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The Effects of Deregulating Retail Operating Hours: Empirical Evidence from Italy

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Abstract

We estimate the impact of deregulating shop hours on the structure of the retail sector and the size and composition of its labor force. To identify the effect of interest, we exploit the staggered implementation of a reform that allowed Italian municipalities to adopt fully flexible operating hours in the late 1990s. Our findings indicate that lifting restrictions on hours increased retail employment by 2.4 percent and increased the number of shops in the affected municipalities by 1.8 percent. In combination with estimates using individual-level data, our results further suggest that retail employment grew more in larger retail operations, with a corresponding movement of the labor force toward permanent employees and away from the self-employed.

1. Introduction

The extent to which governments should regulate economic activity has been debated in the economics literature from its origins (Coase 1937). Indeed, the regulatory framework adopted directly shapes the economic environment, influences firms' dynamics, and conditions markets' structures (see, for example, Peltzman 1976; Levine 2011; Schwartzstein and Shleifer 2013; Arnold et al. 2016; Barone and Cingano 2011; Gal and Hijzen 2016). Today, a consensus that competition-friendly product market regulation (PMR) can ease the functioning of markets and boost economic growth is growing (Égert 2018).

However, despite the numerous contributions highlighting the negative link between the stringency of PMR and firms' average size, productivity, and invest-

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ment (Andrews, Cingano, and Conconi 2014; Alesina et al. 2005), the relative impact of particular pieces of regulation is still disputed, with little empirical evidence on their real effects. While several contributions focus on the impact of entry barriers on productivity (Köke and Renneboog 2005; Schivardi and Viviano 2011; Maican and Orth 2018), employment (Bertrand and Kramarz 2002; Viviano 2008), and market structure (Liao and Chen 2011; Sadun 2015), which advocate for liberalization as a way to foster competition, less is known about the effects of other regulatory barriers.[1]

In this paper, we focus on a particular type of regulation—restrictions on shops' hours of operation in the retail sector—and estimate to what extent such regulations affect the size and structure of the market. This dimension of PMR has been a controversial issue in the policy debate, as it involves social, political, economic, and even religious considerations. The debate originated in the United States in the 17th century with blue laws—namely, laws designed to restrict or ban activities on Sundays for religious reasons, specifically to enforce the observance of a day of worship or rest—and was revived in the mid-19th century when several Supreme Court appeals upheld the legitimacy of such bans. In Europe, the debate was much less heated until recent years, when a wave of deregulation—with much cross-country heterogeneity—involved northern countries like Denmark, the United Kingdom, and the Netherlands as well as Germany, Italy, and Spain. Despite a clear trend toward the liberalization of operating hours on Sunday and public holidays in the US (see Burda and Weil 2004) and EU regulatory frameworks (see Maher 1995), the timing and extent of the deregulation still vary across countries, states, and regions.

From a purely theoretical economic perspective, the effects of deregulating shops' operating 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 for more hours; moreover, there may be either an increase in the number of workers employed or an increase in the number of weekly hours worked, depending on the capability of firms to redistribute workloads. In the longer run, there are more complex general equilibrium effects on market structure whose final impact on employment and economic growth remains ex ante ambiguous. On the one hand, larger and more competitive shops are likely to benefit more than small players (Schivardi and Viviano 2011), to the point of forcing the latter out of the market—for example, if increased shopping hours facilitate price comparison (Clemenz 1990; Inderst and Irmen 2005) or if Sunday openings lower the travel costs to reach large shops (de Meza 1984). On the other hand, the increased competition may promote small shops' specialization, and small retailers may benefit from the augmented demand due to increased shopping hours. Accordingly, small players might end up with a lower share of the market but higher turnover.[2]

[1] A recent notable contribution is Kim, Leung, and Wagman (2017) on the effects of short-term rental regulation.

[2] Ferris (1990) develops a model of retail consumer demand that explicitly takes into account shopping hours and argues that the duration of operating hours allowed and a store's location drive the dynamics of aggregate demand.

In this paper, we tackle these questions empirically and estimate the effects of full deregulation of retail shops' hours on the level and composition of employment and on the number of shops and their size distribution to shed light on the retail sector's implied market structure. To identify the effect of interest, we build a novel data set of Italian municipalities, including their regulatory status in 2007–16, and exploit the variation provided by the staggered implementation at the municipal level of a deregulation reform enacted from 1998 onward. For the core empirical analysis, we rely on administrative data reporting the number of workers and outlets located in each Italian municipality by year and bracket of shop size. We complement the 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 the labor force in terms of individuals' characteristics and employment relationships.

Our estimates show that, in the context of a general contraction in the retail sector and the economy as a whole, deregulating shops' hours of operation mitigated the decrease in the numbers of workers and outlets in the retail sector, with estimated positive impacts of 2.4 percent and 1.8 percent, respectively. Our estimates further suggest that such effects were mainly driven by large retailers, which increased their workforces and were less likely to exit the market. Such dynamics thus reinforce the recomposition of the retail sector in favor of larger players. This finding is corroborated by estimates based on individual-level survey data showing that the retail sector's labor force changed to have a higher prevalence of employees than self-employed workers. Moreover, employees' earnings increased by about 2.4 percent although the number of hours worked remained substantially unchanged, which hints at a more efficient allocation of work time. 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 liberalizations.

The findings in this paper are consistent with the scant evidence documenting the effects that deregulating shops' operating hours produced in other countries. Some papers focusing on the United States (Goos 2004; Burda and Weil 2004) find a negative effect of blue laws on employment in the retail sector for part-time and full-time work (with estimates ranging from 2 to 6 percent). Similarly, Skuterud (2005) estimates that deregulating Sunday openings in Canada generated a positive effect on total retail employment of about 4 percent. As for the European context, the existing literature mainly focuses on Germany, where the deregulation of shopping hours was progressively enacted by different federal states between 2006 and 2007.[3] In this context, exploiting the variation in deregulation across time and states to inform a difference-in-differences identification strategy, Paul (2015) estimates that the reform increased the probability of individuals' employment by 2.5 percent on average, with marked differences across subgroups of workers and types of firms. Bossler and Oberfichtner (2017)

[3] The jurisdiction for regulating shops' operating hours was transferred from the federal government to state governments, whose interventions led to the progressive liberalization of regulations between November 2006 and July 2007.

estimate a positive impact on retail employment driven by part-time workers and workers in larger shops. Senftleben-König (2014), on the other hand, estimates a negative effect driven by a loss of full-time jobs in smaller firms. Finally, Bensnes and Strøm (2019) analyze the effects of a deregulation of shops' operating hours in Norway in the mid-1980s and show that, by increasing employment opportunities for low-skilled workers, it negatively affected youths' human capital investment.

We add to the existing literature in several ways. First, thanks to the peculiar nature of the treatment in the Italian regulatory setting—which is confined to municipal boundaries—we are able to identify the average treatment effect at a very fine level, controlling for potential confounding factors, policy-related distortions, and regional peculiarities through several layers of fixed effects. Second, our data allow us to consistently estimate the effects of deregulation on local market structures by looking not just at the absolute numbers but also at the size distribution of active outlets and firms' dynamics. Third, the use of worker-level data allows us to detail the impact on employment by shedding light on the composition effects generated by the reforms. Finally, unlike previous studies, we focus on a very fragmented setting, the Italian retail market structure, which is characterized by a small average shop size and a low average level of productivity (Ciapanna and Rondinelli 2014).[4] In turn, this implies that the impact of a deregulation intervention like the one analyzed here may ex ante be different from that observed in countries like Germany or the United Kingdom. Indeed, in a context dominated by microfirms, typically family-run businesses, it is less likely that shops will be able to adjust their labor forces to fully take advantage of the new opportunities. In line with this interpretation, and consistent with most previous findings, our results show that the deregulation effect was positive regardless of firms' size, but smaller firms gained relatively less than bigger ones, which led to a reallocation toward larger outlets.

The remainder of the paper is structured as follows. In Section 2, we introduce the Italian regulatory framework of the retail sector and the deregulation reform. Section 3 is devoted to a description of the data set and 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, and Section 6 is dedicated to the individual-level analysis. Section 7 concludes.

2. Institutional Background

In all Organisation for Economic Co-operation and Development (OECD) countries, retail trade is regulated on several dimensions. Restrictions generally apply to the locations of stores, the licenses needed to start a business, the periods of sales promotions and maximum discounts applicable, and operating hours. Concerning the latter, regulations typically either specify the opening and closing

[4] See Table OA3 in the Online Appendix for a comparison of the market structure and firms' dynamics in the retail sectors in Italy, Germany, Spain, and the United Kingdom.

hours and the maximum number of hours a shop can stay open or simply ban or restrict opening on Sundays and public holidays.

The legislation on operating hours varies significantly across countries, even if different liberalization measures have been adopted in most OECD countries in recent years. According to the PMR indicators, in 1998 operating hours were completely liberalized in eight (of 26) countries; this figure increased to 16 (of 37) in 2018. In any case, no legislation provides for a complete ban on opening on Sundays or public holidays. Exceptions apply to tourist areas or a limited number of Sundays or public holidays per year. In some jurisdictions, restrictions do not apply to small shops. In other cases, rules are fixed at the regional or local level; for example, in Germany and Spain the restrictions vary significantly across areas and are less constraining in capital cities. Even in countries where some limitations are maintained, restrictions have been loosened. For example, in 2015 the French Parliament passed a law (*loi* Macron) to increase the number of Sunday openings allowed each year from four to 12.

In Italy, retail trade regulation dates to the first half of the 20th century. Until the end of the 1990s, Sunday openings were generally prohibited, and hours were strictly limited. Yet exceptions could be made by regional governments, which provided a disparate regulatory setting across the nation.

In 1998, a comprehensive reform of the retail sector was passed (the Bersani Decree, March 31, 1998, n.114, G.U. April 24, 1998, n.95) to loosen some restrictions and boost competition. In particular, the national reform fixed the set of administrative authorizations required to open new shops and the relevant criteria to be considered (for example, urban planning constraints) that varied by the shop's size, with smaller shops generally subject to milder requirements.[5] Moreover, the reform established general regulation of shops' hours that restricted both the number of Sunday openings (eight per year) and the daily hours of operation (a maximum of 13 hours between 7 a.m. and 10 p.m.). However, two main exceptions were introduced: in all municipalities, shops could stay open every day in December, and all restrictions were removed in municipalities that most rely on tourism. Those municipalities were included in a list compiled at the regional level. Regions set the criteria that municipalities should fulfill to obtain such status and thus be exempted from any restriction on hours, and each municipality could apply to be included. Regional governments were entrusted with the power to include municipalities that complied with the criteria and to apply limitations on the periods or areas of the municipality that were subject to the deregulated regime (namely, periods or areas most subject to tourist flows).[6] The lists created by the regional administrations were updated over time following

[5] In implementing the new entry rules, some regions imposed a more restrictive interpretation of the Bersani Decree provisions. Such cross-regional variation is exploited in Schivardi and Viviano (2011) to estimate the impact of entry barriers on productivity. Note, however, that different strictness in applying the Bersani Decree was unrelated to the characteristics of the municipalities that determined the applicable regime for operating hours.

[6] The regional regulations could not impose limits more rigid than the national-level benchmark law.

the same process. In December 2011 the rules were repealed: decree 201/2011 (Salva Italia, December 6, 2011, n.201, G.U. December 6, 2011, n.284) completely liberalized the days and hours of shopping across the country, which erased the distinction between touristic and nontouristic municipalities.

3. Data and Descriptive Details

To analyze the impact of deregulation, we reconstructed the regulatory framework that applied to each Italian municipality between 1998 and 2011. To do this, we collected the relevant regional legislation, examined the administrative acts adopted by the regional governments, and identified the municipalities granted touristic status at each point in time.[7] In addition, we checked whether, according to the regional law, a municipality that qualified as touristic allowed shops to open 365 days a year or whether the regional authorities imposed some restrictions.[8] We were not able to gather the relevant information for Liguria, Toscana, and Umbria and thus exclude them from the analysis.[9]

The resulting data set comprises two subsets of touristic municipalities: the fully deregulated, which are located in regions where laws did not provide any restrictions on hours, and the partially deregulated, which were subject to some time or spatial restriction despite their touristic status. Figure 1 plots the fraction of municipalities in each group by year. The graph highlights the process of gradual deregulation until complete deregulation took place at the end of 2011.[10]

To give an idea of the regulatory regime in place before full deregulation, Figure 2 shows the maps of Piedmont and Lazio—two of the largest regions in Italy—in 2010. The differences between the two regions reflect the distinct regulatory approaches taken: while in Piedmont there were no regional-level limitations to liberalization, in Lazio limitations were in place at the central level, and therefore fully deregulated status was an exception.

We merge the municipality-year data on the regulatory framework with data for 2007–16 from the Statistical Register of Active Companies (Archivio statistico delle imprese attive [ASIA]; Italian National Institute of Statistics 2019) database, which contains information about the numbers of workers and firms by year and sector of activity at the municipal level.[11] It also includes information about firms' size in three brackets: small (fewer than three workers), medium (between three

[7] The regional legislation is published on the websites of the regional governments. This is also the case for some administrative acts; for the others, we required the regional administrations to disclose them using the Italian law on administrative transparency.

[8] Several regions had limitations on time—for example, stores were allowed to be open for a maximum number of Sundays during the year—or on areas—for example, only central neighborhoods in large municipalities were typically deregulated.

[9] In these regions, touristic status was autonomously decided by the municipalities through local administrative acts.

[10] Table OA1 in the Online Appendix reports the number of municipalities that transitioned from nontouristic to touristic status, by year and region, and their relative share of all municipalities. It also reports municipalities that reintroduced limits to shops' opening hours, which we exclude from the analysis.

[11] We refer to all shops, stores, and outlets as retail firms.

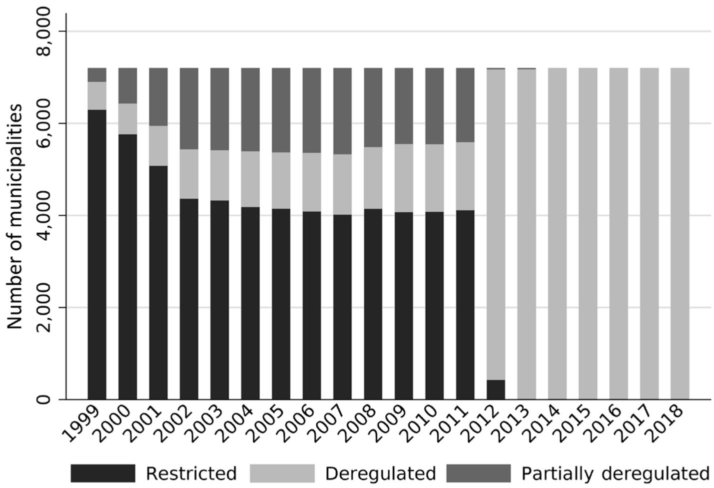


Figure 1. Frequency of deregulated municipalities

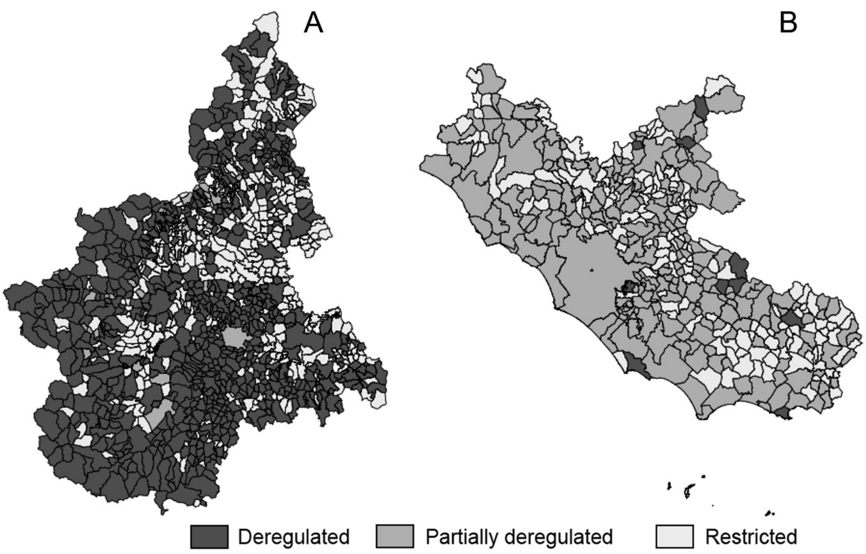


Figure 2. Municipalities in (*A*) Piedmont and (*B*) Lazio before deregulation

and 20 workers), and large (20 or more workers) entities. The final data set is a balanced panel of 6,710 Italian municipalities that includes information about touristic status, numbers of workers and firms in the retail sector (in total and by size bracket), and sociodemographic variables taken from the 2011 census (Italian National Institute of Statistics 2012).

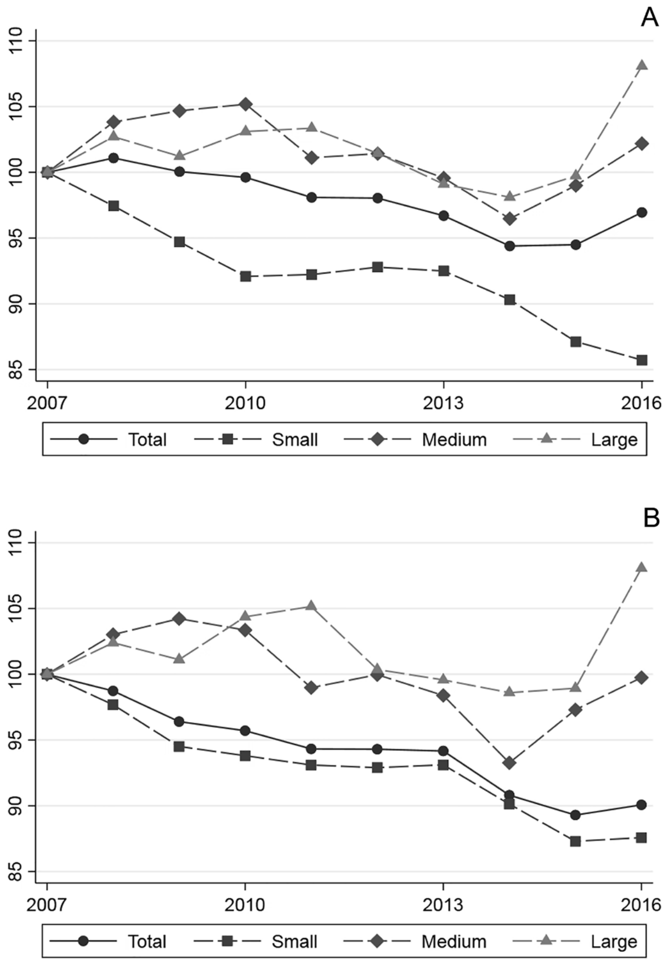


Figure 3. Dynamics of (*A*) workers and (*B*) firms, 2007–16

Figure 3 plots the variation in retail workers and firms (2007 is set at 100 for each). A general decreasing trend starts after the financial crisis of 2008 and the sovereign debt crisis of 2011, which translates to a countrywide 5 percent loss in workers and a 10 percent loss in firms in 2016 relative to 2007. The effect, however, is composite and reflects the deep changes in the market structure of the retail sector. While large and medium-sized firms show a discontinuous pattern, with an expansion until 2009–10 followed by a retraction until 2014 and then renewed growth, small firms alternated harsh declines with periods of relative stagnation on both dimensions. As a result, over the period of analysis the market share of large firms increased at the expense of small and individual firms, and the resulting firm-size distribution shifts to the right (see Figure 4).

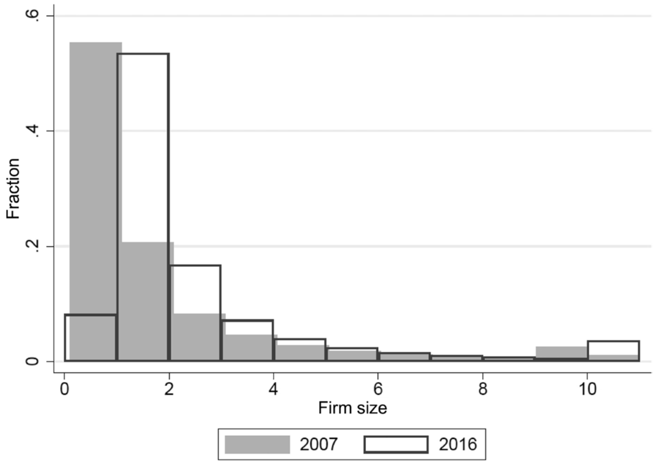


Figure 4. Distribution of firms' sizes

4. Empirical Strategy

The designation of touristic status, the time and spatial limits on liberalization, and the national-level deregulation introduced with the 2011 reform provide exogenous variation in the regulatory framework at the municipal level. We exploit the variation to identify the effect of deregulating firms' operating hours in a (dynamic) difference-in-differences setting or two-way linear fixed-effects estimation. In particular, we are able to quantify the effects of extended operating hours and Sunday openings on the stock of workers and firms in the retail sector.

The existence of a subset of municipalities with limited or no prior restrictions on hours at the beginning of our analysis—namely, the touristic municipalities in 2007—allows us to cluster our sample in two main groups that we compare in our empirical analysis. The control group includes all municipalities that were fully deregulated in 2007; the treatment group is composed of the municipalities where the restrictions were completely repealed between 2007 and 2016.[12] A third group of municipalities, which we label the partial control group, is composed of municipalities that were initially only partially deregulated and whose regulatory status was therefore only partly affected by the 2011 reform.

Table 1 presents descriptive statistics for the three groups in 2007.[13] The groups were rather different at the beginning of the period of analysis. Those in the control group were small but relatively rich; they had the smallest population and the lowest numbers of retail workers and firms but the highest average income and

[12] A total of 86 percent of these firms were deregulated in 2011 with the *erga omnes* act, while the others were added to the list of touristic municipalities in preceding years. Only Veneto and Trentino Alto-Adige were deregulated in 2013 because of late reception of the norm (Table OA1).

[13] To maintain consistency with the empirical applications, variables are in logarithmic form. In Table OA2, the corresponding values are in levels.

Table 1
Descriptive Statistics

	Treated (1)	Control (2)	Partial Control (3)	Differences 2 − 1 (4)	Differences 3 − 1 (5)
(Log)Retail Workers	4.471	3.505	4.682	−.966**	.211**
	(1.648)	(1.553)	(1.809)	(.0718)	(.0474)
(Log)Retail Firms	3.802	2.902	4.012	−.900**	.210**
	(1.422)	(1.324)	(1.614)	(.0613)	(.0419)
(Log)Income Tax	9.125	9.366	9.161	.241**	.036*
	(.378)	(.159)	(.306)	(.00904)	(.00895)
(Log)Population	7.907	6.989	8.038	−.918**	.131**
	(1.212)	(1.078)	(1.388)	(.0502)	(.0359)
Share of Graduates	.0723	.0706	.0787	−.0017	.0064**
	(.0264)	(.0249)	(.0297)	(.00115)	(.000773)
Unemployment Rate	.112	.0617	.102	−.0505**	−.010**
	(.0695)	(.0238)	(.0573)	(.00150)	(.00167)
N	4,113	537	2,060		

Note. Data for firms, workers, and income taxes are for 2007, while data for population, share of graduates (number of graduates/population ages 6 and older), and unemployment rate are for 2011. Differences are the results of t-tests on the difference of means.
** $p < .01$.

the lowest unemployment rate. Municipalities in the partial control group were on average the most populous, had the highest numbers of workers and firms, and had the highest level of human capital, which we proxy with the share of graduates. This is not surprising, as the partial control group eventually included the largest cities in Italy (Rome, Milan, and Naples), where typically deregulation applied only to firms in the city center. Finally, treated municipalities lie in between the other groups in terms of almost all measures, being generally larger but less affluent than the municipalities in the control group.

To identify the effect of the deregulation of operating hours, we employ a model that 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 differences, on the other. The estimating equation is

$$y_{it} = \alpha + \beta \text{Liberalization}_{it} + \mu_i + [\tau_t] + \psi_{rt} + \varepsilon_{it}, \tag{1}$$

where y_{it} is either the (log) number of workers employed in the retail sector or the (log) number of retail firms in municipality i and year t; Liberalization$_{it}$ is an indicator function for municipality i in year t being (fully) deregulated; τ_t and μ_i are year and municipality fixed effects, respectively; and ψ_{rt} is a group of region-year fixed effects included in the preferred specification to additionally control for possible regional trends. The parameter of interest—β—captures the differential changes in y_{it} due to the deregulation. As the reform offered only the possibility for shops to stay open more hours and we have no information about the degree of compliance, the coefficient of interest is interpreted as an intention-to-treat

(ITT) effect. Standard errors are clustered at the municipal level, namely, the level that defines inclusion in the treatment or control group.

Note that this approach differs from a standard difference-in-differences setting in that we use as a control group the always-treated municipalities instead of the usual never-treated units, which are not available in our framework. This difference-in-differences-in-reverse design (Kim and Lee 2019) is applied in several notable papers (Kotchen and Grant 2011; Chemin and Wasmer 2009), and the only required assumption for it to hold is that, absent the treatment, the control and treatment groups would follow a parallel trend, as in the usual difference-in-differences setting. Indeed, the two strategies yield exactly the same result as long as time does not differentially affect treated and untreated units.[14]

Our empirical design compares the treatment group with the control group and excludes the partial control units. Our results should thus be interpreted as the effect of moving from a regime of strictly regulated opening hours to one of fully flexible hours. Indeed, we believe that the space and time limitations that applied to the partial control units make such municipalities unsuitable for being either part of the treatment group or part of the control group.[15]

5. Results

5.1. Main Results

Table 2 presents the baseline results for the effect of liberalization on the number of workers and the number of firms in each municipality. As both outcomes are expressed in logarithmic scale, the estimated coefficients are semielasticities. For specifications with municipality and year fixed effects, the estimated effect of liberalization in treated municipalities amounts to a 3.4 percent increase in the number of individuals working in the retail sector and a 2.1 percent increase in the number of firms. The magnitude of the effects is lower but still significantly different from 0 in the more demanding specification with region-year fixed effects to account for local idiosyncratic shocks.

The validity of our difference-in-differences model relies on the assumption that, absent the treatment, the difference in outcomes for control and treatment groups would have been constant over time (the parallel-trends assumption). Figure 5 provides evidence supporting the absence of pretreatment trends that may bias the estimations. The specification includes region-year fixed effects. We

[14] Online Appendix OB discusses the relationship between the two empirical approaches.

[15] Indeed, the 2011 countrywide deregulation represented a treatment for the partial control units, but the intensity of the treatment changed depending on the initial restrictions. For example, consider two municipalities, A and B, with a maximum of 32 and 40 Sunday openings allowed, respectively. In that case, liberalization would have an intensity of $52 - 32 = 20$ for A but only $52 - 40 = 12$ for B, and the same reasoning applies to all levels of partial deregulation. The inclusion of municipalities with ex ante spatial limitations would constitute an even worse source of bias, as we cannot control for the share of treated and untreated firms in a municipality (for example, the numbers of firms in city centers versus those in peripheral neighborhoods). Results obtained including partial control units (in the control group) have smaller estimated effects in terms of magnitude, but they are still positive and significant for employment and the number of firms in the retail sector.

Table 2
Baseline Results

	(Log)Workers		(Log)Firms	
	(1)	(2)	(3)	(4)
Liberalization	.0343**	.0237**	.0211**	.0181**
	(.0062)	(.0090)	(.0049)	(.0070)
R^2	.990	.990	.992	.992
Region-year fixed effects	No	Yes	No	Yes

Note. Values are difference-in-differences estimates, with robust standard errors clustered at the municipal level in parentheses. All regressions include year and municipal fixed effects. $N = 46{,}500$.
** $p < .01$.

combine all treatment variables for $t > t + 5$ and $t < t - 5$ because the number of treated units shrinks as the distance from the treatment increases (Duflo, Glennerster, and Kremer 2008); these coefficients are estimated but not reported in Figure 5 (Goodman-Bacon 2021).[16]

The results in Figure 5 confirm that the estimated coefficients are null before the treatment and become positive as soon as the municipality is liberalized (time t) for both workers and firms. This reassures that any preexisting difference between the two groups of municipalities is correctly accounted for.[17] The coefficients after the treatment turn out to be constantly positive however, which suggests that the treatment effect is permanent or at least long-lasting. Note finally that the estimated effects, especially when considering the number of firms, scale up gradually: the first period after the reform corresponds to a slightly lower treatment effect.

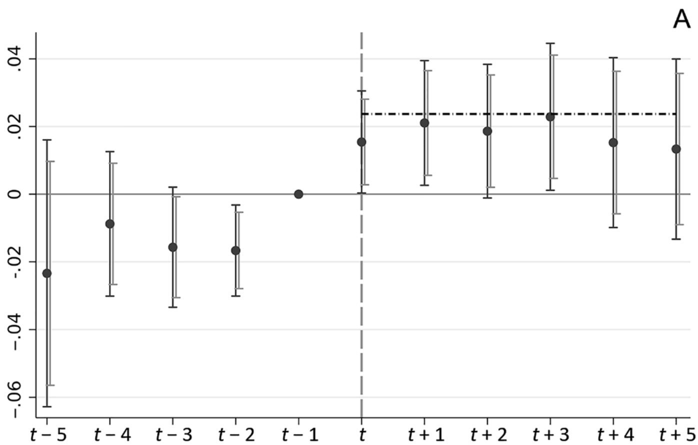
5.2. Robustness Checks

A battery of robustness checks on three dimensions are aimed at ensuring that the results are not driven by the specification, sample, or identification strategy chosen. Results are reported in Table 3.

A first concern is that the selection into the pool of touristic municipalities may be endogenous because the municipalities that could gain the most from liberalization had the greatest incentive to lobby regional authorities or because the authorities took the potential gains in the retail sector into account in selecting the touristic municipalities. If that was the case, the ordinary least squares estimates of equation (1) would be upward biased. To rule out this possibility, in columns 1 and 5 the treatment group is restricted to the municipalities subject to the main wave of liberalization at the end of 2011—which, being an *erga omnes* measure, is by definition exogenous to the individual incentives to be treated. The new treat-

[16] Note that only the municipalities that were deregulated in 2011 or earlier are observable 5 or more years later in our sample. Symmetrically, only the municipalities deregulated in 2013 and 2012 (those in Veneto and Trentino Alto-Adige; see Table OA1) can be observed at $t - 5$, and hence the confidence intervals are larger.

[17] A similar figure with only year and municipality fixed effects has some significant negative coefficients before the deregulation date, which signals that there may be some diverging regional trends.



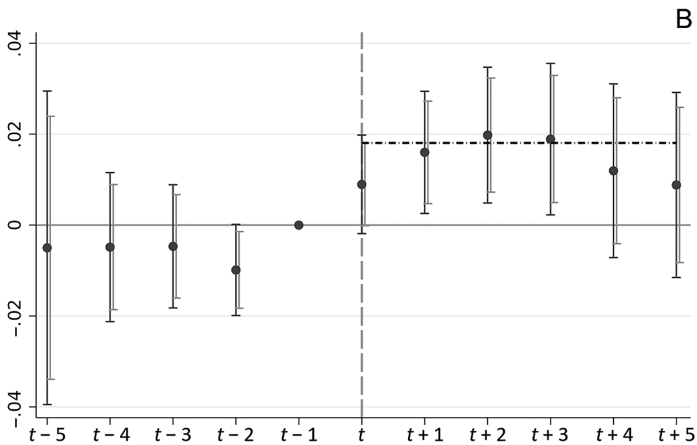


Figure 5. Baseline specification: testing the parallel-trends assumption for (*A*) (log)Workers and (*B*) (log)Firms.

ment group amounts to about 75 percent of the full group, but the exclusion of all units not treated in 2011 allows us to estimate a classic 2 × 2 difference-in-differences model:

$$y_{it} = \alpha + \beta(\text{Treated}_{it} \times \text{After } 2011_{it}) + \mu_i + \psi_{rt} + \varepsilon_{it}. \qquad (2)$$

If self-selection is driving the baseline estimates, there should be a lower or no effect when using the 2011-only treatment group. Instead, the estimated coefficients are either in line with (for workers) or larger than (for firms) the baseline.

Structural differences between treatment and control groups may influence the main results. To ease this concern, we estimate a propensity-score model using the municipality's population, the share of graduates, and the local unemployment rate, all measured in 2011, as predictors. The estimated results, reported

Table 3
Robustness Checks

	(Log)Workers				(Log)Firms			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Liberalization	.0228	.0244*	.0343**	.0207*	.0238+	.0187*	.0213**	.0161*
	(.0167)	(.0122)	(.00624)	(.00919)	(.0128)	(.00914)	(.00520)	(.00760)
N	36,180	38,050	46,500	46,500	36,180	38,050	46,500	46,500
R^2	.989	.990	.990	.990	.991	.992	.992	.992
2012 only	Yes	No	No	No	Yes	No	No	No
Propensity-score weights	No	Yes	No	No	No	Yes	No	No
Goodman-Bacon estimator	No	No	Yes	No	No	No	Yes	No
Treatment-year fixed effects	No	No	No	Yes	No	No	No	Yes

Note. Columns 1 and 5 report the results of a 2 × 2 difference-in-differences estimation that includes in the treatment group only the municipalities treated in 2012. Columns 2 and 6 report the results of a propensity-score weighted version of the baseline model, with the municipality's area, population, and region as predictive variables. Columns 3 and 7 report the results using Goodman-Bacon (2021) with bootstrapped standard errors. The treatment-year fixed effects capture possible differential pretreatment trends. All regressions include municipality and region-year fixed effects. Robust standard errors clustered at the municipal level are in parentheses.

$^{+}\ p < .1$.
$^{*}\ p < .05$.
$^{**}\ p < .01$.

in columns 2 and 6, are consistent with the baseline model, and if anything they have a slightly larger magnitude.

Goodman-Bacon (2021) proposes a method to address the concern that the treatment effect estimates may be biased when units are treated at different points in time. In those cases, the parameter of interest should be a weighted average of all possible two-group, 2-period difference-in-differences estimators, provided that the parallel-trends assumption holds for each estimation subgroup.[18] Columns 3 and 7 present the estimates obtained using the estimator proposed in Goodman-Bacon (2021), which are larger than the baseline ones in magnitude and strongly significant.

Columns 4 and 8 include a battery of fixed effects at the treatment-group-year level. This allows the model to absorb any possible trend that might have affected treated and control municipalities differentially. Such a specification captures cohort-specific effects like yearly shocks to the retail sector. The results of this specification are not significantly different.

5.3. Spatial Spillovers

Potential spillover and relocation effects of place-based policy interventions are widely documented in regional science and urban economics (Glaeser and Gottlieb 2008; Monte, Redding, and Rossi-Hansberg 2018; Ehrlich and Seidel 2018; Falck, Koenen, and Lohse 2019) and in industrial organization (Kerr and Kominers 2015; Lychagin et al. 2016). The displacement effects induced by the differences in regulation across neighboring municipalities may affect our results in two ways. On the one hand, if sellers chose to move their firms from restricted to liberalized municipalities, our estimate of β would be downward biased, given that the demand for retail services in newly liberalized municipalities would already be captured, at least partially, by those relocated sellers. On the other hand, even if sellers do not adjust their location choices according to liberalization status, buyers may adjust their consumption choices by traveling to nearby municipalities to shop. In such cases, our estimates would be upward biased; in fact, once firms are allowed to open flexibly in the municipality of residence of the firm's owner, individuals will switch to local suppliers to minimize their transaction costs. In turn, this will reduce employment in control municipalities and increase it in treated municipalities without a real increase in the overall size of the sector. In light of such reasoning, it becomes of utmost importance to find out whether the estimated coefficients result from a geographical shift of retail activities or whether they represent an effective boost of the sector.

To ensure that our results are not biased by spillovers, displacement, or relocation effects, we propose three strategies; the results are reported in Table 4. First, we aggregate the data into larger administrative units whose boundaries are designed to be large enough to contain the spillovers and repeat the estimation.

[18] Note that the 2011-only estimates, being from a simple 2×2 difference-in-differences model, already partially address this issue.

Second, we follow a spatial exclusion approach (Ehrlich and Seidel 2018) and exclude control units with a shared boundary with a treated municipality, as they are potentially more affected by local relocation. Third, to characterize and directly estimate the magnitude of the spillover effects, we augment our difference-in-differences model with the numbers and composition of neighboring municipalities before and after deregulation.[19]

In the first exercise, we aggregate the data at the local labor market (LLM) level. Because LLMs are self-contained labor markets where most of the people live and work,[20] they are the most suitable geographical units for examining the labor market effects of local shocks. 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 ($Q_{\mathrm{LLM},t}$) and proceed with a slightly modified model:

$$y_{\mathrm{LLM},t} = \alpha + \beta Q_{\mathrm{LLM},t} + \mu_{\mathrm{LLM}} + \psi_{rt} + \varepsilon_{\mathrm{LLM},t}. \tag{3}$$

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

$$y_{\mathrm{LLM},t} = \alpha + \beta \mathrm{Liberalization}_{\mathrm{LLM},t} + \mu_{\mathrm{LLM}} + \psi_{rt} + \varepsilon_{\mathrm{LLM},t}. \tag{4}$$

The results of the exercises are reported in columns 1 and 5 (equation [3]) and 2 and 6 (equation [4]) of Table 4. When all municipalities in an LLM are liberalized, employment in the retail sector increases by 3.7 percent, and the effect is even larger with the more restricted identification strategy (4.1 percent). The estimated increase in the number of firms amounts to about 1.6 percent in both specifications, which is in line with the baseline results in Table 2.

In columns 3 and 7 we implement a spatial exclusion approach and reestimate the baseline model, excluding the control units that directly neighbor the treated municipalities. Note that the sample size shrinks very little because the control group is sensibly smaller than the treatment group. The estimated coefficient is slightly larger than the baseline for both retail employment (2.7 percent) and retail firms (almost 2 percent).

As explained above, in our setting there are two possible spillover effects at play depending on the status of the municipality: first, firms in liberalized municipalities may capture part of the demand from neighboring municipalities (baseline

[19] The same empirical approach to the difference-in-differences method in the presence of spillover effects is independently developed by Berg, Reisinger, and Streitz (2021). They show the consistency of the method and quantify the bias in the baseline estimation without accounting for the spillover effects.

[20] Each local labor market (LLM) contains about 13 municipalities on average, with significant variability depending on the accessibility of the area. The boundaries of each LLM are defined by the Italian National Institute of Statistics on the basis of commuting matrixes, which account for the numbers of workers commuting from and to each location in the country.

Table 4
Spatial Spillovers

	(Log)Workers				(Log)Firms			
	Local Labor Market (1)	Restricted Local Labor Market (2)	No Neighbors (3)	Spillover Effects (4)	Local Labor Market (5)	Restricted Local Labor Market (6)	No Neighbors (7)	Spillover Effects (8)
Liberalization	.0369**	.0412**	.0270**	.0250*	.0163**	.0159*	.0197**	.0172*
	(.0089)	(.0117)	(.0066)	(.0099)	(.0053)	(.0071)	(.0053)	(.0080)
$\hat{\gamma}^{t}$				−.0027+				−.0008
				(.0016)				(.0013)
$\hat{\gamma}^{tt}$				.0022				.0015
				(.0019)				(.0015)
N	5,130	3,497	43,110	46,170	5,130	3,497	43,110	46,170
LLM fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
Municipality fixed effects	No	No	Yes	Yes	No	No	Yes	Yes

Note. The results in columns 1 and 5 aggregate data at the local labor market (LLM) level and redefine the treatment as the share of treated population per LLM per year. Columns 2 and 6 focus on fully treated or fully control LLMs. Columns 3 and 7 excludes control municipalities that share a boundary with a treated unit. Columns 4 and 8 include measures for the numbers of treated and control neighbors. All regressions include region-year fixed effects. Robust standard errors clustered at the LLM level or the municipality level are in parentheses.

$^{+}p < .1$.
$^{*}p < .05$.
$^{**}p < .01$.

spillover). This demand-capturing effect is stronger for restricted municipalities; however, once liberalized they would be able to regain part of that demand (spillover on the treated group). The robustness of the liberalization parameter in the LLM-level exercises indicates that the sum of the spillover effects within the boundaries of a given LLM is essentially null. This could mean either that the spillover effects are in fact negligible or that the spillover effect on the treated group fully offsets the baseline spillover effect. 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 contiguous to the ith municipality at each time t, and with its interaction with Liberalization:[21]

$$\begin{aligned} Y_{it} = \alpha + \beta \text{Liberalization}_{it} + \mu_i + \tau_t + \gamma^t \text{Lib\_Neigh}_{it} \\ + \gamma^{tt}(\text{Lib\_Neigh}_{it} \times \text{Liberalization}_{it}) + \varepsilon_{it}, \end{aligned} \tag{5}$$

where γ^t captures the effect on the ith unit of neighboring one extra liberalized municipality (baseline spillover), while γ^{tt}, which captures the interaction of liberalized neighbors and the liberalization treatment, provides the estimate of the spillover effect on the treated. Through equation (5), we are able to disentangle the direct effect of the deregulation from that induced by relocation.[22] The results in columns 4 and 8 show that the baseline spillovers have a negative sign. Hence, the liberalized municipalities do capture some of the demand coming from their neighbors, but it applies only to the most mobile input: workers. The magnitude is very small compared with the direct treatment effect, on the order of one-tenth of the direct effect for every already liberalized neighboring municipality, and the effect is almost completely offset by the spillover on the treated. More formally, $\hat{\gamma}^t + \hat{\gamma}^{tt} = 0$.

5.4. *Effects on Market Structure*

While the baseline results are informative about the average effect of the reform, it is especially important to understand whether and how the deregulation of firms' operating hours affected the underlying market structure. Indeed, in the context of a progressive sectoral recomposition in favor of larger firms, with a declining trend in the presence of small shops and a growing presence of large chain stores (Ciapanna and Rondinelli 2014), it becomes of first-order importance to understand whether liberalization affected the two types of firms differently and thus mitigated or accentuated the gap. Moreover, such differences are particularly interesting and relevant in the case of Italy given its peculiar retail market structure, both before and after deregulation reform. The Italian retail sector, indeed, shows a marked cone-shaped structure, with a lot of microfirms and very few large companies: in 2011, family-run businesses with no employees accounted for

[21] The estimated effects are identified thanks to the staggered implementation of the liberalization, which affects the status of the ith unit and that of its neighbors at different times.

[22] See Online Appendix OC for a more detailed discussion of the implications of this model.

Table 5
Effects on Market Structure

	(Log)Workers			(Log)Firms		
	<3 Workers	3–20 Workers	>20 Workers	<3 Workers	3–20 Workers	>20 Workers
Liberalization	.0216**	−.0125	.0454	.0144*	.0029	.0192*
	(.0071)	(.0018)	(.0310)	(.0064)	(.0099)	(.0079)
R^2	.989	.966	.901	.991	.979	.937

Note. Values are difference-in-differences estimates at the municipal level, with robust standard errors clustered at the municipality level in parentheses. All regressions include municipality and region-year fixed effects. $N = 46{,}500$.
* $p < .05$.
** $p < .01$.

over two-thirds of active firms in the retail sector, whereas those with more than 10 employees made up less than 2 percent of businesses.[23]

Table 5 shows the effects for firms in the three size brackets described in Section 3. In our sample, 78 percent of firms had fewer than three workers, about 20 percent had between three and 20 workers, and just 1.2 percent had more than 20. However, medium-sized firms employed about 42 percent of workers in the sector, with an average of 5.8 workers, and large firms employed about 23 percent, with an average of 53 workers, compared with 1.2 workers in small firms.

Table 5 reports the estimates from the baseline model with municipality and region-year fixed effects. Figure 6 shows that the model correctly accounts for preexisting trends in these specifications. The effect was concentrated in small and large firms and was marginally larger in the latter case.[24]

Table 6 adds evidence for the impact of flexible hours on the distribution of firms' size. Firms that were small in 2007 experienced a 1.6 percent growth in workforce. However, the impact was considerably larger for firms that were already large in 2007, with an estimated growth in workforce of almost 7 percent. Medium-sized firms did not grow. This exercise allows us to evaluate the impact of the reform on the intensive margin, leaving out any consideration of market recomposition. Table 6 also estimates the impact of the reform on the (log) number of new firms becoming active and on the (log) number of firms exiting the

[23] Table OA3 reports data for Italy, Germany, Spain, and the United Kingdom in 2011 and 2016. The only country with a pyramidal market structure comparable to the Italian one is Spain, but that system is much less bottom heavy. Germany and the United Kingdom have rather different structures, with firms being on average larger and older. Such differences are particularly relevant when comparing our results with previous studies focusing on these countries (ffor Germany, see Bossler and Oberfichtner 2017; Senftleben-König 2014).

[24] Table OA4 shows the results by size of the firm under different empirical specifications. While the specification at the municipality level excluding neighboring control towns has results very similar to the baseline levels, the LLM regressions show null effects on small firms. We believe this difference may be due to the extent of spatial spillovers on small and large firms. Indeed, while the effect on smaller firms is likely to be local, that on larger firms may more likely extend beyond municipal boundaries. For example, consider a potential customer living in a nonderegulated municipality: she would be more willing to move to a nearby deregulated municipality (perhaps in a neighboring region if she is in a municipality at the border) and thus pay the transportation and opportunity costs to shop in a large department store instead of a small, family-owned firm.

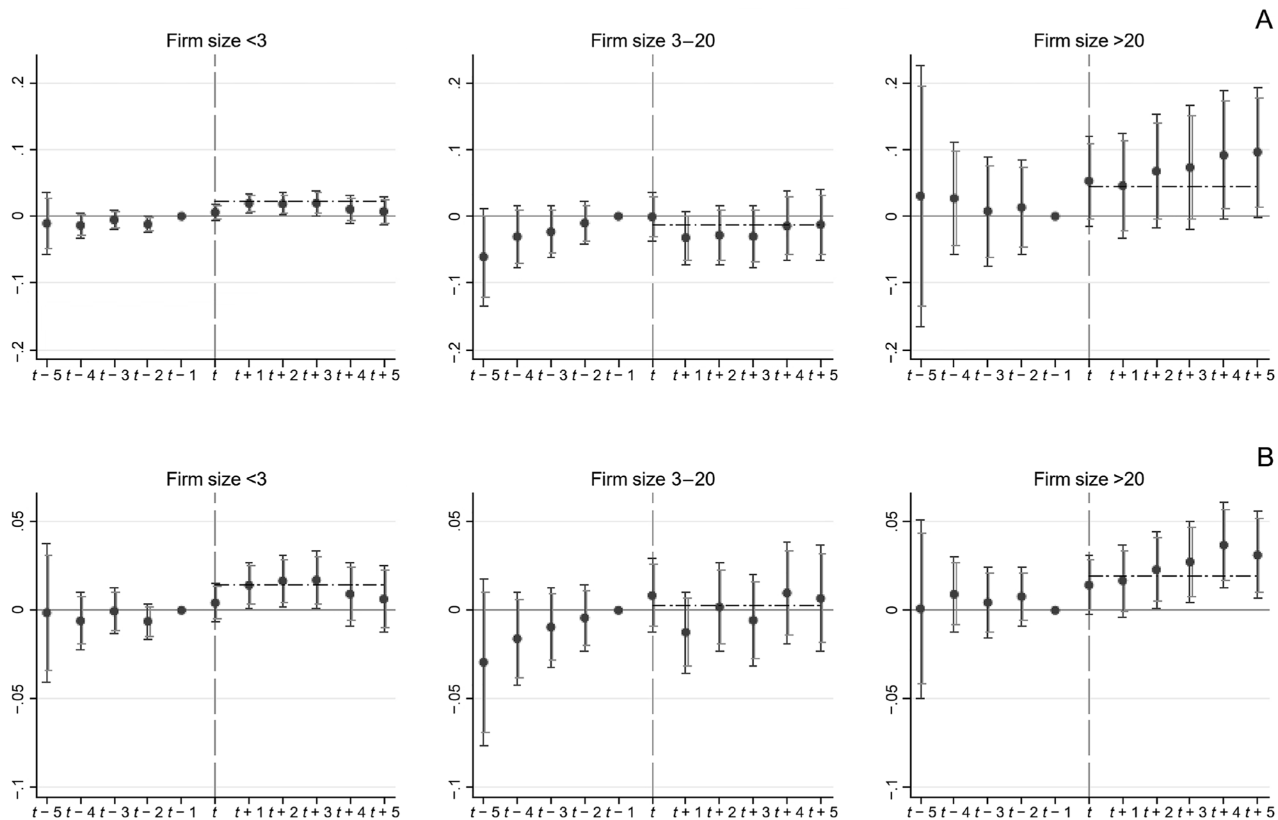


Figure 6. Estimated effects by firm size for (*A*) (log)Workers and (*B*) (log)Firms

Table 6

Effects on Firms' Dynamics

	(Log)Workers in 2007			(Log)Firm Entries			(Log)Firm Exits		
	<3 Workers	3–20 Workers	>20 Workers	<3 Workers	3–20 Workers	>20 Workers	<3 Workers	3–20 Workers	>20 Workers
Liberalization	.0162+	−.0067	.0688*	.0240*	.0747**	.0277**	.0149	−.0517**	−.0161**
	(.0092)	(.00164)	(.0267)	(.0115)	(.0131)	(.0058)	(.0109)	(.0120)	(.0061)
R^2	.987	.981	.945	.920	.810	.493	.919	.818	.507

Note. All regressions include municipality and region-year fixed effects. $N = 46{,}500$.
+ $p < .1$.
* $p < .05$.
** $p < .01$.

market each year in a given municipality. The results show that the introduction of fully flexible hours favored the opening of new firms in all size brackets, with a particularly large effect on medium-sized firms. On the other hand, small firms were not more likely to exit after the reform, whereas medium and large firms became significantly less likely to cease their activities.

This evidence, especially considered in combination with the results reported in Section 6, points to the conclusion that the deregulation of shops' operating hours reinforced the process of recomposition of the retail market structure in favor of larger operators, which grew more and were less likely to exit the market. Indeed, large firms accounted for about 60 percent of the total increase in the number of workers in the retail sector relative to the baseline period (2007) while employing only about 20 percent of the workforce.

5.5. Sectoral Spillover Effects

We finally consider the possibility that the deregulation of shops' operating hours may induce spillover effects on sectors other than retail. To explore this channel, we estimate our baseline model on the number of workers and firms in all sectors covered by the ASIA data. This exercise is both a robustness check for the identification strategy—there should be no effect on sectors unaffected by the deregulation—and a test for possible spillover effects. Sectors may be indirectly affected by the liberalization of the retail sector either positively—those complementing retail activities—or negatively—if workers and entrepreneurs are induced to flee from a given sector toward retail activities. Figure 7 plots the estimated coefficients for each sector of economic activity.

The deregulation of retail operating hours generally generated no effect on employment in other sectors. Interestingly, however, employment increased significantly for hotels and restaurants and in the financial sector, which suggests that those sectors benefited from the fact that more shops were open at night or on Sundays. If we consider the effects on the number of firms instead, we find evidence of a significant increase in the number of banks and other financial sectors' branches only. This may also be due to a general increase in the volume of economic activity.

As a final exercise, in Figure 8 we estimate the effect of deregulation on overall employment and the number of firms. To this end, we pool all sectors and augment our baseline specification with sector-year fixed effects to account for sector-specific shocks. There is no significant impact on employment or the number of active firms.[25]

6. Individual-Level Evidence

In this section, we complement the main analysis with evidence at the individual level to both corroborate the results from the main analysis and better qual-

[25] Note that as retail employs about 20 percent of workers in the private sector, a 2 percent increase would algebraically translate into a .4 percent increase in total employment.

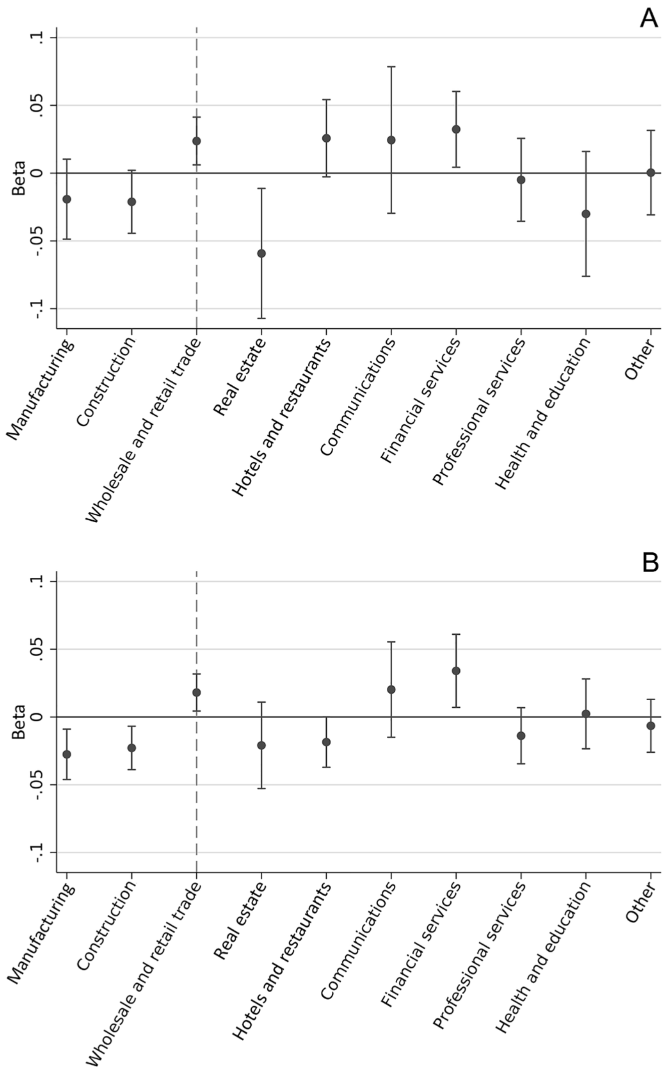


Figure 7. Estimated sectoral spillover effects for (*A*) (log)Workers and (*B*) (log)Firms

ify the employment relationships created and workers involved in the transition, that is, the compositional effects of the deregulation. To this end, we use the data from the LFS, which contains quarterly individual-level and household-level information about education, employment history, and demographic characteristics. We merge the LFS data with a quarterly version of the data set on retail de-

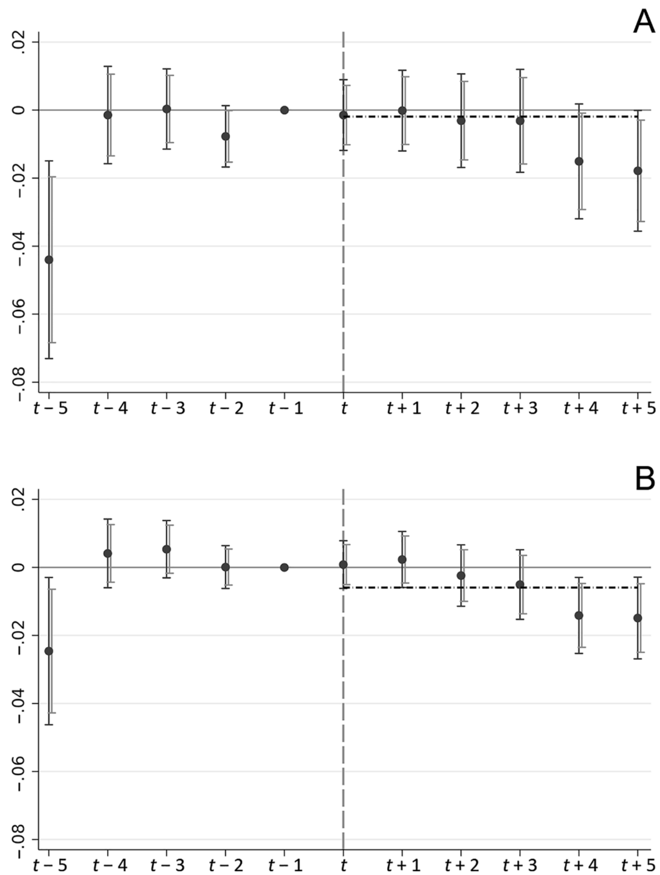


Figure 8. Estimated aggregate effects for (*A*) (log)Workers and (*B*) (log)Firms

regulation used in the main analysis, exploiting information about households' municipality to match the two.[26]

Table 7 reports the main descriptive analysis of the sample in 2007, that is, before the deregulation of treated municipalities. It confirms the evidence in Table 1: individuals living in treated municipalities were on average more educated than those in the control group but less than those in the partial control group, and the overall employment rate was lower. The share of workers in the retail sector was similar across groups, around 15 percent. The share of self-employed workers was highest in control municipalities, and so was the incidence of work-

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

Table 7
Labor Force Survey Descriptive Statistics, 2007

	Treated (1)	Control (2)	Partial Control (3)	Differences 2 − 1 (4)	Differences 3 − 1 (5)
Population ages 15–64:					
Female	.510	.507	.511	−.003	.001
	(.500)	(.500)	(.500)	(.005)	(.002)
Age	40.599	42.573	41.019	1.974**	.420**
	(13.838)	(13.645)	(13.841)	(.134)	(.047)
Tertiary education	.102	.093	.134	−.009*	.032**
	(.302)	(.290)	(.341)	(.003)	(.001)
Employed	.540	.625	.565	.085**	.025**
	(.498)	(.484)	(.496)	(.005)	(.002)
N	215,698	10,900	149,111		
Employed in retail sector	.148	.151	.148	.003	−.000
	(.355)	(.358)	(.355)	(.004)	(.002)
N	116,555	6,814	84,240		
Self-employed	.421	.484	.417	.063**	−.004
	(.494)	(.500)	(.493)	(.016)	(.006)
Large distribution	.227	.244	.241	.017	.014*
	(.419)	(.430)	(.427)	(.014)	(.005)
Worked Sundays	.179	.243	.209	.064**	.030**
	(.384)	(.429)	(.406)	(.014)	(.005)
Weekly hours worked	38.643	38.163	38.313	−.480	−.330
	(14.751)	(15.032)	(14.884)	(.482)	(.175)
N	17,285	1,031	12,487		
Retail employees:					
Permanent contract	.865	.803	.850	−.062**	−.015*
	(.341)	(.398)	(.357)	(.018)	(.005)
N	10,005	532	7,283		
Monthly earnings (euros)	1.097	1.066	1.096	−30.73	−1.15
	(434)	(466)	(490)	(19.76)	(7.45)
N	9,371	589	6,726		

Note. Values for earnings are for 2009. Differences are the results of t-tests on the difference of means.
* $p < .05$.
** $p < .01$.

ing on Sundays. In the treated municipalities, retail workers were generally more likely to have permanent work contracts and slightly higher wages.

The individual-level empirical specification necessarily differs from the baseline specification in equation (1), because the LFS does not include households in all municipalities in all years. Each wave covers about 1,200 municipalities that are rotated so that smaller municipalities are not sampled each quarter.[27] This structure of the data does not allow us to run regressions with municipality fixed effects, and thus we resort to a specification with LLM fixed effects. The estimating equation is

$$Y_{jiq} = \alpha + \beta \text{Liberalization}_{iq} + \boldsymbol{X}_{jiq} + \lambda_l + \tau_{qr} + \varepsilon_{ilq}, \quad (6)$$

where Liberalization is for municipality of residence i, $\boldsymbol{X}_{jiq}$ is a group of individual-level characteristics in quarter q, λ_l is LLM fixed effects, and τ_{qr} is a set of quarter-region fixed effects to capture possible region-specific time trends, as in equation (1).[28] The results are reported in Table 8.

The main outcome of interest (columns 1 and 2) is an indicator variable for whether individual j residing in municipality i in quarter q is employed in the retail sector; the group of those not in retail include the unemployed and those employed in other sectors. Results are consistent with those in the main analysis: the deregulation of shops' operating hours induced an overall increase in the probability of being employed in retail of about .5 of a percentage point. Given the overall share of individuals employed in the retail sector—roughly 8 percent of this population—we estimate a marginal effect of around 6 percent. The result is strongly robust to the inclusion of individual control variables.[29]

In column 3, we restrict our analysis to employed individuals and estimate a model of workers' relocation to investigate whether the reform induced a change in the sectoral composition of employment in the affected municipalities. Conditional on being employed, the probability of holding a job in the retail sector rose by 1.2 percentage points over a baseline of about 15 percent (Table 7).

The evidence presented in Section 5.4 suggests that deregulation favored a recomposition of the sector toward larger firms. This should correspond, at a microlevel, to an increased probability of being an employee instead of being self-employed. Column 4, which estimates equation (6) on an indicator function for being self-employed in the retail sector, shows that deregulation lowered the share of self-employed by about 3 percentage points (around 7 percent). In line with this result, column 5 shows that the probability of working in a chain store—defined as a multibranch employer—increased.[30] Finally, column 6 tests

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

[28] The controls in the main specification include gender, age, age squared, and indicator functions for education level.

[29] Table OA5 reports some heterogeneous effects of the estimation of equation (6). There is a stronger increase in participation for men, individuals over 25, and those with less than tertiary education.

[30] Figure OA1 estimates the probability of working for a given size of employer among retail-sector employees. It shows evidence of a reallocation of workers from small to large firms.

Table 8

Individual-Level Estimates for Employment in Retail: Extensive Margin and Composition

	(1)	(2)	(3)	Self-Employed (4)	Chain (5)	Permanent Employee (6)
Liberalization	.0049**	.0059**	.0123**	−.0310**	.0297**	.0032
	(.0017)	(.0017)	(.0032)	(.0057)	(.0049)	(.0058)
Female		−.0294**	.0082**	−.1158**	.0860**	−.0342**
		(.0012)	(.0018)	(.0023)	(.0022)	(.0022)
Age		.0131**	−.0058**	.0126**	.0001	.0624**
		(.0002)	(.0004)	(.0006)	(.0006)	(.0008)
Age^2		−.0002**	.0000**	.0000*	−.0001**	−.0007**
		(.0000)	(.0000)	(.0000)	(.0000)	(.0000)
Secondary		.0189**	−.0106**	.0196**	.0099**	.0090**
		(.0012)	(.0022)	(.0028)	(.0025)	(.0026)
Tertiary education		−.0314**	−.0957**	.0087+	.0077+	−.0178**
		(.0018)	(.0031)	(.0046)	(.0044)	(.0044)
N	1,981,343	1,981,343	1,070,641	153,699	151,470	91,790
Employed	No	No	Yes	Yes	Yes	Yes
Employed in retail	No	No	No	Yes	Yes	Yes
Employees in retail	No	No	No	No	No	Yes

Note. Results are difference-in-differences estimates for the population ages 15–64, with robust standard errors clustered at the local labor market level in parentheses. All regressions include local labor market and region-year-quarter fixed effects.

+ $p < .1$.

** $p < .01$.

whether the deregulation induced a recomposition of the retail sector toward permanent or fixed-term employment relationships. The results suggest no significant change in this respect.

We also analyze the intensive margin to understand whether the deregulation affected the likelihood of working on Sunday, the number of hours worked weekly, and the incidence of part-time work. We also estimate the impact on (log) reported monthly earnings for employees. The results are reported in Table 9.

Overall, the share of retail workers working on Sundays rose by 1.7 percentage points from a baseline probability of 18 percent. The effect was concentrated among self-employed workers and those not working at chains.

Analysis of the responses on the intensive margin provides additional evidence on the mechanisms triggered by the liberalization. There was no increase in the number of hours worked weekly. This result, together with the baseline results on the number of workers, suggests that most stores adapted to the new regime by increasing the number of individuals employed rather than the length of their shifts. Overall, there is no effect on the incidence of part-time work. However, there is a decrease in workers with fixed-term contracts.

Finally, we investigate how the observed compositional effects and the changes in the intensive margin affected employees' earnings. There is a baseline average increase of 2.4 percent, which is stable across types of firms. There is, however, a remarkable difference in the effects on earnings for permanent workers, whose increase of 2.6 percent contrasts with the null effect for temporary workers. The finding that earnings increased whereas hours worked did not suggests that the introduction of flexible working hours allowed retailers to allocate their (and their employees') working time more efficiently.

7. Conclusions

In this paper, we estimate the causal effects of deregulating shops' operating hours on employment, market structure (in particular, the number and size distribution of firms), and working arrangements in the retail sector in Italy. We exploit the staggered implementation of a reform that allowed firms to adopt any schedule for hours of operation 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 previous contributions, we find a positive effect of the reform on employment, with an increase of 2.4 percent in the number of workers and 1.8 percent in the number of firms. Our results are very similar in magnitude to those of Viviano (2008), who finds that the introduction of flexible entry regulations in Italy increased the share of those working in the sector by .8 of a percentage point, and this number is slightly lower than our estimates of the share of individuals working in the sector, conditional on being employed. Moreover, the increase in employment that we find is equivalent to the effect that Berton et al. (2018) find would be produced by a 10 percent increase in credit to firms. In light of such comparisons, we argue that the effects of such deregulation are sizable.

Table 9

Individual-Level Estimates of Liberalization: Intensive Margin and Earnings

Explanatory Variable	Overall (1)	Employee (2)	Self-Employed (3)	Nonchain (4)	Chain (5)	Temporary (6)	Permanent (7)
Worked Sundays	.017**	.007	.023**	.018**	−.007	.001	.005
	(.005)	(.007)	(.007)	(.005)	(.013)	(.021)	(.007)
N	153,475	91,646	61,825	114,999	36,264	11,713	78,591
(Log)weekly hours	−.002	.007	−.001	−.002	.001	.029	.004
	(.004)	(.006)	(.006)	(.005)	(.009)	(.020)	(.006)
N	144,102	85,029	59,067	108,967	33,209	11,172	72,596
Part-time work	−.000	−.014*	.005	.004	−.018	−.039+	−.011
	(.004)	(.007)	(.004)	(.004)	(.012)	(.022)	(.007)
N	153,699	91,790	61,905	115,158	36,296	11,731	78,715
(Log)monthly wage		.024**		.018+	.025+	.004	.026**
		(.008)		(.011)	(.013)	(.025)	(.008)
N		69,659		40,972	27,849	9,101	60,531

Note. Results are difference-in-differences estimates for retail workers, with robust standard errors in parentheses. All regressions include local labor market and year-quarter-region fixed effects.

+ $p < .1$.

* $p < .05$.

** $p < .01$.

Interestingly, results from aggregate-level administrative data and individual-level survey data point to more significant growth for larger firms than for smaller ones and a recomposition of the sector toward large chain stores and away from small, family-run businesses. The adoption of flexible operating hours translated into an average increase in the real wages of employees, especially those holding permanent contracts, given that the increase in earnings was more than proportional to the increase in hours worked. Finally, the reform also had a positive effect on the activity of complementary services such as restaurants and financial services. Taken together, the evidence presented in this paper suggests that removing restrictions on operating hours can improve growth in the retail sector, and larger firms are better able to exploit the full potential of such a reform.

To better understand the external validity of our exercise, it is important to quantify the treatment generating the estimated effects. Our results should thus be read as the effects of moving from a regime in which shops can remain open 316 days a year to one in which they can stay open any day of the year (an increase of approximately 15 percent). Any other policy intervention should be evaluated on such a scale for comparison. Of course, the effects we estimate are to be interpreted as ITT parameters in that the reform did not impose any particular schedule.

All in all, from a policy perspective our results provide support for the idea that more flexible regulation of the business environment boosts economic growth, and the positive effect is reinforced by mechanisms of reallocation toward larger, more productive retail firms. We find no evidence that this leads to a worsening of employment conditions overall, though only permanent-contract workers benefited from the reform via an increase in their real wages. Our work, nevertheless, remains silent on the effects that softening regulation may have on consumers' welfare. Such effects would impact potential changes in prices, quantities, varieties, and quality of the goods sold and services provided to consumers. These questions are beyond the scope of the paper and are left to future research.

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