano, Silvia Giacomelli, Sauro Mocetti, Sergio Santoro, and participants to the Bank of Italy Lunch seminar for useful suggestions. We are also indebted to Nicola De Carne (Nielsen) for data assistance. The views expressed here are our own and do not necessarily reflect those of the Bank of Italy.

[2] Fast-moving consumer goods, also known as packaged goods, are products that sell quickly at relatively low cost. FMCGs have a short shelf life because of high consumer demand (e.g., soft drinks and confections) or because they are perishable (e.g., meat, dairy products, and baked goods). These goods are purchased frequently, are consumed rapidly, are priced low, and are sold in large quantities. They also have a high turnover when they are on the shelf at the store.

once accounting for the large composition effects at play. As of product categories, the most sustained dynamics concerned medical, pharmaceutical and food products. The type of outlet also represents an interesting dimension of heterogeneity throughout the spreading of the pandemics: restrained mobility favoured proximity stores of smaller size (superettes and smaller supermarkets), while penalizing larger ones (e.g. hypermarkets), generally located in more peripheral areas. The temporary nature of such shocks - which peaked for all categories at the onset of the lockdown, just to slowly fade away when restrictions were lifted - traces their source back to Government restrictions, rather than being the signal of structural changes in the market structure. Along these lines, in the second part of the analysis, we propose an attempt to causally identify the effects of mobility restrictions on sales dynamics, by disentangling them from the general impact of fear of contagion risks. To this aim, we perform a difference-in-difference estimation, exploiting the time lapse between the institution of the first partial red zone, involving 10 municipalities within the Lodi province on February 23rd (treated group), and the total national lockdown, starting on March 9^{th} (control group). Our results indicate that during the partial lockdown week, large grocery chain stores (super and hypermarkets) in the treated province registered revenues around 10 percent lower than those of their control group. The effect is stronger for larger stores (e.g. hypermarkets), more exposed to mobility restrictions, due to their peripheral location, and for "less essential goods", such as cosmetics and alcoholic beverages (-19 and -12 percent respectively). Our results prove robust to different specifications. In particular, by exploiting the quasi-random location of the first Covid-19 clusters and the diffusion that ensued, we are able to split our sample according to the levels of contagion risk, and show that the latter does not bias our estimates.[3]

Our work is related to several recent empirical research contributions examining many aspects of the COVID-19 pandemic, and using different data sources. A first strand of literature analyses the effects of lockdown measures and contagion on consumers' behavior. Three notable studies for Denmark, Spain, and the US present evidence on the impact of COVID-19 on consumer spending. Andersen et al. (2020) use transaction-level bank account data from a large Danish bank to find a decline in spending – following the COVID-19 outbreak – which varies across product categories and correlates with Government's restrictions. Carvalho et al. (2020) utilise a large high-frequency point-of-sale transaction dataset from a major Spanish commercial bank to find large overall spending declines across various product categories, following a Government lockdown.

[3] See Diao et al. (2017) for details on the spatial difference-in-differences.

Baker et al. (2020a,b) use transaction-level household financial data from a personal-finance website to observe a substantial increase in consumer spending as COVID-19 cases increase, followed by a significant decline in general spending. The authors also observe heterogeneity in spending responses across states (depending on the severity of the virus outbreak). Chronopoulos et al. (2020) examine consumer-spending responses to the onset and spread of the pandemics and the subsequent government-imposed lockdown in Great Britain. Based on data from a personal finance app, which aggregates all transactions from linked bank accounts and credit or debit cards, they find that overall consumer spending declined as the UK government lockdown became imminent and has continued to decline since, although with some heterogeneity by age, gender, and income level. Our work is especially related to two other recent contributions. The first one is an article by O'Connell et al. (2020), where the authors use household-level scanner data for the UK to analyse purchase dynamics during the pandemics. They document large spikes in spending on storable products in the four weeks preceding the lockdown, particularly visible for FMCGs. The second piece of literature is the work by Goolsbee and Syverson (2020), which proposes a causal estimate of the effects of the shutdown policy conditions at the county and city level in the US on foot traffic, a proxy for economic activity. The results indicate that legal shutdown orders have accounted for a modest share of the massive overall changes in consumer movements. However, Goolsbee and Syverson (2020) consider how often people visit shops rather than how much do they actually spend, which makes their findings perfectly compatible with a surge in grocery stores' revenues overall.

Compared to the the aforementioned contributions, the present article adopts a somewhat different perspective. In fact, our focus is on outlets' performance (revenues and sold quantities) rather than on consumers' preferences and spending behavior. The latter is clearly a fundamental driving force within the analysis, yet our main objective is to disentangle the effect on the observed dynamics of Government restrictions (imposed by decree with different timing) from those associated to contagion risks fear.

The original contribution of our analysis is twofold: on the one hand, with respect to the aforementioned literature, we propose an attempt of causal identification of the lockdown effect on sales, on the other hand, our estimation method presents some elements of novelty in the designing of distance-depending control groups, borrowed from the spatial econometrics techniques (Diao et al., 2017).

The paper is structured as follows. In Section 2 we present the institutional setting and the timing of the Covid-19 restriction measures in the different Italian regions, in Section

3 we outline the dataset and document revenues dynamics across broad sets of outlet types, regions and products. In Section 4 we propose our identification analysis of the the effects of restricted mobility on revenues dynamics, and discuss our results. A final section concludes. Robustness checks, additional tables and figures are relegated to the Appendix.

2 Institutional background

In this section we provide a general description of the relative weight of the grocery chain-store sector in Italy, with particular reference to market share dynamics of the different types of outlets in the last decade with a geographical detail. Successively, we present the timing of the COVID-19 crisis from its onset in late February, until the end of the restrictions period and we show how the containment measures have differently affected the grocery retail sector, depending on store type as well as on geographical location.

2.1 The grocery chain-store sector in Italy

In the food sector alone, the market share of modern distribution (grocery retail chain stores) represents 74.5%, in Italy, corresponding to about 88 billion of food consumption. The number of outlets has decreased of around 11% within the last decade, from around 29 thousand in 2009 to 26 thousand in 2018, due to the sharp reduction in smaller-size shops, having a sales area between 100 and 400 square meters (-26.6%), partially offset by the expansion of discounters (+ 24.2%). Supermarkets are also growing, albeit to a less extent (1%), while hypermarkets are downsizing (figure 2). Grocery large-scale retail in Italy remains one of the most atomised markets in Europe. The top five operators share just over half of the market (51.8%) while in countries such as Germany, France and the United Kingdom the share is between 75 and 80%. A closer look, however, unveils local as well as structural differences, which characterize both the market structure and its evolution - including different types of dynamic responses to shocks.

The most striking difference between geographical areas concerns the distribution of outlet types. In figure 3, panel (a), we report the relative market shares of store categories by NUTS1 in 2019.[4] While *Supermarkets* are the best-selling format in most

[4]The distinction between *hypermarkets*, *supermarkets* and *superettes* hinges on the store size, and more specifically if the facilities are bigger than 2,500, 1,500 or 450 sqm, respectively. *Discounts*, are specialized in private label and non branded products.

of the country, the market share of large-scale stores strictly depends on the macro-area, being very popular in the North West, while representing only the 10% of the market in Southern regions - where consumers seem to prefer smaller-scale stores. The wide gap in *Discount* market shares - more than 10% between the South and the North West - reflects crucial differences in market structure and consumer habits toward the low price policies followed in those stores. In panel (b), we report the dynamics of the average store size between 2003 and 2017: in line with the (static) market share figures, in the Northwest stores are bigger, and have been growing at a higher rate than those located in other areas of the country, since the early 2000's.

Not surprisingly, there is a very strong and positive association between the number of outlets and population density ($\rho = .95$) on the one hand, and total sales area ($\rho = .45$) on the other hand. We also find a negative correlation between population density and the presence of large sales surfaces ($\rho = -.13$). In fact, hypermarkets arise mostly in peripheral areas, which implies the need for customers to drive to the store location (for a detailed discussion on the topic, see Viviano et al. 2012). As we show in the next sections, this feature helps us to explain the more modest dynamics in sales registered by this category of stores compared to the others during the period of mobility restrictions, which we exploit for identification purposes later in the discussion.

2.2 The timing of the COVID-19 crisis

Figure 4 depicts the main phases of the Covid-19 crisis. The first acknowledgment of the existence of a suspect "viral pneumonia" cluster of cases in Wuhan, China, dates back to December 31, 2019, and was closely followed by the first reported cases in the EU, and by the lockdown imposed to the Hubei region in mid-January. In Italy, the first reported cases, not directly linked to the Wuhan cluster, date back to mid-February and were detected in Lombardy. On February 23, 2020, the Government declared the first -partial- lockdown, involving 10 municipalities in the Lodi province, which were quarantined to prevent the spread of contagion. Soon after, the sudden increase in the number of infected patients, the pressure on the Intensive Care Units (ICUs) in the most affected areas, and the rise in the count of Covid-19-related deaths prompted the authorities to take unprecedented actions. On March 7 2020, all of Lombardy and 14 additional provinces were quarantined.[5] After only 48 hours, on March 9 2020, the whole

[5]More specifically, the provinces of Parma, Piacenza, Reggio Emilia, Rimini, Pesaro and Urbino, Venezia, Padova, Treviso, Asti, Alessandria, Novara, Vercelli, and Verbano-Cusio-Ossola in four regions (Emilia-Romagna, Piedmont, Veneto and Marche).

Italian territory was placed under lockdown. Moreover, the closure of all "non-essential" activities (including bars, cinemas, restaurants) was imposed, in order to ensure social distancing, and reduce the rate of contagion. COVID-19 cases started to decline in May 2020, thanks to the two-months lockdown. Freedom of movements was re-established on May 4, and other not essential activities re-opened later in the month, putting an end to the so-called "phase 1" of the first wave to enter "phase 2", or *Respite period.* After the summer lull, the virus regained strength in late September, giving rise to the second wave.

3 Data and Descriptive Statistics

This section presents the data sources that are employed throughout the analysis and the first descriptive evidence regarding the dynamics of grocery stores' revenues following the introduction of Government restrictions in effect between early March and the first week of May 2020 in Italy. We first characterize sales dynamics with detail for geographical area, type of outlet and product category; then we propose some preliminary regressions to investigate the response of our outcome variables to the COVID-19 shock (distinguishing between the anticipation period, the lockdown weeks and the respite phase throughout the summer). Finally, we attempt to disentangle the role of price and quantity in explaining the observed patterns.

3.1 Data

In this paper, we employ NUTS1 and NUTS3 level scanner data on revenues and sold quantities, collected by Nielsen and covering about 85% of total grocery chain stores in Italy.[6] The Nielsen scanner data are structured according to the general classification provided by the Efficient Consumer Response (ECR) community, used for category management by both industrial and distribution companies. The ECR classification involves a hierarchical, multi-layer structure, with increasingly granular and more specific clusters of products. In the analysis, we match its broader categories (*Product Area*) with corresponding *Classification of Individual Consumption by Purpose* (COICOP, UN Statistical Division, 2018) classes. Despite the differences in the classification scope - distributive and consumption-based, respectively - the two overlap pretty well.

In figure 5, we report an example of the Nielsen data structure alongside the general

[6] The dataset covers sales for the universe of hypermarkets and supermarkets, 90% of discounts and around 67% of superettes.

nomenclature of the ECR classification. In particular, out of the 6 layers available, our data is aggregated at L3 (*product group*, black font) - as a result, we are able to track down the dynamics of the granular products, but we miss potentially interesting information related to the marketability and packaging (L4 and L5, respectively), as well as individual product information like brand, producer, etc. (red font layers), which all appear aggregated and not distinguishable.

We employ weekly data for 462 product categories, sold in Italy from January 1, 2018 to October 30, 2020. The NUTS1-level data (North-West, North-East, Center and Sardinia, and South and Sicily) further identify the type of store, whereas the NUTS3-level one (province) only accounts for total sales in larger outlets (i.e. all the super and hypermarkets). In table 1 we describe the two datasets (*Channels* for NUTS1-level, and *Provinces* for NUTS3) in terms of coverage, granularity, and variables reported. Based on the COICOP, we identify 19 products at the most granular 4-digits level, further aggregated into seven 2-digits clusters: *food* - which is the main segment, *soft drinks*, *alcoholics*, *household equipment*, *medical appliances*, and *personal care supplies* (see table 2).

For exposition purposes, in what follows we distinguish three sub-periods along the first wave of the Covid-19 crisis: *Anticipation* (February 1 until March 8 2020), *Lockdown* (March 8 until May 4 2020), and finally the *Respite*, from the end of the lockdown to October 30 2020. In figure 6 we report the overall sum of revenues per week in 2018 (maroon dashed), 2019 (green dotted), and 2020 (yellow solid) for the whole sample in the period January-October. The graph highlights the presence of a structural break in the 2020 series, coinciding with the first wave of the pandemics, starting around the last week of February (anticipation period), until the beginning of May, which gradually vanishes throughout the Summer (respite period). The peak is registered on March 8th, i.e. the first week of national lockdown. Looking at revenues dynamics, the year on year growth rate of weekly revenues[7] at the national or NUTS1-regional level (and/or by product categories), writes:

$$r(y_w) = \frac{y_w - y_{w-52}}{y_{w-52}} * 100 \tag{1}$$

In figure 7, panel (a), we report the time series of $r(y_w)$ from October, 2019 and October, 2020, for the whole country. The series presents two spikes: the first coincides with the national lockdown on March 8, and the second with the end of the lockdown

[7]The year-on-year rate is defined as the percentage change between sales in week w of revenues y with respect to revenues of the corresponding week one year before (i.e., y_{w-52}).

- i.e., the first week of May, 2020. The plot, however, suggests that the lockdown regulatory measures did not fully channel the increase in revenues: the "excess" demand (i.e., precautionary spending) appeared already on early February, as the first news of the pandemics outbreak started spreading (consumption hoarding), and abnormal revenues ceased only in June, when mobility across regions was fully re-established, just to reemerge with the onset of second wave in September.

Despite the acceleration in revenue growth registered in aggregate terms, the lockdown effect appears to be composite, depending on store types. In panel (b) we report the same $r(y_w)$ series by outlet category: while the anticipation effect generates common upward trends, these diverge as soon as the restrictions to the mobility take place. While medium- and small-scale stores, including *Discounts*, reach unprecedented levels of revenues, with +20 to +40% increases with respect to the same week of 2019, hypermarkets experienced negative fluctuations for the whole lockdown sub-period. We argue that a similar diverging pattern, far from being the result of changes in consumption habits, is rather attributable to the more peripheral position of large retail surfaces, less easily accessible throughout the time lapse of mobility restrictions. Indeed, especially in smaller municipalities, hypermarket customers were unable to drive beyond municipal boundaries and were induced to shop in vicinity outlets (generally superettes or specialized stores), within few hundreds meters from their homes. This intuition finds further confirmation in the sudden reversion of the trend to the old habits immediately following the end of lockdown (see the positive spike since May 4).

Apparently, as shown in panel (c), the same revenue patterns have been recorded all over the country, with only minor differences across NUTS1 regions. Finally, in panel (d), we exploit the 2-digit COICOP classification to show how rather diverging trends emerge across products: while relatively important product clusters (in terms of total market share), such as *food* and *household appliances* experienced sizeable increases, the *personal* products were negatively affected by the lockdown. Unsurprisingly, *medical equipments* (including masks, pharmaceuticals, etc.) recorded unprecedented peaks both during and after the lockdown period.

3.2 Descriptive Analysis: the Lockdown Effect

The descriptive evidence shown in figure 7, although extremely informative, fail to address more specific questions related to the quantification of the lockdown effect, and do not account for the differentials between Northern and Southern regions. In order to shed further light on these features, we propose a simple fixed effects linear regression model, aimed at capturing the impact of the pandemics on product revenues for each sub-period. More specifically, we run a fixed-effects regression of product revenues (in logarithms) of the following form

$$y_{i,w,r,s} = \beta\, L_{i,w,r,s} + \gamma_k + \tau_t + \alpha_r + \sigma_s[+\psi_i] + \epsilon_{i,w,r,s} \tag{2}$$

where i, w, a, k, and s represent the product, the week, the region and the store type, respectively. $L_{i,w,r,s}$ is an indicator function for the weeks from March 9 to May 4, 2020, and captures the gross effect of the lockdown period on product revenues, *net* of all factors related to the specific period of the year ($\tau_t = week_t + month_t + year_t$, which controls for week of the year, month of the year, and year fixed effects), macro-region characteristics (α_r), store type (σ_s) and product group specificity (γ_k in the baseline model, replaced by ψ_i - i.e., product fixed effects - in a more saturated specification). We augment model (2) in several ways. First, we add indicator functions to capture the anticipation effect ($A_{i,w,r,s}$, active between Feb 1 and Mar 8, 2020) and the respite period ($R_{i,w,r,s}$, since the lockdown end) - i.e., *All Periods* model:

$$y_{i,w,r,s} = \beta\, L_{i,w,r,s} + \delta\, A_{i,w,r,s} + \eta\, R_{i,w,r,s} + \gamma_k + \tau_t + \alpha_r + \sigma_s[+\psi_i] + \epsilon_{i,w,r,s} \tag{3}$$

Second, we interact the lockdown indicator with dummies for area and store type, in order to estimate the marginal lockdown effect for each sub-category (*Marginal Effects*); finally, we substitute the lockdown indicator with date dummies, one per each week from January 1st to May 24, 2020, in order to allow for further flexibility in the parameters' estimation. In the latter exercise, the estimating equation writes: $y_{i,w,r,s} = \delta_{t*|t*>=Jan1,2020} + \gamma_k + \alpha_r + \sigma_s + \tau_t$. In table 3, we report the results of *Baseline* and *All periods* models, while relegating the results of the other cited models to the appendix B.

The estimated βs are all positive and statistically significant, although less and less as we add fixed effects. Notice that in the *All periods* specification, the estimated lockdown effect gets reduced by about a half when controlling for each individual product instead of product group (column *Prod*). Thus, the extent of within-product group variation is an important driver of revenue growth. Despite the generally positive trend, we retrieve a negative *Anticipation* parameter ($\hat{\delta}$). The latter seems to indicate that, conditioning on product, area, and outlet type fixed effects, revenues decreased in the weeks immediately preceding the restrictions. Accordingly, this result could hinge on a pattern of spending substitution towards products already highly represented within the consumption basket (e.g., bottled water, canned goods), at the expenses of products unaffected by precautionary stockpiling (e.g. cosmetics and perishables), which experienced a significant reduction. Finally, the *Respite* parameter ($\hat{\eta}$) is never statistically significant, in line with the descriptive evidence.

3.3 The Drivers of Revenue Growth: Quantity and Price.

In order to investigate whether, and to what extent, the changes in revenues are driven by changes in quantities sold or in prices charged, we correlate all variables before and during the lockdown. However, before discussing our results, a caveat is needed on our price variable. In our dataset, we can directly observe revenues and quantities, whereas unit prices P_{jt} are unknown. We can only retrieve an average price for each product class as the ratio between revenues and quantities at the most possible granular level (i.e., $\hat{P}_{jt} = \frac{PQ_{jt}}{Q_{jt}}$). Our measure, however, suffers from at least two sources of bias. First and foremost, we are unable to control for different product formats - i.e., we cannot tell whether 100 litres of sparkling water have been sold in packs of 6 bottles of 2 l each, or in 500 ml bottles individually. Second, several product groups feature different products with sometimes substantial differences in *unit* prices, but we miss enough detail to correctly compute the single products shares. These limitations have to be taken into account when interpreting the results we are about to present.

In table 5 we report the correlations between year-on-year changes in revenues ($r(y)_w$) on one side, and in quantity or price on the other ($r(q)_w$ and $r(p)_w$, respectively). In order to test the effects of the lockdown also on the relationship between these variables, we report the exercise both for the January 2018-December 2019 period (panel a), and for the sole lockdown weeks (panel b), by type of outlet. Along the whole time range and for all store categories, changes in quantities seem to be the main drivers of revenue growth, with correlation coefficients in the baseline period ranging between a minimum of 0.77 (superettes) to a maximum of 0.85 in supermarkets (panel a). As soon as the mobility restrictions were introduced, the increase in sold quantities seems to have reinforced its role as the main determinant of revenue growth in all store types - with correlation coefficients higher than 0.95 in all cases. On the other hand, the price dynamics did not follow a similar pattern. In all store types but superettes, its relatively weak positive correlation with the revenue growth fades away and is replaced by negative values of ρ. This result suggests that bigger, more structured stores were less able to adjust their prices in response to the mobility-induced demand shock, whereas agile neighborhood outlets quickly coped with it by rising their price level accordingly,. Moreover, the level of product aggregation, combined with price rigidity may induce the negative correlation we retrieve for larger outlets. Thus, because heterogeneous patterns in quantity and price by individual products could trigger offsetting effects within the more aggregate COICOP class, we conduct further analyses by individual good and implement three sequential exercises.

First, we descriptively quantify the lockdown effect on sold quantities by regressing their (log) value on an indicator for the lockdown period. The estimated model, run *product by product*, writes

$$q_t = \alpha + \beta_q L_t + \epsilon_t \tag{4}$$

where q_t is the log of total quantity sold at time t, summed across NUTS1 regions and distributive channels, and L_t is an indicator function equal to 1 for the lockdown weeks and zero otherwise. Thus, the coefficient β_q captures the dynamics observed during the restriction period. It is crucial to stress that such effect is averaged across all areas and distributive channels, and provides an indication of the overall "appeal" of individual products between March and May. In figure 8, panel a), we plot all the 450 estimated β_q - with their confidence intervals - divided by product category. Results, as expected, show a high degree of heterogeneity across and within categories - e.g., on average, *Food* products show marked increases, whereas *Personal Care* goods have usually recorded less sustained growth, except for hygiene-related products, whose request surged, following recommendation on hygiene protocols to limit contagion.

Second, we further investigate the differences in quantity demanded among products, conditional on store types, by looking at the distribution of the product-store fixed effects interacted with the lockdown period indicator. In formulas, we estimate a fixed effects model of the form

$$r(q)_w = \mathit{lockdown} \times \psi_i \times \sigma_s \tag{5}$$

where $\mathit{lockdown} \times \psi_i \times \sigma_s$ is an interaction term that captures the combined (fixed) effects of store types, individual products, and the lockdown period. Such a saturated model yields $462 \times 4 = 1,848$ parameters for the baseline estimate, and likewise for the lockdown period. In order to find the lockdown net effect for each product/store couple, we subtract one another, and in figure 8, panel b), we plot the resulting densities. The results confirm a massive detrimental effect of the lockdown on hypermarkets, where most products show negative fluctuations (i.e., estimates below zero) during the mobility restriction period, whereas supermarkets, superettes and mostly discounts faced positive demand shocks.

Third, we check whether products affected by such unanticipated shock were characterized by price changes and, if it is the case, whether the two effects followed similar patterns. To this aim, we estimate the price effect *per product* (as in equation 4, using $\hat{p}_t$) as our dependent variable. For the subset of products showing significant lockdown-driven changes in quantity,[8] we test the correlation between the estimated βs. Results are summarized in table 6, panel A. Columns 1 and 2 report the average value of the estimated parameter, whereas column 3 refers to the correlation coefficient (ρ) between the two βs. Finally, column 4 displays the weekly revenues averaged across products (in million €), which proxies the "share" of products within the consumer's basket. The results on the full sample (*All Products*) show a weak positive correlation between the two measures. When classifying products according to the sign (*Positive/Negative*) and magnitude (tertiles of $\hat{\beta}_q$) of their response to the shock, we find that the negative correlation coefficient on positively affected products is entirely driven by the set of most affected goods - e.g., so called "superstar" products like flour, yeast and parapharmaceuticals, but also stockpiling goods like chocolate bars and frozen products. All the other products show positive correlations - even though to different extents.

Our primary concern relates to potential composition effects acting as confounding factors on the results for prices. In fact, while total quantity is invariant to the choice of distributive channel, the latter heavily affects the average price level - which is what we measure with $\hat{P}_{it}$. As a result, changes in consumption habits, like those highlighted in section 3, may be the actual drivers of price changes captured by $\hat{\beta}_p$. Consider the case of yeast: its price on hypermarkets amounted to around the 87% of the average price charged in other outlet types, whereas the same figure for supermarkets and superettes is 113% and 134%, respectively. Hence, if smaller outlets face an excess demand while hypermarkets experience drops in quantity purchased, our measure of aggregate prices increases mechanically, due to such composition effects - without further modifications of posted prices. In order to account for that, we exploit the full information in our data, and we augment regression 4 with parameters aimed at capturing regional and outlet type (fixed) effects. The results, reported in panel B, confirm that the composition in terms of store type and area matters for price effect estimation only: while $\hat{\beta}_Q$ is slightly different,

[8] For this exercise, we restrict the sample to all products with statistically significant estimates of $\hat{\beta}_q$. Out of 462 products, we focus on 321.

the distribution of $\hat{\beta}_P$ changes dramatically, with their correlations ranging from .58 (1^{st} positive tertile) to .99 (*All Products*). Hence, despite a few attempt to control prices of products, which were subject to strong demand shocks - e.g., the government capped the price of surgical masks at 0.5 €, irrespective of the outlet - our results indicate that average prices closely followed the dynamics of quantity changes, once the area and store type are accounted for. After providing a detailed description of the raise in revenues during the restriction period, we are now ready to propose our identification strategy aimed at disentangling the sole impact of the lockdown measures from the perceived contagion risk effect.

4 Identifying the Lockdown Effect

In this section, we attempt to identify the causal impact of the mobility restriction measures adopted during the first wave on sales of large grocery stores in Italy. We first describe our empirical strategy, which exploits the gradual entry into force of the provisions by different geographical areas; then we report our results and discuss their significance and limitation. Finally, we conduct a battery of robustness checks to corroborate our evidence. The present analysis is based on NUTS3 data, which are available for the universe of large stores only (as stated in section 3), so all our results in this section solely apply to this sub-category.

4.1 Empirical Strategy

The partial lockdown imposed on 10 municipalities within the province of Lodi on February 23, 2020 (see figure 9, left panel), provides an exogenous variation in the treatment status that we exploit to estimate the causal lockdown effects on grocery stores revenues. More specifically, we exploit the fact that the red zone - nearly the whole province- had been isolated from the rest of Lombardy with the prohibition for its citizens of moving from home except for reasons of emergency and for the supply of essential and basic necessities. We propose the following difference-in-difference specification:

$$y_{ipt} = \alpha + \beta treatment_{ipt} + \tau_t + \gamma_{ip} + \epsilon_{ipt} \tag{6}$$

where our outcome variable y is the log of revenues for product i in province p at week t; $treatment_{ipt}$ is an indicator function for products sold in the Lodi province after February 23, 2020, while τ_t and γ_{ip} are time and province/product fixed effects, respectively. [9]

In figure 10, panel (a), we plot revenues dynamics (index February 5, 2020 = 1) for the treatment (solid blue line) and the control group (dashed maroon line) - the latter including all Italian provinces but Lodi.[10] The plot confirms that there are no detectable pre-trends that differently affected the treatment and the control group. Moreover, despite the upward dynamics, the graph highlights how the treated units "catch-up" the level of spending of control units right after the national lockdown date (black dotted line). Figure 10, panel (b), depicts the within-province mobility for the treatment and the control group: in the time lapse between the treatment date (maroon vertical line) and the national lockdown (black vertical line), the mobility within the treated area decreased dramatically, lying well below the average level in control provinces. The gap was closed only when the national lockdown imposed the same measures on the whole country.

The proposed model faces a few potential threats to the identification of the correct treatment effect, that we discuss below and address with a series of robustness checks - reported in the body of the paper or in appendix C.

First, and most important, one could argue that our results are mainly driven by the heterogeneous perceived risk of contagion in the two groups rather than by the different restrictions applied to treated and controls. The descriptive evidence shown in figure 10, panel (a), could induce to believe that the treatment affects provinces belonging to the control group(s) as well as Lodi, and that would violate the main assumption for the validity of the difference-in-differences identification - i.e., absent the treatment, the outcome of treatment and control groups would hold parallel. We argue that the regulatory intervention imposed on February 23, 2020 - the *lockdown* - had two main effects: on the one hand, it generated a wave of concern about contagion risk (*signalling effect*) that invested the whole country, and caused a spike of consumption hoarding for precautionary reasons (O'Connell et al., 2020). On the other hand, for quarantined municipalities only, it imposed strict constraints to personal mobility (*mobility effect*). As long as the extent of the *signalling effect* is the same for treated and control units, the proposed identification captures an unbiased estimate of the *mobility effect*.

In order to correctly identify the impact of lockdown measures on total revenues, as

[9] Given that the treatment period goes from February 23 to March 8 (3 points in time given the weekly frequency of our data), for the sake of consistency in our exercise, we consider the three weeks before the restrictive measures are enforced, i.e. form the week of February 1st to the first week of lockdown.

[10] In order to avoid the possibility of store detection, the Nielsen data only cover 87 provinces in Italy, out of 107. In most cases, the "missing" provinces are located in the Southern regions.

distinct from the effect of perceived contagion risk, we propose an exercise with *distance-dependent control groups*, whose size in terms of included provinces is an increasing function of the distance from the epicenter of contagion (i.e. the province of Lodi). Following the methodology proposed in Diao et al. (2017), we exploit the heterogeneous geography of the first wave infections - which we report, in terms of total cases per 100,000 inhabitants within the province, in figure 11, panel (a). Most heavily affected provinces are located in the North-West, and in particular around Lodi (which is by far the worst case, with an incidence of around 175). The "quasi-exogenous" geographical distribution of the starting clusters allows us to partition provinces into gradually less exposed units, depending on the physical distance from the epicenter. More specifically, we compute the distance between each province and Lodi,[11] and use the deciles of the distance distribution to generate gradually narrower balls around the treated province. For each decile, all provinces whose centroid lies within the relative ball belong to the control group. In figure 11, panel (b), we report the trends for each distance-based control group: all of them confirm the absence of any significant pre-trend. In addition, we run the baseline model excluding all provinces which share any boundary with Lodi: in this way, we are able to exclude from the analysis the units facing the highest (perceived) contagion risk.

Second, our analysis suffers from data limitations. In fact, the maximum degree of geographical detail available in the Nielsen data-set is NUTS3, i.e. the province level. On the other hand, the quarantine was imposed at the municipal level, for a subset of municipalities within the Lodi province. The latter account for 22% of the whole province population, and are home to more than 40% of sales area within the grocery retail sector, respectively. Provided that the data points in our treatment group include both the treated and a subset of control units, the estimated β is downward biased by construction and attains a *lower bound* of the actual effect.

Third, the province of Lodi may have unique features possibly driving our results, and our model could be capturing them, rather than the lockdown effect. Italian provinces are in fact characterized by an extreme degree of heterogeneity. In this context, the very small size of the treatment group relative to the control raises similar concern. To address both issues, we propose i) an inverse propensity score-weighted version of model (6) - which corrects for possible selection of treated units into the treatment, and ii) a synthetic control group analysis, with province level data. Both exercises yield very

[11] In order to do that, we first compute the centroids for each province, then we generate a measure of distance, in kilometers, to the centroid of Lodi.

robust, and statistically significant results (see appendix C).

4.2 Results and Robustness Checks

Table 7 reports the results of the difference-in-differences estimation on total revenues (column 1, *Baseline*). Our results point to an impact of lockdown measures of around 10 percent lower revenues for grocery stores located in the red zone with respect to those located in areas not subject to mobility restrictions. These findings are remarkably robust to the choice of the model: columns 2 to 10 report the estimates using the distance-dependent control groups, increasingly more restrictive - from the 9^{th} (corresponding to a radius of around 750 km around Lodi) to the 1^{st} decile (which corresponds to a very narrow 64 km radius) - with qualitatively the same estimates. In column 11, instead, we focus on the contiguity rather than the distance between provinces, and exclude from the sample all Lodi's neighbor provinces: estimated parameters are again very robust. These results provide a strong support to the correctness of our identification: irrespective of the level of *contagion risk* considered in the control group, in fact, the point estimate is relatively unaffected.

In table 8, we run the baseline exercise *by product group*, both for revenues (panel A), and for its main component, quantity (panel B). As expected, the product dimension provides an interesting source of heterogeneity: for instance, the estimated parameters for *Food* and *Medical equipment* are significantly lower in absolute value than the aggregate model ones, indicating a lower elasticity of demand for goods deemed as "more essential", even in the quarantined zones. Consistently, coefficients referred to *Soft Drinks* and *Personal Appliances* are higher in absolute value (-12 and -19%, respectively): when forced to limit their shopping trips, consumers choose to reduce their consumption of those products. Finally, the quantity channel is the one driving the estimates: apart from the *Household* products, the estimates virtually don't change when using Q_{ipt} instead of PQ_{ipt}.

5 Conclusions

The grocery chain stores have played a crucial role during the first wave of the Covid-19 pandemics. The sector faced a huge positive demand shock for most product categories - which means, in turn, being able to cope with shortages, adjust the logistics, increase hours and working shifts, all of it right in the middle of an unprecedented crisis, which raised uncertainty and blocked most economic activities. The so-called "second wave" of the pandemics, will very likely have similar characteristics: hence, understanding the effects of containment measures on new consumption habits and therefore on retailers' performance has become of first-order importance.

This paper contributes by showing that i) changes in quantity purchased, rather than price movements, drive most of the revenue dynamics, ii) there exist huge variations in both quantity demanded, prices charged and dynamic response across areas and distributive types, iii) lockdown measures soften revenue increases by a considerable amount; in particular, they do so through the mobility channel, which mainly affects larger peripheral outlets, whose revenues were around 10 percent lower than those in their control group; iv) the contagion risk plays a minor role, its effects being quite homogeneous across provinces.

References

Andersen, A. L., Hansen, E. T., Johannesen, N., and Sheridan, A. (2020). Consumer responses to the covid-19 crisis: Evidence from bank account transaction data. *Available at SSRN 3609814*.

Baker, S. R., Farrokhnia, R. A., Meyer, S., Pagel, M., and Yannelis, C. (2020a). How does household spending respond to an epidemic? consumption during the 2020 covid-19 pandemic. Technical report, National Bureau of Economic Research.

Baker, S. R., Farrokhnia, R. A., Meyer, S., Pagel, M., and Yannelis, C. (2020b). Income, liquidity, and the consumption response to the 2020 economic stimulus payments. Technical report, National Bureau of Economic Research.

Carvalho, V. M., Hansen, S., Ortiz, A., Garcia, J. R., Rodrigo, T., Rodriguez Mora, S., and Ruiz de Aguirre, P. (2020). Tracking the covid-19 crisis with high-resolution transaction data.

Chronopoulos, D. K., Lukas, M., and Wilson, J. O. (2020). Consumer spending responses to the covid-19 pandemic: An assessment of great britain. *Available at SSRN 3586723*.

Diao, M., Leonard, D., and Sing, T. F. (2017). Spatial-difference-in-differences models for impact of new mass rapid transit line on private housing values. *Regional Science and Urban Economics*, 67:64–77.

Goolsbee, A. and Syverson, C. (2020). Fear, Lockdown, and Diversion: Comparing Drivers of Pandemic Economic Decline 2020. NBER Working Papers 27432, National Bureau of Economic Research, Inc.

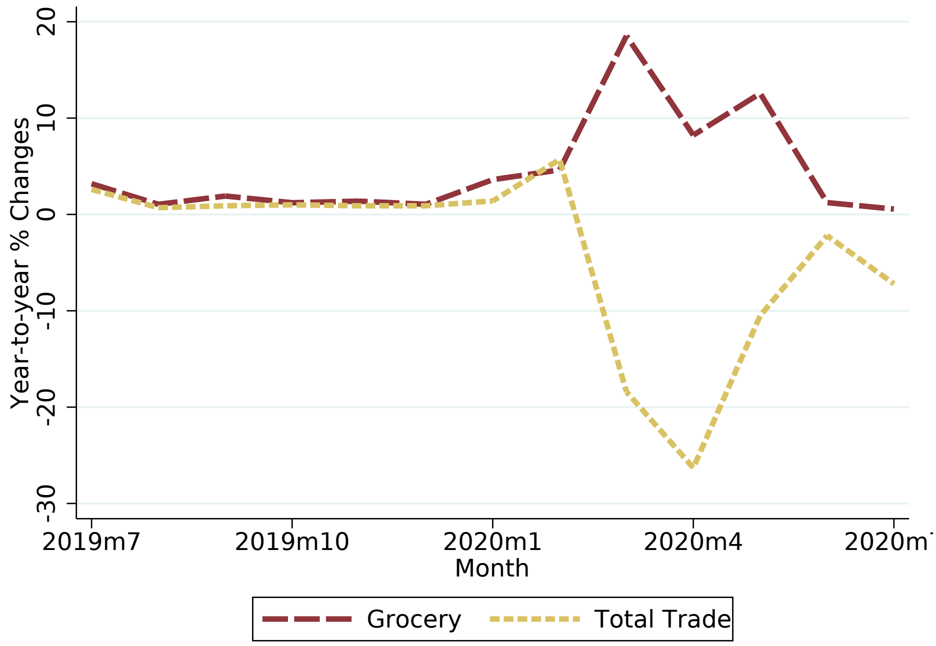
O'Connell, M., de Paula, A., and Smith, K. (2020). Preparing for a pandemic: Spending dynamics and panic buying during the covid-19 first wave. *CEPR Working Papers*, (15371).

Pepe, E., Bajardi, P., Gauvin, L., Privitera, F., Lake, B., Cattuto, C., and Tizzoni, M. (2020). Covid-19 outbreak response, a dataset to assess mobility changes in italy following national lockdown. *Scientific data*, 7(1):1–7.

Viviano, E., Aimone Gigio, L., Ciapanna, E., Coin, D., Colonna, F., Lagna, F., and Santioni, R. (2012). The retail trade sector and the food industry in italy. *Bank of Italy Occasional Paper*, (119).

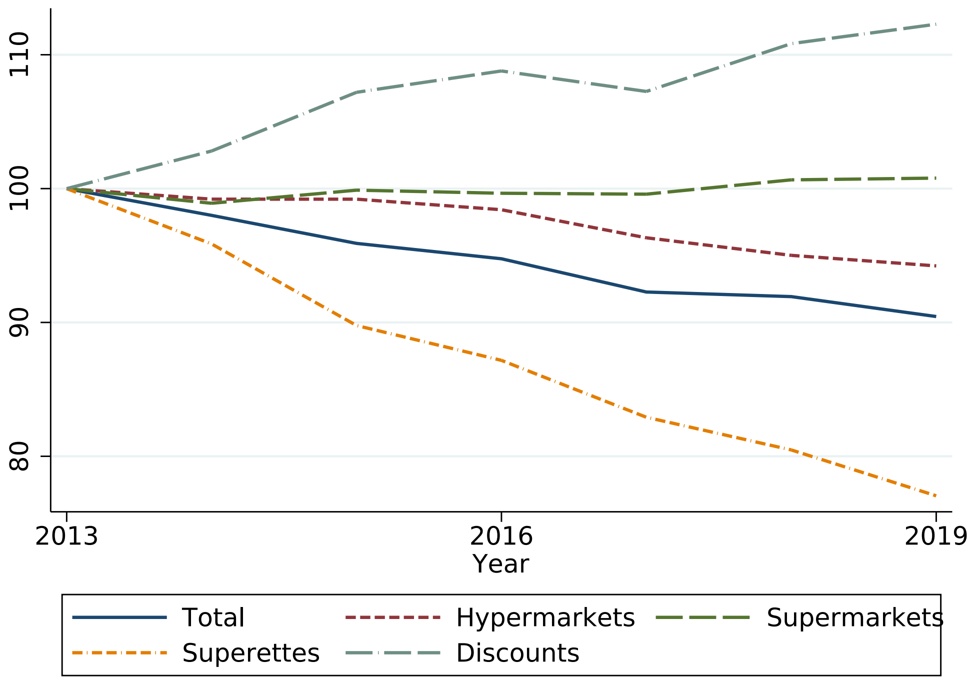
Figures and Tables

Figure 1: Sales in grocery and total retail trade; year-on-year changes



Notes: Year-on-year % changes of total grocery sales (Nielsen, maroon line), and of total trade sector (Istat, yellow line).

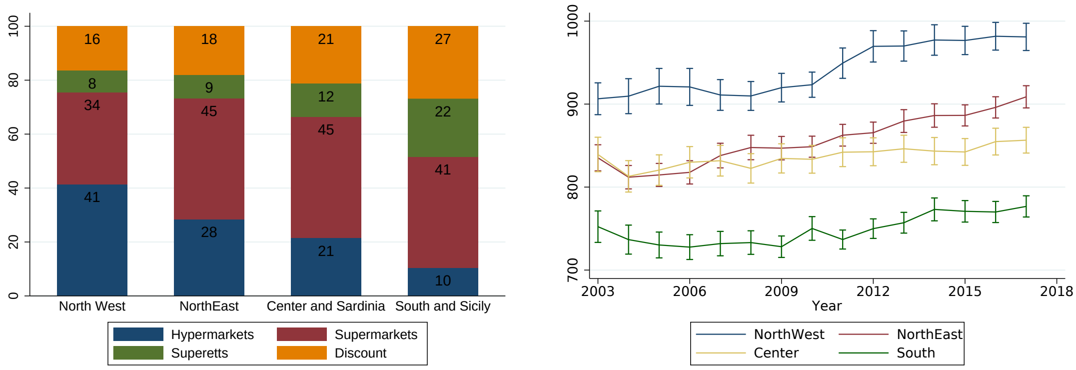
Figure 2: Evolution in the number of outlets in the grocery retail sector in Italy (index 2013=100).



Notes: Number of grocery stores per type in Italy, 2013-2019. Sources: Federdistribuzione, Economic Development Ministry, Gnlc Nielsen, Tradelab.

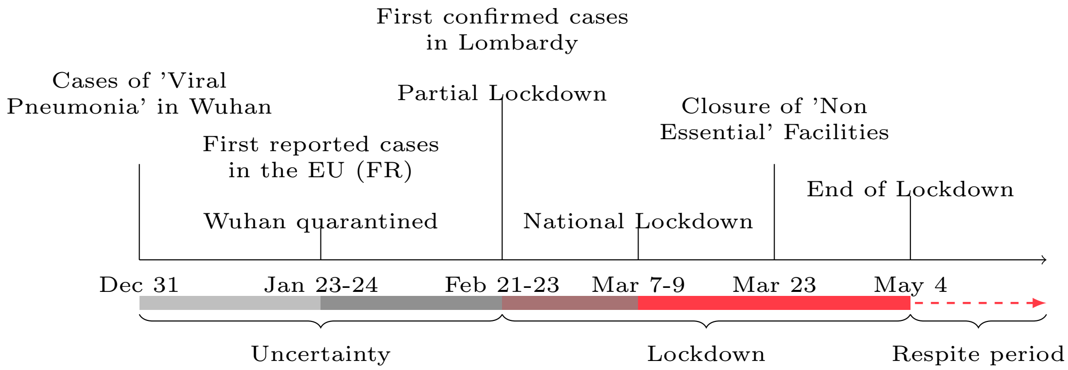
Figure 3: Market shares and average market size

(a) Market Shares by type in %, 2019 (b) Average Size in m^2, 2013-2017



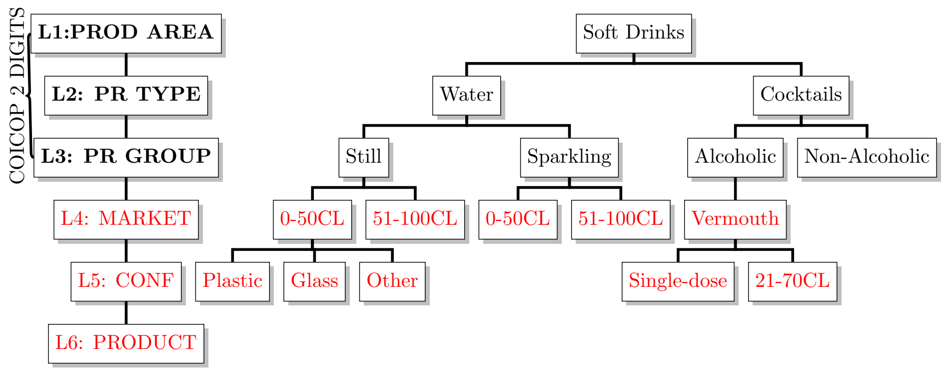
Notes: (a) Market shares per distributive category and macroarea in 2019. *hypermarkets* are facilities bigger than 2,500 sq. meters, *supermarkets* bigger than 1,500, and *superettes* are bigger than 450. *Discounts* sell different product brands. (b) Average store size per municipality, aggregated at macroarea. Authors' elaborations on Nielsen data.

Figure 4: Covid-19 first wave Timeline in Italy



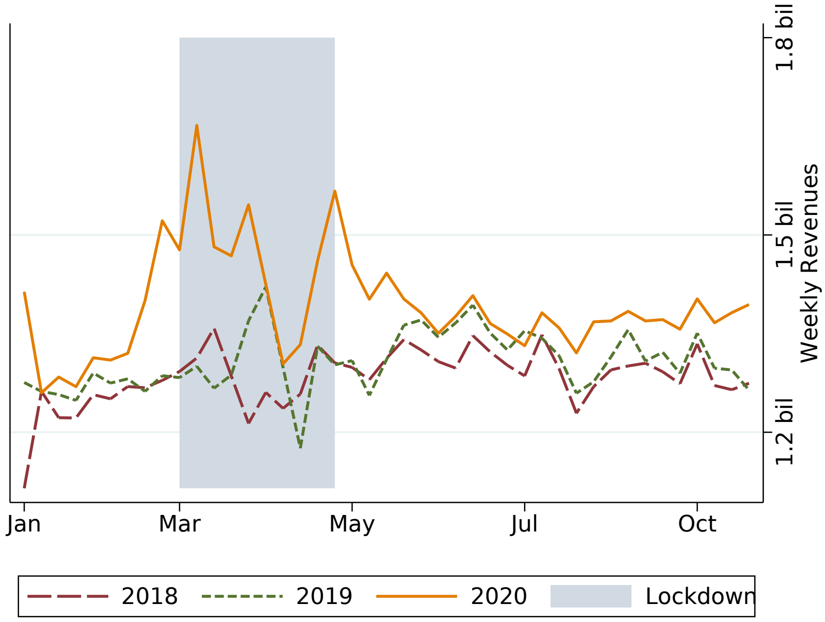
Notes: Timeline of the Covid-19 pandemics evolution in Italy. Red areas correspond to partial/national lockdown periods.

Figure 5: Data Structure: the Nielsen Data and the ECR classification



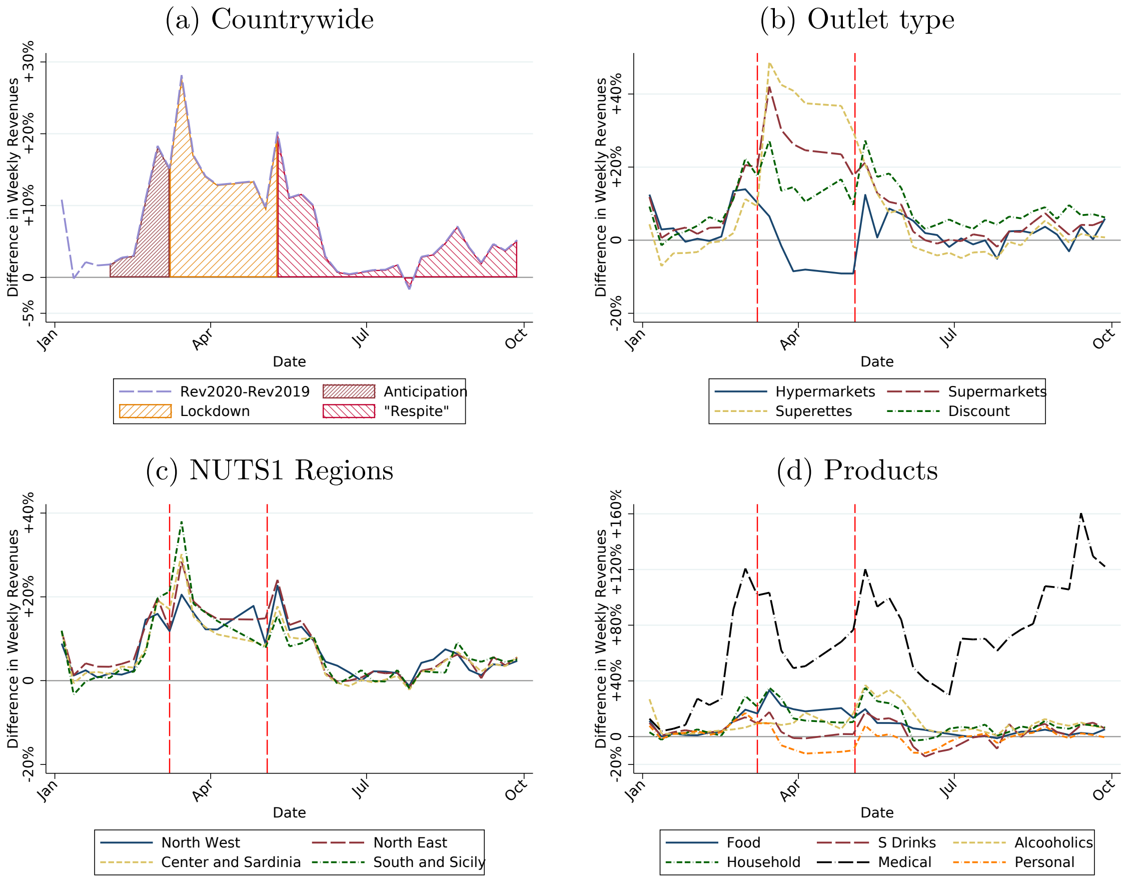
Notes: Nielsen/ECR scanner data structure, split in information available (black font) and missing (red font) in our dataset.

Figure 6: Total Revenues 2018-2020



Notes: Italian grocery retail sector total weekly revenues, 2018-2020 - Nielsen data. In each series, we exclude Easter week. The shaded area corresponds to the weeks of the national lockdown in 2020.

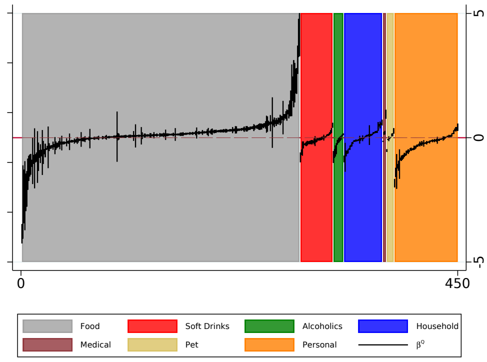
Figure 7: Time Series of $r(y)$, Weekly revenue growth 2019-2020



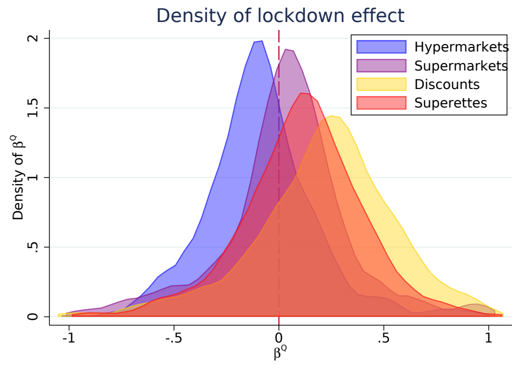
Notes: Year-on-year % changes in revenues. (a) countrywide, split in subperiods - *Anticipation* in maroon, *Lockdown* in yellow, and *Respite* in red; (b) divided by outlet type: hypermarkets (solid blue), supermarkets (dashed red), superettes (dotted yellow) and discounts (dotted green); (c) divided by NUTS1 regions: North West (solid blue), North East (dashed red), Center and Sardinia (dotted yellow), and South and Sicily (dotted green); (d) divided by product groups: *Food* (solid blue), *Soft Drinks* (dashed red), *Alcoholics* (dotted yellow), *Household products* (dotted green), *Medical appliances* (dashed black), and *Personal goods* (dotted orange). The vertical, dashed lines mark the starting and end date of the lockdown.

Figure 8: Lockdown estimated effect per product and store type

(a) Lockdown effect per product

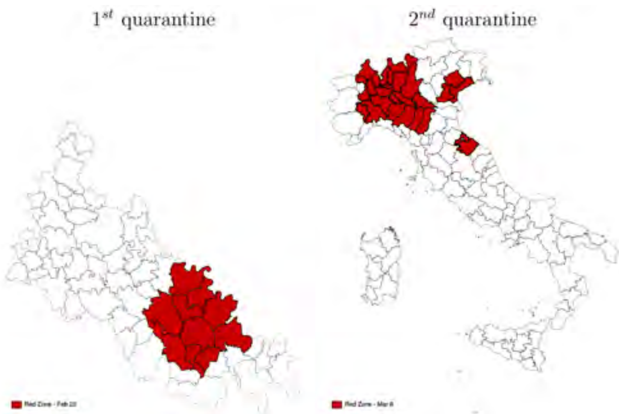


(b) Lockdown effect density per store type



Notes: (a) Distribution of the lockdown effect on quantity purchased *per product* ($\hat{\beta}^Q$ as defined in (4)). Estimated parameters are sorted in ascending order. The horizontal line is set at $\hat{\beta}^Q = 0$ (b) Density distributions of the lockdown effect on quantity purchased *per store type* ($\hat{\beta}^Q$ as defined in (4)). The vertical line is set at $\hat{\beta}^Q = 0$.

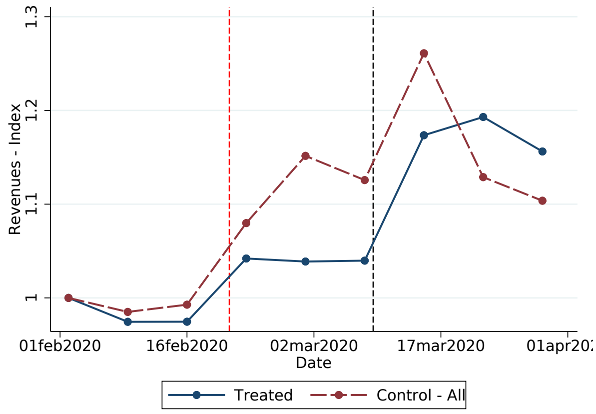
Figure 9: Quarantined zones: within Lodi province, and at national level



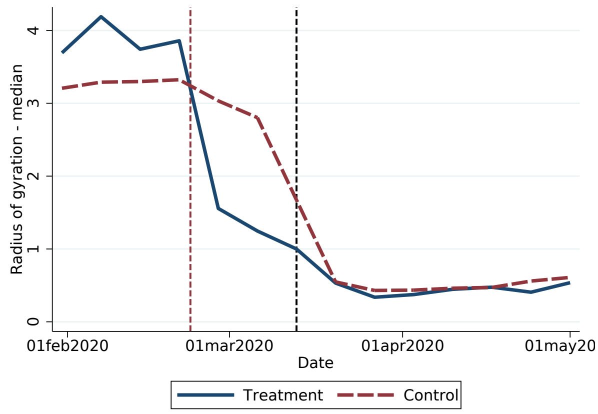
Notes: Map of the municipalities subject to the first lockdown within Lodi (left panel), and all provinces subject to the second, partial lockdown (right panel).

Figure 10: Pre-trends and mobility patterns

(a) Revenues - trends per group



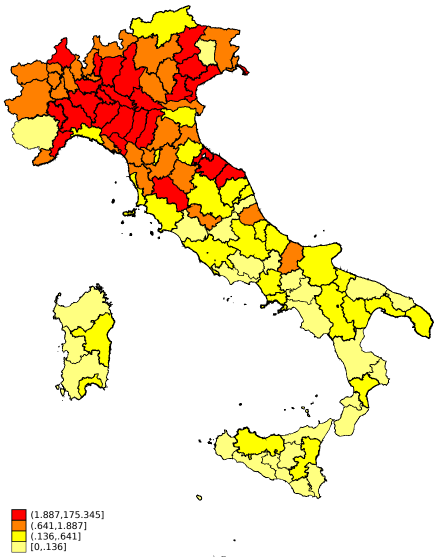
(b) Mobility Patterns



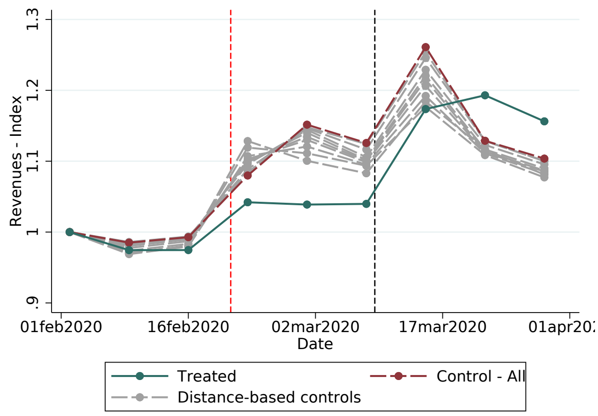
Notes: (a) Revenues indexed at February 1, 2020, reported for the treatment group (Lodi, solid blue), averaged across the control group provinces for the baseline control (*Control - All*, dashed maroon), and across all Lombardy provinces but Lodi (dashed green). (b) Within-province mobility patterns in Lodi (solid blue) and averages across control group provinces (dashed maroon). The mobility is proxied through the median value of the radius of gyration - Pepe et al. (2020).

Figure 11: Covid incidence and distance-based trends

(a) Total cases over 100,000 population



(b) Distance-based controls



Notes: (a) Total Covid-19 identified cases between March and September, 2020, per 100,000 inhabitants at the province level (February/August, 2020). (b) Revenues indexed at February 1, 2020, reported for the treatment group (Lodi, solid blue), averaged across the control group provinces for the baseline control (*Control - All*, dashed maroon), and across all distance-based control groups (dashed grey).

Table 1: Nielsen: available data

	Channels	*Provinces*
Time Dimension		
Frequency	Weekly	Weekly
Coverage	Jan 1, 2018-today	Jan 1, 2018-today
Granularity		
ECR	L3	L3
Geography	NUTS-1 and Store Type	NUTS-3 and H+S
Coverage	Big-box Sector	Big-box Sector
Variables		
Measured	Revenues and Quantity (P*Q e Q)	Revenues and Quantity (P*Q e Q)
Approximate*	Prices (P)	Prices (P)

* The computation of weekly prices $\hat{P} = P * Q/Q$ is influenced by i) changes in the composition of product baskets, ii) discounts and offers, and iii) the product formats purchased.

Notes: Available Nielsen dataset description and main characteristics.

Table 2: Revenues per product

	(1)	(2)	(3)	(4)	(5)
				T-tests	
	Treated	All controls	Lomb Controls	Δ_{A-T} (2)-(1)	Δ_{L-T} (3)-(1)
Food	2937.9	6274.5	13909.1	3336.6***	10971.2***
	(325.8)	(8347.9)	(17428.4)	(79.37)	(455.4)
Soft Drinks	430.5	922.2	1935.4	491.7***	1504.9***
	(37.97)	(1120.6)	(2296.2)	(10.50)	(59.97)
Alcoholics	266.8	578.7	1377.9	311.8***	1111.1***
	(45.96)	(849.0)	(1833.1)	(8.489)	(47.96)
Household	211.5	374.1	864.4	162.6***	652.9***
	(21.72)	(493.6)	(1062.1)	(4.765)	(27.76)
Medical Appl	26.85	47.17	117.3	20.32***	90.47***
	(5.552)	(68.53)	(149.8)	(0.765)	(3.933)
Personal	419.2	776.8	1772.6	357.6***	1353.4***
	(29.70)	(1024.5)	(2207.7)	(9.487)	(57.63)

Notes: Elaborations on Nielsen data. Descriptive statistics: average weekly revenues per product group and provinces groups - treatment (i.e., Lodi, in column 1), all other provinces (2), and other Lombardy provinces (3). In columns 4 and 5 we report the results of t-tests on the difference of the means between the control and treatment municipalities.

Table 3: Regression Results

Model 1): Baseline					
	Base	Time	ST+Clust	All	Prod
Lockdown	0.025***	0.023*	0.024*	0.025*	0.019**
	(0.010)	(0.013)	(0.013)	(0.013)	(0.008)
Model 2): All Periods					
	Base	Time	ST+Clust	All	Prod
Lockdown	0.039***	0.033*	0.033*	0.038**	0.023**
	(0.010)	(0.018)	(0.018)	(0.018)	(0.011)
Anticipation	-0.107***	-0.071***	-0.066***	-0.041**	-0.044***
	(0.012)	(0.019)	(0.019)	(0.019)	(0.012)
Respite	0.038***	0.021	0.020	0.023	0.010
	(0.007)	(0.018)	(0.017)	(0.017)	(0.010)
Observations	1,079,654	1,079,654	1,079,654	1,050,004	1,050,004
τ_t		✓	✓	✓	✓
α_r			✓	✓	✓
γ_k				✓	
σ_s				✓	✓
ψ_i					✓

Notes: 1) Estimated results of model (2) with increasing number of fixed effects: week (column 2), area (column 3), the product class and the store type (column 4), and the individual products (column 5). 2) Estimated results of model (3) with increasing number of fixed effects.

Table 4: Regression Results: Store type

Panel a): Store type analysis				
	Discount	Hypermarket	Superette	Supermarket
Lockdown	0.026***	-0.162***	0.212***	0.069***
	(0.010)	(0.009)	(0.009)	(0.009)
Observations	249,859	257,831	253,708	258,213
Panel b): Interacted Coefficients				
	Discount	Hypermarket	Superette	Supermarket
Lockdown	0.118	-0.169	0.193	0.100
Observations	1,079,654	1,079,654	1,079,654	1,079,654
τ_t	✓	✓	✓	✓
α_r	✓	✓	✓	✓
ψ_i	✓	✓	✓	✓

Notes: (a) Estimated results of model (2) per store type. All estimated models include week, area, and individual product fixed effects. (b) Lockdown parameter estimates interacted with store type fixed effects: we report the difference between the lockdown and the no-lockdown periods estimates.

Table 5: Correlation between year-on-year growth rates of revenues ($r(y)_w$) and quantities ($r(q)_w$) or prices ($r(p)_w$)

	Panel a: Baseline, 2018-2019							
	Hypermarkets		Supermarkets		Superettes		Discounts	
	$r(q)_w$	$r(p)_w$	$r(q)_w$	$r(p)_w$	$r(q)_w$	$r(p)_w$	$r(q)_w$	$r(p)_w$
$r(y)_w$	0.84	0.51	0.85	0.36	0.77	0.15	0.84	0.12
	Panel b: Lockdown Period							
	Hypermarkets		Supermarkets		Superettes		Discounts	
	$r(q)_w$	$r(p)_w$	$r(q)_w$	$r(p)_w$	$r(q)_w$	$r(p)_w$	$r(q)_w$	$r(p)_w$
$r(y)_w$	0.97	-0.30	0.98	-0.22	0.98	0.41	0.96	-0.19

Notes: Decomposition of the year-on-year growth rates of revenues ($r(y)_w$) into their main components: y-o-y growth in quantity demanded ($r(q)_w$) and prices charged ($r(p)_w$). The table reports the decomposition results per outlet type - hypermarkets (columns 2 and 3), supermarkets (4 and 5), superettes (6 and 7) and discounts (8 and 9) - and distinguishing between the pre-Covid, baseline period (panel a) and the lockdown weeks (panel b).

Table 6: Correlation between $\hat{\beta}_q$ and $\hat{\beta}_p$

Panel a): Country-level regressions

	$\hat{\beta}_Q$	$\hat{\beta}_P$	ρ	avg PQ (mil.)
All Products	0.06	-0.03	0.08	2.24
Positive Q Shocks	0.41	0.16	-0.48	2.79
1^{st} Tercile	0.12	0.11	0.44	3.84
2^{nd} Tercile	0.26	0.22	0.33	3.24
3^{rd} Tercile	0.85	0.15	-0.59	1.27
Negative Q Shocks	-0.49	-0.33	0.50	1.39
1^{st} Tercile	-1.00	-0.68	0.41	0.37
2^{nd} Tercile	-0.32	-0.21	0.51	1.43
3^{rd} Tercile	-0.14	-0.09	0.38	2.40

Panel b): Area-Store type regressions

	$\hat{\beta}_Q$	$\hat{\beta}_P$	ρ	avg PQ (000)
All Products	0.02	0.03	0.99	326.97
Positive Q Shocks	0.36	0.37	0.98	414.32
1^{st} Tercile	0.11	0.12	0.58	560.29
2^{nd} Tercile	0.25	0.28	0.79	439.46
3^{rd} Tercile	0.73	0.72	0.98	241.23
Negative Q Shocks	-0.50	-0.49	0.98	192.63
1^{st} Tercile	-1.05	-1.03	0.96	59.37
2^{nd} Tercile	-0.32	-0.30	0.80	234.29
3^{rd} Tercile	-0.12	-0.11	0.70	286.99

Notes: panel a) Results of equation (4) on total quantity (column 2) and prices (column 3) - whole country level. In column 4 we compute the correlation of the two measures across all products, while column 5 reports the average weekly revenues in million €. We report all figures for the full sample (row 1), and divide between positive (rows 2 to 5) and negative (6 to 10) estimated parameters; finally, we further divide the sample according to tertiles of the distribution. In panel b) we report the same figures estimated controlling for area and outlet type fixed effects.

Table 7: Baseline results and distance-based robustness checks

	Baseline	d9	d8	d7	d6	d5	d4	d3	d2	d1	No Neighbors
treatment	-0.106***	-0.106***	-0.106***	-0.105***	-0.103***	-0.102***	-0.102***	-0.099***	-0.097***	-0.089***	-0.108***
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)	(0.006)	(0.009)	(0.002)
Observations	226,329	206,126	182,533	158,935	137,937	114,401	90,759	69,656	45,658	24,297	215,536

Notes: Difference-in-differences results, as in equation (6). In column 1 (*Baseline*) we use all available data and keep the control group as broad as possible. In columns 2 to 10 we use increasingly restrictive definitions of control groups, using the deciles of distance from the epicenter. In column 11 (*No Neighbors*), we exclude from the sample the provinces contiguous to Lodi. In all models, we cluster the standard errors at the province level.

Table 8: Difference-in-differences: results per product

	Panel A) Revenues						
	Overall	Food	S Drinks	Alcohol	Medic	Household	Personal
treatment	-0.106***	-0.096***	-0.121***	-0.080***	-0.037***	-0.000	-0.160***
	(0.002)	(0.003)	(0.003)	(0.006)	(0.004)	(0.009)	(0.005)
Observations	226,329	136,925	17,023	5,675	19,697	2,064	33,088
	Panel B) Quantity						
	Overall	Food	S Drinks	Alcohol	Medic	Household	Personal
treatment	-0.104***	-0.095***	-0.133***	-0.071***	-0.027***	0.036***	-0.150***
	(0.002)	(0.003)	(0.003)	(0.006)	(0.004)	(0.008)	(0.005)
Observations	226,306	136,902	17,023	5,675	19,697	2,064	33,088
τ_t	✓	✓	✓	✓	✓	✓	✓
γ_{ip}	✓	✓	✓	✓	✓	✓	✓

Notes: panel a) Difference-in-differences results, as in equation (6), run per product group. Column 1 reports the baseline estimates, whereas in columns 2 to 7 we restrict the estimation sample to the individual product groups. Panel b) Difference-in-differences results, as in equation (6), run per product group and on quantity demanded. In all models, we cluster the standard errors at the province level.

A Appendix

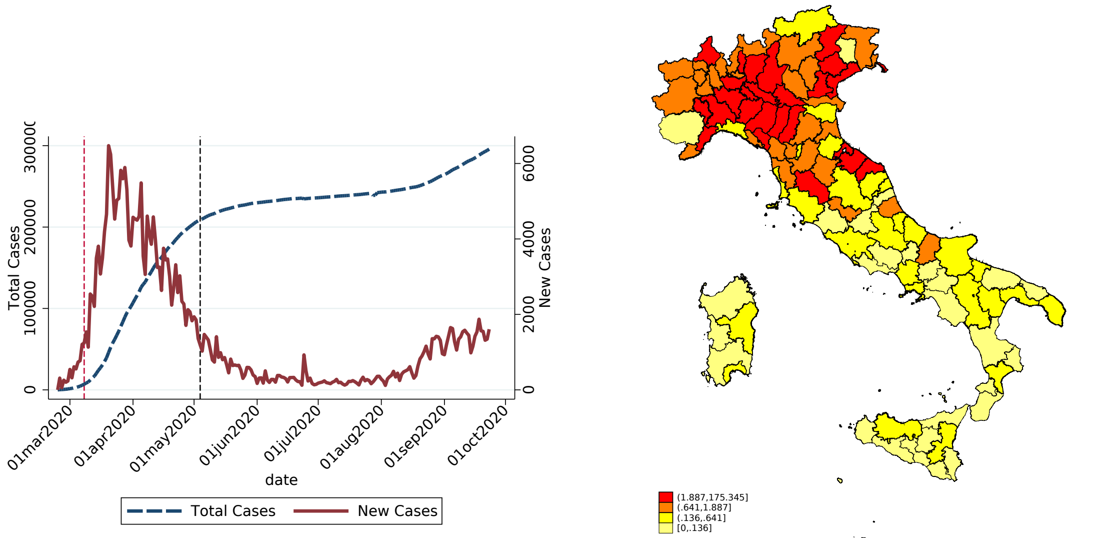
Appendix A: the evolution of the pandemics in Italy

The evolution of the "first wave" of Covid-19 pandemics in Italy is reported in figure 12, panel (a). In particular, we plot the total number of cases - dashed blue line - and the number of new cases recorded - solid red line. The dashed vertical lines mark the starting (red) and ending (black) dates of the national lockdown. The number of cases peaked a few days after the regulatory measures, and slowly decreased afterwards: during the summer, the "respite" period has been characterized by a very low number of new infections. Panel (b) reports the geographical dispersion of the *incidence* across provinces - i.e., the total number of cases over total population - during the first wave.

The two plots highlight the main features of the pandemics in Italy: first, its impact peaked during the lockdown, whereas the rate of infections was sensibly lower from May on, second, it was characterized by a strong local variation: while all provinces recorded at least a few cases, by far the most hit places were located in the northwestern regions (Lodi, Bergamo, Milano) which experienced the worst consequences in terms of lost lives and healthcare systems collapses.

Figure 12: Covid-19 evolution in Italy

(a) Country-level new and total cases (b) Incidence over provincial population

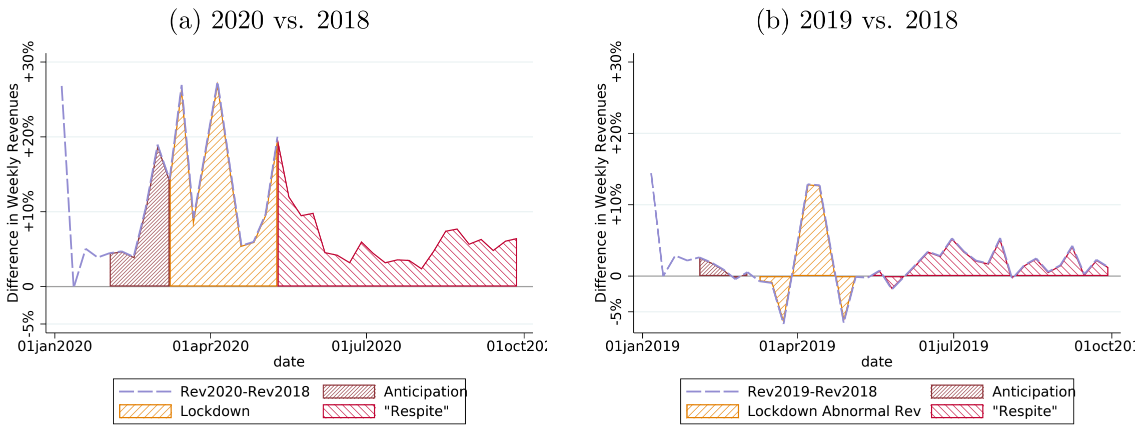


Notes: (a) total number of cases (dashed blue line) and daily number of new cases (solid red line) in Italy between february, 23 and september 30, 2020. Vertical dashed lines mark the national lockdown dates (b) geographical variation of first-wave *incidence* (defined as the ratio between the total number of cases and the province population).

Appendix B: the effect of the lockdown

The length of the available data allows us to check the robustness of the year-on-year changes reported in figure 7, panel (a). Indeed, a similar picture can be retrieved by using data on 2018 revenues (figure 13, panel a), which supports the hypothesis that the 2020 revenue dynamics were actually abnormally high with respect to previous periods. A further test for robustness is provided in panel (b), where we plot $r(y)$ referring to the 2019 series ("control" sample): aside from increases due to Easter dates, there is no detectable trend in the series, as expected.

Figure 13: $r^2(y)$ - (2020 vs. 2018) and $r(y)_{2019}$ (2019 vs. 2018)



Notes: (a) Year-on-two-years % changes in revenues split in subperiods - *Anticipation* in maroon, *Lockdown* in yellow, and *Respite* in red. (b) Year-on-year % changes in revenues split in subperiods - *Anticipation* in maroon, *Lockdown* in yellow, and *Respite* in red - relative to the confront between 2019 and 2018.

In table 9 we analyze the three time intervals individually, for each adjusted series (i.e., $r(y)$, $r^2(y)$ and $r(y)$ 2019 vs. 2018). We report their sum, period by period, in levels (first three rows, in million €) and in percentage change for the full period (fourth row). Results confirm that the pandemic period dramatically affected total revenues of the GDO. Despite a very modest increase between 2019 and 2018 - amounting to about 2% for the whole period, column 3 - the effect of the pandemics is clearly identified both by contrasting the 2020 revenues with those in 2019 (+11.3%), and with those in 2018 (+13.6%). The most striking difference between the 2020 and the control series regards the "Respite" period: even after the end of the lockdown, abnormal revenues amount to more than $\times 33$ times those in the 2018-2019 period.

Finally, in table 10 we report the estimated marginal effects of the full equation (2). More specifically, we interact the lockdown indicator function with indicator functions for

Table 9: Empirical Distribution of $r(y)$ and $r^2(y)$ - All Periods

	Treated		Control
	$y^{2020} - y^{2019}$	$y^{2020} - y^{2018}$	$y^{2019} - y^{2018}$
Anticipation (mln €) (Feb 1 - Mar 8)	689	824	133
Lockdown (mln €) (Mar 9 - May 9)	1.818	2.141	322
Respite (mln €) (May 10 - Sep30)	560	578	17
Full period (% change) (Feb 1 - Sep30)	11.3	13.6	2

Notes: Sum of $r(y)$ (columns 1 and 3) and $r^2(y)$ (column 2) per period, in million euros. The **Full period** figure is reported in % change.

areas (column 1), stores (column 2), product clusters (column 3), or full model (column 4), and report all estimates obtained for the lockdown period. Note that, in order to retrieve the full effect per category, one should subtract from the reported parameter the one obtained for lockdown == 0 (see e.g. panel b of table 4).

Appendix C: robustness checks

Propensity score matching and weighting In order to relax the concern that our estimates are driven by unique features of the Lodi province, we both run a propensity score matching model and reweight the difference-in-difference model by the inverse of the propensity score: this way, we make sure that the estimation is driven more by provinces that are, ex-ante, more similar to Lodi, according to the chosen observables. More specifically, to estimate the propensity score we use measures related to both the physical/demographic characteristics and to the market structure whose relative weight might likely influence grocery chain stores revenues (data source in parenthesis):

- **Demography**
  - share of population aged 75+ (Istat, 2019)
  - population density in 2018 (Istat, 2018)
- **Market Structure**
  - stores per kilometer (Asia/Istat, 2016)

Table 10: Regression Results

Model 3): Marginal Effects				
	Area	Store	Cluster	All
Lockdown × NorthWest	0.090***			0.580***
	(0.021)			(0.034)
Lockdown × NorthEast	-0.044**			0.162***
	(0.022)			(0.034)
Lockdown × Center+Sardinia	0.002			0.199***
	(0.022)			(0.034)
Lockdown × South+Sicily	-0.595***			-0.126***
	(0.021)			(0.034)
Lockdown × Discount		0.107***		-0.803***
		(0.021)		(0.026)
Lockdown × Drugstore		-2.183***		-3.039***
		(0.037)		(0.042)
Lockdown × Iper		0.173***		-0.727***
		(0.021)		(0.026)
Lockdown × Free Services		-0.327***		-1.235***
		(0.021)		(0.026)
Lockdown × Super		0.905***		0.000
		(0.021)		(.)
Lockdown × Food			0.090***	0.486***
			(0.015)	(0.026)
Lockdown × Soft Drinks			0.129***	0.599***
			(0.035)	(0.040)
Lockdown × Alcoholics			0.577***	0.961***
			(0.061)	(0.062)
Lockdown × Household Equipment			-0.248***	0.325***
			(0.032)	(0.037)
Lockdown × Medical Appliances			0.377***	0.950***
			(0.096)	(0.094)
Lockdown × Pet Products			-0.258***	0.315***
			(0.068)	(0.069)
Lockdown × Personal Care			-0.575***	0.000
			(0.026)	(.)
Observations	1,079,654	1,079,654	1,050,004	1,050,004
τ_t	✓	✓	✓	✓
α_a	✓			✓
σ_s		✓		✓
γ_k			✓	✓

Notes: Interacted estimated parameters of (2) for area (column 1), outlet type (column 2), product cluster (column 3) and all categories (column 4) indicators. We only report the parameter estimated for the lockdown period (i.e., *lockdown* == 1).

– grocery chain stores employees per population (Nielsen, 2017)

Using the above variables, we are able to compute the propensity score at the province/product level, and run a full battery of robustness checks. The results, reported in table 11, include propensity score-weighted models (using kernel-based weights for ATT, column 1, and ATE, column 2), the inverse propensity score weighted model (column 3) and a classical propensity score matching difference-in-differences (column 4). All of them are extremely robust with respect to the baseline results.

Table 11: Inverse Propensity Score Weighting

	Kernel-based PS		IPW	PSM
	ATT	ATE	ATE	ATE
treatment	-0.104***	-0.106***	-0.105***	-0.106***
	(0.0061)	(0.00245)	(0.0200)	(0.0153)
N	222,786	222,786	37,131	37,131

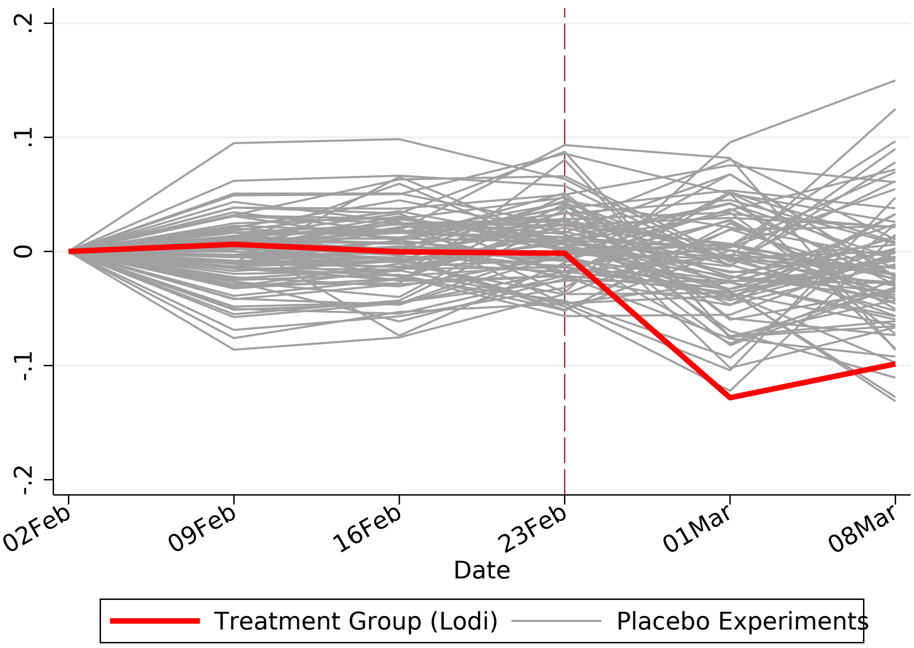
Notes: Propensity score-weighted models using kernel-based weights for ATT (column 1) and ATE (column 2), inverse propensity score weighted (column 3) and propensity score matching difference-in-differences (column 4).

Synthetic control group analysis We use a similar approach to overcome the concerns related to the small number of treated units with respect to the control group product/provinces - in fact, a 1/86 ratio. To address this, we run a synthetic control group analysis: in a nutshell, we build a "synthetic Lodi province" using all provinces within the control group, reweighted in order to mimic the observables of the treated province. Once obtained the optimal weights, we confront the (untreated) synthetic Lodi with the actual Lodi, and estimate the effect of the treatment - with extremely robust results. We also repeat the same approach with all untreated provinces, and we use these placebo experiments to yield a sort of inferential analysis on the goodness of the results. Figure 14 reports the very satisfactory results: the counterfactual Lodi leads to a bigger drop in revenues (red line) with respect to all its placebo counterparts (grey lines).

Appendix D: The "contagion risk" effect

The estimate of the lockdown effect does not provide information on a more subtle, yet real effect related to the objective risk of contagion. Even in the absence of direct restrictions to personal mobility, in fact, customers may decide to loose their shopping habits and avoid social contacts while shopping the stores. On the other hand, a higher

Figure 14: Inferential analysis: placebo Synthetic Control Group



Notes: results of the estimated treatment effects in a synthetic control group approach (red line) contrasted with all available placebo exercises (gray lines).

contagion risk may lead to higher levels of precautionary spending and stockpile shopping, because of the uncertainty related to the evolution of the pandemics, to supply capacity of stores, and to the regulatory measures.

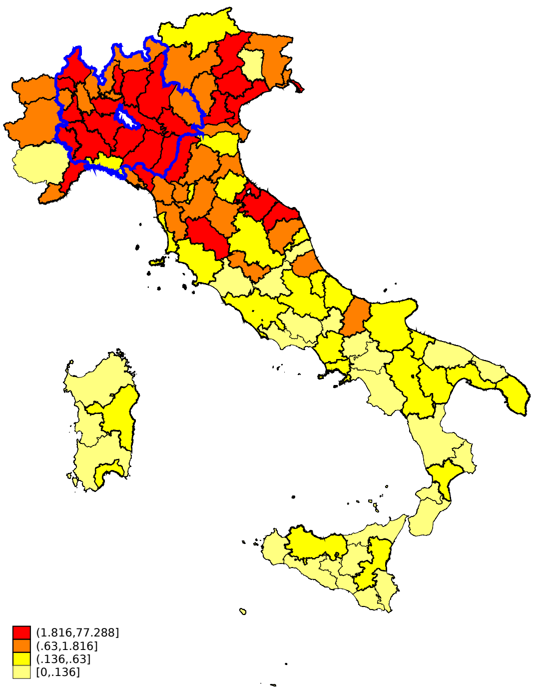
In order to disentangle which of the two effects is stronger, and quantify the net effect on total revenues, we exploit the heterogeneous geography of the first wave infections - which we report, in terms of total cases per 100,000 inhabitants within the province, in figure 15. Most heavily affected provinces are located in the North-West, and in particular around Lodi (which is by far the worst case, with an incidence of around 175). The "quasi-exogenous" geographical distribution of the starting clusters allows us to partition provinces into treated and control units, depending on the physical distance from the epicenter - i.e., the Lodi province, which we exclude from the analysis. More specifically, we compute the distance between each province and the epicenter,[12] and, using the first quartile of the distance distribution, we generate a ball of radius 118 km around Lodi. All provinces whose centroid lies within the ball belong to the treatment group, because they faced the highest contagion risk (they are marked by the solid blue line).

We then use model (6) to obtain estimates of the *contagion risk* effect. Despite the similar identification approach, the interpretation of the parameter estimates changes, alongside the definition of treatment group. The risk of infection is in fact proportional to the vicinity of the province subject to lockdown, and the imposition of extreme regulatory measures signals the real extent of such risk (it is our *treatment*). Therefore, comparing neighbour provinces with distant ones after the partial lockdown captures the differential effect of being at high risk of infection versus facing a somehow "baseline" risk, shared by all the country.

Results are shown in table 12. As expected, the lockdown of Lodi caused an increase in precautionary demand in treated provinces of around 1 percent higher than their control counterpart. Consistently with a distance-dependent contagion risk, the estimated effect is even stronger if we compare Lodi neighbors to Central+Southern or to Southern provinces only (1.3 and 1.8 points, respectively, reported in columns 2 and 3).

[12]In order to do that, we first compute the centroids for each province, then we generate a measure of distance, in kilometers, to the centroid of Lodi.

Figure 15: Total cases over 100,000 provincial population



Notes: Total Covid-19 identified cases between March and September, 2020, per 100,000 inhabitants at the province level (February/August, 2020). The province of Lodi is in white; the solid blue lines mark all provinces within the first quartile of dsitance from Lodi.

Table 12: Estimates of "contagion risk" effect

	Distance		
	All	CS	S
lockdown	0.011*	0.013*	0.018**
	(0.007)	(0.007)	(0.009)

Notes: Difference-in-differences results, as in equation (6). The treatment group includes all provinces within the first quartile of distance from Lodi, the control group all remaining provinces. Lodi is dropped from the analysis. In all models, we cluster the standard errors at the province level.

RECENTLY PUBLISHED "TEMI" (*)

N. 1314 – *Working horizon and labour supply: the effect of raising the full retirement age on middle-aged individuals*, by Francesca Carta and Marta De Philippis (February 2021).

N. 1315 – *Bank credit and market-based finance for corporations: the effects of minibond issuances*, by Steven Ongena, Sara Pinoli, Paola Rossi and Alessandro Scopelliti (February 2021).

N. 1316 – *Is inflation targeting a strategy past its sell-by date?*, by Alberto Locarno and Alessandra Locarno (February 2021).

N. 1317 – *Declining natural interest rate in the US: the pension system matters*, by Jacopo Bonchi and Giacomo Caracciolo (February 2021).

N. 1318 – *Can we measure inflation expectations using Twitter?*, by Cristina Angelico, Juri Marcucci, Marcello Miccoli and Filippo Quarta (February 2021).

N. 1319 – *Identifying deposits' outflows in real-time*, by Edoardo Rainone (February 2021).

N. 1320 – *Whatever it takes to save the planet? Central banks and unconventional green policy*, by Alessandro Ferrari and Valerio Nispi Landi (February 2021).

N. 1321 – *The power of text-based indicators in forecasting the Italian economic activity*, by Valentina Aprigliano, Simone Emiliozzi, Gabriele Guaitoli, Andrea Luciani, Juri Marcucci and Libero Monteforte (March 2021).

N. 1322 – *Judicial efficiency and bank credit to firms*, by Giacomo Rodano (March 2021).

N. 1323 – *Unconventional monetary policies and expectations on economic variables*, by Alessio Anzuini and Luca Rossi (March 2021).

N. 1324 – *Modeling and forecasting macroeconomic dowside risk*, by Davide Delle Monache, Andrea De Polis and Ivan Petrella (March 2021).

N. 1325 – *Foreclosures and house prices*, by Michele Loberto (March 2021).

N. 1326 – *inancial structure and bank relationships of Italian multinational firms*, by Raffaello Bronzini, Alessio D'Ignazio and Davide Revelli (March 2021).

N. 1327 – *Foreign investors and target firms' financial structure: cavalry or lucusts?*, by Lorenzo Bencivelli and Beniamino Pisicoli (April 2021).

N. 1328 – *Board composition and performance of state-owned enterprises: quasi experimental evidence*, by Audinga Baltrunaite, Mario Cannella, Sauro Mocetti and Giacomo Roma (April 2021).

N. 1329 – *Can internet banking affect households' participation in financial markets and financial awareness?*, by Valentina Michelangeli and Eliana Viviano (April 2021).

N. 1330 – *(In)Efficient separations, firing costs and temporary contracts*, by Andrea Gerali, Elisa Guglielminetti and Danilo Liberati (April 2021).

N. 1331 – *The catalytic role of IMF programs*, by Claudia Maurini and Alessandro Schiavone (April 2021).

N. 1332 – *Dating the euro area business cycle: an evaluation*, by Claudia Pacella (April 2021).

N. 1333 – *Population aging, relative prices and capital flows across the globe*, by Andrea Papetti (April 2021).

N. 1334 – *What drives investors to chase returns?*, by Jonathan Huntley, Valentina Michelangeli and Felix Reichling (April 2021).

N. 1335 – *Managerial talent and managerial practices: are they complements?*, by Audinga Baltrunaite, Giulia Bovini and Sauro Mocetti (April 2021).

(*) Requests for copies should be sent to:
Banca d'Italia – Servizio Studi di struttura economica e finanziaria – Divisione Biblioteca e Archivio storico – Via Nazionale, 91 – 00184 Rome – (fax 0039 06 47922059). They are available on the Internet www.bancaditalia.it.

2019

ALBANESE G., M. CIOFFI and P. TOMMASINO, *Legislators' behaviour and electoral rules: evidence from an Italian reform,* European Journal of Political Economy, v. 59, pp. 423-444, **WP 1135 (September 2017).**

APRIGLIANO V., G. ARDIZZI and L. MONTEFORTE, *Using the payment system data to forecast the economic activity,* International Journal of Central Banking, v. 15, 4, pp. 55-80, **WP 1098 (February 2017).**

ARNAUDO D., G. MICUCCI, M. RIGON and P. ROSSI, *Should I stay or should I go? Firms' mobility across banks in the aftermath of the financial crisis,* Italian Economic Journal / Rivista italiana degli economisti, v. 5, 1, pp. 17-37, **WP 1086 (October 2016).**

BASSO G., F. D'AMURI and G. PERI, *Immigrants, labor market dynamics and adjustment to shocks in the euro area,* IMF Economic Review, v. 67, 3, pp. 528-572, **WP 1195 (November 2018).**

BATINI N., G. MELINA and S. VILLA, *Fiscal buffers, private debt, and recession: the good, the bad and the ugly,* Journal of Macroeconomics, v. 62, **WP 1186 (July 2018).**

BURLON L., A. NOTARPIETRO and M. PISANI, *Macroeconomic effects of an open-ended asset purchase programme,* Journal of Policy Modeling, v. 41, 6, pp. 1144-1159, **WP 1185 (July 2018).**

BUSETTI F. and M. CAIVANO, *Low frequency drivers of the real interest rate: empirical evidence for advanced economies,* International Finance, v. 22, 2, pp. 171-185, **WP 1132 (September 2017).**

CAPPELLETTI G., G. GUAZZAROTTI and P. TOMMASINO, *Tax deferral and mutual fund inflows: evidence from a quasi-natural experiment,* Fiscal Studies, v. 40, 2, pp. 211-237, **WP 938 (November 2013).**

CARDANI R., A. PACCAGNINI and S. VILLA, *Forecasting with instabilities: an application to DSGE models with financial frictions,* Journal of Macroeconomics, v. 61, **WP 1234 (September 2019).**

CHIADES P., L. GRECO, V. MENGOTTO, L. MORETTI and P. VALBONESI, *Fiscal consolidation by intergovernmental transfers cuts? The unpleasant effect on expenditure arrears,* Economic Modelling, v. 77, pp. 266-275, **WP 1076 (July 2016).**

CIANI E., F. DAVID and G. DE BLASIO, *Local responses to labor demand shocks: a re-assessment of the case of Italy,* Regional Science and Urban Economics, v. 75, pp. 1-21, **WP 1112 (April 2017).**

CIANI E. and P. FISHER, *Dif-in-dif estimators of multiplicative treatment effects,* Journal of Econometric Methods, v. 8. 1, pp. 1-10, **WP 985 (November 2014).**

CIAPANNA E. and M. TABOGA, *Bayesian analysis of coefficient instability in dynamic regressions,* Econometrics, MDPI, Open Access Journal, v. 7, 3, pp.1-32, **WP 836 (November 2011).**

COLETTA M., R. DE BONIS and S. PIERMATTEI, *Household debt in OECD countries: the role of supply-side and demand-side factors,* Social Indicators Research, v. 143, 3, pp. 1185–1217, **WP 989 (November 2014).**

COVA P., P. PAGANO and M. PISANI, *Domestic and international effects of the Eurosystem Expanded Asset Purchase Programme,* IMF Economic Review, v. 67, 2, pp. 315-348, **WP 1036 (October 2015).**

ERCOLANI V. and J. VALLE E AZEVEDO, *How can the government spending multiplier be small at the zero lower bound?,* Macroeconomic Dynamics, v. 23, 8. pp. 3457-2482, **WP 1174 (April 2018).**

FERRERO G., M. GROSS and S. NERI, *On secular stagnation and low interest rates: demography matters,* International Finance, v. 22, 3, pp. 262-278, **WP 1137 (September 2017).**

FOA G., L. GAMBACORTA, L. GUISO and P. E. MISTRULLI, *The supply side of household finance,* Review of Financial Studies, v.32, 10, pp. 3762-3798, **WP 1044 (November 2015).**

GERALI A. and S. NERI, *Natural rates across the Atlantic,* Journal of Macroeconomics, v. 62, article 103019, **WP 1140 (September 2017).**

GIORDANO C., M. MARINUCCI and A. SILVESTRINI, *The macro determinants of firms' and households' investment: evidence from Italy,* Economic Modelling, v. 78, pp. 118-133, **WP 1167 (March 2018).**

GOMELLINI M., D. PELLEGRINO and F. GIFFONI, *Human capital and urban growth in Italy,1981-2001,* Review of Urban & Regional Development Studies, v. 31, 2, pp. 77-101, **WP 1127 (July 2017).**

LIBERATI D. and M. LOBERTO, *Taxation and housing markets with search frictions,* Journal of Housing Economics, v. 46, article 101632, **WP 1105 (March 2017).**

MAGRI S., *Are lenders using risk-based pricing in the Italian consumer loan market? The effect of the 2008 crisis,* Journal of Credit Risk, v. 15, 1, pp. 27-65, **WP 1164 (January 2018).**

MERCATANTI A., T. MAKINEN and A. SILVESTRINI, *The role of financial factors for european corporate investment,* Journal of International Money and Finance, v. 96, pp. 246-258, **WP 1148 (October 2017).**

MIGLIETTA A., C. PICILLO and M. PIETRUNTI, *The impact of margin policies on the Italian repo market,* The North American Journal of Economics and Finance, v. 50, **WP 1028 (October 2015).**

MONTEFORTE L. and V. RAPONI, *Short-term forecasts of economic activity: are fortnightly factors useful?,* Journal of Forecasting, v. 38, 3, pp. 207-221, **WP 1177 (June 2018).**

NERI S. and A. NOTARPIETRO, *Collateral constraints, the zero lower bound, and the debt–deflation mechanism,* Economics Letters, v. 174, pp. 144-148, **WP 1040 (November 2015).**

PANCRAZI R. and M. PIETRUNTI, *Natural expectations and home equity extraction,* Journal of Housing Economics, v. 46, 4, **WP 984 (November 2014).**

PEREDA FERNANDEZ S., *Teachers and cheaters. Just an anagram?*, Journal of Human Capital, v. 13, 4, pp. 635-669, **WP 1047 (January 2016).**

RIGGI M., *Capital destruction, jobless recoveries, and the discipline device role of unemployment,* Macroeconomic Dynamics, v. 23, 2, pp. 590-624, **WP 871 (July 2012).**

2020

ALESSANDRI P. and M. BOTTERO, *Bank lending in uncertain times,* R European Economic Review, V. 128, **WP 1109 (April 2017).**

ANTUNES A. and V. ERCOLANI, *Public debt expansions and the dynamics of the household borrowing constraint,* Review of Economic Dynamics, v. 37, pp. 1-32, **WP 1268 (March 2020).**

ARDUINI T., E. PATACCHINI and E. RAINONE, *Treatment effects with heterogeneous externalities,* Journal of Business & Economic Statistics, , v. 38, 4, pp. 826-838, **WP 974 (October 2014).**

BOLOGNA P., A. MIGLIETTA and A. SEGURA, *Contagion in the CoCos market? A case study of two stress events,* International Journal of Central Banking, v. 16, 6, pp. 137-184, **WP 1201 (November 2018).**

BOTTERO M., F. MEZZANOTTI and S. LENZU, *Sovereign debt exposure and the Bank Lending Channel: impact on credit supply and the real economy,* Journal of International Economics, v. 126, article 103328, **WP 1032 (October 2015).**

BRIPI F., D. LOSCHIAVO and D. REVELLI, *Services trade and credit frictions: evidence with matched bank – firm data,* The World Economy, v. 43, 5, pp. 1216-1252, **WP 1110 (April 2017).**

BRONZINI R., G. CARAMELLINO and S. MAGRI, *Venture capitalists at work: a Diff-in-Diff approach at late-stages of the screening process,* Journal of Business Venturing, v. 35, 3, **WP 1131 (September 2017).**

BRONZINI R., S. MOCETTI and M. MONGARDINI, *The economic effects of big events: evidence from the Great Jubilee 2000 in Rome,* Journal of Regional Science, v. 60, 4, pp. 801-822, **WP 1208 (February 2019).**

COIBION O., Y. GORODNICHENKO and T. ROPELE, *Inflation expectations and firms' decisions: new causal evidence,* Quarterly Journal of Economics, v. 135, 1, pp. 165-219, **WP 1219 (April 2019).**

CORSELLO F. and V. NISPI LANDI, *Labor market and financial shocks: a time-varying analysis,* Journal of Money, Credit and Banking, v. 52, 4, pp. 777-801, **WP 1179 (June 2018).**

COVA P. and F. NATOLI, *The risk-taking channel of international financial flows,* Journal of International Money and Finance, v. 102, **WP 1152 (December 2017).**

D'ALESSIO G., *Measurement errors in survey data and the estimation of poverty and inequality indices,* Statistica Applicata - Italian Journal of Applied Statistics, v. 32, 3, **WP 1116 (June 2017).**

DEL PRETE S. and S. FEDERICO, *Do links between banks matter for bilateral trade? Evidence from financial crises,* Review of World Economic, v. 156, 4, pp. 859 - 885, **WP 1217 (April 2019).**

D'IGNAZIO A. and C. MENON, *The causal effect of credit Guarantees for SMEs: evidence from Italy,* The Scandinavian Journal of Economics, v. 122, 1, pp. 191-218, **WP 900 (February 2013).**

ERCOLANI V. and F. NATOLI, *Forecasting US recessions: the role of economic uncertainty,* Economics Letters, v. 193, **WP 1299 (October 2020).**

MAKINEN T., L. SARNO and G. ZINNA, *Risky bank guarantees,* Journal of Financial Economics, v. 136, 2, pp. 490-522, **WP 1232 (July 2019).**

MODENA F., E. RETTORE and G. M. TANZI, *The effect of grants on university dropout rates: evidence from the Italian case,* Journal of Human Capital, v. 14, 3, pp. 343-370, **WP 1193 (September 2018).**

NISPI LANDI V., *Capital controls spillovers,* Journal of International Money and Finance, v. 109, **WP 1184 (July 2018).**

PERICOLI M., *On risk factors of the stock–bond correlation,* International Finance, v. 23, 3, pp. 392-416, **WP 1198 (November 2018).**

RAINONE E., *The network nature of OTC interest rates,* Journal of Financial Markets, v.47, article 100525, **WP 1022 (July 2015).**

RAINONE E. and F. VACIRCA, *Estimating the money market microstructure with negative and zero interest rates*, Quantitative Finance, v. 20, 2, pp. 207-234, **WP 1059 (March 2016).**

RIZZICA L., *Raising aspirations and higher education. Evidence from the UK's widening participation policy,* Journal of Labor Economics, v. 38, 1, pp. 183-214, **WP 1188 (September 2018).**

SANTIONI, R., F. SCHIANTARELLI and P. STRAHAN, *Internal capital markets in times of crisis: the benefit of group affiliation*, Review of Finance, v. 24, 4, pp. 773-811, **WP 1146 (October 2017).**

SCHIANTARELLI F., M. STACCHINI and P. STRAHAN, *Bank Quality, judicial efficiency and loan repayment delays in Italy,* Journal of Finance , v. 75, 4, pp. 2139-2178, **WP 1072 (July 2016).**

2021

ALBANESE G., E. CIANI and G. DE BLASIO, *Anything new in town? The local effects of urban regeneration policies in Italy,* Regional Science and Urban Economics, v. 86, **WP 1214 (April 2019).**

FIDORA M., C. GIORDANO and M. SCHMITZ, *Real exchange rate misalignments in the Euro Area,* Open Economies Review, v. 32, 1, pp. 71-107, **WP 1162 (January 2018).**

LI F., A. MERCATANTI, T. MAKINEN and A. SILVESTRINI, *A regression discontinuity design for ordinal running variables: evaluating central bank purchases of corporate bonds,* The Annals of Applied Statistics, v. 15, 1, pp. 304-322, **WP 1213 (March 2019).**

LOSCHIAVO D., *Household debt and income inequality: evidence from Italian survey data,* Review of Income and Wealth. v. 67, 1, pp. 61-103, **WP 1095 (January 2017).**

NISPI LANDI V. and A. SCHIAVONE, *The effectiveness of capital controls,* Open Economies Review, v. 32, 1, pp. 183-211, **WP 1200 (November 2018).**

PEREDA FERNANDEZ S., *Copula-based random effects models for clustered data,* Journal of Business & Economic Statistics, v. 39, 2, pp. 575-588, **WP 1092 (January 2017).**

FORTHCOMING

ACCETTURO A., A. LAMORGESE, S. MOCETTI and D. PELLEGRINO, *Housing Price elasticity and growth: evidence from Italian cities,* Journal of Economic Geography, **WP 1267 (March 2020).**

ALBANESE G., G. DE BLASIO and A. LOCATELLI, *Does EU regional policy promote local TFP growth? Evidence from the Italian Mezzogiorno,* Papers in Regional Science, **WP 1253 (December 2019).**

ANZUINI A. and L. ROSSI, *Fiscal policy in the US: a new measure of uncertainty and its effects on the American economy,* Empirical Economics, **WP 1197 (November 2018).**

APRIGLIANO V. and D. LIBERATI, *Using credit variables to date business cycle and to estimate the probabilities of recession in real time*, The Manchester School, **WP 1229 (July 2019).**

BALTRUNAITE A., C. GIORGIANTONIO, S. MOCETTI and T. ORLANDO, Discretion and supplier selection in public procurement, Journal of Law, Economics, and Organization, **WP 1178 (June 2018)**

COVA P., P. PAGANO, A. NOTARPIETRO and M. PISANI, *Secular stagnation, R&D, public investment and monetary policy: a global-model perspective,* Macroeconomic Dynamics, **WP 1156 (December 2017).**

DE PHILIPPIS M., *Multitask agents and incentives: the case of teaching and research for university professors,* Economic Journal, **WP 1042 (December 2015).**

DEL PRETE S. and M. L. STEFANI, *Women as "Gold Dust": gender diversity in top boards and the performance of Italian banks,* Economic Notes, Monte dei Paschi di Siena, **WP 1014 (June 2015).**

HERTWECK M., V. LEWIS and S. VILLA, *Going the extra mile: effort by workers and job-seekers,* Journal of Money, Credit and Banking, **WP 1277 (June 2020).**

METELLI L. and F. NATOLI, *The international transmission of US tax shocks: a proxy-SVAR approach,* IMF Economic Review, **WP 1223 (June 2019).**

MOCETTI S., G. ROMA and E. RUBOLINO, *Knocking on parents' doors: regulation and intergenerational mobility,* Journal of Human Resources, **WP 1182 (July 2018).**

PERICOLI M. and M. TABOGA, *Nearly exact Bayesian estimation of non-linear no-arbitrage term-structure models,* Journal of Financial Econometrics, **WP 1189 (September 2018).**