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THE EFFECTS OF SHOP OPENING HOURS DEREGULATION: EVIDENCE FROM ITALY

by Lucia Rizzica[*], Giacomo Roma[*] and Gabriele Rovigatti[*]

Abstract

We estimate the effects of the deregulation of shop opening hours on the market structure of the retail sector and on the size and composition of the labour force employed there. To identify these effects, we exploit the staggered implementation of a reform that allowed Italian municipalities to adopt fully flexible opening hours in the late 1990s. Our findings indicate that the possibility of opening shops 24/7 increased employment in the retail sector by about three per cent and raised the number of shops in the affected municipalities by about two per cent. The effects were concentrated amongst workers employed in larger commercial outlets that were better able to exploit the flexibility introduced by the new regime. An analysis of individual-level evidence suggests that the deregulation also produced a recomposition of employment towards regular employees rather than self-employed workers.

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1 Introduction*

To what extent countries should regulate the entry and activity in the service sector has traditionally been a debated topic, both from the policy (Gal and Hijzen, 2016) and academic perspectives (Arnold et al., 2016; Barone and Cingano, 2011). According to the OECD (Égert, 2018), well-functioning and competitive markets require i) a sound legal and judicial infrastructure, ii) an effective competition regime, iii) an efficient insolvency regime and, finally, iv) a competition-friendly Product Market Regulation (PMR).[2] Despite a widespread consensus on a negative link between the stringency of PMR on one side and average firm size, productivity and investment on the other (Égert, 2016; Andrews and Cingano, 2014; Alesina et al., 2005), the relative impact of specific pieces of regulation is still disputed, with little empirical evidence on their real effects. More specifically, while there is a substantial number of contributions focusing on the impact of entry barriers on productivity (Schivardi and Viviano, 2011; Maican and Orth, 2015, 2018), employment (Bertrand and Kramarz, 2002; Viviano, 2008) and market structure (Sadun, 2015), which advocate the liberalization as a way to foster competition, less is known on the effects of other regulatory barriers and whether and to what extent they hinder market competitiveness and eventually affect social welfare.

In this paper we focus on how the regulation of shops opening hours affects the relevant market size and structure. This dimension of PMR has traditionally been a controversial issue in the policy debate, as it involves social, political, economic, and even religious considerations. The debate originated in the US as back as in the seventeenth century with the Blue Laws - i.e., the laws designed to restrict or ban some or all Sunday activities for religious reasons, particularly to promote the observance of a day of worship or rest - and was revived in the mid-nineteenth century thanks to several Supreme Court appeals which,

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[2]In its periodic survey, which results in the publication of the PMR indicators (both sectoral and economy-wide) the OECD quantifies the extent of several regulatory elements: from the administrative barriers to entry to the role of state-owned firms in the market, from the provision of anti-competitive codes of conduct for professionals to price and shop opening hours limitations. For further details see Koske et al. (2015).

in most cases, upheld the legitimacy of bans. In Europe the debate has been much less heated until recent years, when a wave of deregulation involved - with much cross-country heterogeneity - northern countries like Denmark, UK and the Netherlands as well as Germany, Italy and Spain. Despite the existence of a clear trend towards the liberalization of Sunday and public holiday openings in both the US and the EU regulatory frameworks, the timing and extent of the deregulation still vary across countries, states, regions.[3]

Those in favor of regulatory interventions not only underline the importance of the respect of the same day of rest for all workers but also advocate welfare losses due to the negative effects on small retailers which would suffer an unfair competition coming from large players, the latter being better able to cover longer work shifts. On the other hand, the advocates of liberalization stress that longer shopping hours have a positive impact not only on consumers' welfare, but also on employment, productivity and, ultimately, economic growth.

From a purely theoretical economic perspective, however, the effects of deregulating shops opening hours are ex-ante uncertain. In the short-run, the effect on employment may be null or positive depending on the degree to which shops respond by effectively opening longer hours; moreover, there may be either an increase in the number of workers employed or just an increase in the number of weekly hours worked, depending on the capability of firms to redistribute the working loads. In the longer run, instead, there are more complex general equilibrium effects on the market structure whose final effect on employment and economic growth remain ex-ante ambiguous. On the one hand, larger and more competitive shops are likely to benefit more than the small players (Schivardi and Viviano, 2011), to the point to force them out of the market - e.g., if longer shopping hours facilitate price comparison (Clemenz, 1990) or if Sunday openings lower travel costs to shop locations (De Meza, 1984) - on the other hand, the increased competition could promote small shops' specialization, and small retailers could benefit from the augmented demand due to longer shopping hours (i.e., small players might end up with a lower share of a market whose size is nonetheless bigger than in the regulated period).

[3]For a comprehensive review of regulatory changes in the US, see Burda and Weil (2004), see Maher (1995) for the UK case and Dijkgraaf and Gradus (2007) for the Netherlands.

In this paper, we tackle these questions empirically and estimate the effects of full deregulation in shop opening hours on the level and composition of employment and on the number of shops and their size distribution, thus shedding light on the implied market structure. To identify the effect of interest, we focus on Italy and build a novel dataset of Italian municipalities 2007-2016, including their regulatory status, and exploit the variation provided by the staggered implementation at the municipal level of a deregulation reform enacted from 1998 onwards. The core empirical analysis, then, relies on administrative data reporting the number of workers and plants located in each Italian municipality by year, sector of activity and plant size. We complement this analysis with individual-level evidence based on the Italian Labor Force Survey (LFS) which allows us to obtain a more detailed picture of the effects of the reform on the composition of employment in terms of individual and employment relation characteristics.

Our estimates show that, in the context of a general contraction of the retail sector and of the economy as a whole, deregulating shops opening hours helped lowering the decrease in both the number of workers and establishments, with an estimated positive impact of about 3% and 2%, respectively. Such effects were mainly driven by the large retailers, thus leading to a marked recomposition of the sector in favor of the latter. On top of it, individual-based estimates show that the sector's labor force structure changed towards a higher prevalence of employees over self-employed, together with a general increase in the number of hours worked and earnings of employees, especially of those with permanent contracts. Our results are robust to a number of checks that account for the possibility of geographical spillovers, the potential selection of deregulated municipalities and the different timing of the liberalization.

Our results are consistent with the scant evidence documenting the effects that deregulating shops opening hours produced in other countries. Some papers focusing on the US (Goos, 2004; Burda and Weil, 2004) found a negative effect of the "Blue Laws" on employment in the retail sector, both for part- and full-time work (with estimates ranging from 2 to 6%). Similarly, for Canada, Skuterud (2005) estimated that deregulating Sunday openings generated a positive effect on total employment between 5 and 12%. As for the European context, the existing literature focused on the case of Germany, where a deregulation of shopping hours was progressively enacted by the different states between 2006 and 2007.[4] In this context, Bossler and Oberfichtner (2017) and Senftleben-König (2014) found mixed evidence - no effects and mildly positive effects on part-time workers, respectively. On the other hand, exploiting the variation in deregulation across time and states to inform a difference-in-differences identification strategy, Paul (2015) estimated that the reform increased the overall sector employment rate, and that the individual employment probability rose by 2.5%, with marked differences across workers' subgroups and firm types.

We add to the existing literature in several ways. First, thanks to the very peculiar nature of the treatment in the Italian regulatory setting - i.e., confined within municipal boundaries - we are able to identify the average treatment effect at a very fine level, controlling for any potential confounding factor, policy-related distortion and regional peculiarity through several layers of fixed effects. Moreover, we manage to identify and control for geographical spillovers so as to account for possible bias due to the sorting of employers, workers and consumers across municipalities with different regulatory frameworks. Second, our data allow us to consistently estimate the effects of the deregulation on local market structures by looking not just at the number but also at the size distribution of active plants. Third, the use of worker-level data allows us to detail the impact on employment shedding light on the compositional effects generated by the reform. Finally, we are the first to study the impact of such reforms in the Italian setting, which is characterized by a very small average size of the shops and a low average level of productivity (Ciapanna and Rondinelli, 2014).

The remainder of the paper is structured as follows: in Section 2 we introduce the Italian regulatory framework of the retail sector; Section 3 is devoted to the description of the dataset and the discussion of the main features of the market; in Section 4 we introduce and discuss our identification strategy; Section 5 illustrates our main empirical findings; while Section 6 is dedicated to the individual level analysis; finally, Section 7 concludes.

[4]More specifically, the jurisdiction on shop opening hours' regulation was transferred from the federal government to the state governments, whose interventions led to a progressive liberalization between November, 2006 and July, 2007.

2 Institutional Background

In all OECD countries retail trade is regulated under several dimensions. Restrictions generally apply to the localization of stores, the licenses needed to start a business, the periods of sales promotions and maximum discounts applicable, and shop opening hours. With respect to the latter, regulation typically either specifies the opening and closing hours and the maximum number of hours a shop can stay open, or simply ban or restrict Sunday and public holidays shop openings.

The legislation on shop opening hours significantly varies across countries, even if different liberalization measures have been adopted in most of the OECD countries in recent years. According to the OECD PMR indicators, in 1998 shop opening hours were completely liberalized in 8 countries (out of 26); this number raised to 16 (out of 34) in 2013. In any case, no legislation provides for a complete ban of Sunday or public holidays opening. Exceptions generally regard touristic areas or a limited number of Sunday or public holidays per year. In some jurisdictions, restrictions do not apply to small shops. In some cases, rules are fixed at the regional or local level: for example, in Germany and Spain the restrictions significantly vary across areas, and are less constraining in the capital cities. In addition to this, even in countries where some limitation has been maintained, restrictions were loosened lately. For example, in 2015 the French Parliament passed a law ("loi Macron") to increase the number of Sunday openings allowed yearly from 4 to 12.

In Italy, the retail trade regulation dates back to the first half of the XX century. Until the end of the 1990s Sunday openings were generally prohibited and opening hours strictly limited; exceptions, yet, could be decided by the regional governments, thus returning a scattered regulatory picture over the national territory.

In 1998, then, a comprehensive reform of the sector was passed (the "Bersani Decree") with the aim of loosening some of the restrictions and boost competition. In particular, the reform fixed, for the whole national territory, the set of administrative authorizations required to open new shops and the relevant criteria to be considered (e.g., urban planning constraints) that varied on the basis of the shop's size, smaller shops being generally subject to milder requirements.[5] Moreover, the reform established a general regulation of shops opening hours that restricted both the number of Sunday openings (8 per year) and the daily opening hours (a maximum of 13 hours within the 7AM-10PM period) for all shops. However, two main exceptions were introduced: (i) shops could stay open every day in December in all municipalities; (ii) all the restrictions were removed in the municipalities which most relied on tourism. Such municipalities (henceforth "touristic municipalities") were included in a list compiled at the regional level. First, the regions set the criteria that municipalities should fulfill in order to obtain such status and thus be exempted from any restriction on shop opening hours. Then, each municipality could apply to be included in the list. The regional governments were entrusted with the power to include in the list the municipalities that complied with the criteria and to apply limitations on the periods of the year or the areas of the municipality that were subject to the deregulated regime (i.e., periods or areas most subject to touristic flows).[6] The lists made by the regional administrations were updated over time, following the same process.

In December 2011, eventually, all these rules were repealed: the Law Decree 201/2011 ("Salva Italia") completely liberalized days and hours of shopping all over the country, thus overcoming the distinction between touristic and non touristic municipalities.

3 Data and Descriptives

In order to analyze the impact of shop opening hours deregulation we first had to reconstruct the regulatory framework that applied to each Italian municipality over the period of interest. To do this we collected the relevant regional legislation and examined the administrative acts adopted by all regional governments between 1998 and 2011 so as to identify

[5]In the implementation of these new rules concerning entry, some regions imposed a more restrictive interpretation of the provisions of the Bersani Decree. Such cross-regional variation has been exploited by Schivardi and Viviano (2011) to estimate the impact of entry barriers on productivity. Note however that such different strictness in the application of the Bersani Decree was unrelated to the characteristics of the municipalities that determined the shop opening hours regime applicable.

[6]In all cases the regional regulations could not impose limits more rigid than the national-level benchmark discipline.

the municipalities that were granted the "touristic" status at some point in time.[7] On top of recovering the list of touristic municipalities, we also checked whether, according to the regional law, the qualification of a municipality as touristic allowed shops to open 365 days a year or whether some restrictions were imposed at the regional level.[8] We were not able to gather information on three regions: Liguria, Toscana and Umbria, which we are forced to exclude from the analysis.[9]

The dataset comprises two subsets of municipalities: i) the *fully deregulated* i.e., those located in regions where regional laws did not provide any restriction to Sunday and holiday openings, and ii) the *partially deregulated* i.e., those subject to some time or spatial restriction. In Figure 1 we plot the fraction of fully and partially deregulated municipalities by year. The figure highlights the process of gradual deregulation to which Italian municipalities were subject until the complete deregulation passed at the end of 2011.[10]

To give an idea of the regulatory regime in place before the enactment of the full deregulation, in Figure 2 we show the maps of Piedmont and Lazio - two of the biggest regions in Italy - in 2010: dark blue areas correspond to touristic areas, whereas light blue marks the partially deregulated municipalities; finally, grayish areas are relative to not touristic municipalities (i.e., restricted). The difference between the two plots highlights the distinct regulatory approaches followed by the two regional authorities: while in Piedmont we do not record regional-level limitations to liberalization, in Lazio there were limitations in place at the central level, therefore the *fully deregulated* status represented an exception to the general rule.

[7] The regional legislation is published on the institutional websites of the regional governments. This is also the case for some of the administrative acts under consideration; for the others, we required the regional administrations to disclose them upon request.

[8] There were several regions with limitations in terms of time - e.g., stores were allowed to open up to a maximum number of Sundays during the year - or space (typically, vast municipalities were deregulated in their central neighborhoods only).

[9] In these regions the "touristic" status was autonomously decided by the municipalities themselves with local administrative acts.

[10] In Table B.1 we report the number of municipalities which transited from the "non-touristic" to the touristic status, per year and region, and the relative share on the total number of regional municipalities in parentheses.

We merge the municipality-year data on the regulatory framework with the Asia database, from 2007 to 2016.[11] Asia is an administrative dataset containing information on the total number of workers and plants by year and sector of activity at the municipal level; it also contains information on firms size - in particular, the data on both the number of plants and the number of workers, is split into large (≥ 3 workers) and small plants (< 3 workers).[12] The final dataset is a balanced panel of 6,710 Italian municipalities that reports the touristic status, the number of workers and plants in the retail sector (total and split between small and large plants), plus socio-demographic variables taken from the latest available Census (2011).

In Figure 3 we plot the variation in the number of retail workers (left) and establishments (right) between 2007 and 2016. Alongside country-level measures, we also separately plot the figures for large and small firms. We document a general decreasing trend starting after the financial crisis of 2008 and the sovereign debt crisis of 2011, which translates in a 5%loss in terms of number of workers and in a loss of about 10% in terms of total plants in 2016 relative to 2007 countrywide. The effect, however, is composite, and reflects the deep changes which characterized the market structure of the retail sector. While large firms show a discontinuous pattern, with an expansion until 2009/2010, followed by a retraction until 2014, and a renewed growth until 2016, small firms alternated harsh declines to periods of relative stagnation, in both dimensions. As a result, over the period of analysis the market share of large firms has increased at the expense of small and individual firms, and in most cases they have taken the lead in driving the recovery.

The patterns observed were significantly different across macro-areas: in Figure 4 we provide some descriptive evidence on geographical differences, separately considering northern, central and southern regions. For large and small firms we report the number of workers

[11] Asia (*Registro Statistico delle Imprese Attive*), Istat.

[12] The median plant size for the retail sector over the period of observation is 4.4.

and plants per inhabitant, 2007-2016. First, the figures reveal significant and persistent differences between the South and the rest of the country: the rate of employment in the retail sector is about 23% lower in the South relative to the North; on the other hand, the overall number of plants per inhabitant is quite similar but its composition differs significantly, shops in the South being predominantly small. Secondly, as for the dynamics, we observe that all macroregions recorded sizeable drops in both employment and the number of plants over the period, with the trend being driven by small firms. The South experienced a partially different pattern, with a stable employment level in retail, but with its composition changing in favor of large plants.

4 Empirical Strategy

The application of the touristic status, the time and spatial limits, and the national-level deregulation introduced with the 2011 reform provide exogenous variation in the regulatory framework at the municipal level that we exploit for identification in a (dynamic) difference-in-differences or event study setting (Autor, 2003). In particular, we are able to quantify the effects of extended opening hours and Sunday opening on measures related to employment on the one hand (e.g., number of workers in the retail sector), and on the market structure on the other (i.e., total number of plants and size distribution) in the main analysis.

The existence of a subset of municipalities with limited or no prior restrictions on opening hours (the touristic municipalities) allows us to cluster our sample in three groups: first, the *control group* which includes all the municipalities that, at the beginning of our period of analysis (2007), were already fully deregulated; second, the *treatment group*, which comprises all municipalities where the restrictions were totally repealed at some point in time between 2007 and 2016;[13] third, the *partial control group*, made of all touristic municipalities which

[13] 86% of these were deregulated in 2011 with the *erga omnes* act, while the others entered the list of touristic municipalities in the preceding years. Only the municiaplities of Veneto and Trentino Alto-Adige were deregulated in 2013 due to a late reception of the norm (Figure B.1).

were deregulated only partially, and whose regulatory status was only partially affected by the 2011 reform.

In Table 1 we present descriptive statistics for the three groups at the beginning of the period analyzed (2007). The figures suggest that the three groups of municipalities are rather different. Those in the control group are small but relatively rich: they display the lowest levels of economic activity in the retail sector (both in terms of total workers and plants and for small and large units separately), the smallest population and surface, but the highest average income and the lowest unemployment rate. Municipalities in the partial control group, instead, are on average those with the highest number of workers and plants in the retail sector, the largest population and surface and the highest level of human capital (which we proxy by the share of graduates). This group, indeed, includes all the largest cities in Italy, (e.g., Rome, Milan and Naples) where the deregulation applied to the shops located in the city center only.

In order to identify the effect of the deregulation of opening hours, we employ a model which is flexible enough to exploit the variation in time provided by the treatment at the municipal level on the one hand, and to correctly sort out the cross-sectional (i.e., municipal) differences on the other. The resulting estimating equation reads:

$$Y_{it} = \alpha + \beta \ Liberalization_{it} + \mu_i + \tau_t \,[+ \psi_{r,t}\,] + \varepsilon_{it} \tag{1}$$

where $Y_{it} = [(log)workers_{it}, (log)plants_{it}]$, $Liberalization_{it}$ is an indicator function for municipality i in year t being (fully) deregulated, and τ_t and μ_i are year and municipality fixed effects, respectively. $\psi_{r,t}$ are region-year fixed effects which we include for robustness to additionally control for potential regional trends. The parameter of interest - β - captures the differential changes in Y_{it} due to the deregulation. As the norm was offering only the possibility for shops to stay open longer hours, the coefficient of interest is yet to be interpreted as an Intention To Treat (ITT) effect of Sunday Openings or Average Treatment effect on the Treated (ATT) of the deregulation. Standard errors are clustered at the municipal level, that defines inclusion into the treatment or control group.

Note that our approach differs from a standard Diff-in-Diff in that we are using as control group the "always treated". Such design, already applied in several notable papers (Kotchen and Grant, 2011; Chemin and Wasmer, 2009), follows a methodology that has been labeled as "Difference in differences in reverse" (DDR) by Kim and Lee (2019). As for the standard Diff-in-Diff setting, the only assumption required for the DDR to hold is that, absent the treatment, the control and treatment groups would follow a parallel trend, indeed, the two strategies yield exactly the same result as long as time does not affect differently treated and untreated units.[14]

In the main analysis, we restrict the estimation sample to the treatment and control groups, leaving the analysis of the partial control group as a robustness check (Section 5.2). The space and time limitations that applied to the partial control units make such municipalities unsuitable for both being part of the treatment and of the control groups. Indeed, the deregulation of 2011 represented an actual treatment for them, but its "intensity" changed according to the initial limitations in terms of space or time.[15]

5 Results

5.1 Main results

In Table 2 we present our baseline results. In columns (1)-(2) we report the estimates of the liberalization effect on the number of workers, and in columns (3)-(4) those on the number of plants in each municipality. As both outcomes are expressed in logarithmic scale, the estimated coefficients are to be interpreted as semi-elasticities. In columns (1) and (3) we include municipality and year fixed effects, the resulting estimated effect of liberalization in

[14]See Appendix A for a more formal discussion.

[15]For example, in a municipality with a maximum of 32 Sunday openings allowed, the deregulation would have had an "intensity" of 52 - 32 = 20, with 40 initial Sunday openings it would have amounted to 12, and so on.

the newly liberalized municipalities is a 3.4% increase in the number of individuals working in the wholesale and retail sector, and a 2.1% increase in the number of shops.

The magnitude of these effects is lower, respectively 2.1% and 1.6%, but still significantly different from zero when we impose the more demanding specification with region-year fixed effects to account for local idiosyncratic shocks (columns 2 and 4).

Note that the validity of our difference-in-differences relies on the assumption that, absent the treatment, the difference in outcomes between control and treatment groups would have been constant over time (Parallel Trend Assumption, PTA). In order to test this assumption, we run an event study type of analysis à la Autor (2003); more specifically, we interact the treatment group indicator with *time-to-treatment* binary indicators, from $t-5$ (i.e., five years before the treatment) to $t+9$, and regress the outcome variable(s) on them. To satisfy the PTA, we require no parameter for terms *before* the treatment to be significantly different from zero.

In Table 3 we report the estimated parameters, with the baseline (excluded) year at $t-1$. We present the results for both specifications - i.e., with (columns 1 and 3) and without (columns 2 and 4) region-year fixed effects. The results confirm that the estimated coefficients are null *before* the liberalization and become positive as soon as the municipality is liberalized (time t), for both specifications. The one with year and municipality fixed effects only returns two negative coefficients before the liberalization date, thus signaling that there may be some concerns in terms of diverging trends; yet, the more demanding specification with region-year fixed effects reassures us that any pre-existing difference between the two groups of municipalities is correctly accounted for. On the other hand, the coefficients *after* the treatment turn out to be constantly positive, thus suggesting that the treatment effect is permanent - or, at least, long-lasting. The low level of statistical significance for any $t > t+4$ is due to the fact that the number of treated units shrinks a lot as we increase the distance from the treatment (Duflo et al., 2008).[16]

[16] Only the municipalities that were deregulated *before* 2011 would be observable five or more years later

In the spirit of Autor (2003), Figures 5 and 6 provide graphical evidence supporting the absence of pretends that may bias the estimations. Following Dobkin et al. (2018), in the graphs we also plot the counterfactual trend, i.e., the one that would have been observed in the absence of the deregulation. This is a simple linear trend imputed on the basis of the coefficients estimated before the treatment. This exercise confirms that the treatment effects estimated after the liberalization are all significantly different from zero and from the imputed counterfactual trend.

5.2 Robustness Checks

Our baseline results shall be interpreted as the effect of moving from a regime of strictly regulated opening hours to one of fully flexible hours - i.e., 24/7. There are several municipalities, however, which already enjoyed forms of liberalization limited in time, space, or both (the *partial control group*). For those cases, we would still expect a positive effect of the full liberalization, but to a lesser extent: in Table 4, columns 1 and 6, we run the esti-mation on a sample that includes the partially deregulated units within the control group, so as to estimate the effect of being granted fully flexible opening schedules versus being subject to any type of restrictions. As expected, the estimated coefficients are smaller in magnitude: in our preferred specification the effects of being fully liberalized amount to 2.2%extra employment and to 1.1% extra plants in the wholesale and retail sector.[17]

We then propose a battery of robustness checks aimed at ensuring that all results obtained do not depend on the specification, sample, or identification strategy chosen.

in our sample. Symmetrically, only the municipalities deregulated in 2013 (those in Veneto and Trentino Alto-Adige, Table B.1) can be observed at $t-5$.

[17]Similarly, in Appendix Table B.2 we replicate our estimates including the "partial control" group in the treatment group instead. Again, the results we obtain are smaller in magnitude than those of our baseline exercise and closer to the ones in Table 4.

A prime concern is that selection into the pool of touristic municipalities may have been somehow endogenous - e.g., because the municipalities which could gain the most from the liberalization had the highest incentive to lobby the regional authorities, or because these took the potential gains in the retail sector into account in selecting the touristic municipalities. If this were the case, our results could turn out to be significantly upward biased. To rule out this possibility, in columns 2 and 7 we restrict our treatment group to the municipalities that were subject to the main wave of liberalization i.e., at the end of 2011. The new treatment group amounts to about 75% of the full group, but with this selection we are able estimate a classical 2×2 difference-in-differences model:

$$Y_{it} = \alpha + \beta \ (Treated_{it} \times Post\ 2011_{it}) + \gamma\ Treated_{it} + \delta\ Post\ 2011_{it} + \varepsilon_{it} \tag{2}$$

If it was the selection effect that drove our baseline estimates, we should find lower or no effect with the 2011-only sample: instead, the estimated coefficients are sensibly larger than the baseline, and strongly significant.[18]

The next robustness check that we perform directly addresses the concern that structural differences between the municipalities in the treatment and in the control group may be driving our results. To this end, we estimate a propensity score model to weight the control group during the estimation, using the municipality's population, the share of graduates and the local unemployment rate, all measured in 2011, as predictive variables. The results, reported in columns 3 and 8, are in line with the baseline model and, if any, they show a slightly larger magnitude.

An additional test of robustness follows the approach introduced by Goodman-Bacon (2018). The proposed method addresses the concern that the treatment effect estimates may be biased when units are treated at different points in time. In those cases, as the author argues, the parameter of interest should be a weighted average of all possible two-

[18]Moreover, in Appendix Table B.3 we estimate our baseline model restricting the sample to the pre-2012 full deregulation. In this case the treated municipalities are those that were deregulated between 2007 and 2011 and the control ones are the ones that were then deregulated in 2012 and thus at the time were not treated. This more standard exercise produces positive but not statistically significant coefficients of a slightly smaller magnitude. Again, this points against the existence of an ex-ante selection into treatment.

group/two-period difference-in-differences estimators in the data, provided that the PTA holds for each estimation subgroup.[19] In columns 4 and 9 we present the estimates obtained with the Goodman-Bacon estimator, which are extremely robust.

In columns 5 and 10, finally, we add a battery of fixed effects at the treatment group-year level. This allows us to absorb any possible trend that might have affected treated and control municipalities differentially: still, the baseline results hold.

5.3 Heterogeneous effects

While the baseline results are informative on the average effect of the deregulation, to better inform the policy implications of the reform we are interested in exploring the differentials of such effects depending on several firm and local market characteristics.[20]

First, in Table 5, columns 1 and 2, we separately estimate equation (1) for municipalities in the Center-North and in the South of Italy. We are forced to bind together central and northern regions because our data for the Center are incomplete and most regions opted for partial liberalizations. The results reveal that the effect on both employment and plants has been stronger for central and northern regions. In particular, we estimate the effect on additional workers and plants in the Center-North to be one third higher (+3.5% versus +2.4%) and more than double (+2.5% versus +1.1%) that in southern regions. In turn, this evidence seems to suggest that the size of the plants increased more sensibly in the South, whereas in the North this was also accompanied by an increase in the number of shops.[21]

In the context of a progressive sectoral recomposition in favor of larger plants, i.e., a

[19]Note, however, that the 2011-only exercise that we perform, being a simple 2×2 difference-in-differences, already partially addressed this issue.

[20]In Appendix table B.4 we run the same estimates controlling for Region-Year fixed effects, the estimated coefficients are qualitatively similar but more imprecisely estimated because of the smaller sample size.

[21]In Appendix Table B.5, we provide support to this result by using data on employment and number and total surface of large chain store units provided by the data analytics company Nielsen. More specifically, we find that the increase in chain stores employees and plants was driven by units in the Center-North (while we don't find significant effects in southern regions).

declining trend for the small shops and a growing presence of large chain stores,[22] it becomes of first-order importance to understand whether the liberalization affected the two types of firms differently thus mitigating or accentuating the gap. In columns 3 and 4, we repeat the baseline exercise on the number of workers and plants for small and large firms (i.e., below or above three workers). We find that the increase was higher for large plants (+3.6% and +2.5% for workers and plants, respectively) than for small shops (+2.2% and +1.8%), although both significantly different from zero. In subsection 5.4 we extend our results to account for possible geographical spillovers, which may display differently in the case of large and small shops, and show the robustness of the results to this dimension, too.[23]

Finally, we estimate the effects along the distribution of municipalities' size. In columns 5 and 6, we split our sample in small (i.e., $< 5,000$ inhabitants) and large towns and find that in small centers the liberalization effects were slightly stronger than in the larger ones (i.e., +3.6% against +2.4% in terms of workers, and +2.1% against +1.6% for plants). As for the case of plant size heterogeneities, again in this case, our results may be biased by potential spillover effects that differently affect small and large towns.[24] In the following paragraph we directly tackle this issue so as to corroborate the proposed model.

5.4 Spatial spillovers

Potential spillover and relocation effects of place-based policy interventions have been widely documented in the fields of regional science and urban economics (Glaeser and Gottlieb, 2008; Monte et al., 2018; Ehrlich and Seidel, 2018; Falck et al., 2019)), as well as in industrial organization (see, for instance, applications to public procurement in Branzoli and Decarolis

[22] see e.g. Ciapanna and Rondinelli (2014).

[23] For example, consider the case of a potential customer living in a non-deregulated municipality: she would be more willing to move to a nearby deregulated municipality (i.e., to pay the transportation and the opportunity costs) in order to shop in a large department store than to shop in a small, family-owned unit. If this is the case, the estimates of the effect on large firms may be downward biased.

[24] For example, small towns, mostly those surrounded by bigger, and liberalized centers, might potentially enjoy additional benefits from retaining the demand previously absorbed by the neighbors. Our estimated coefficient for small towns would then be upward biased.

(2015) and Conley and Decarolis (2016)). The displacement effects induced by the differences in regulation across neighboring municipalities may affect our results in two main ways. On the one hand, if sellers chose to relocate their shops from restricted to liberalized municipalities, our estimate of β would be downward biased, given that the demand of treated units would already be met, at least partially, by sellers located in nearby municipalities. On the other hand, even if sellers do not adjust their location choices according to the liberalization status, buyers may adjust their consumption choices by traveling to nearby municipalities to go shopping. In such case our estimates would be upward biased: in fact, once the shops are allowed to open flexibly in the municipality of residence, individuals will *switch* suppliers opting for the local ones (i.e., will minimize transaction costs). In turn, this would shift down employment in the control municipalities and increase it into the treated ones without a real increase in the overall sector size. In the light of such reasonings, it becomes of utmost importance to find out whether the estimated coefficients result from a geographical shift of the retail activities or whether they represent an effective boost of the sector.

In order to ensure that our results are not biased by spillover, displacement or relocation effects, we propose three different strategies. First, we aggregate our data in larger administrative units, whose boundaries are designed to be big enough as to "contain" the spillovers, and repeat the estimation. Second, we exclude all control units with a shared boundary with a treated municipality, as these are potentially more affected by local relocation - "spatial exclusion approach", Ehrlich and Seidel (2018). Third, in order to directly estimate the magnitude of the spillover effects, we augment our difference-in-differences with variables reflecting the number and composition of neighboring municipalities, before and after the deregulation (Table 6).[25]

The first exercise to account for local spillovers consists in aggregating the data at the Local Labor Market (LLM) level. LLMs are self-contained labor markets where most of

[25]The same empirical approach to the difference-in-difference method in the presence of spillover effects has been independently developed by Berg and Streitz (2019). In their paper, they show the consistency of the method and quantify the bias in the baseline estimation without accounting for the spillover effects.

the people live and work[26] and therefore are the most suited geographical units to examine labor market effects (e.g., employment, consumption) of local shocks accounting for possible spatial spillovers. Each LLM contains both control and treated municipalities - hence, the full sample cannot yield a binary treatment. Therefore, for each LLM (and year) we compute the share of population living in deregulated municipalities ($Q_{LLM,t}$) and proceed with a slight modification of the estimation model, which now reads:

$$Y_{LLM,t} = \alpha + \beta \; Q_{LLM,t} + \mu_{LLM} + \tau_t + \varepsilon_{LLM,t} \tag{3}$$

However, we are able to identify a subset of Local Labor Markets in which either all the municipalities were deregulated in the same year or all the municipalities were deregulated before 2005. This distinction allows us to run the baseline equation at the LLM level, although on a restricted sample:

$$Y_{LLM,t} = \alpha + \beta \; Liberalization_{LLM,t} + \mu_{LLM} + \tau_t + \varepsilon_{LLM,t} \tag{4}$$

The results of the two exercises are reported in columns 1-4 and 2-5 of Table 6, respectively. We find that when all municipalities in a Local Labor Market are liberalized, employment in the retail sector increases by 3.1% and the effect is even larger in our cleanest identification strategy (3.7%). On the other hand, we find a small and not statistically significant effect on the total number of shops in the area, suggesting that shops increased their scale of activity (intensive margin), but did not increase their number (extensive margin).

In columns 3 and 5, then, we re-estimate our baseline model excluding the control units that directly neighbor the treated ones. Note that the sample size shrinks very little because in our setting the number of control units was sensibly smaller than that of the treated ones. The resulting estimated coefficients are slightly smaller with respect to the baseline model: 2.7% and 2%, respectively for workers and plants.

[26] Each LLM contains about 13 municipalities on average, with significant variability depending on the accessibility of the area. The boundaries of each LLM are released by the National Statistical Office based on the so-called commuting matrix, which account for the number of workers commuting from and to each location in the country.

In our setting, we see two possible spillover effects at play, depending on the status of the municipality: first, shops in liberalized municipalities may capture part of the demand from neighbor municipalities (*baseline spillover*). These are stronger for restricted municipalities; however, once liberalized they would be able to regain part of that demand (*spillover on the treated*). The substantial robustness of the liberalization parameter in our LLM-level exercises indicates that the sum of the spillover effects within a given LLMs is essentially null. In turn, this could either mean that the spillovers are in fact negligible, or that the spillover effect on the treated fully offsets the baseline spillovers. In order to disentangle these counteracting effects, we augment the baseline model of Equation (1) with the variable lib_neigh_{it}, which measures the number of control municipalities that are contiguous to the i^{th} municipality at each time t, and with its interaction with the *liberalization* variable.[27] The resulting model reads:

$$
\begin{aligned}
Y_{it} = \alpha + \beta\ liberalization_{it} + \mu_i + \tau_t \\
+ \gamma^t\ lib\_neigh_{it} + \gamma^{tt}\ (lib\_neigh_{it} \times liberalization_{it}) + \varepsilon_{it} \quad (5)
\end{aligned}
$$

where γ^t captures the effect on the i^{th} unit of being surrounded by one extra liberalized municipality (*baseline spillover*), while γ^{tt}, by capturing the interaction of liberalized neighbors and the liberalization treatment, provides the estimate of the *spillover on the treated*. Through model (5), we are able to disentangle the direct effect of the deregulation from that induced by relocation.[28] The results in columns 4 and 8 show that the baseline spillovers have positive sign, i.e., the liberalized neighbors do in fact steal demand from their neighbors, but i) it only applies to the most mobile input, the workers, ii) the magnitude of the effect is very small compared to the direct treatment effect (in the order of one tenth of the direct effect for every already liberalized neighboring municipality), and iii) the effect is almost completely offset by the spillover on the treated (more formally, $\hat{\gamma^t} + \hat{\gamma^{tt}} = 0$).

[27] The estimated effects are identified thanks to the staggered implementation of the liberalization, which affects both the status of the i^{th} unit and that of its neighbors at different points in time.

[28] See Appendix C for a more detailed discussion of the implications of this model.

In Table 7, we replicate all these analyses of the spillover effects splitting our sample into small/large plants and small/large towns (respectively in panel A and panel B). The first exercise shows that the positive effect of the deregulation was largely concentrated across larger plants, while the second reveals no significant differences between the effects for small and large towns.

In turn, the positive, significant and robust estimates of the treatment effect for all variables and subsamples confirm that the liberalization had net positive effects that go well beyond the simple geographical recomposition of the demand.

5.5 Sectoral spillovers

We finally consider the possibility that the deregulation of shop opening hours may induce spillover effects on sectors other than the retail. To explore this channel we estimate the baseline model on the number of workers and plants in sectors other than wholesale and retail trade. In our view, this exercise is both a robustness check, to the extent that we should not find any effect on the sectors that were not affected by the deregulation, and a test for possible spillover effects. Indeed, there are sectors that were indirectly affected by the liberalization of the retail sector either positively - it is the case for sectors complementing retail activities - or negatively, if workers and entrepreneurs are induced to flee from a given sector towards retail activities. In figure 7 we plot the estimated coefficients for each sector of economic activity.

Our results show that the deregulation of shop opening hours generally generated no effect on employment in other sectors. Interestingly, yet, we find that employment increased significantly in the sector of hotels and restaurants, thus suggesting that these activities benefited from the fact that more shops were open at night or on Sundays. If we consider the effects on the number of plants, instead, we find evidence of a significant increase in the number of banks and other financial sector's branches. This may also be due to a general increase in the volume of economic activity.

As a final exercise, in Table 8 we estimate the effect of shop opening hours deregulation on overall employment and number of plants. We present results for three specifications: the baseline model in (1), and the two specifications estimated at the Local Labor Market level (equations (3) and (4)). All specifications include sector-year fixed effects to account for sector-specific shocks. We find a positive and significant increase in the level of overall employment in the economy in the order of 1.5% and no effect on the total number of plants, the effects being robust across the three specifications.

6 Individual-level evidence

In this section we complement the main analysis with evidence at the individual level in order to better qualify the type of employment relationships created and the type of workers involved in the transition, i.e, the compositional effects of the deregulation. To this pur-pose, we use the data from the Italian *Labor Force Survey* (LFS) which contains quarterly individual and household level information on education, employment history and demographic characteristics. We merge the LFS data with an augmented (i.e., quarterly) version of the dataset on retail deregulation used in the main analysis, exploiting information on household's municipality of residence to match the two.[29]

Table 9 reports the main descriptive analysis of the sample in 2007, *before* the deregulation of treated municipalities. It confirms the evidence in Table 1 for which individuals living in treated municipalities are on average more educated than those in the control group (but less than those in the partial control group) and that the overall employment rate

[29]Note that we do not have information on the municipality where individuals work. This may bias our results to the extent that we may misclassify individuals into treatment and control groups. On the other hand, the municipality of residence is less subject than that of work to endogeneity concerns.

is lower. Among the workers of the retail sector, the share of self-employed higher in the control municipalities where workers, before the treatment, were also more likely to work on Sundays. Focusing on the employees only, we observe that in the already deregulated municipalities they were generally more likely to hold a permanent work contract and were paid slightly higher wages.

In this setting, our empirical specification will necessarily differ from the baseline specification in (1), because the LFS design does not require to survey households in all municipalities in all years. In fact, each wave covers about 1,200 municipalities and they rotate, in a way that smaller municipalities change from one quarter to the other.[30] This structure of the data does not allow us to run regressions with municipality fixed effects, and we resort to a specification with LLM fixed effects. The resulting estimating equation reads:

$$Y_{ilq} = \alpha + \beta \; Liberalization_{lq} + X_{ilq} + \lambda_l + \tau_q + \varepsilon_{ilq} \tag{6}$$

where $Liberalization$ is defined as before, λ_l and τ_q are LLM and quarter FE, respectively, and X_{ilq} are individual-level characteristics.[31] The estimates are reported in Table 10.

The main outcome of interest (columns 1 and 2) is an indicator variable for whether individual i, residing in municipality m in quarter q is employed in the retail sector or not, the latter option including both non employment and employment in other sectors. Results are consistent with those of the main analysis: the deregulation of shop opening hours induced an overall increase in the probability of being employed of about 0.2 percentage points. Given the share of individuals who are employed in the retail sector (i.e., roughly

[30]The survey is designed as to build a representative sample of the whole population in each quarter hence, large municipalities are always surveyed, whereas small and very small towns appear occasionally.

[31]In our main specification, we include an indicator function for the gender, the age, the age squared, and indicator functions for the education level.

5%), we estimate a marginal effect of around 4%, qualitatively similar (slightly larger) to the one reported in Table 2. The result is strongly robust to the inclusion of individual control variables.[32] Second (column 3), we restrict our analysis to employed individuals only and estimate a model of workers' relocation, i.e. whether the reform induced a change in the sectoral composition of employment in the affected municipalities. We find that, conditional on being employed, the probability of holding a job in this sector rose by 0.8 percentage points.

The evidence presented in Section 5 suggested that the deregulation favored a recomposition of the sector towards larger plants. This should correspond, at a micro level, to an increased probability of being an employee instead of self-employed. In column 4, thus, we estimate Equation (6) on an indicator function for being self-employed conditional on working in the retail sector, and find that the deregulation lowered the share of self-employed among retail sector workers by about 1.6 percentage points (about 7 percent). In line with this result, in column 5, we also find that the probability of working in a chain store increased.[33] Finally, in column 6, we test whether the deregulation further induced a recomposition of the sector towards permanent or fixed term employment relationships. The results suggest no significant change in this respect.

We then focus on the intensive margin and test whether the deregulation affected the likelihood of working on Sunday and the total number of hours worked weekly.

In panel A we show the estimates of the likelihood of working on Sunday. This is, in a way, a test of the degree of compliance to the the reform. We find that overall the share of retail sector workers working on Sundays rose by 3.4 percentage points from a baseline probability of 19%. Employees were more affected than the self-employed (+5 p.p. vs. 2.3 p.p.). Coherently with this, we also find that those working in large multi-branch stores

[32]In Appendix Table B.6 we report some heterogeneous effects of the estimation of Equation (6). We find a stronger increase in participation for men, individuals aged over 25 and those with less than tertiary education.

[33]LFS data do not contain an indicator variable for being employed in a chain store. We proxy this variable with the interaction between the indicator for stores with multiple branches, and with 10+ workers.

(chain stores) experienced a larger increase in their propnsity to work on sunday that the workers of the small shos (+4 p.p. vs. 2.9 p.p.). Finally, and perhaps most interestingly, we find that temporary workers adapted their working shifts to the Sunday openings more than those with permanent contracts (+5.5 p.p. vs. 3.4 p.p.).

The analysis of the responses on the intensive margin (weekly hours worked), that we report in panel B, provides additional evidence on the mechanisms triggered by the opening hours liberalization. First, and most strikingly, we do not find a significant average effect on the number of hours worked (column 1); this result, together with our baseline results on the number of workers, suggests that most stores adapted to the new hours regime by increasing the number of individuals employed, rather than the length of their shifts. There are, however, differences in this approach depending on the type of workers: while employees slightly increased their working hours (+1.3%, column 2), self-employed did not react significantly (column 3); moreover, the increase in average working hours affected to a similar extent employees of both large and small stores (columns 4 and 5), and those with a fixed-term and with a permanent contract (columns 6 and 7).

Finally, in panel C, we investigate how, and to what extent, the observed compositional effects and the changes in the number of hours worked, were passed-through to employees' earnings. We find a baseline average increase of 1.9% (column 1), which is stable across firm types (columns 4 and 5). There is, however, a remarkable difference in the effects on wages for permanent workers (columns 6 and 7), whose increase of 2.2% in monthly earnings is significantly higher than that experienced by temporary workers. Such difference likely reflects an increased likelihood of being employed by large employers who presumably offer better contracts. Temporary workers, on the other hand, were not equally protected and thus did not experience any wage increase despite their increased likelihood of working on Sundays.

Conclusions

In this paper we estimate the causal effects of shop opening hours deregulation on the number of workers, the market structure (i.e., the number and size distribution of plants), and the working conditions of the retail sector in Italy. We exploit the staggered implementation of the liberalization to retrieve an unbiased estimate of the parameters of interest, taking into account possible confounding factors and potential spillover effects that could harm the identification. In line with the previous contributions, we find a positive effect of the reform on employment, with an increase of about 3% in the number of workers and of 2% on the number of establishments. If we compare our results to the effects of other type of interventions we can conclude that our results are very similar in magnitude to those by Viviano (2008), who found that the introduction of flexible entry regulations in the retail sector in Italy increased the share of those working in the sector by 0.8 p.p., indeed this number is very much in line with our estimates on the share of individuals working in the sector, conditional on employment (Table 10, column 3). Moreover, the increase in firms' employment that we find is equivalent to the effect that, according to Berton et al. (2018), would be produced by a 10% increase in credit to firms. In the light of such comparisons we argue that the effects of such deregulation are sizable.

Interestingly, our results point at a more significant growth for larger plants than for smaller ones, favoring a recomposition of the sector in favor of large chain stores vis á vis small, family-run businesses. Individual level estimates confirmed the increase in employment in the retail sector and further pointed at a recomposition of the sector's labor force towards dependent work. Large chain stores took advantage of the reform by increasing the employed labor force, the number of Sunday openings and the hours worked by each employee more than small shops. This growth of large chain stores vis á vis small retailers eventually translated into an average significant increase in the total labor earnings of dependent workers holding a permanent contract (the increase in earnings being more than proportional to the that in the number of hours worked). Finally, we show that the reform also had a positive effect on the activity of complementary services, such as restaurants and financial services and, overall, on total employment in affected areas.

Take together, therefore, the evidence collected in this paper suggests that removing restrictions to opening hours promoted the growth of the retail sector, with the larger firms being better able to exploit the full potential of the reform and thus growing most.

To better understand what is the external validity of our exercise, it is important to quantify the treatment that is generating the estimated effects. Our results should thus be read as the effects of passing from a regime in which shops could remain open 316 days a year, to one in which they could stay open any day of the year.[34] Any policy intervention should be measured on such scale.

All in all, from a policy perspective, our results provide support to the idea that a more flexible regulation of the business environment boosts economic growth, the positive effect being reinforced by mechanisms of reallocation towards larger, more productive units. We find no evidence that this leads to a worsening of employment conditions, on the contrary permanent dependent workers enjoyed an increase in their earnings. Our work, nevertheless, remains silent on the effects that softening regulation, as in this case, may have on consumers' welfare. Such effect would pass through the potential changes in prices, quantities, varieties and quality of the goods sold and services provided. These questions are beyond the scope of the paper and left to future research.

[34] In the pre-deregulation regime shops could stay open 316 days, i.e., 365 minus 52 Sundays, minus 12 public holidays, plus 8 Sundays or public holidays allowed, plus 7 other Sundays or public holidays allowed in December. In the post-deregulation regime shops could potentially stay open 365 days a year.

Tables

Table 1: ASIA descriptive statistics in 2007, by regulation type.

	(1)	(2)	(3)	(4)	(5)
				T-Tests	
	Treated	Control (Fully deregulated)	Partial Control (partially deregulated)	Δ_{RC-T} (2) - (1)	Δ_{PC-T} (3) - (1)
(log) workers	4.471	3.505	4.682	-0.966***	0.211***
	(1.648)	(1.553)	(1.809)	(0.0718)	(0.0474)
$\geq$ 3 employees	4.012	3.242	4.191	-0.771***	0.179***
	(1.686)	(1.537)	(1.847)	(0.0826)	(0.0521)
< 3 employees	3.871	3.000	4.091	-0.870***	0.220***
	(1.396)	(1.299)	(1.592)	(0.0602)	(0.0413)
(log) plants	3.802	2.902	4.012	-0.900***	0.210***
	(1.422)	(1.324)	(1.614)	(0.0613)	(0.0419)
$\geq$ 3 employees	2.261	1.610	2.483	-0.651***	0.222***
	(1.426)	(1.303)	(1.622)	(0.0700)	(0.0453)
< 3 employees	3.633	2.738	3.835	-0.895***	0.202***
	(1.379)	(1.271)	(1.579)	(0.0589)	(0.0409)
(log) income tax per capita	9.125	9.366	9.161	0.241***	0.0361***
	(1.297)	(1.128)	(1.452)	(0.0527)	(0.0379)
(log) population	7.907	6.989	8.038	-0.918***	0.131***
	(1.212)	(1.078)	(1.388)	(0.0502)	(0.0359)
(log) surface	2.955	2.815	3.376	-0.140***	0.421***
	(0.998)	(0.829)	(0.953)	(0.0390)	(0.0261)
Share of graduates (6+ age)	0.0723	0.0706	0.0787	-0.00174	0.00636***
	(0.0264)	(0.0249)	(0.0297)	(0.00115)	(0.000773)
Unemployment rate	0.112	0.0617	0.102	-0.0505***	-0.0105***
	(0.0695)	(0.0238)	(0.0573)	(0.00150)	(0.00167)
Observations	4,113	537	2,060		

Notes: Elaborations on ASIA and Istat Census data. Descriptive statistics per type of municipality. Data on plants, workers, and income taxes refer to 2007, while data on population, surface, share of graduates and unemployment rate source from the latest available census, in 2011. In columns 4 and 5 we report the results of t-tests on the difference of the means between the control and treatment municipalities, and the partial control and treatment groups, respectively.

Table 2: Baseline results.

	(1)	(2)	(3)	(4)
	(log) workers		(log) plants	
Liberalization	0.0343***	0.0237***	0.0211***	0.0181***
	(0.0062)	(0.0090)	(0.0049)	(0.0070)
Observations	46,500	46,500	46,500	46,500
R^2	0.990	0.990	0.992	0.992
Year FE	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓
Region-Year FE		✓		✓

Notes: Elaborations on ASIA data. Difference-in-differences estimates on the number of workers (in logarithm, columns 1-2) and plants (3-4) in the retail sector. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Testing for parallel trend assumption.

	(1)	(2)	(3)	(4)
	(log) workers		(log) plants	
Treated ×:				
$t-5$	-0.0200**	-0.0108	-0.0222***	-0.0014
	(0.0099)	(0.0187)	(0.0071)	(0.0151)
$t-4$	-0.0110**	-0.0005	-0.0030	-0.0025
	(0.0052)	(0.0103)	(0.0039)	(0.0076)
$t-3$	-0.0074	-0.0092	-0.0044	-0.0028
	(0.0050)	(0.0087)	(0.0035)	(0.0065)
$t-2$	0.0016	-0.0102	0.0014	-0.0080
	(0.0043)	(0.0067)	(0.0032)	(0.0049)
t	0.0093*	0.0219***	0.0049	0.0108*
	(0.0052)	(0.0078)	(0.0039)	(0.0058)
$t+1$	0.0274***	0.0273***	0.0179***	0.0178**
	(0.0059)	(0.0095)	(0.0044)	(0.0070)
$t+2$	0.0330***	0.0247**	0.0237***	0.0215***
	(0.0071)	(0.0102)	(0.0054)	(0.0078)
$t+3$	0.0401***	0.0294***	0.0233***	0.0208**
	(0.0080)	(0.0113)	(0.0060)	(0.0087)
$t+4$	0.0327***	0.0212*	0.0158**	0.0139
	(0.0095)	(0.0129)	(0.0072)	(0.0097)
$t+5$	0.0389***	0.0186	0.0176**	0.0103
	(0.0107)	(0.0136)	(0.0080)	(0.0103)
$t+6$	0.0224	0.0114	0.0166	0.0202
	(0.0237)	(0.0249)	(0.0219)	(0.0231)
$t+7$	0.0478*	0.0449	0.0258	0.0332
	(0.0281)	(0.0293)	(0.0263)	(0.0271)
$t+8$	0.0309	0.0285	0.0332	0.0398
	(0.0363)	(0.0374)	(0.0300)	(0.0309)
$t+9$	0.0203	0.0196	0.0202	0.0237
	(0.0457)	(0.0467)	(0.0347)	(0.0357)
Observations	46,500	46,500	46,500	46,500
R^2	0.990	0.990	0.992	0.992
Year FE	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓
Region-Year FE		✓		✓

Notes: Elaborations on ASIA data. Event study estimates - à la Autor (2003) - on the number of workers (in logarithm, columns 1-2) and plants (3-4) in the retail sector. The coefficients relate to the interaction between the treatment group dummy and the distance (in years) from the treatment date (t). $t-1$ is the baseline period. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	(log) workers					(log) plants				
Liberalization	0.0228***	0.0502***	0.0403***	0.0343***	0.0218***	0.0113***	0.0253***	0.0213***	0.0211***	0.0166***
	(0.00431)	(0.0105)	(0.00942)	(0.00619)	(0.00645)	(0.00332)	(0.00834)	(0.00705)	(0.00494)	(0.00536)
Observations	67,100	36,310	38,050	46,500	46,500	67,100	36,310	38,050	46,500	46,500
R^2	0.991	0.989	0.990	0.990	0.990	0.993	0.991	0.992	0.992	0.992
Partial	✓					✓				
2012 Only		✓					✓			
Propensity Score Weights			✓					✓		
Goodman-Bacon				✓					✓	
Treatment-Year FE					✓					✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: ASIA data elaborations on the number of workers (columns 1 to 5, in logs) and plants (6 to 10, in logs) . In columns 1 and 6 we add the *partial* control group to the estimation sample, in 2 and 7 we report the results of a 2×2 difference-in-differences keeping in the treatment group only the municipalities which were treated in 2012, with the enactment of the *Salva Italia*. In 3 and 8 we run a propensity score weight version of the baseline model, where we used the municipality surface, population and region as predictive variables for the PSM; in 4 and 9 we report the results obtained with the methodology by Goodman-Bacon (2018) - with bootstrapped standard errors - and columns 5 and 10 add Treatment-Year fixed effects, aimed at capturing possible differential pre-trends in treated units. All models are estimated with municipal and year fixed effects; Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Heterogeneous effects.

	(1)	(2)	(3)	(4)	(5)	(6)
	Macroarea		Plant size		Population size	
	log(workers)					
Liberalization	0.035***	0.024***	0.022***	0.036***	0.036***	0.024***
	(0.008)	(0.008)	(0.006)	(0.010)	(0.008)	(0.005)
Observations	30,280	16,220	46,450	38,889	33,380	13,120
R^2	0.989	0.992	0.987	0.974	0.978	0.990
	log(plants)					
Liberalization	0.025***	0.011*	0.018***	0.025***	0.021***	0.016***
	(0.006)	(0.006)	(0.006)	(0.008)	(0.007)	(0.003)
Observations	30,280	16,220	46,450	38,889	33,380	13,120
R^2	0.991	0.994	0.989	0.975	0.981	0.996
Subsample	C-N	S	< 3	≥ 3	$< 5,000$	$\geq 5,000$
Year FE	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓

Notes: Elaborations on ASIA data. Plants are small when they have less than three workers, large when three or more. C-N refers to Center and North of Italy, S to South. Municipalities are small when they have less than 5,000 inhabitants, large when they have 5,000 or more. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Accounting for spatial spillovers.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(log) workers				(log) plants			
Liberalization	0.0307***	0.0369***	0.0270***	0.0251***	0.0087	0.0082	0.0197***	0.0172***
	(0.0095)	(0.0115)	(0.0066)	(0.0040)	(0.0056)	(0.0076)	(0.0053)	(0.0029)
$\hat{\gamma}^t$				-0.0027***				-0.0008
				(0.0007)				(0.0006)
$\hat{\gamma}^{tt}$				0.0022*				0.0015
				(0.0011)				(0.0010)
Observations	5,130	2,440	43,110	46,170	5,130	2,440	43,110	46,170
Model	LLM	LLM-clean	No-neigh	Spill	LLM	LLM-clean	No-neigh	Spill
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
LLM FE	✓	✓			✓	✓		
Municipality FE			✓	✓			✓	✓

Notes: Elaborations on ASIA data. In the estimations reported in columns 1 and 5 we aggregate the data at the LLM level, and redefine the treatment as the share of treated municipalities per LLM/year; in 2-6 we further restrict the focus on *fully treated* or *fully control* LLMs, allowing us to use a binary treatment. In column 3 and 7 we exclude from the estimation sample all control municipalities which directly neighbor (i.e., share a boundary with) a treated unit, in order to avoid any possible contamination due to spillover effects. Lastly, in columns 4 and 8, we estimate the spillover effects by adding measures for the number of treated and control neighbors. Robust standard errors clustered at the LLM or municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Accounting for spatial spillovers and heterogeneous effects.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Plant size:	Small				Large			
	log(workers)							
Liberalization	0.0120*	0.0150	0.0194***	0.0274***	0.0530***	0.0607***	0.0274**	0.0330*
	(0.0067)	(0.0093)	(0.0062)	(0.0039)	(0.0180)	(0.0209)	(0.0109)	(0.0179)
Observations	5,130	2,440	43,062	46,120	5,115	2,425	36,490	38,638
	log(plants)							
Liberalization	0.0020	0.0011	0.0184***	0.0165***	0.0541***	0.0540***	0.0161*	0.0221
	(0.0063)	(0.0086)	(0.0059)	(0.0032)	(0.0141)	(0.0169)	(0.0087)	(0.0174)
Observations	5,130	2,440	43,062	46,120	5,115	2,425	36,490	38,638
B. Population size:	Low Population				High Population			
	log(workers)							
Liberalization	0.0329**	0.0324**	0.0248**	0.0380***	0.0277**	0.0425**	0.0287***	0.0467***
	(0.0134)	(0.0149)	(0.0125)	(0.0059)	(0.0132)	(0.0177)	(0.0096)	(0.0115)
Observations	2,560	1,530	21,102	33,030	2,570	910	21,923	13,090
	log(plants)							
Liberalization	0.0128	0.0111	0.0200*	0.0248***	0.0039	0.0037	0.0258	0.0331**
	(0.0092)	(0.0111)	(0.0119)	(0.0037)	(0.0055)	(0.0091)	(0.0190)	(0.0108)
Observations	2,560	1,530	21,102	33,030	2,570	910	14,567	13,090
Model	LLM	LLM-clean	No-neigh	Spill	LLM	LLM-clean	No-neigh	Spill
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
LLM FE	✓	✓			✓	✓		
Municipality FE			✓	✓			✓	✓

Notes: Elaborations on ASIA data. Panel A: we split the sample according to plant size (i.e., above, columns 1-4, or below three employees, columns 5-8), and report the results for the number of workers (in logs) above and for the number of plants (logs) below. Panel B: we split the sample in small and large units according to median population levels - around 44,000 for LLMs and 5,000 for municipalities. For all panels, in columns 1 and 5 we aggregate the data at the LLM level, and redefine the treatment as the share of treated municipalities per LLM/year; in 2-6 we further restrict the focus on *fully treated* or *fully control* LLMs, allowing us to use a binary treatment. In column 3 and 7 we exclude from the estimation sample all control municipalities which directly neighbor (i.e., share a boundary with) a treated unit, in order to avoid any possible contamination due to spillover effects. Lastly, in columns 4 and 8, we estimate the spillover effects by adding measures for the number of treated and control neighbors. Robust standard errors clustered at the LLM or municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Effects on total employment and number of plants.

	(1)	(2)	(3)	(4)	(5)	(6)
	(log) workers			(log) plants		
Liberalization	0.0129***	0.0177**	0.0168*	0.0036	0.0046	0.0066
	(0.0045)	(0.0076)	(0.0095)	(0.0030)	(0.0069)	(0.0089)
Observations	417,285	51,028	24,157	418,388	51,058	24,177
R^2	0.844	0.929	0.912	0.890	0.939	0.926
Sector-Year FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Municipality FE	✓			✓		
LLM FE		✓	✓		✓	✓

Notes: Elaborations on ASIA data. Difference-in-differences estimates on the number of workers (in logarithm, columns 1 to 3) and plants (4 to 6) in all sectors of the economy. In columns 1 and 4 an observation is a municipal/sector couple, whereas the other specifications are at the LLM/sector level. Robust standard errors clustered at the municipality (1 and 4) or at the LLM level, and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 9: LFS descriptive statistics in 2007, by regulation type.

	(1)	(2)	(3)	(4)	(5)
				T-Tests	
	Treated	Control (Fully deregulated)	Partial Control (Partially deregulated)	Δ_{RC-T} (2) - (1)	Δ_{PC-T} (3) - (1)
A. Population 15-64					
Female	0.522	0.525	0.523	0.003	0.000
	(0.500)	(0.499)	(0.499)	(0.004)	(0.001)
Citizen	1.056	1.071	1.071	0.014***	0.015***
	(0.321)	(0.353)	(0.360)	(0.003)	(0.001)
Age	43.453	47.165	44.358	3.712***	0.905***
	(23.396)	(23.264)	(23.400)	(0.179)	(0.063)
Secondary	0.255	0.262	0.272	0.006	0.017***
	(0.436)	(0.440)	(0.445)	(0.003)	(0.001)
Tertiary	0.207	0.175	0.225	-0.032***	0.018***
	(0.405)	(0.380)	(0.417)	(0.003)	(0.001)
Employed	0.349	0.390	0.364	0.041***	0.016***
	(0.477)	(0.488)	(0.481)	(0.004)	(0.001)
Observations	339,732	17,827	236,098	357,559	575,830
B. Employed					
Employed in Retail	0.149	0.153	0.150	0.004	0.000
	(0.356)	(0.360)	(0.357)	(0.004)	(0.002)
Self-employed	0.242	0.291	0.247	0.049***	0.005*
	(0.428)	(0.454)	(0.431)	(0.006)	(0.002)
Large Distribution	0.332	0.356	0.346	0.024	0.014*
	(0.471)	(0.479)	(0.476)	(0.018)	(0.006)
Sunday Working	0.184	0.186	0.197	0.002	0.013***
	(0.387)	(0.389)	(0.398)	(0.005)	(0.002)
Weekly Hours Worked	38.639	38.159	38.275	-0.480	-0.364*
	(14.812)	(15.071)	(14.905)	(0.476)	(0.173)
Observations	118,506	6,957	86,021	125,463	204,527
C. Employees in Retail					
Permanent Contract	0.865	0.803	0.849	-0.062***	-0.016**
	(0.342)	(0.398)	(0.358)	(0.018)	(0.005)
Monthly Wage*	1,097	1,066	1,095	-30.97	-2.01
	(434)	(466)	(490)	(19.7)	(7.4)
Observations	10,034	533	7,314	10,567	17,348

Notes: Elaborations on LFS data. Descriptive statistics per type of municipality - Treated, Fully or Partially Deregulated. Panel A reports average values of gender, citizenship, age, education level and employment rate for the full sample in 2007 - i.e., all population between the age 15-64 surveyed. Panel B deals with labor market-related variables for all employed individuals - reporting the probability of working in the retail sector, in the large distribution, being self-employed or working on Sundays, plus the average number of hours worked. In panel C we restrict the focus on retail employees and report the share of permanent contract employees and the monthly wage. All reported variables refer to 2007, but the wage - which relates to 2009, when the question was added to the LSF. Columns 4 and 5 report the results of t-tests on the difference of the means between the control and treatment municipalities, and the partial control and treatment groups, respectively. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Individual-level estimates: extensive margin and composition.

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment in retail			Self-Employed	GDO	Permanent
Liberalization	0.0024***	0.0022**	0.0079***	-0.0159***	0.0156***	-0.0051
	(0.0009)	(0.0008)	(0.0024)	(0.0052)	(0.0060)	(0.0055)
Female		-0.0185***	0.0092***	-0.1395***	-0.0145***	-0.0288***
		(0.0007)	(0.0018)	(0.0030)	(0.0034)	(0.0031)
Age		0.0044***	-0.0097***	0.0149***	0.0140***	0.0627***
		(0.0001)	(0.0004)	(0.0006)	(0.0007)	(0.0011)
Age^2		-0.0001***	0.0001***	0.0000	-0.0002***	-0.0007***
		(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Secondary education		0.0263***	-0.0116***	0.0095***	-0.0041	-0.0052
		(0.0010)	(0.0022)	(0.0035)	(0.0040)	(0.0036)
Tertiary education		-0.0170***	-0.0970***	0.0008	-0.0505***	-0.0289***
		(0.0010)	(0.0032)	(0.0058)	(0.0060)	(0.0061)
Observations	3,205,296	3,205,296	1,093,363	93,925	65,515	52,045
LLM FE	✓	✓	✓	✓	✓	✓
Year-Quarter FE	✓	✓	✓	✓	✓	✓
Conditional on Employment			✓	✓	✓	✓
Conditional on Employment in retail				✓	✓	✓

Notes: Elaborations on LFS data. Difference-in-differences estimates on LFS data. In columns 1 to 4 the dependent variable is an indicator variable for workers in the retail sector, and models feature increasing controls (2) and Region-Year fixed effects (3). In column 4 we restrict the sample to employed individuals. In columns 5 and 6 we condition for retail sector employees, and investigate the recomposition effects towards the self-employee status (5) or workers in the great distribution (Chain, column 6). All specifications include province and quarter (i.e., wave) fixed effects. Robust standard errors clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

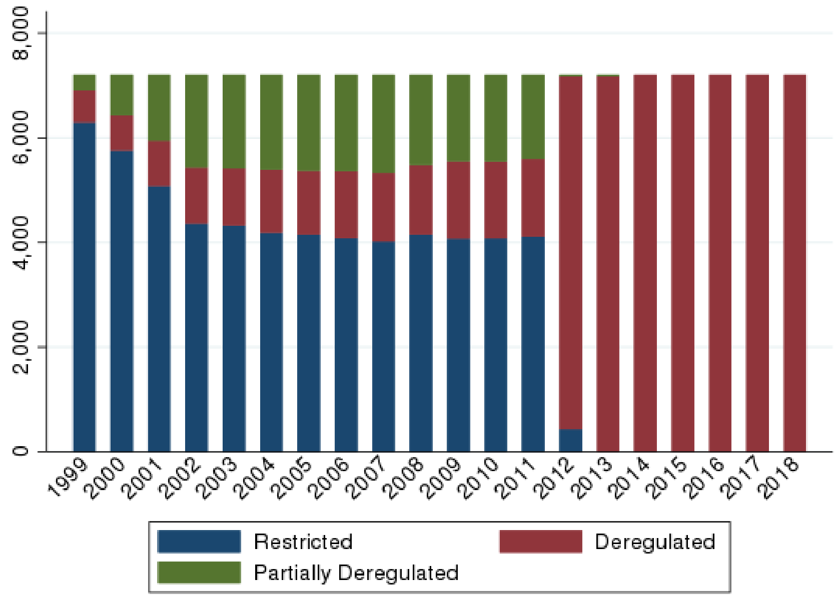
Table 11: Individual-level estimates: intensive margin and earnings.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Overall	Self		Chain		Contract	
	A: Sunday working						
Liberalization	0.034***	0.050***	0.023***	0.029***	0.041***	0.055***	0.034***
	(0.005)	(0.007)	(0.007)	(0.009)	(0.014)	(0.021)	(0.008)
Observations	93,774	52,006	41,814	49,329	16,095	7,465	43,629
	B: (log) weekly hours						
Liberalization	0.006	0.013**	0.008	0.017***	0.019*	0.008	0.012*
	(0.005)	(0.006)	(0.006)	(0.007)	(0.010)	(0.019)	(0.006)
Observations	88,411	48,209	40,197	46,680	14,612	7,130	40,268
	C: (log) monthly wage						
Liberalization	-	0.019**	-	0.020**	0.019	0.003	0.022***
	-	(0.008)	-	(0.010)	(0.013)	(0.023)	(0.008)
Observations	-	40,634	-	27,674	12,498	5,897	34,713
Model	Overall	Employee	Self	Non-chain	Chain	Temp	Perm
LLM FE	✓	✓	✓	✓	✓	✓	✓
Year-Quarter FE	✓	✓	✓	✓	✓	✓	✓
Conditional on Employment in retail	✓	✓	✓	✓	✓	✓	✓

Notes: Difference-in-differences estimates on LFS data, subset of retail workers only. Panel A: effects on the likelihood of Sunday working; B: number of weekly hours (log); C: monthly wage (log) - for employees only. Column 1 reports the plain estimation in (6), in columns 2 and 3 we distinguish between employees and self-employed, respectively. In columns 4 and 5 we investigate differential effects between employees of large chains and all the others. Finally, in columns 6 and 7 we divide the sample of employees according to the contract type (either temporary or permanent). Robust standard errors clustered at the municipality level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

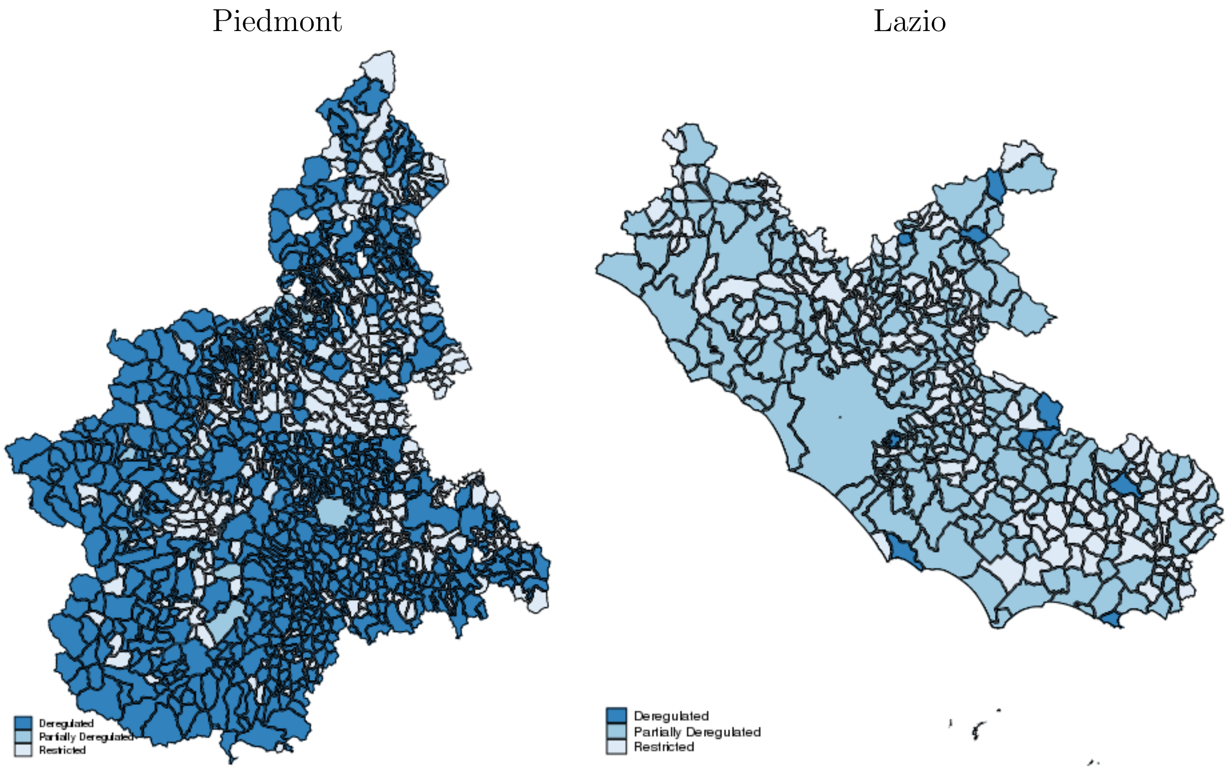
Figures

Figure 1: Deregulated municipalities, by year.



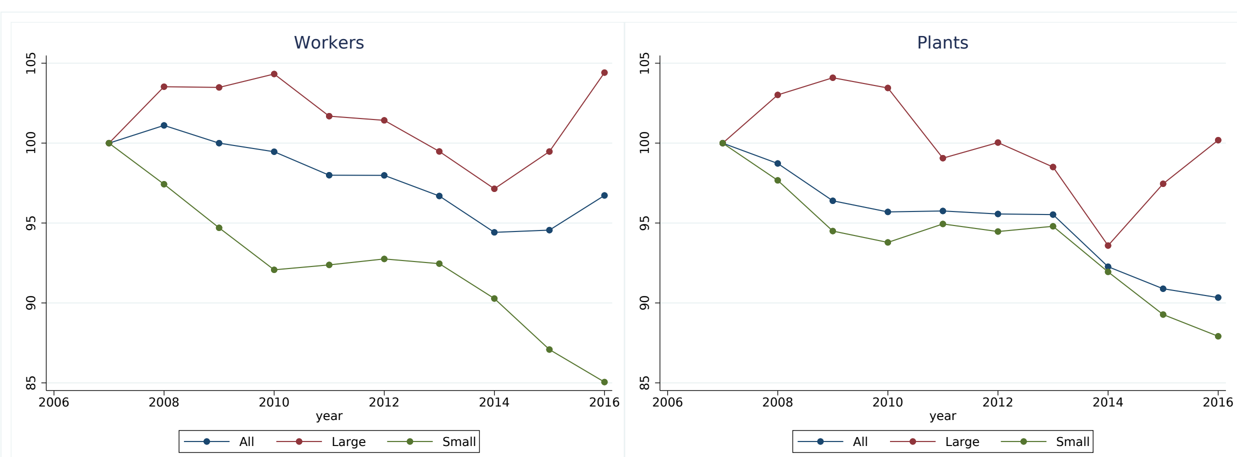
Notes: Frequency of municipalities by year and type: restricted (not touristic), partially deregulated (touristic with restrictions) and fully deregulated (touristic).

Figure 2: Piedmont and Lazio before the full deregulation.



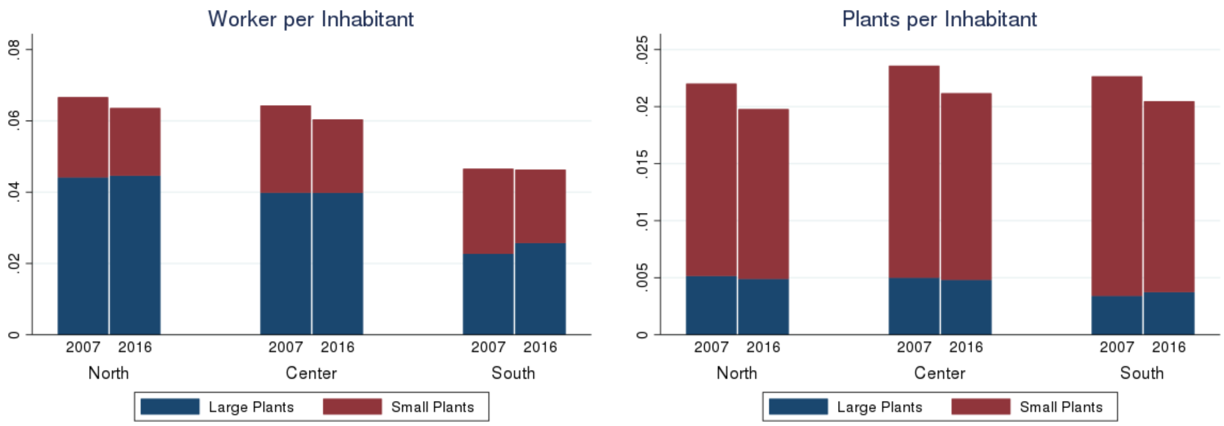
Notes: Restricted (not touristic), partially deregulated (touristic with restrictions) and fully deregulated (touristic) municipalities in Piedmont and Lazio regions in 2010, before the introduction of the *Salva Italia*.

Figure 3: Variation of retail employment and plants.



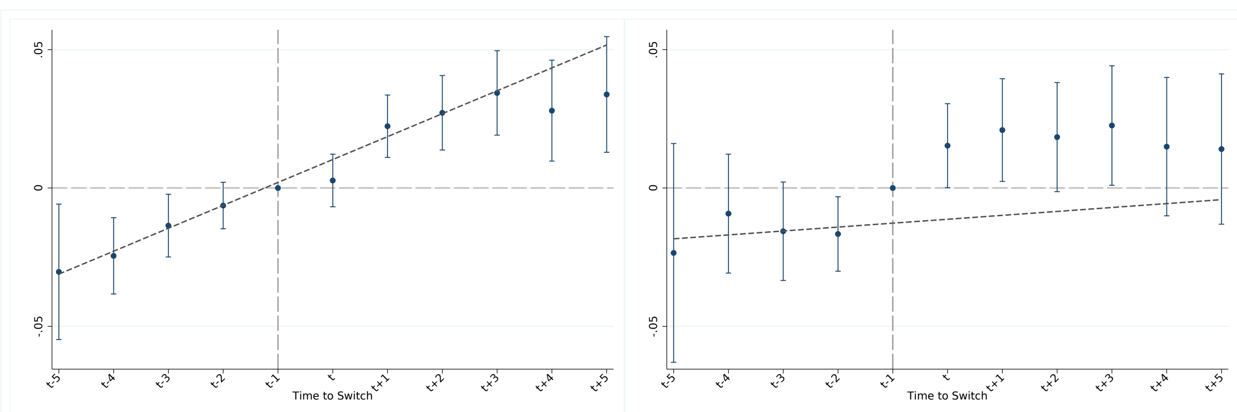
Notes: Elaborations on ASIA data. The plot reports the dynamics of the number of workers (left panel) and the number of plants (right panel) in the retail sector 2007-2016, with 2007 as base year (i.e., $Y_{2007} = 100$). Both panels report the total value - the blue line - those relative to small firms - ≤ 3 employees, green line - and to large firms - > 3 employees, red line.

Figure 4: Descriptive evidence, by macro-area.



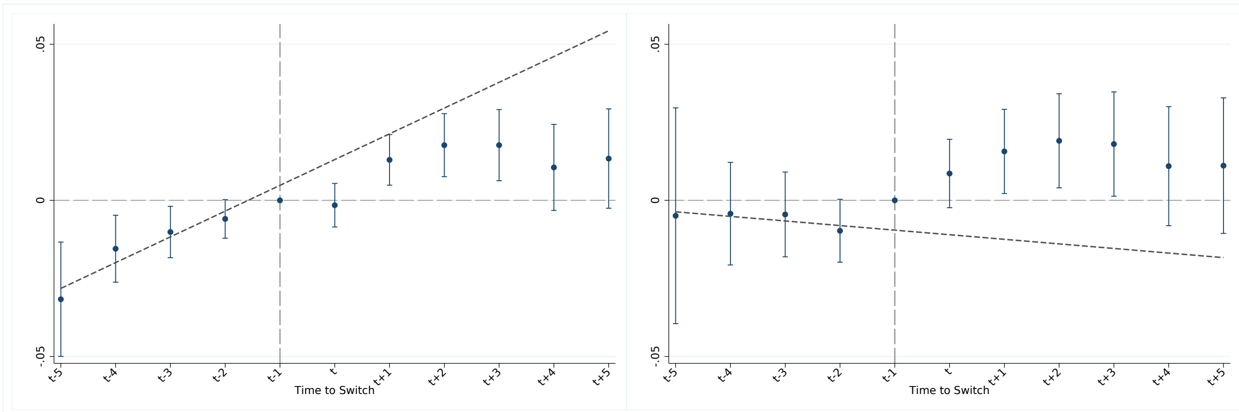
Notes: Number of workers in the retail sector per inhabitant (left panel) and number of plants in the retail sector per inhabitant (right panel) in 2007 and 2016, per macro-region in Italy. Bars reported the stacked values for small - ≤ 3 employees, maroon bars - and large firms - > 3 employees, blue bars.

Figure 5: Testing for the Parallel Trend Assumption. Workers.



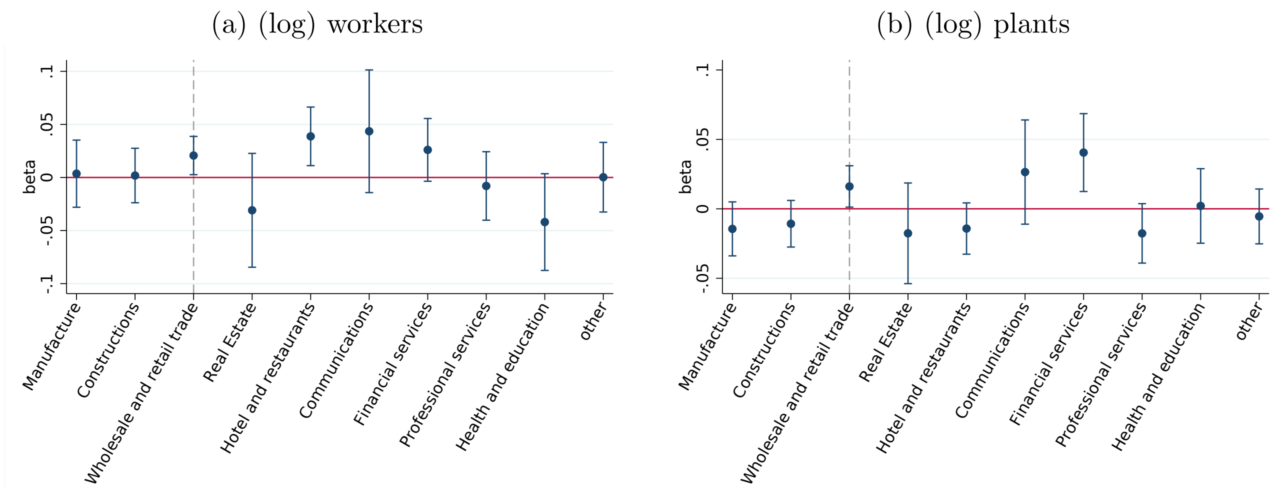
Notes: dots are the point estimates of the event study estimation of (log) workers on indicator variables for time to treatment, while blue bars indicate their confidence intervals. The upward sloping, dashed back line is the linear fit of the estimates in the pre-treatment years, projected on the post-treatment period.

Figure 6: Testing for the Parallel Trend Assumption. Plants.



Notes: dots represent the point estimates of the event study estimation of (log) plants on indicator variables for time to treatment, while blue bars indicate their confidence intervals. The upward sloping, dashed back line is the linear fit of the estimates in the pre-treatment years, projected on the post-treatment period.

Figure 7: Sectoral spillovers.



Notes: dots represent point estimates of *Liberalization* as in model (1), sector by sector, while blue bars indicate their confidence intervals. The dotted vertical line indicates the retail sector

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Appendices

A Difference in Differences in Reverse

This section presents the "*Difference in differences in reverse*" (DDR) framework as formalized in Kim and Lee (2019), from which we also borrow part of the notation. In particular, we label the model with an *always treated* control group as DDR, and as DD the "usual" difference in differences strategy - i.e., with the *never treated* control group. We consider the simplest possible case - a 2×2 DD strategy - with two periods $t = 0, 1$, a treatment and a control group ($G_i = T$ and $G_i = C$, respectively), and a treatment $D_{it} = 0, 1$, depending on the liberalization status of the i^{th} municipality. The difference between DD and DDR lies on the treatment status of the control group: the former reads $D_{it} = 0$ if $G_i = C$ - in words, no municipality is treated in the control group at each point in time - while in the latter case (the one we implement) $D_i = 1 \quad \forall\, i, t \ | G_i = C$, i.e. an "always treated" control group. To complete the notation, we label the outcome of interest with the treatment status, the group and the time (i.e., $Y^D_{t,G}$).

The DD strategy boils down to comparing the differences between treatment and control group at $t = 1$ with the same difference at $t = 0$. In formulas

$$\begin{aligned} DD^{2\times 2} &= \underbrace{\big(\mathbb{E}(Y^1_{1,T}) - \mathbb{E}(Y^0_{1,C})\big)}_{Post} - \underbrace{\big(\mathbb{E}(Y^0_{0,T}) - \mathbb{E}(Y^0_{0,C})\big)}_{Pre} = \\ &\underbrace{\big(\mathbb{E}(Y^1_{1,T}) - \mathbb{E}(Y^0_{0,T})\big)}_{T\,Group} - \underbrace{\big(\mathbb{E}(Y^0_{1,C}) - \mathbb{E}(Y^0_{0,C})\big)}_{C\,Group} \end{aligned} \tag{A.1}$$

To obtain the identification, the DD requires a single assumption: that, *absent the treatment*, the control and treatment groups would hold parallel, keeping the same distance before and after the treatment period (Parallel Trends assumption, PT). More formally

$$ID_{DD} \Rightarrow \underbrace{\left(\mathbb{E}(Y^0_{1,T}) - \mathbb{E}(Y^0_{0,T})\right)}_{Counterfactual\,T\,Group} = \underbrace{\left(\mathbb{E}(Y^0_{1,C}) - \mathbb{E}(Y^0_{0,C})\right)}_{C\,Group}$$

and, substituting the counterfactual treatment in (A.1), we show that the DD yields an estimate of the treatment effect

$$DD^{2\times 2} = \mathbb{E}(Y^1_{1,T} - Y^0_{1,T}) \tag{A.2}$$

Moving to the DDR case, the definition in (A.1) differs with respect to the treatment level of the control group - i.e., always 1 - and of the "counterfactual" identification: in the DDR case, it depends on the (unobservable) level of outcome in the control group at $t = 0$, *present the treatment*. It leads to the identification condition

$$ID_{DDR} \Rightarrow \underbrace{\left(\mathbb{E}(Y^1_{1,T}) - \mathbb{E}(Y^1_{0,T})\right)}_{Counterfactual\,T\,Group} = \underbrace{\left(\mathbb{E}(Y^1_{1,C}) - \mathbb{E}(Y^1_{0,C})\right)}_{C\,Group}$$

In turn, it shows that the DDR reads

$$DDR^{2\times 2} = \mathbb{E}(Y^1_{0,T} - Y^0_{0,T}) \tag{A.3}$$

i.e., it identifies the treatment effect on the treated *before* the treatment.

Despite the formal differences, it is straightforward to see that equations (A.2) and (A.3) capture the very same measure under very mild conditions - namely, that time does not affect differentially treated and untreated units.[35] In particular, DD and DDR yields the same estimate - i.e., the **same number**, both in magnitude and in sign; hence, in our setting there is no need to assume symmetry of any sort, because both measure would reflect the liberalization parameter - being it the only treatment at play.

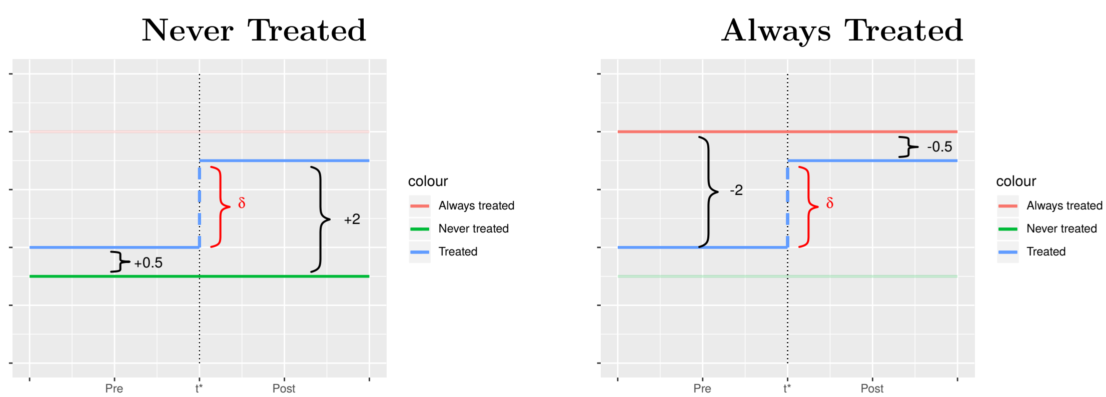
We offer a graphical and numerical example in figure A.1. Consider the case in which

[35] we also note that, if that would not be the case, the PT assumption would be violated and the DD estimate would be invalid in the first place

the liberalization effect (δ) amounts to 1.5, and the sample is divided into a treatment group (light blue line, T), a control group of never treated (green line, CL) and a control group of always treated (red line, CH).[36] For sake of simplicity - and without loss of generality - we assume no trends of any sort, and the starting values of T, CL and CH groups are 2, 1.5 and 4, respectively. It is crucial to stress that DD and DDR identification strategy are exactly the same (i.e., difference-in-differences).

We refer to the left panel, and substitute the example numbers in equation (A.1) to obtain: $\hat{\delta}_{DD} = +2 - 0.5 = 1.5$, which is in fact an unbiased estimate of the treatment effect. Moving to the right panel, we can estimate the DDR in exactly the same way, to yield $\hat{\delta}_{DDR} = -0.5 - (-2) = 1.5$.[37] Again, the estimated effect is unbiased, and the DDR captures the very same effect. It is the interpretation, though, that is slightly different - see above.

Figure A.1



The DDR has been extensively used in the literature: on top of the already cited Kim and Lee (2019) who detail all the econometric implications, there are several examples of empirical contributions (Kotchen and Grant, 2011; Chemin and Wasmer, 2009; Giuffrida and Rovigatti, 2020). In the case of staggered treatments, then, the DDR is implicitly used when already treated units are included into the control groups (e.g. in (Autor et al., 2006) and (Gentzkow and Shapiro, 2008)), as clearly modeled by Goodman-Bacon (2018).

[36] *Ceteris paribus*, in our setting the very same situation happens in each year before 2012, when we have the already treated, the treatment and the untreated municipalities.

[37] It is crucial to note that the numbers used in the example don't matter, and in particular there is no need to have a symmetric distance. The same result applies to any other configuration.

B Additional figures and tables

Table B.1: Number of municipalities being deregulated by year of deregulation and region.

	before 2006	2006	2007	2008	2009	2010	2011	2012	2013
Abruzzo	302		31					50	
	(1.00)		(0.10)					(0.17)	
Val d'Aosta	56							17	
	(0.77)							(0.23)	
Basilicata	21				6			104	
	(0.16)				(0.05)			(0.79)	
Calabria	139						3	259	
	(0.35)						(0.01)	(0.64)	
Campania	265	1		3	2	3	1	284	
	(0.48)	(0.00)		(0.01)	(0.00)	(0.01)	(0.00)	(0.52)	
Emilia-Romagna	180							134	
	(0.56)							(0.42)	
Friuli-Venezia-Giulia	209	27			128				
	(1.00)	(0.13)			(0.61)				
Lazio	222							156	
	(0.59)							(0.41)	
Lombardia	337	5		38	2			1327	
	(0.23)	(0.00)		(0.03)	(0.00)			(0.89)	
Marche	184								
	(0.82)								
Molise	5							136	
	(0.04)							(1.00)	
Piemonte	698	12	28	6	1	2	2	431	
	(0.60)	(0.01)	(0.02)	(0.01)	(0.00)	(0.00)	(0.00)	(0.37)	
Puglia	28	7						229	
	(0.11)	(0.03)						(0.89)	
Sardegna	63							157	
	(0.29)							(0.71)	
Sicilia	176		15	8	4	23	6	185	
	(0.45)		(0.04)	(0.02)	(0.01)	(0.06)	(0.02)	(0.47)	
Trentino-Alto Adige	141							29	107
	(0.52)							(0.11)	(0.40)
Veneto	93	6	2	1	1		1	2	314
	(0.23)	(0.01)	(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.76)
Total	3,119	58	76	56	144	28	13	3,500	421

Notes: Number of municipalities transiting from not touristic to touristic, by region and year. In parentheses we report the relative share on the total number of municipalities.

Table B.2: Robustness Check: including the "Partial Treatment" group.

	(1)	(2)	(3)	(4)
	(log) workers		(log) plants	
Liberalization	0.0098**	0.0145***	0.0107***	0.0127***
	(0.0042)	(0.0056)	(0.0035)	(0.0047)
Observations	61,730	61,730	61,730	61,730
R^2	0.991	0.991	0.993	0.993
Year FE	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓
Region-Year FE		✓		✓

Notes: Elaborations on ASIA data. Difference-in-differences estimates on the number of workers (in logarithm, columns 1-2) and plants (3-4) in the retail sector. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.3: Robustness Check: pre-2011 deregulations only ("Never Treated Control")

	(1)	(2)	(3)	(4)
	(log) workers		(log) plants	
Liberalization	0.0158	0.0120	0.0102	0.0143
	(0.0246)	(0.0251)	(0.0171)	(0.0172)
Observations	16,452	16,452	16,452	16,452
R^2	0.995	0.995	0.996	0.996
Year FE	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓
Region-Year FE		✓		✓

Notes: Elaborations on ASIA data. Difference-in-differences estimates on the number of workers (in logarithm, columns 1-2) and plants (3-4) in the retail sector. Years 2007-2011. Treated municipalities are the ones deregulated before 2012, control municipalities the ones deregulated in 2012 (with the Law Decree 201/2011). Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.4: Heterogeneous effects with Region-Year fixed effects.

	(1)	(2)	(3)	(4)	(5)	(6)
	Macroarea		Plant size		Population size	
	log(workers)					
Liberalization	0.026**	0.020**	0.021**	0.019	0.028**	0.016*
	(0.013)	(0.010)	(0.008)	(0.016)	(0.012)	(0.009)
Observations	30,280	16,220	46,450	38,889	33,380	13,110
R^2	0.989	0.992	0.987	0.974	0.978	0.991
	log(plants)					
Liberalization	0.029***	0.001	0.015**	0.019	0.028***	0.003
	(0.010)	(0.007)	(0.008)	(0.012)	(0.009)	(0.005)
Observations	30,280	16,220	46,450	38,889	33,380	13,110
R^2	0.991	0.994	0.989	0.975	0.981	0.996
Subsample	C-N	S	< 3	≥ 3	$< 5,000$	$\geq 5,000$
Year FE	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓
Region-Year FE	✓	✓	✓	✓	✓	✓

Notes: Elaborations on ASIA data. Plants are small when they have less than three workers, large when three or more. C-N refers to Center and North of Italy, S to South. Municipalities are small when they have less than 5,000 inhabitants, large when they have 5,000 or more. Robust standard errors clustered at the municipal level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.5: Evidence from data on supermarkets.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Nielsen Data on Large Distribution								
	Italy			Center-North			South		
	Plants	Employees	m^2	Plants	Employees	m^2	Plants	Employees	m^2
Liberalization	0.017***	0.049***	0.124***	0.017***	0.051***	0.134***	0.015	0.038	0.097
	(0.005)	(0.014)	(0.032)	(0.005)	(0.015)	(0.034)	(0.015)	(0.035)	(0.080)
Observations	46,500	46,500	46,500	30,280	30,280	30,280	16,220	16,220	16,220
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Elaborations on Nielsen data. Nielsen data report the number of plants, the total number of employees and the shop surface for all large distributors in Italy, 2007 to 2016. We report the estimation for all these measures on the whole country (columns 1 to 3), on central and northern regions (4 to 6) and for southern regions (7 to 9). Robust standard errors clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.6: Individual-level estimates: extensive margin, heterogeneous effects.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Employment in Retail									
	Males	Females	Italians	Foreigners	15-24	25-44	45-64	Less than Sec.	Secondary	Tertiary
Liberalization	0.0026**	0.0017*	0.0022**	0.0051	-0.0005	0.0059***	0.0048***	0.0024**	0.0050**	0.0018
	(0.0013)	(0.0009)	(0.0009)	(0.0033)	(0.0008)	(0.0022)	(0.0017)	(0.0011)	(0.0022)	(0.0011)
Female			-0.0169***	-0.0417***	-0.0027***	-0.0274***	-0.0392***	-0.0267***	-0.0205***	-0.0101***
			(0.0007)	(0.0026)	(0.0005)	(0.0018)	(0.0013)	(0.0011)	(0.0015)	(0.0006)
Age	0.0059***	0.0032***	0.0044***	0.0035***				0.0041***	0.0075***	0.0041***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)				(0.0001)	(0.0002)	(0.0001)
Age^2	-0.0001***	-0.0000***	-0.0001***	-0.0000***				-0.0000***	-0.0001***	-0.0000***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)				(0.0000)	(0.0000)	(0.0000)
Secondary education	0.0181***	0.0344***	0.0277***	0.0004	0.0364***	0.0240***	0.0196***			
	(0.0013)	(0.0014)	(0.0011)	(0.0032)	(0.0015)	(0.0020)	(0.0016)			
Tertiary education	-0.0229***	-0.0092***	-0.0182***	-0.0062**	-0.0227***	-0.0414***	-0.0209***			
	(0.0014)	(0.0010)	(0.0011)	(0.0025)	(0.0008)	(0.0025)	(0.0020)			
Observations	1,531,180	1,674,116	3,026,776	178,519	756,081	775,522	892,297	1,312,661	674,804	1,217,831
LMA FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year-Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

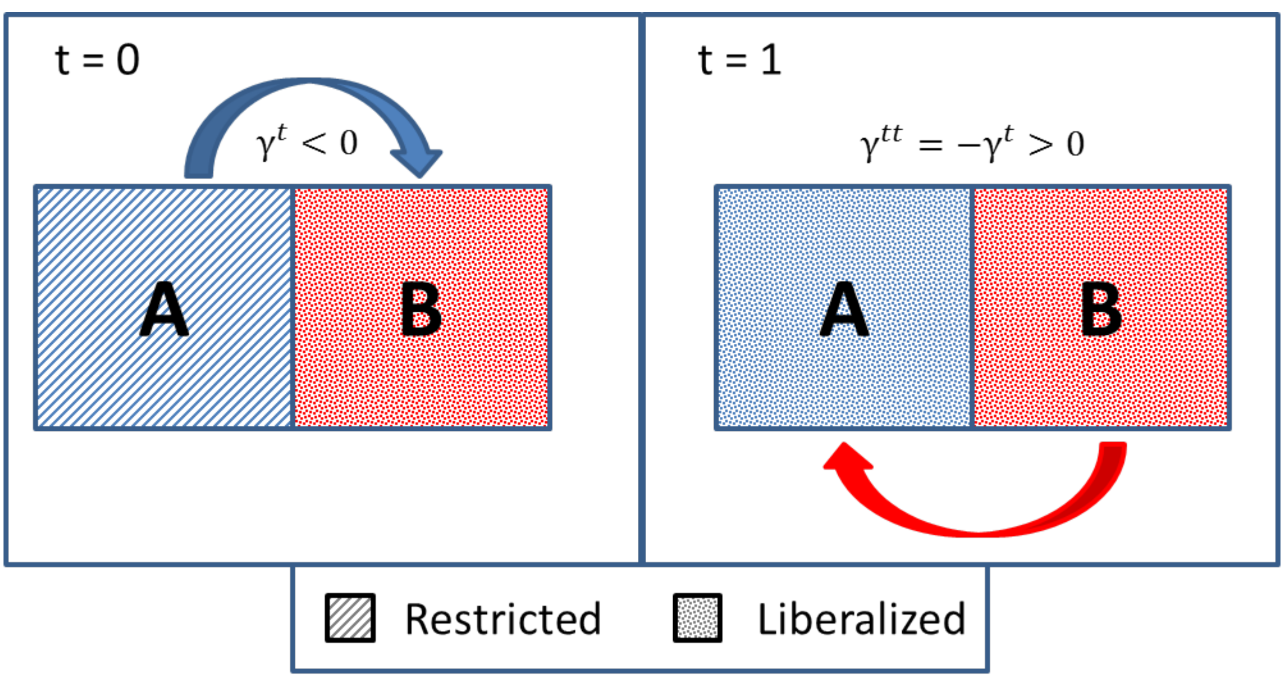
Notes: Difference-in-differences estimates on LFS data. With each model we investigate the employment probability for different subsets of individuals, including/excluding controls accordingly: males (1) and females (2); Italian citizens (3) or not (4); young (5), young adults (6) and adults (7); individuals with at most the primary education (8), secondary (9) and tertiary or more (10). All specifications include LLM and quarter (i.e., wave) fixed effects. Robust standard errors clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

C Spatial Spillovers

There are spatial spillovers of different nature that must be taken into account in dealing with regulation effects. In our framework, in particular, there are partial and general equilibrium effects that should not be overlooked, and might drive our main results.

Consider the case depicted in figure C.1. In left panel, at $t = 0$, municipality A is not liberalized, while its neighbor (B) is. In that case, part of the demand (e.g., Sunday shoppers or late-night buyers) is forced to move from A to B in order to enjoy the longer opening hours; as a result, both the number of stores and the retail employees in B increase at the expense of A's shops market. In terms of equation (5), our prior would be $\hat{\gamma}^t < 0$ (i.e., evidence of negative spillovers of liberalized neighbors). However, as depicted in the right panel, when A gets deregulated ($t = 1$) and the institutional setting allows A's shops to open longer hours and meet the part of the demand previously diverted toward B, we expect to estimate no spillover effect - hence, the interaction between the treatment and the treated neighbor should offset the negative spillover; $\hat{\gamma}^{tt} = -\hat{\gamma}^t > 0$.

Figure C.1: Spillover Effects on Treated and Control Units



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