ENTRY REGULATION IN RETAIL MARKETS

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Abstract

This article studies the impact of entry regulation on the market structure of the retail sector. I show that a reform that increased the statutory cap on the number of pharmacies in Italian cities is not sufficient to remove the distortions brought about by entry regulation; the new cap becomes binding two semesters after the reform. I exploit variation in the reform's intensity to provide suggestive evidence that regulation shielded incumbents from additional competitors. Using a structural model of entry, I find that full liberalization would increase the number of firms by 60% and decrease the number of cities with only one pharmacy by 130%. I assess the model's predictive accuracy by comparing postreform outcomes with simulated ones to find that it correctly forecasts market structure in half of the cities affected by the reform.

1 Introduction

A guiding principle of antitrust policy is that restricting competition generally hurts consumers. Incumbents in retail markets, however, are frequently shielded from new competitors by entry regulations (Biggar, 2001). Regulators argue that these instruments work in the public interest because they increase the quality of sellers and, combined with a subsidy scheme, ensure broad levels of coverage. Opponents contend that entry restrictions increase the market power of incumbents and ultimately harm consumers. This controversy is at the heart of the debate on deregulating retail and professional services, as implemented by the United States and Japan and proposed by the European Comission.

I inform this debate with a study of entry regulation in the Italian pharmacy market. This setting is attractive because Italy, like other European countries, restricts the maximum number of pharmacies in a city based on a population criterion that was reformed in 2012. I exploit the staggered introduction of this reform across Italian regions to provide reduced-form evidence on the distortionary effects of entry regulation. Furthermore, I estimate a static structural model of entry with homogeneous firms to estimate the competitive effects of entry. I then use this model to simulate counterfactual policies and I evaluate the model's predictive performance by comparing its predictions with *postreform* outcomes.

I find that regulating entry has a substantive impact on the structure of the pharmacy market in Italian cities. In particular, I show that reducing the *statutory* minimum number of residents per pharmacy leads to an equivalent decrease in the *effective* ratio of population to pharmacies after two semesters. I exploit heterogeneity in the intensity of this reduction across cities to find that postreform ratios always converge to the minimum allowable value. I argue that this adjustment suggests that further entry is likely to be profitable; this implies that pharmacists are shielded from additional competition through entry regulation.

I then model firms' profits as a function of city-level observables and an unobservable profit shock that is common across all firms in a city, following Bresnahan & Reiss (1991). The specification of profits captures heterogeneity between firms that operate in cities with different market structures. I assume that firms enter as long as profits are positive and the entry restriction is not binding; this equilibrium condition allows me to estimate the profit function by maximum likelihood. I show that markets become more competitive with the entry of additional firms, the bulk of these changes takes place with the third and fourth entrants. The minimum number of customers that an average pharmacy needs to break even is around 30% of the current statutory minimum number, which rationalizes my reduced-form results. I use the model to simulate outcomes under free entry and I find that full liberalization would increase the overall number of establishments by 60% and decrease the number of cities with only one pharmacist by more than 130%. Finally, I use *ex-post* data to assess the model's out-of-sample performance and show that it accurately predicts postreform outcomes in 50% of markets. My findings suggest that the model exaggerates profits and overstates the benefits of liberalizing entry.

My results contribute to the literature on the effects of entry regulation in Italy. Empirical evidence suggests that regulation of retail in Italy is distortionary, reducing both consumer surplus and productivity. Calzolari *et al.* (2018) show that entry restrictions enable pharmacists in small Italian towns to appropriate more surplus from inelastic consumers relative to markets with more competitors. Schivardi & Viviano (2011) show that entry barriers in the Italian retail sector lead to higher profit margins and lower productivity. My contribution is to provide evidence on the effects of regulation in a different market and to assess alternative regulatory schemes. Evidence on the distortionary effects of regulation, as noted by Pozzi & Schivardi (2016), is yet insufficient to comprehensively judge the current regulatory scheme. In particular, the potential benefits of regulation are largely unknown, mostly because they involve dimensions which are hard to quantify, such as quality of life or harmonic urban development.

My findings are also methodologically relevant since I evaluate the predictive accuracy of a widely used model in empirical studies of entry. The literature combining simulations from structural models with retrospective analysis of policy reforms is small, recent and focused on the analysis of models about mergers; the relevance of retrospective strategies has been stressed by both Nevo & Whinston (2010) and Angrist & Pischke (2010). My strategy is closest to the approach in Peters (2006), who uses structural models to predict postmerger prices and then confronts these predictions with observed data from airline mergers in the United States. Björnerstedt & Verboven (2016) have a similar strategy using detailed data on a large merger between all firms in a segment of the analgesics market in Sweden. My contribution is to apply this retrospective analysis to a standard entry model. In particular, I assess the predictive power of the model in Schaumans & Verboven (2008), an adaptation of Bresnahan & Reiss (1991) to a setting with entry restrictions.

2 Institutional setting

A pharmacy in Italy can sell prescription drugs (with regulated profit margins), over-the-counter (OTC) drugs (some with regulated profit margins) and other health products (with unregulated profit margins). Pharmacies have the monopoly on the sale of prescription drugs while the remaining products can also be sold by non-pharmacies. Italians have regulated pharmacies since 1241, when Federico II in the *Costituzione di Melfi* instituted limits to the number of pharmacies and required pharmacists to be registered in professional boards. Current regulation of the Italian pharmacy market is given by law and it is composed by three instruments: **(i)** population caps, **(ii)** distance restrictions and **(iii)** minimum educational standards of pharmacy owners[1].

The property of each pharmacy is licensed exclusively to an individual with a degree in pharmacy or chemistry registered in the professional board of pharmacists (*Albo dei Farmacisti*), either alone or in partnership. In 2017, property and management were separated allowing pharmacies to be licensed to private companies provided that the store director is a registered pharmacist. Pharmacies must be at least 200 metres apart from one another.

Entry is regulated through a statutory cap on the maximum number of pharmacies in a municipality (*comune* in Italian, henceforth referred to as city) that is a function of the city's population. Before 2012, cities with a population under 12,000 could have a pharmacy every 5,000 residents, larger cities one every 4,000. For example, a city with a population of 10,000 could have at most 2 pharmacies and a city with a population of 20,000 could have at most 5 pharmacies. The legislation allowed for exceptions to this cap based on "topographic conditions" to be determined by local authorities.

In 2012 the maximum number of residents per pharmacy was reduced to 3,300 for all cities regardless of their population. Cities could also authorize additional pharmacies in train stations, airports and shopping centers. Incumbent pharmaceutical license-holders were not eligible to bid for new pharmacies. Furthermore, the reform liberalized non-price competition by eliminating restrictions on opening hours and allowing them to offer discounts on OTC drugs.

This reform was part of a broader set of measures to address the Italian debt crisis during the Monti administration. This government received parliamentary support from the majority of political parties in a time of emergency and enacted several provisions, most notably a pension reform. The cap reduction, however, was the only substantive reform in the pharmacy market during this time. The regulation on margins remained unchanged except for a 0.43% reduction for larger pharmacies resulting from an increased contribution to the national health system's budget.

Entry is not regulated for all firms in the pharmaceutical market. In particular, since 2006 non-regulated establishments known as parapharmacies are authorized to sell

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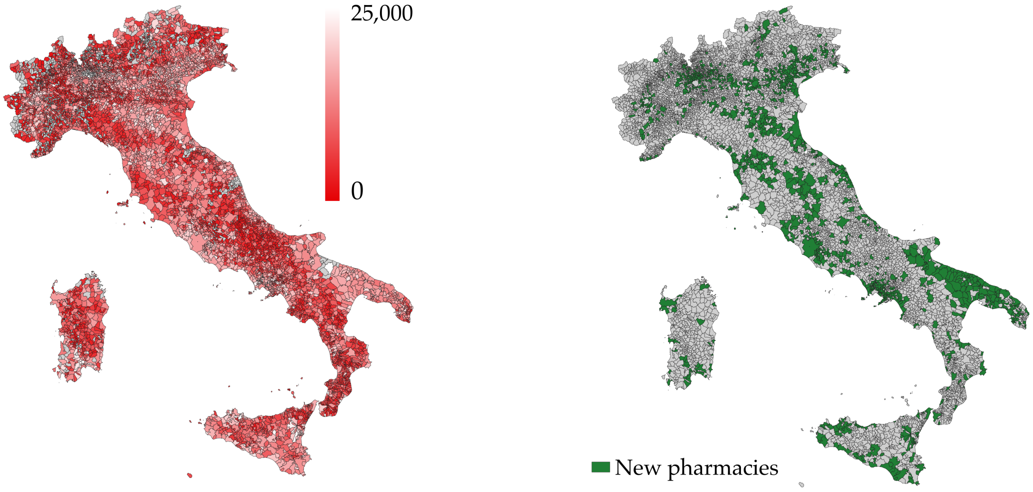
[1] This section is largely drawn from Selmin (2013) and Stagnaro (2019).

OTC drugs and other related products (such as cosmetic article s, hygiene products and veterinary drugs). The law only requires parapharmacies to hire a licensed pharmacist to oversee the sale of all drugs. The 2012 reform introduced minor changes for this sector: it allowed parapharmacies to offer discounts and it outlawed discrimination between pharmacies and parapharmacies by suppliers. Initial drafts of the reform allowed parapharmacies to sell prescription drugs not subsidized by the health system (*fascia C*, in Italian); these changes were rejected by Parliament. Parapharmacies are important competitors of traditional pharmacies, but the legal monopoly on the sale of prescription drugs limits their substitutability. Since the majority of the yearly expenditure on drugs accounts for prescription drugs, I exclude parapharmacies from my main analysis; the results are unchanged in specifications that account for the number of parapharmacies, included in the Appendix C.



(a) Population per pharmacy in 2011 **(b)** Cities with new pharmacies after 2012

Figure 1: Geographic distribution of pharmacies

The reform awarded 2,138 new pharmacy licenses in 1,343 cities, increasing the number of existing licenses in Italy by 13% (16,437 in 2011). Although only 17% of Italian cities received new licenses (shown in figure 1b), these were home to 56% of the Italian population. The reform mostly increased the maximum number of firms by one; as shown in table C.1, 74% of the 1,343 cities were awarded one additional pharmacy, 15% two, 5% 3 and the remaining 6% 4 or more.

The implementation of the reform was delegated to the regional authorities, thereby creating variation in the introduction of the reform throughout the Italian territory. I illustrate this process in figure 2. The process started with a regional publication of the full list of cities that would receive additional licenses in a later call for applications. After opening the call and receiving applications, these were assessed and ranked. Both the list of new pharmacies and the ranking of applications could be challenged and brought to court to be revised. These legal issues caused significant delays in the assignment process. Once the ranking issues were settled[2], the regions proceeded to assign pharmacies using a system of subsequent calls (*interpelli* in Italian).

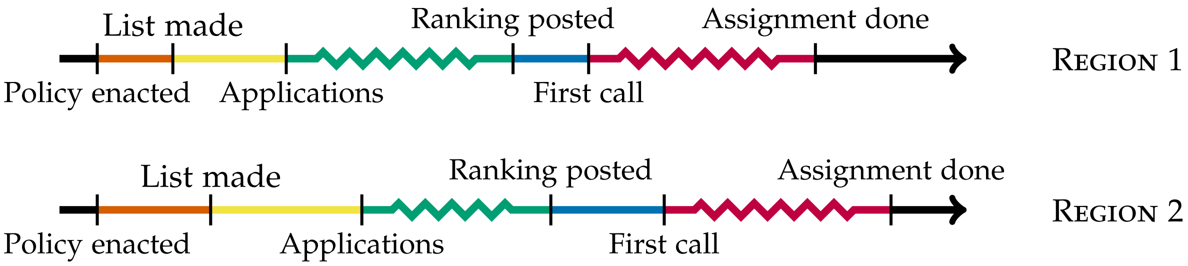


Figure 2: Policy timeline

This assignment process can be summarized as follows. Let N be the number of new licenses to be awarded in a given region. The first call summoned the N best-ranked candidates. Each candidate was asked to provide a list of ordered preferences that could contain at most as many locations as the position of the candidate in the ranking. In other words, the first candidate chose only one (since his choice is unconstrained) but the candidate at the 10th position could provide up to 10 possible alternatives. The system offered the pharmacy that was placed at the top of each candidate's preference list, among those that remain unassigned. Candidates could then accept or decline this offer. This call would give rise to the assignment of N_1 new pharmacies. Not all pharmacies will necessarily be assigned in this round for two reasons: **(i)** candidates might not provide a preference order, **(ii)** candidates might refuse the pharmacy offered to them. If at the end of this process, some licenses are still unassigned the region would implement a second call where the remaining best-ranked candidates ($N - N_1$) were summoned to choose and so forth until all pharmacies were chosen. The timing of the calls by semester is summarized in figure 3; the first call is shown in orange, subsequent ones in gray [3].

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[2] In some cases, regions proceeded with the assignment process with legal issues regarding whether the pharmacy could be installed. This was notified to applicants who chose aware of this information.

[3] Data on the content and timing of regional calls was scraped from the different regional Official Gazzettes (BUR, Italian for *Bollettino Ufficiale della Regione*) when available online. Data on the assignment of pharmacies is not available for the regions of Trentino-South Tyrol, Campania, Apulia and Sicily. Furthermore, although Tuscany carried out 7 calls data is only available online for the first one. These regions are thus excluded from the analysis.

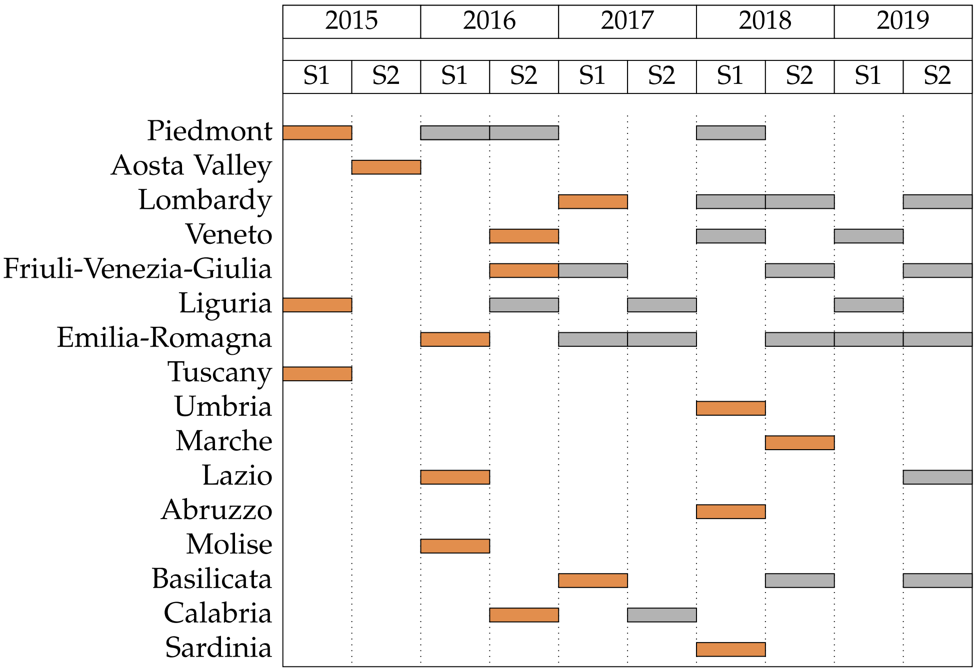

Figure 3: Timing of the calls by region and semester

3 Data

I construct the dataset used in this article by joining multiple sources. I obtain the data on the number of pharmacies on each city from the Open Data of the Italian Health Ministry. Demographic characteristics at the city level come from the 2011 Census carried out by the Italian National Statistics Institute (ISTAT). Geographical information of distance and commuting time between cities was obtained from ISTAT.

I compute the number of pharmacies in a city as the sum of the number of active licenses in that city each year. Due to legal changes, the same pharmacy can appear multiple times with different licenses. I group all the licenses that share a common address into a single pharmacy. I consider a pharmacy to be active between the earliest date and the latest date in which any of these licenses appears in the register maintained by the Ministry. I assume that the pharmacy is open without interruptions in the period of time between these dates. Section A details the construction of the market counts in detail.

1,107 of the 1,820 additional pharmacies (60%) in the regions contained in the sample were chosen in the first call[4]. 453 (25%) of all pharmacies have not been assigned[5]. Descriptive statistics for cities with available data are shown in table 1. Cities with pharmacies that were chosen in the first call are larger, with higher incomes and less unemployment than cities without additional pharmacies (i.e. non-assigned) and than cities with additional pharmacies that were not chosen in the first call.

Table 1: Descriptive statistics, by choice status (2011 values)

	Mean		Diff.	Std. Error	Obs.
	First call	**Non-chosen**			
Unemployment rate	0.0879	0.0895	-0.0017	0.0028	1077
Mean taxable income (EUR 1000's)	19.2411	17.2382	2.0029***	0.1895	1083
Outbound commuters (1000's)	4.2283	2.4383	1.7900***	0.1825	1083
Population (1000's)	16.2928	9.7924	6.5003***	0.8064	1083
Share of residents over 65	0.1871	0.2192	-0.0322***	0.0035	1083
Share of residents under 65	0.1707	0.1552	0.0155***	0.0017	1083
Share of residents with university degree	0.0846	0.0720	0.0127***	0.0017	1083

	Mean		Diff.	Std. Error	Obs.
	First call	**Non-assigned**			
Unemployment rate	0.0879	0.1026	-0.0148***	0.0027	7175
Mean taxable income (EUR 1000's)	19.2411	16.3591	2.8819***	0.1465	7195
Outbound commuters (1000's)	4.2283	0.9213	3.3070***	0.0633	7195
Population (1000's)	16.2928	3.9662	12.3266***	0.3157	7195
Share of residents over 65	0.1871	0.2165	-0.0295***	0.0023	7195
Share of residents under 65	0.1707	0.1577	0.0130***	0.0013	7195
Share of residents with university degree	0.0846	0.0678	0.0168***	0.0010	7195

4 Descriptive analysis

I provide descriptive evidence that regulation effectively protects incumbents from additional competition using an event study strategy that exploits the staggered introduction of pharmacies across cities. I define the event as the *date of the first assignment* of a pharmacy in a city. The regression specification is

$$ratio_{c,s} = \alpha + \mu_c + \gamma_s + \sum_{j=-15}^{7} \beta_j \, \mathrm{I}\{s = k_c + j\} + \sum_{j=-15}^{7} \psi_j \left(large_c \times \mathrm{I}\{s = k_c + j\} \right) + \varepsilon_{c,s}$$

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[4] Some pharmacies are chosen in multiple calls due to a posterior refusal of the assignee to start business. I consider a pharmacy to be chosen in the first round regardless of the fact that it might be also available in a later call.

[5] This might be due to refusal by applicants, lack of data on later calls (Tuscany) or because the region has not finished the call process.

where $ratio_{c,s}$ is the log ratio of population to pharmacies in city c in semester s and $large_c$ is an indicator variable that is equal to 1 if the city had a population over 12,000 in 2011. Notice that population data are only available at a yearly frequency, so within year variations are due solely to variations in the number of pharmacies. I choose to work at the semester level to recover within year responses to policy. The specification includes city (μ_c) and semester (γ_s) fixed effects. k_c is the event time at city c, i.e. the date of the first assignment of a pharmacy in city c. I exclude the indicator corresponding to $j = 0$, so all coefficients are relative to the event time. I collapse all the event times for $s \leq k_c - 15$ into a single bin. Furthermore, I include all cities that were never assigned a pharmacy (never-takers) in the sample with all their $\mathrm{I}\{s = k + j\}$ indicators set to 0 (Abraham & Sun, 2020). The coefficients of interest are β_j which measure the average change in the dependent variable j semesters relative to the first assignment of a pharmacy in cities under 12,000 and $\beta_j + \psi_j$ in cities over 12,000.

The event time is not random within a region since markets that are more profitable will be chosen before than unprofitable ones. I address this concern by estimating the regression without cities that were assigned a pharmacy after the first regional call. This reduces the relevance of strategic timing of within-region assignments due to profitability and identifies the coefficients solely through between-region variation.

The results are partly mechanical, given that entry automatically reduces the dependent variable. However, I argue that the precise amount of this reduction is informative of the degree of protection from competition that incumbents enjoy as a result of the regulation. A binding restriction before the reform might suggest that further entry could be profitable but this possibility is precluded, protecting incumbents from the threat of entry. If postreform ratios are close to the updated cap or, alternatively, if the observed reduction in the *effective* ratio is close to the reduction in the *statutory* ratio, this might suggest that further entry would be profitable even under the more lenient policy.

I exploit the variation in the treatment intensity to show that the postreform ratio always converges to the minimum statutory level. The reform lowered the population cap from $4,000$ to $3,300$ for towns with a population above 12,000 (a reduction of 17.5%) and from $5,000$ to $3,300$ (34%) for smaller towns. My specification recovers the heterogeneous effect of the policy across population categories, as shown in figure 4. Pre-reform coefficients are close to 0 for most periods, the systematic difference from 0 reflects a pre-trend that is a result of population growth. Notice that this pre-trend can only create a downward bias in the coefficients since population growth might offset the increase in pharmacies. Therefore, the estimated coefficients, if anything, underestimate the true effect.

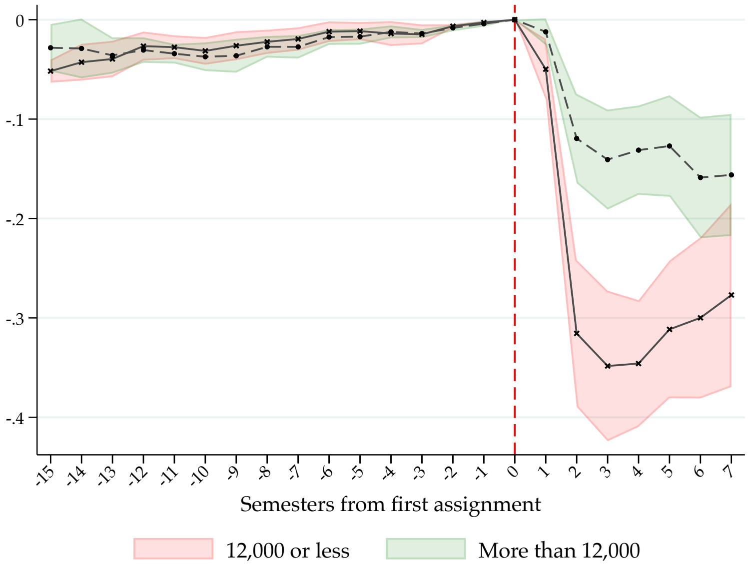


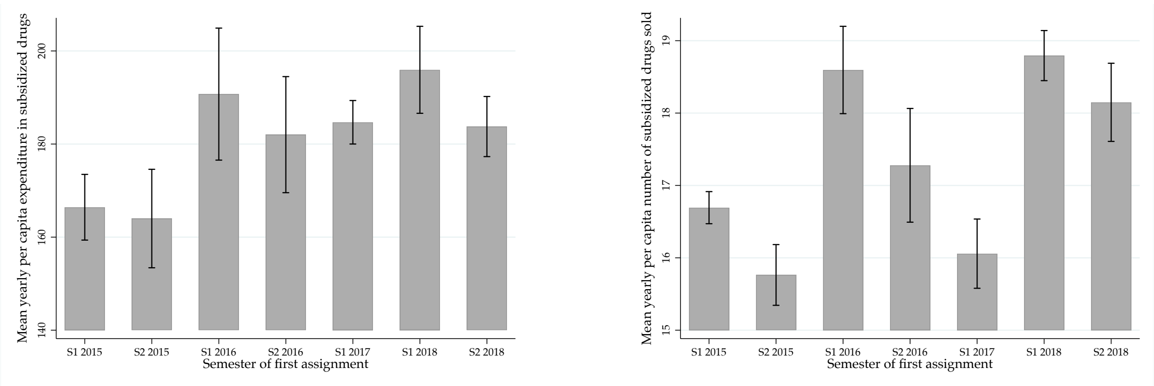
Figure 4: Event study (assigned in first call only)

Note: Plotted values are the β_j coefficients. The shaded area represents a 95% confidence interval. I exclude cities that were assigned more pharmacies in subsequent calls to avoid multiple treatments. The final sample includes $5,305$ cities and the number of observations (city-semester pairs) is $83,127$ covering the timespan from 2010 to 2018. Standard errors are robust and clustered at the province level. The specification includes city and semester fixed effects.

A year after the first assignment, the average number of population by pharmacy is reduced by more than 30% in small cities and by 15% in larger cities in the year after the first assignment; these figures are remarkably close to the statutory decrease induced by the policy. This swift reaction suggests that further entry was profitable in many markets and did not take place as a result of entry restrictions. Figure C.1 supports this interpretation: after the reform the share of affected markets with a binding entry restriction returns to its pre-reform levels, revealing the existence of profits from entry in previously regulated markets. The precision of the coefficients diminishes as j increases reflecting the smaller number of available observations since most calls take place after 2016. Results remain similar, although smaller, changing the event to the date of first assignment in *the region* and using the entire sample of cities regardless of whether they were chosen in later calls (figure C.2 in the Appendix C). The coefficients are also robust to excluding never-takers but they become less precisely estimated.

The crucial identifying assumption is that the timing of the event is random. If the baseline trend absent the assignment is different across k_c, the specification above will yield biased coefficients. The policy's rollout is arguably exogenous since it is affected mainly to bureaucratic delays that are uncorrelated with the profitability of a market in a given region. Figure 5 shows two variables[6] which are correlated with profitability: yearly per capita expenditure on subsidized drugs[7] and yearly per capita number of packs of subsidized drugs. As seen in figures 5a and 5b, there is no systematic pattern that correlates with the timing of the first call; although regions with early assignments seem to be more frugal than the remaining ones.

Furthermore, to assess whether the timing of assignments correlates with some proxies of profitability I regress a set of observables in 2011 against a time variable $diff_r$ that measures the number of semesters between the first call in Italy and the first call in region r. Results are presented in table 2 and they are all statistically not significant. The data, therefore, support the claim that the timing of the initial assignment is unrelated to the profitability of pharmacies.



(a) Per capita expenditure on drugs

(b) Per capita subsidized drug packs

Figure 5: Observables by semester of first call

Note: Reported statistics are the average value of the indicated variable throughout the period 2008-2018, where each observation is a region-year pair. Black lines indicate 95% confidence intervals. Each observation is weighted by the region's population in the corresponding year.

An additional concern is that baseline outcomes are different across regions. This could be argued for two different reasons. Firstly, regional disparities across Italy, in particular between the industrial northern regions and the less developed ones in

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[6] This information is obtained from a yearly government publication known as *L'uso dei farmaci in Italia - Rapporto OsMed* that provides aggregate data at the regional level on a number of indicators related to pharmaceutical consumption in Italy.

[7] Drugs in Italy are categorized according to their reimbursement status by the national health system. Drugs that are eligible for reimbursements (in Italian, *fascia A*) will be henceforth referred to as subsidized.

the south, have persisted across time and continue to be one of the country's main economic concerns. This might be reflected in differential outcomes absent the policy that get confounded by the event time. I address this concern by estimating the regression focusing solely on northern regions, where the assumption of homogeneity is arguably less severe. Estimates of this specification remain similar although somewhat larger in magnitude as shown in figure C.3 in the Appendix C[8].

Table 2: Regression estimates

	COEFFICIENT	STD. ERR
Total yearly expenditure on subsidized drugs	14.301	(68.432)
Total yearly expenditure on non-subsidized drugs	2.174	(24.029)
Total yearly expenditure on OTC drugs	1.793	(17.388)
Total yearly number of prescriptions of subsidized drugs	-205.058	(3337.427)
Total yearly number of packs of subsidized drugs sold	453.573	(6850.635)
Yearly per capita expenditure on subsidized drugs	3.441	(2.141)
Yearly per capita number of prescriptions of subsidized drugs	0.029	(0.188)
Mean household income	-147.773	(300.867)
Number of people with one chronic disease every 100	0.100	(0.223)
Number of people with two chronic diseases every 100	0.004	(0.254)

Note: Reported coefficients correspond to a weighted regression of the variable in the first column against diff$_r$, a variable that is equal to the difference (in semesters) between region r first call date and the first call in the entire country. The sample contains 2011 data for 16 regions. Each observation is weighted by the region's population. Standard errors are robust to heteroskedasticity.

Secondly, as it was said before, the timing of the assignment might be affected by the efficiency of the regional bureaucracy which might be heterogeneously fast in their handling of the call for applications. If the efficiency of the regional bureaucracy correlates with market profitability, then the coefficients might be biased. However, as previously argued, it is unlikely that the profitability of pharmacies in all cities are correlated with the efficiency of the regional bureaucracy. Some evidence supporting this claim is presented in figure 3 where no readily apparent pattern emerges; table 2 reinforces the idea that the rollout is uncorrelated with variables that affect profitability.

5 Model

Ideally, recovering the effect of entry on competition would amount to observing how the price-cost margins ($p_N - MC_N$, where p_N is the price and MC_N is the marginal cost with N active firms) evolve as N increases. Since margins are unobserved in my

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[8] Results are also similar restricting the sample to central or southern regions, although less precisely estimated due to the smaller number of calls in those regions.

data, I use the structural model proposed by Bresnahan & Reiss (1991) in order to estimate firms' profits and analyze the competitive effects of entry.

Let an individual firm's profits in a market with N homogeneous firms be given by

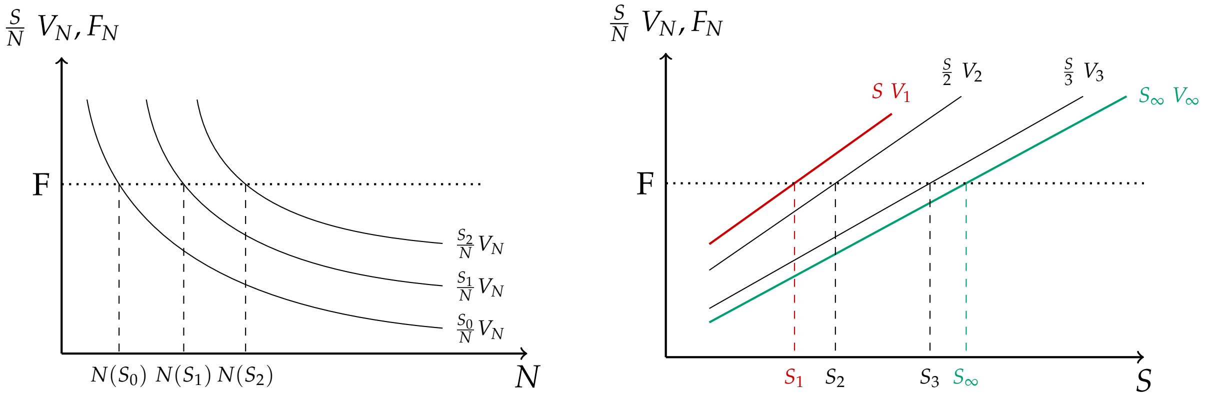
$$\Pi_N = \frac{S}{N} V_N - F_N$$

where S denotes market size, V_N denotes income derived from a representative consumer, F_N denotes fixed costs. It is useful to think of $V_N(\cdot)$ as the average profit per customer, i.e. $V_N = [p_N - AVC_N]\, d_N$ where AVC_N are average variable costs and d_N is the demand level of the representative consumer. The firm breaks even when

$$\Pi_N = \frac{S}{N} V_N - F_N = 0 \Rightarrow \frac{S}{N} \equiv s_N = \frac{F_N}{V_N} = \frac{F_N}{[p_N - AVC_N]\, d_N}$$

This magnitude is the *per firm minimum market size*, which equals the ratio of fixed costs to variable profits per customer. As the number of firms in a market increases, standard oligopoly models predict a fall in the price-cost margin due to increased competition until this margin converges to perfectly competitive levels. A graphical intuition is provided in figure 6b. For any market size S, monopolists, *ceteris paribus*, can extract the most surplus from customers; monopoly variable profits (in red) are the highest possible (relative to other market configurations). As N increases $p_N - AVC_N$ diminishes, shifting the variable profit curve downwards until it converges to the perfectly competitive curve (in green). Assuming constant fixed costs, the minimum market size per firm (s_N) increases as a result of the change in conduct induced by the larger number of incumbents.

This is the main intuition behind the use of minimum per firm market sizes as a measure of the competitive effects of entry. Furthermore, denote the minimum efficient scale as $s_\infty = \lim_{N \to \infty} \frac{S_N}{N}$. A scale-free measure of the compettive effects of entry is given by the evolution of the ratio $\frac{s_\infty}{s_N}$ as N varies. Intuitively, this statistic measures the percentage increase in the minimum scale of a competitive firm relative to that of a firm in an oligopoly with N firms. For instance, $\frac{s_\infty}{s_1}$ measures the percentage increase in customers a firm should have to break even in perfect competition relative to the amount it would need if it were a monopolist. If this statistic decreases with N, margins plausibly decrease with entry since firms are forced to sell more quantities in order to cover their fixed costs. Observing that the ratio converges to 1 suggests that further entry has no discernible effects on competitive conduct.



(a) Equilibrium number of firms

(b) Minimum market size

Figure 6: Entry model: illustration of equilibrium

This interpretation must be nuanced by the fact that entry thresholds only measure *changes* in competitive conduct instead of levels of effective competition. Consider the case in which entrants join a collusive agreement. Clearly, since margins would remain unchanged the per firm market size would remain unchanged. Thus, entry thresholds trivially converge to 1 although the market is not actually competitive.

5.1 Estimation

I empirically estimate entry thresholds by imposing a functional form to firm's profits composed by three sets of variables: market size S, variable profits V_N and fixed costs F_N. I assume that profits of a given firm in market m with N active firms are given by

$$\Pi_{m,N} = S(Y_m)V_N(Z_m) - F_N(W_m) - \varepsilon_m$$

where $S(Y)$ is a function of a vector of observables Y that proxies for market size, Z is a vector of profit shifters and W is a vector of cost shifters. ε is the unobserved component of profits assumed to be distributed as a standard normal, identical and independently distributed across markets. Furthermore, the unobserved component of profits is assumed to be orthogonal to the observables Y, W and Z.

Market size is proxied by the following equation

$$S(Y_m) = pop_m + \lambda_1 pos_m + \lambda_2 neg_m + \lambda_3 comm_m$$

where pop_m is the population of city m in thousands, pos_m and neg_m are indicator variables that are equal to 1 if the city experienced a positive or negative population growth, respectively, between 2005 and 2011 and $comm_m$ is the number of daily outbound commuters of city m in thousands. The unit coefficient on the population variable allows to translate units of market demand into units of population. The growth variables capture entrants' expectations and lagged responses to past growth. Including the number of outbound commuters allows for consumers to acquire goods outside of the city.

Variable profits are proxied by the following equation

$$V_N(Z_m) = \alpha_1 + \beta_1 inc_m + \beta_2 old_m + \beta_3 young_m + \beta_4 unemp_m - \sum_{j=2}^{N} \alpha_{j,m}$$

where inc_m is mean taxable income, old_m and $young_m$ are the share of residents over 65 and under 18, respectively, and $unemp_m$ is the unemployment rate. Notice that $V_1 = \alpha_1 + \beta_1 inc_m + \beta_2 old_m + \beta_3 young_m + \beta_4 unemp_m$ equals the per customer variable profits of a monopolist firm. The fixed effects $a_{j,m}$ indicate whether city m has up to j active firms. Thus, $\alpha_{j,m} = V_{j,m} - V_{j-1,m}$ represents the change in variable profits when the j-th firm enters market m; this approach recovers the gradient of variable profits as a function of market structure non-parametrically.

Finally, fixed costs are proxied by

$$F_N(W_m) = \gamma_1 + \gamma_n north_m + \sum_{j=2}^{N} \gamma_{j,m}$$

where $north_m$ is an indicator variable if the city is located in the northern regions and the $\gamma_{j,m}$ terms allow later entrants to have different fixed costs.

The model is estimated using an equilibrium condition on the observed number of firms. If entry restrictions are not binding, we observe N firms in market m if and only if

$$\Pi_{m,N} \geq 0 \quad \text{and} \quad \Pi_{m,N+1} < 0$$

which represent the intuitive conditions that N is an equilibrium if **(i)** entrants are best responding ($\Pi_N \geq 0$, i.e. entrants have non-negative profits) and **(ii)** non-entrants are best responding ($\Pi_{N+1} < 0$, i.e. non-entrants have negative profits)[9]. Using the functional form for profits leads to the condition

$$S(Y_m)V_N(Z_m) - F_N(W_m) - \varepsilon_m \geq 0 \quad \text{and} \quad S(Y_m)V_{N+1}(Z_m) - F_{N+1}(W_m) - \varepsilon_m < 0$$

from where it follows that the probability of observing n firms in a market with unrestricted entry ($e = 0$) is given by

$$P(N = n, e = 0) = \begin{cases} 1 - \Phi\left[S(Y_m)V_1(Z_m) - F_1(W_m)\right] & \text{if } n = 0 \\ \Phi\left[S(Y_m)V_N(Z_m) - F_N(W_m)\right] - \Phi\left[S(Y_m)V_{N+1}(Z_m) - F_{N+1}(W_m)\right] & \text{otherwise} \end{cases}$$

---

[9] I assume that the outside option (profits from not entering) is 0.

where $\Phi(\cdot)$ denotes the standard normal cumulative distribution function.

The presence of entry restrictions introduces a different equilibrium condition for markets where the restriction is binding. As noted by Schaumans & Verboven (2008), under binding entry restrictions it is no longer possible to infer a lower bound on profits from a particular market configuration. Thus, with binding entry restrictions $\bar{N}$ is an equilibrium if and only if

$$\Pi_{\bar{N}} \geq 0 \Rightarrow S(Y_m)V_{\bar{N}}(Z_m) - F_{\bar{N}}(W_m) - \varepsilon_m \geq 0$$

so the probability of observing n firms in a market with restricted entry is

$$P(N = n, e = 1) = \begin{cases} 1 - \Phi\left[S(Y_m)V_1(Z_m) - F_1(W_m)\right] & \text{if } n = 0 \\ \Phi\left[S(Y_m)V_N(Z_m) - F_N(W_m)\right] & \text{otherwise} \end{cases}$$

Using these results, the likelihood contribution of a given market is given by

$$l(n_m, e_m) = (1 - e_m)P(N = n_m, e_m = 0) + e_m\left[P(N = n_m, e_m = 1)\right]$$

so the likelihood function becomes

$$L(\alpha, \beta, \gamma, \lambda) = \prod_{m=1}^{M} l(n_m, e_m, \alpha, \beta, \gamma, \lambda)$$

I estimate the parameters of interest $\theta = \{\alpha, \beta, \gamma, \lambda\}$ by maximum likelihood. I impose two constraints on the parameters, namely **(i)** $\alpha_j \geq 0$ and **(ii)** $\gamma_j \geq 0$ for all $j \geq 2$. This ensures that later entrants have lower profits than monopolists given the same market size. I also collapse all cities with 5 pharmacies or more into a single residual category, so $N \leq 5$. Coefficients α_5 and γ_5 measure the average variation in profits and costs across cities with different market structures.

Available pharmacies in the call for applications can be of two types: new or vacant. New pharmacies are those available as a result of the change in the cap. I consider entry in 2011 to be restricted in a city if **(i)** the city is featured on the list of *new* pharmacies in the reform or **(ii)** the city is not featured on the list of cities in the call for applications (either new or vacant). I consider entry in 2011 to be free if **(i)** the city has no pharmacies or **(ii)** the city is featured on the list of vacant pharmacies.

Following Bresnahan & Reiss (1991), I restrict the sample in order to reduce the problem of overlapping markets. I adopt the criterion proposed by and Schaumans & Verboven (2008) who exclude urban towns. These are defined as towns that have **(i)** a population density of more than 800 per km^2 or **(ii)** a population of over 15,000. Table 3 presents summary statistics of this sample. This approach differs from than that in Bresnahan & Reiss (1991) who study isolated towns. Due to the characteristics of European cities, considering isolated towns would introduce significant sample selection in order to avoid the problem of overlapping markets. However, the Appendix C shows results using isolated towns. Namely, I consider cities that **(i)** are at least 10 kilometers from the nearest city of 1,000 people or more and **(ii)** at least 60 kilometers from the nearest city of 100,000 people or more and **(iii)** have a population under 15,000. These towns are presented in figure C.4 and their summary statistics are shown in table C.2.

Table 3: Summary statistics

	MEAN	STD. DEV.	50%	25%	75%	MAX	MIN
Number of pharmacies	1.14	0.77	1.00	1.00	1.00	8.00	0.00
Unemployment rate	0.10	0.06	0.08	0.06	0.13	0.42	0.01
Binding entry restriction	0.84	0.36	1.00	1.00	1.00	1.00	0.00
Mean taxable income (in 1000's)	16.33	3.35	16.56	13.72	18.64	36.62	7.14
Positive population growth (2005-2011)	0.51	0.50	–	–	–	–	–
Negative population growth (2005-2011)	0.49	0.50	–	–	–	–	–
Number of daily outbound commuters (in 1000's)	0.82	0.87	0.50	0.23	1.08	5.53	0.00
Total population (in 1000's)	3.10	3.12	1.96	0.90	4.16	14.99	0.05
Share of residents over 65	0.22	0.06	0.21	0.18	0.25	0.63	0.05
Share of residents under 18	0.16	0.03	0.16	0.14	0.18	0.27	0.00
Share of residents with university degree	0.07	0.02	0.06	0.05	0.08	0.27	0.01
Population density	147.88	160.28	87.26	40.15	191.93	799.23	1.04
Northern region	0.65	0.48	–	–	–	–	–
Central region	0.12	0.32	–	–	–	–	–
Southern region	0.27	0.44	–	–	–	–	–
Observations	6703						

6 Results

Table 4 presents the main results. The estimated coefficients have the expected signs: profits are increasing in mean taxable income, the share of elderly residents and decreasing in the unemployment rate and the share of young residents. The coefficient on the number of outbound commuters is negative, suggesting that individuals buy in other markets than those where they live. Costs are larger in the north, reflecting higher labor costs and real estate prices. Most coefficients retain their sign when incorporating entry restrictions to the likelihood function.

Table 4: Parameters of the entry model

	No entry restrictions		With entry restrictions	
	Coef	Std. Err.	Coef.	Std. Err.
Positive growth	0.0329	(0.0382)	-0.0857	(0.0709)
Negative growth	0.1660	(0.0338)	0.0797	–
Commuters	-0.9197	(0.0225)	-1.1547	(0.0621)
Mean taxable income	-0.0053	(0.0016)	0.0003	(0.0005)
Share of residents over 65	1.8442	(0.0736)	1.9581	(0.4091)
Share of residents under 18	-0.4884	(0.0568)	-0.4737	(0.4690)
Unemployment rate	-0.4760	–	-0.3978	(0.2322)
Northern region	0.3829	(0.0448)	0.5232	(0.0762)
α_1	2.176	–	2.4866	(0.2057)
α_2	1.7371	(0.0514)	1.1647	(0.0976)
α_3	0.1653	(0.0171)	0.5724	(0.1344)
α_4	0.1080	(0.0221)	0.5644	(0.1208)
α_5	0.0700	(0.0332)	0.0729	(0.0808)
γ_1	0.8003	(0.1004)	0.4845	(0.2038)
γ_2	1.5575	(0.0879)	0.9232	(0.1663)
γ_3	0.8087	(0.0867)	1.2342	(1.3066)
γ_4	0.5663	(0.1676)	0.5884	(0.6343)
γ_5	0.2855	(0.2818)	0.2816	(0.5455)
Log-likelihood	-3873.761		-1640.986	
Observations	6703		6703	

Note: Asymptotic standard errors computed using the inverted Hessian. A missing (–) standard error implies that the element in the diagonal of the inverted Hessian is negative.

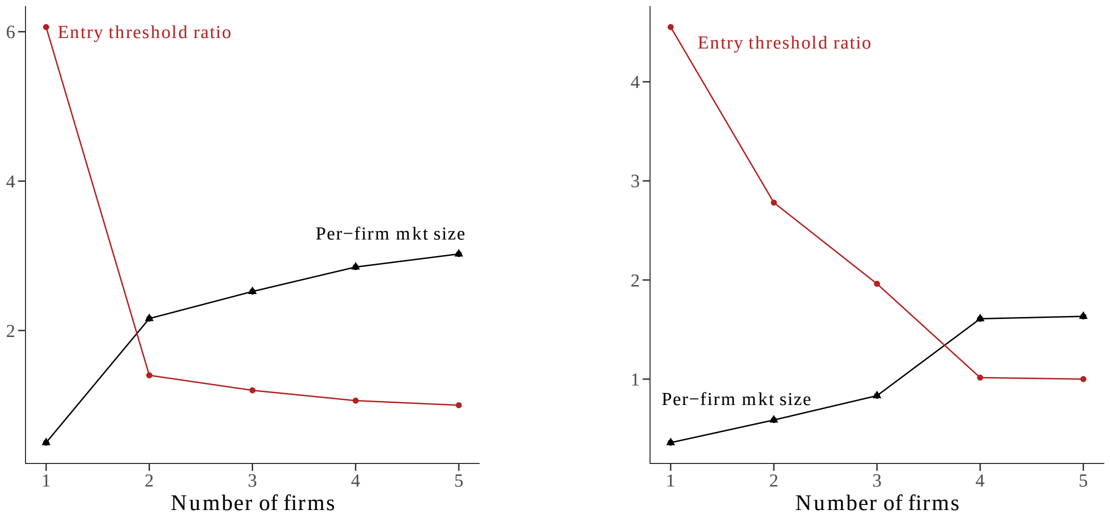
The α_j coefficients are precisely estimated in both specifications. Interestingly, the reduction of profits because of entry is larger in the model that accounts for entry restrictions. The cost shifters γ_j are imprecisely estimated and they are less robust across specifications. As expected, the minimum market sizes are smaller in specifications that account for entry restrictions. As seen in figure 7, the minimum per-firm market size is roughly halved in the model with entry restrictions relative to the model without restrictions. These estimates suggest that a pharmacy breaks even with less than 2,000 customers even in markets with 5 or more pharmacies, requiring a scale under 1,000 in concentrated markets, i.e. with less than 3 firms. This evidence supports the interpretation of the reduced-form estimates discussed in section 4, since the postreform cap is noticeably higher than even the most demanding minimum scales.

Furthermore, entry thresholds converge after the entry of the second firm in the model without entry restrictions and after the entry of the fourth firm in the specification accounting for entry restrictions. Entry restrictions, thus, might be more disruptive for small towns with less pharmacists; in larger cities conduct is less likely to change as a result of entry.

Table 5: Entry thresholds

	Entry thresholds					Ratios			
	S_1	S_2	S_3	S_4	S_5	$\frac{s_2}{s_1}$	$\frac{s_3}{s_2}$	$\frac{s_4}{s_3}$	$\frac{s_5}{s_4}$
No entry restrictions	499	4,320	7,567	11,399	15,122	4.33	1.17	1.13	1.06
With entry restrictions	359	1,174	2,498	6,436	8,165	1.64	1.42	1.93	1.01

Note: Entry thresholds are computed at the mean of the observables included in the profit function, except for indicator variables which are set to specific values: thresholds correspond to northern regions with positive population growth.



(a) No entry restrictions **(b)** With entry restrictions

Figure 7: Entry thresholds

Estimates obtained using the sample of isolated towns are presented in table C.3; coefficients are imprecisely estimated and some entry parameters (α_j and γ_j) violate the non-negativity constraint and are set to 0. The coefficients lead to implausibly low entry thresholds shown in figure C.4. I interpret these issues as a consequence of major sample selection as a result of focusing only on poorer and less populated towns, as shown in table C.2.

These results are robust to changes in the use of five or more firms as a residual category. Figure C.6 presents the evolution of entry thresholds for two alternative residual categories (4 or more and 6 or more) using the model with entry restrictions. For the latter, however, since only 4 cities in the benchmark sample have 6 or more pharmacies I re-estimate the model using a larger sample that includes cities with a population under 25,000 and a density below 800 people per square kilometer. Despite this change of sample, the decreasing entry thresholds and the size of the minimum per-firm market size of figure C.6b are similar to the benchmark model.

6.1 Policy reform

The previous estimates can be used to evaluate counterfactual regulations. Entry predictions are obtained by taking, for each market, 5,000 draws from a standard normal distribution. These shocks are then included in the profit function characterized by city-specific data and the coefficients in table 4. For each market and each shock, I compute the predicted number of firms in a market as the maximum n such that profits (i.e. $S \times V_n(\cdot) - F_n(\cdot) - \varepsilon$) are positive. I then evaluate alternative regulatory schemes by adjusting the maximum allowed number of pharmacies in a particular market. I detail the simulation procedure in section B.

Some markets are profitable in my model although they do not have a pharmacy. Any comparison between the counterfactual distribution and the effective data would pick up this effect although it is unrelated to the policy change. To ensure within-model comparisons, the benchmark case is not the effective number of pharmacies in a city but the *predicted* one under the 2011 regulatory scheme. The model predicts the *status quo* outcomes correctly in 90% of the sample; figure C.7 in the Appendix shows that the benchmark distribution closely follows the effective one.

I compute the predicted number of firms in a city *in a setting without entry restrictions* as the maximum number of firms with positive profits. In the *status quo*, the number of pharmacies in the sample equals 7,910. Eliminating entry regulations would increase this by over 60%, up to 12,892. Entry restrictions, thus, are economically relevant although their effect is smaller than in Schaumans & Verboven (2008), who find that fully deregulating entry in Belgium increases the stock of pharmacies by 173%. The distribution of towns according to their counterfactual number of pharmacies is shown in table 6. Full liberalization would decrease the number of markets with only one firm by more than 130%.

Figure 8 shows the effects of deregulation for different population sizes. Free entry increases the availability of pharmacies in 56% of the cities in the sample. Predictions are similar across income levels and across regions (figure C.8). Although the model is silent on welfare implications, the large decrease in the number of monopoly markets suggests that revising the current regulation might provide substantial benefits to consumers.

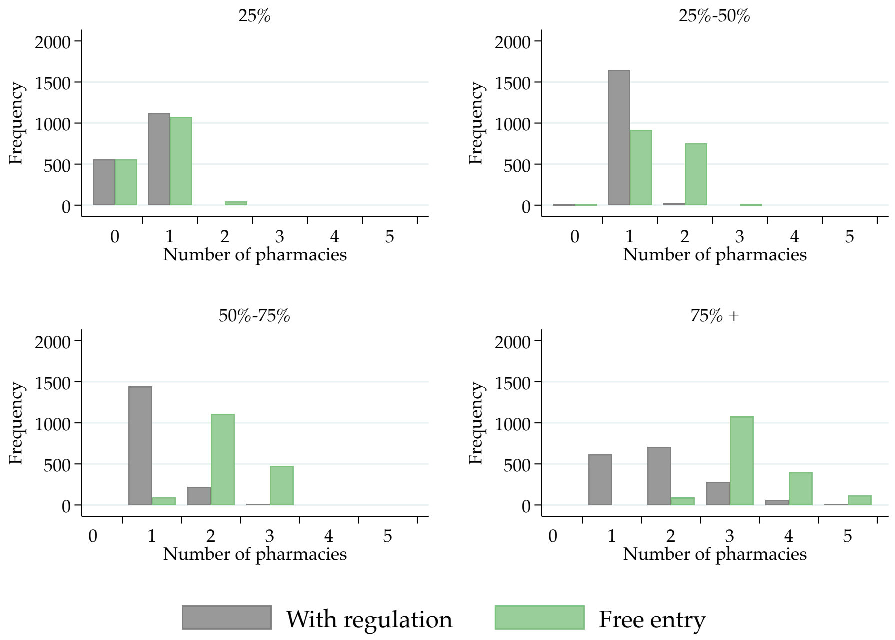


Figure 8: Distribution of market configurations, by population quartiles

Table 6: Baseline and counterfactual market configurations (absolute frequencies)

Without entry restrictions	With cap at 3,300						
	0	**1**	**2**	**3**	**4**	**5+**	**Total**
0	562	0	0	0	0	0	562
1	0	2082	0	0	0	0	2082
2	0	1858	136	0	0	0	1994
3	0	850	596	107	0	0	1553
4	0	30	202	136	29	0	397
5	0	5	15	52	29	14	115
Total	562	4825	949	295	58	14	6703

6.2 Validation

I validate the model by simulating the 2012 reform and confronting my out-of-sample predictions with the effective outcomes observed in 2019. I exclude regions where I do not have assignment data (Trentino-South Tyrol, Campania, Apulia and Sicily). Additionally, some cities in the sample no longer exist in 2019 due to administrative mergers with other cities. Therefore, the validation sample is smaller than the full sample and contains 4,691 markets. I simulate outcomes using updated observables in the profit function except for the unemployment rate and the number of outbound commuters due to data restrictions. I compute the updated cap on pharmacies as the sum of the number of pharmacies in 2011 and the number of new licenses awarded to the city.

My out-of-sample forecast of the total number of pharmacies in the sample is 6,365, largely in line with the *effective* postreform magnitude (6,493). Relative to 2011, the number of pharmacies in the sample increases by 447; the out-of-sample predicted effect of the policy is 453. The fit worsens among the subsample of affected markets, i.e. those that received at least an additional pharmacy: I predict an increase of around 442 relative to the observed increase of 237. My model correctly predicts the postreform number of pharmacies in 49% of the affected markets. Table 7 shows that I systematically overestimate the number of pharmacies. In particular, the model predicts a disappearance of monopolies altogether in affected cities; the number of monopoly markets more than halves but remains slightly over 100. Figure 9 compares the distribution of market configurations using three different criteria; notice that a city may change its market configuration under alternative criteria. The model's fit is lower in southern markets and cities with lower incomes and higher unemployment, as shown in table C.5.

Table 7: Predicted and effective market configurations (absolute frequencies)

Effective	Predicted 2019					
2019	**1**	**2**	**3**	**4**	**5**	**Total**
1	1	71	1	0	0	73
2	1	95	81	7	0	184
3	0	4	77	36	0	117
4	0	0	6	36	5	47
5	0	0	0	2	0	2
Total	2	170	165	81	5	423

My simulation assumes that cities that did not receive additional licenses in the initial reform do not update their caps. This explains why the model underpredicts in the entire sample. Clearly, caps are adjusted due to population growth; 7% of non-assigned cities increase the number of pharmacies between 2011 and 2019. I do not introduce these updates because I do not have data on these modifications and I can only infer them through the effective number of pharmacies in 2019. This approach, however, is not ideal for my validation exercise since the resulting updated counterfactual would be a function of the comparison outcome.

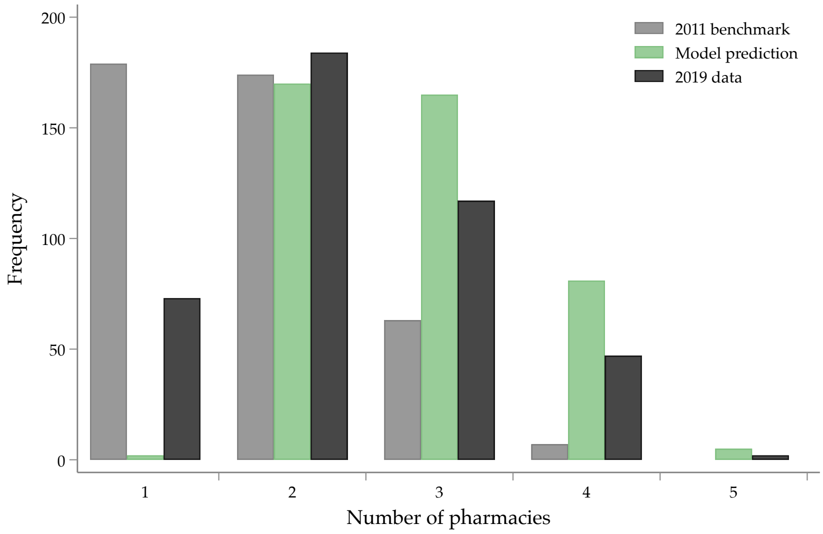


Figure 9: Distribution of market configurations in affected markets

I analyze two dimensions that might explain my predictive errors. The first one is the exclusion of parapharmacies: if parapharmacies are good substitutes of pharmacies, they should be included in the effective number in 2019. For instance, a predicted duopoly might be composed by one pharmacy and one parapharmacy. Excluding the latter from the analysis might create disparities between the model's predictions and the effective number of firms in a market. The performance of the model is not affected by the number of parapharmacies in a city. Including parapharmacies in the number of establishments in 2019 does not bridge the gap between the model's prediction and the number of establishments in a city, as seen in figure C.9 and table C.6. This suggests that the two types of firms are imperfect substitutes, likely due to the legal monopoly of the sale of prescription drugs as noted in section 2. The model in section 5 can be extended to account for strategic interaction among firms of different types, as done in Schaumans & Verboven (2008) who study the interaction between pharmacists and physicians.

Additionally, I study if the model's accuracy depends on the toughness of competition within a market. I measure toughness as the proximity of each firm to other pharmacies and parapharmacies. I compute the number of stores in a range of 500 and 200 metres for each pharmacy and I average this indicator at the city level. Figure C.10 presents the counterfactual distribution of market configurations according to the average number of stores within 200 metres around a pharmacy; the general pattern is unchanged throughout the different categories. The share of correctly predicted cities remains similar across markets with different levels of local competition, measured using two different values for the radius (table C.7).

The results show that the model overstates firms' profits, thus predicting a higher amount of firms than the effective one after the reform. This might be a result of the assumption that entry will take place as long as profits are positive. However, pharmacists may be in short supply and able to choose where to set up shop. For instance, liberalizing entry in relatively more profitable markets might shift firms away from other cities. Even if markets were equally profitable, an inelastic supply may arise if pharmacists have strong location preferences and do not move to small towns to start a business. In my counterfactual, there is an infinite supply of pharmacists and the only limits to entry are a given city's profitability and its regulatory status. Therefore, no city loses coverage as a result of free entry in my counterfactual scenario. A possible approach to incorporate losses into the model could be to combine entry liberalization with markup reductions as in Schaumans & Verboven (2008). I do not assess such instruments since the Italian subsidy scheme is not uniform but conditional on the sales of each pharmacy.

7 Conclusion

Restricting entry in the Italian pharmacy market has substantial economic effects. It reduces the number of establishments by around 60% and more than doubles the number of monopoly markets. Although these results overstate the distortionary effects of regulation, the velocity at which markets become restricted again after increasing the number of allowed pharmacies suggests that many incumbent firms are effectively shielded from additional competition. I do not assess the potential benefits from regulation but my results imply that these can only justify the policy if they are sufficiently large to offset the policy's impact on market power.

These findings are relevant for policy given that many European countries are discussing reforms to their entry regulation policies. I provide estimates that emphasize the potential benefits from deregulation. My findings also are relevant to examine the consequences of new platforms that are close substitutes to highly regulated services, such as Uber vs. taxi services or Amazon vs. traditional retail stores. The development of unregulated alternatives to traditional regulated sectors is analogous to a reduction in entry restrictions.

My results are also methodologically relevant since I provide an assessment of a routinely used structural model to study entry and predict the effects of policy reforms, as in Schaumans & Verboven (2008) who study liberalization in the Belgian pharmacy market and Grant *et al.* (2019) who simulate reforms in the German long-term care market. I show that the model fits the data moderately in my application since it overestimates firms' profits. This simple model, however, is able to predict half of the postreform outcomes and its general prediction is largely in line with the postreform outcomes. The model's shortcomings might be overcome with additional data that improve the specification of the profit function. It is therefore important to retrospectively study other reforms to disentangle the weaknesses of my data from those of the model.

My analysis is limited by the lack of information at the pharmacy level. Incorporating micro-level data on prices and quantities would strengthen the reduced-form strategy significantly by expanding it to study the reform's effect on additional competitive outcomes. The structural model could also be extended to recover strategic interactions between pharmacies and parapharmacies or between pharmacies and physicians using the strategy in Schaumans & Verboven (2008). Additionally, micro-level data can be incorporated to the model to accomodate heterogeneity across firms following Berry (1992).

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Appendix A Data

This section details the procedure for constructing the datasets on pharmacies and new license assignments.

A.1 Pharmacies

The initial dataset contains information at the license level, indicating an identification number, the associated address and the start and end dates for the validity of the license. Due to legal changes, the same pharmacy might change its tax identification number or their license registration. I collapse different licenses into a single pharmacy to eliminate administrative modifications on the same pharmacy. This leads to a dataset where each row is a unique identification number-address pair.

I match all licenses with different identification numbers that correspond to the same address in the same city into a single pharmacy. I match small differences in addresses using the `strgroup` command in STATA that matches strings on their Levenshtein edit distance with a threshold of 0.25. I match pharmacies with identical identification numbers but different addresses if the end date of the earliest one coincides with the start date of the latest one. When there is overlap in these dates, I select the pharmacy with the longest tenure in the market and I delete the younger license. After this procedure, I eliminate duplicates of identification numbers, start and end dates. I delete licenses with the same identification number but located in different cities.

A.2 Call data

The dataset on the assignment process of new pharmacies after the reform is scraped from the online Regional Gazzettes `pdf` files containing the list of new licenses and the ranking (*graduatoria*, in Italian). Each region has different data structures; the data cleaning procedure is thus tailored at the regional level. The STATA code for constructing this dataset is available upon request.

Appendix B Simulation

The counterfactual market configuration under free entry is computed using the following algorithm:

1. Draw, for each market, a realization of a standard normal distribution.
2. Evaluate the profit function using the estimated parameters and the previous draw for $N = 1$. If profits are positive, evaluate the profit function in $N + 1$.
3. The equilibrium market configuration is the last N such that profits are non-negative.
4. Iterate this procedure 5,000 times and take the average of the number of firms across all draws as the predicted market configuration.

The benchmark prediction is computed in the same way but imposing that N cannot be larger than the legal cap computed as the ratio of population to the minimum statutory level depending on the city's population.

The out-of-sample prediction is computed in the same way but updating the maximum allowed number of pharmacies in the cities that received a new license to be equal to the sum of the existing pharmacies in 2011 and the number of additional licenses. I assume cities that did not receive an additional pharmacy in 2012 have the same cap as in 2011.

Appendix C Additional tables and figures

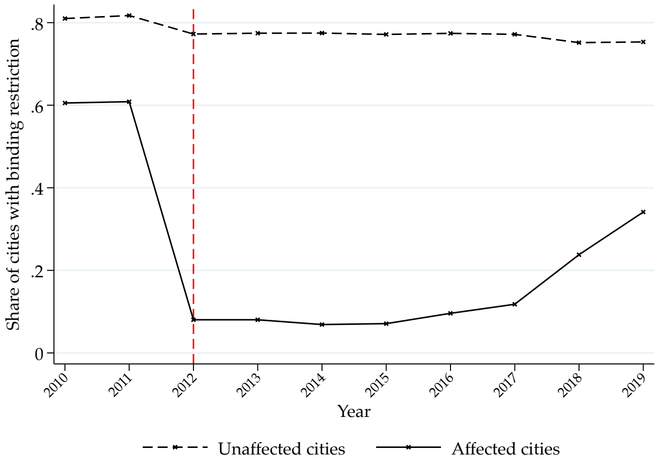
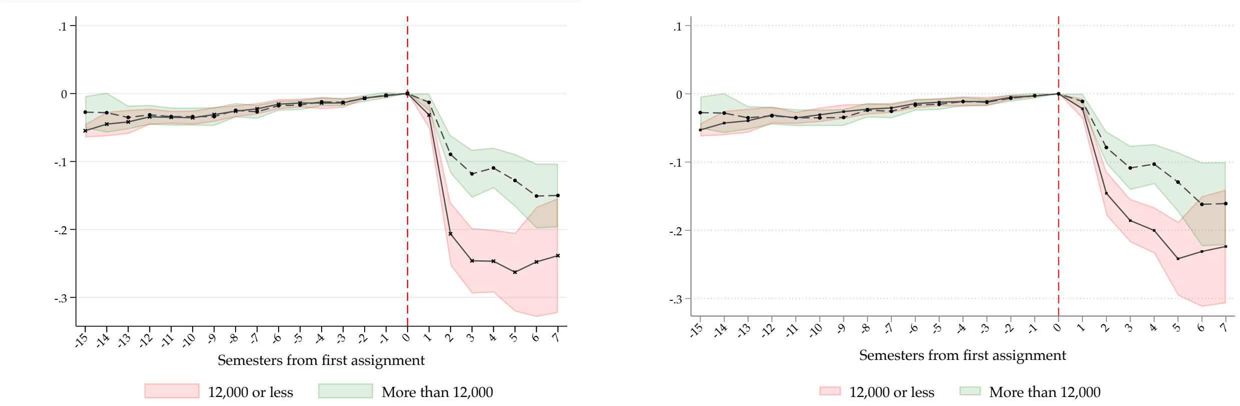


Figure C.1: Share of markets with binding entry restrictions



(a) First call cities, with multiple treatments

(b) All call cities

Figure C.2: Event study, alternative samples

Note: Plotted values are the β_j coefficients. The shaded area represents a 95% confidence interval. The sample in panel (a) includes $5,566$ cities and the number of observations (city-semester pairs) is $87,221$ covering the timespan from 2010 to 2018. The sample in panel (b) includes 5722 cities and the number of observations (city-semester pairs) is $89,644$ covering the timespan from 2010 to 2018. The event time in panel (b) is defined as the date of first assignment of a pharmacy in the entire region, instead of in the city. Standard errors are robust and clustered at the province level. The specification includes city and semester fixed effects.

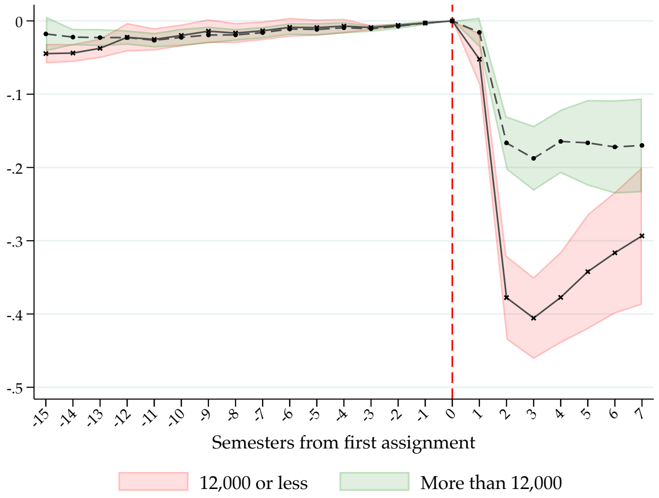


Figure C.3: Event study, northern regions

Note: Plotted values are the β_j coefficients. The shaded area represents a 95% confidence interval. I exclude cities that were assigned more pharmacies in subsequent calls to avoid multiple treatments. The sample includes 3, 686 cities and the number of observations (city-semester pairs) is 57, 776 covering the timespan from 2010 to 2018. Standard errors are robust and clustered at the province level. The specification includes city and semester fixed effects.

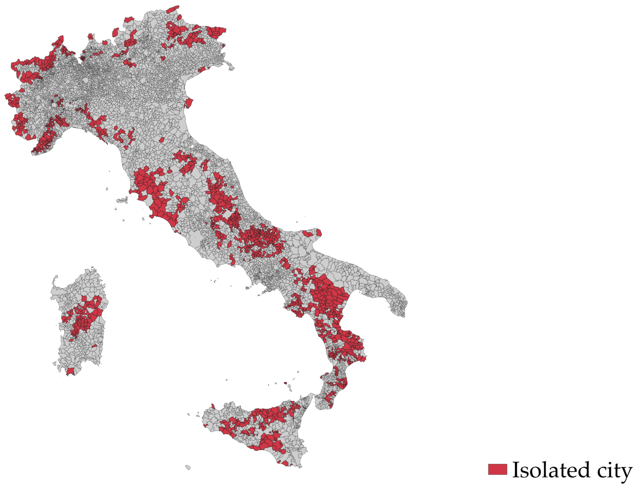
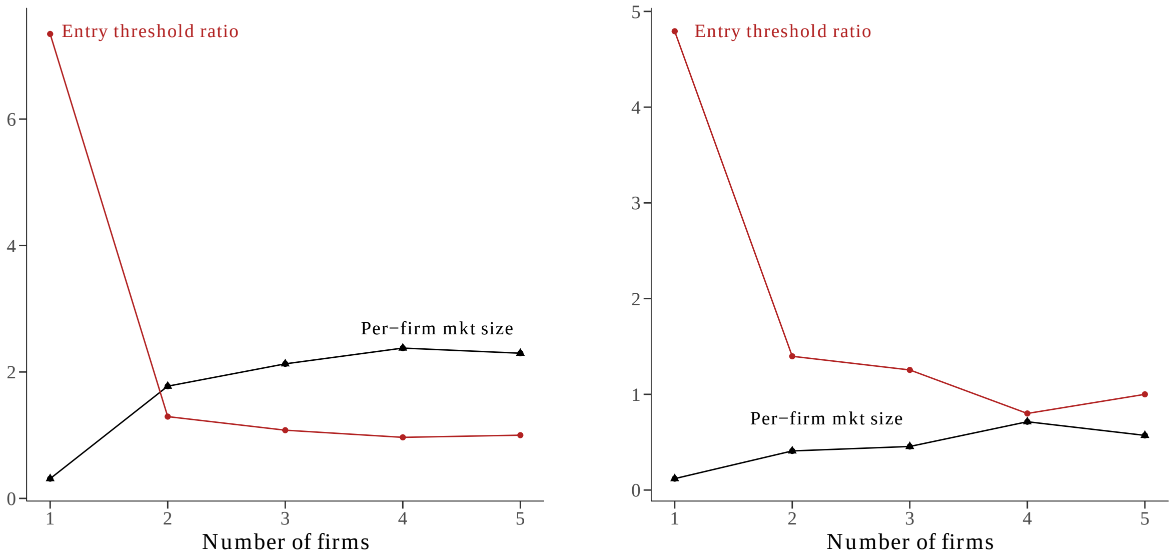
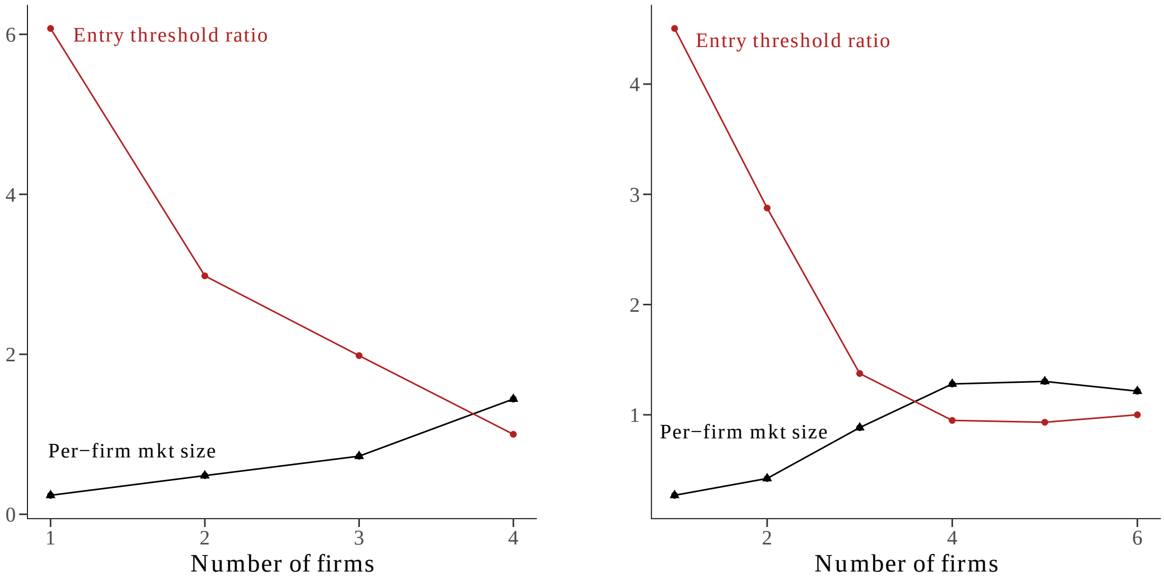


Figure C.4: Isolated cities



(a) No entry restrictions **(b)** With entry restrictions

Figure C.5: Entry thresholds (isolated towns)



(a) Residual category is 4+ **(b)** Residual category is 6+

Figure C.6: Robustness to residual category definition

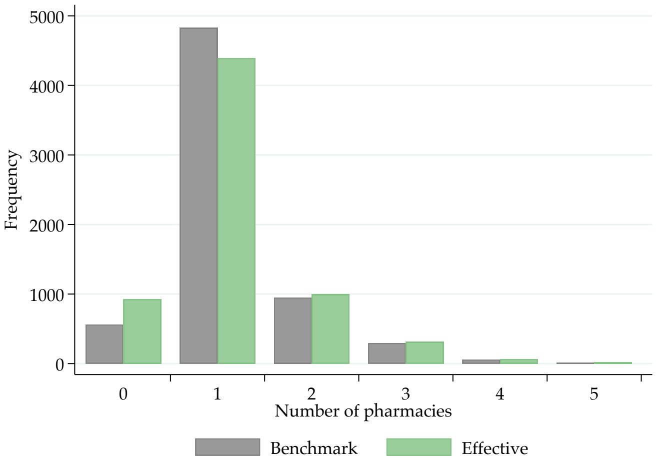


Figure C.7: Comparison of market configurations across effective data and benchmark prediction

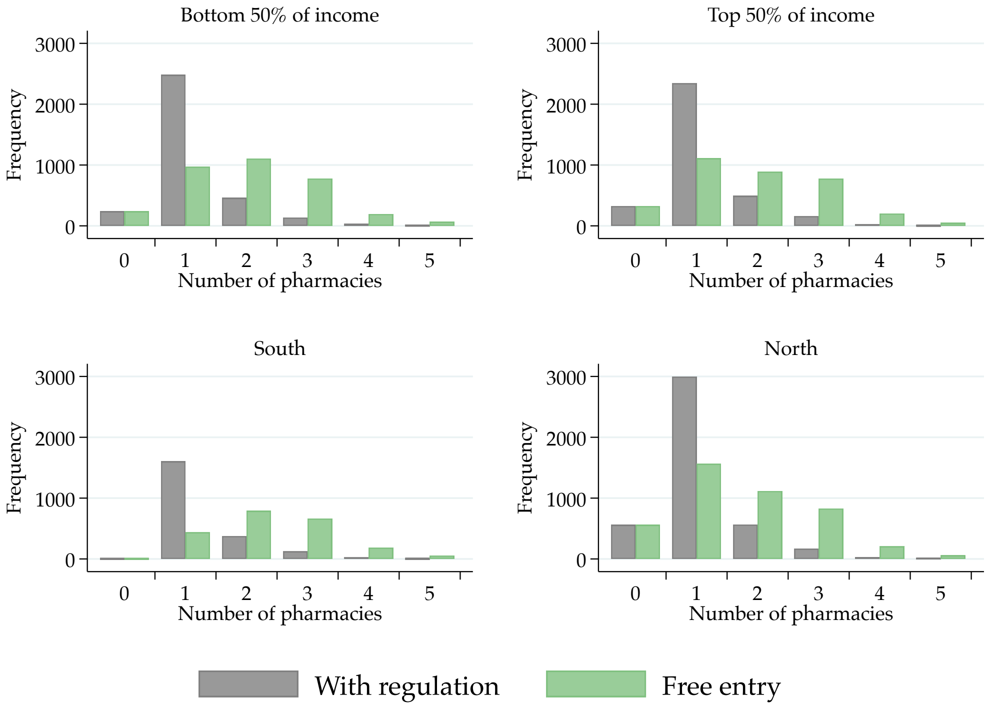


Figure C.8: Distribution of market configurations, by income (top panel) and region (bottom panel)

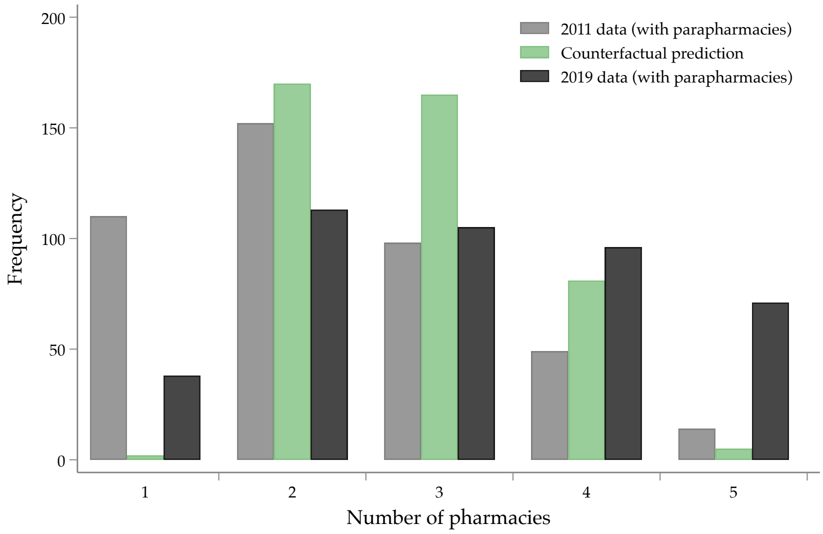


Figure C.9: Comparison of market configurations across effective (including parapharmacies) data and benchmark prediction

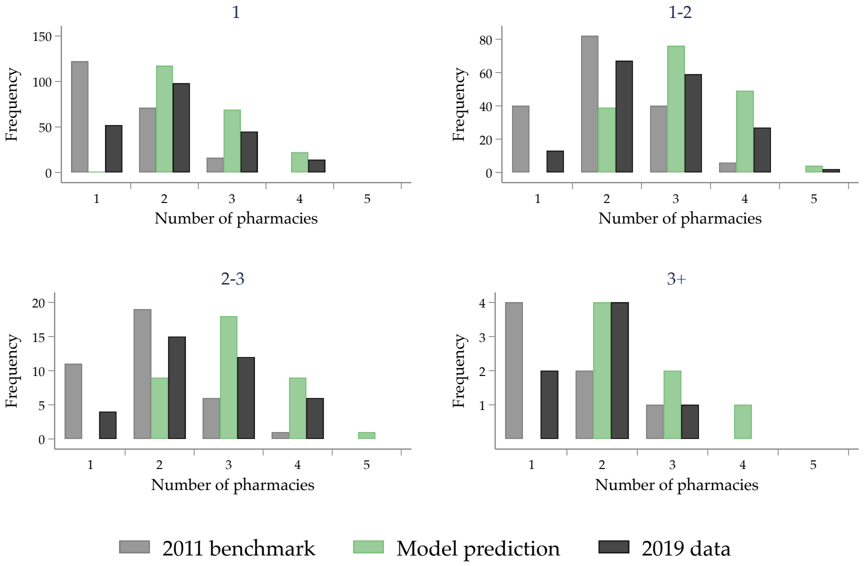


Figure C.10: Distribution of market configurations, by intensity of competition

Note: Each panel indicates a subsample of cities according to their average number of stores. This variable is computed as the mean of the number of competing stores in a radius of 200 metres centered at each pharmacy or parapharmacy in a city in 2019.

Table C.1: Market structure of cities with new assigned pharmacies

	Number of new pharmacies												
Number of incumbents	1		2		3		4		5		6+		**Total**
	N	%	N	%	N	%	N	%	N	%	N	%	N
0	29	72.50	8	20.00	1	2.50	1	2.50	0	0.00	1	2.50	40
1	294	97.67	7	2.33	0	0.00	0	0.00	0	0.00	0	0.00	301
2	282	87.58	37	11.49	2	0.62	0	0.00	0	0.00	1	0.31	322
3	143	90.51	15	9.49	0	0.00	0	0.00	0	0.00	0	0.00	158
4	93	80.17	20	17.24	2	1.72	0	0.00	1	0.86	0	0.00	116
5	76	80.85	17	18.09	1	1.06	0	0.00	0	0.00	0	0.00	94
6+	81	25.96	102	32.69	61	19.55	30	9.62	17	5.45	21	6.73	312
Total	998	74.31	206	15.34	67	4.99	31	2.31	18	1.34	23	1.71	1343

Table C.2: Summary statistics

	Mean	Std. Dev.	50%	25%	75%	Max	Min
Number of pharmacies	1.17	1.15	1.00	1.00	1.00	11.00	0.00
Unemployment rate	0.12	0.07	0.10	0.06	0.16	0.40	0.01
Binding entry restriction	0.77	0.42	1.00	1.00	1.00	1.00	0.00
Mean taxable income (in 1000's)	14.13	2.82	13.74	11.91	16.03	30.09	7.14
Positive pop. growth (2005-2011)	0.23	–	–	–	–	–	–
Negative pop. growth (2005-2011)	0.77	–	–	–	–	–	–
Outbound commuters (in 1000's)	0.35	0.47	0.19	0.10	0.41	4.47	0.00
Total population (in 1000's)	2.49	4.36	1.05	0.45	2.51	40.92	0.05
Share of residents over 65	0.26	0.07	0.25	0.21	0.30	0.63	0.07
Share of residents under 18	0.13	0.03	0.14	0.11	0.16	0.27	0.02
Residents with university degree (%)	0.06	0.02	0.06	0.05	0.08	0.18	0.01
Population density (% national mean)	0.16	0.23	0.10	0.05	0.19	4.16	0.00
Northern region	0.43	–	–	–	–	–	–
Central region	0.13	–	–	–	–	–	–
Southern region	0.46	–	–	–	–	–	–
Observations	1219						

Table C.3: Baseline specifications (isolated towns)

	No entry restrictions		With entry restrictions	
	Coef	Std. Err.	Coef.	Std. Err.
Positive growth	-0.0858	(0.0505)	-0.1226	(0.4209)
Negative growth	0.0797	–	-0.0835	(0.4121)
Commuters	-0.3964	–	-1.1784	(0.3424)
Mean income	0.0025	(0.0030)	0.1123	(0.1374)
Share of residents over 65	1.6224	–	5.3606	(8.8471)
Share of residents under 18	-0.2396	–	-2.4506	(10.7147)
Unemployment rate	-0.2850	(0.1629)	-2.0870	(1.6561)
Northern region	0.4775	(0.0623)	1.1838	(0.1740)
α_1	1.8686	–	1.8859	(5.5677)
α_2	1.5558	–	2.0440	0.6062
α_3	0.1948	(0.0439)	–	–
α_4	0.1150	(0.0373)	0.5038	(0.4339)
α_5	0.0029	(0.0357)	–	–
γ_1	0.5005	(0.0720)	–	–
γ_2	1.7984	(0.1226)	1.3286	(1.0673)
γ_3	0.7577	(0.1890)	1.2342	(1.3066)
γ_4	0.5031	(0.3010)	1.9164	(3.3266)
γ_5	0.7476	(0.4477)	–	–
Log-likelihood	-730.3317		-302.6328	
Observations	1219		1219	

Note: Asymptotic standard errors computed using the inverted Hessian. The model with entry restrictions imposes a non-negativity restriction on γ_1. A missing (–) standard error implies that the element in the diagonal of the inverted Hessian is negative. A missing coefficient (–) implies that its estimate is 0 due to the imposed constraints on parameters.

Table C.4: Entry thresholds (isolated towns)

	Entry thresholds					Ratios			
	S_1	S_2	S_3	S_4	S_5	$\frac{s_2}{s_1}$	$\frac{s_3}{s_2}$	$\frac{s_4}{s_3}$	$\frac{s_5}{s_4}$
No entry restrictions	312.60	3,549.59	6,385.63	9,511.25	11,482.50	11.3550	1.7990	1.4895	1.2073
With entry restrictions	119.12	817.21	1,365.27	2,854.88	2,854.88	3.4300	1.1138	1.5683	0.8000

Table C.5: Difference in observables between incorrectly and correctly predicted markets

VARIABLE	DIFFERENCE	T-STATISTIC
Mean taxable income (in 1000's)	-0.676*	(-2.45)
Share of residents over 65	-0.00160	(-0.61)
Share of residents under 18	0.000577	(0.36)
Population (in 1000's)	0.333	(1.27)
Unemployment rate (2011)	0.0124*	(2.43)
Number of commuters outside city (2011)	-0.0356	(-0.40)
Percent of residents with university degree (2011)	0.00239	(1.27)
Southern region	0.111**	(2.90)
Central region	0.0154	(0.51)
Northern region	-0.0853*	(-2.09)
New pharmacies in reform	0.0566**	(3.01)
Number of parapharmacies	0.150	(1.58)
Observations	554	

Note: The reported coefficient is the difference in the average value of the variable in the first column between incorrectly predicted markets (i.e., cities where the predicted number of firms is different from the effective one) and correctly predicted markets.

Table C.6: Predicted and effective (including parapharmacies) market configurations (absolute frequencies)

EFFECTIVE 2019	PREDICTED 2019					
	1	**2**	**3**	**4**	**5**	**Total**
1	1	37	0	0	0	38
2	1	81	29	2	0	113
3	0	33	61	11	0	105
4	0	9	58	27	2	96
5	0	10	17	41	3	71
Total	2	170	165	81	5	423

Table C.7: Predictive status, by intensity of local competition

Predictive status	Average number of stores 0-1 #	0-1 %	1-2 #	1-2 %	2-3 #	2-3 %	3+ #	3+ %	Total #	Total %
200 metres										
Correct	105	50.2	83	49.4	18	48.6	2	28.6	208	49.4
Overestimate	96	45.9	81	48.2	18	48.6	5	71.4	200	47.5
Underestimate	8	3.8	4	2.4	1	2.7	0	0.0	13	3.1
500 metres										
Correct	59	50.4	77	48.1	50	53.2	22	44.0	208	49.4
Overestimate	55	47.0	76	47.5	41	43.6	28	56.0	200	47.5
Underestimate	3	2.6	7	4.4	3	3.2	0	0.0	13	3.1

Note: The average number of stores is computed as the mean of the number of competing stores in a radius of 200 or 500 metres centered at each pharmacy or parapharmacy in a city in 2019.

Summary

A guiding principle of antitrust policy is that restricting competition generally hurts consumers. Incumbents in retail markets, however, are frequently shielded from new competitors by entry regulations (Biggar, 2001). Regulators argue that these instruments work in the public interest because they increase the quality of sellers and, combined with a subsidy scheme, ensure broad levels of coverage. Opponents contend that entry restrictions increase the market power of incumbents and ultimately harm consumers. This controversy is at the heart of the debate on deregulating retail and professional services, as implemented by the United States and Japan and proposed by the European Comission.

I inform this debate with a study of entry regulation in the Italian pharmacy market. This setting is attractive because Italy, like other European countries, restricts the maximum number of pharmacies in a city based on a population criterion that was reformed in 2012. I exploit the staggered introduction of this reform across Italian regions to provide reduced-form evidence on the distortionary effects of entry regulation. Furthermore, I estimate a static structural model of entry with homogeneous firms to estimate the competitive effects of entry. I then use this model to simulate counterfactual policies and I evaluate the model's predictive performance by comparing its predictions with *postreform* outcomes.

I find that regulating entry has a substantive impact on the structure of the pharmacy market in Italian cities. In particular, I show that reducing the *statutory* minimum number of residents per pharmacy leads to an equivalent decrease in the *effective* ratio of population to pharmacies after two semesters. I exploit heterogeneity in the intensity of this reduction across cities to find that postreform ratios always converge to the minimum allowable value. I argue that this adjustment suggests that further entry is likely to be profitable; this implies that pharmacists are shielded from additional competition through entry regulation.

I then model firms' profits as a function of city-level observables and an unobservable profit shock that is common across all firms in a city, following Bresnahan & Reiss (1991). The specification of profits captures heterogeneity between firms that operate in cities with different market structures. I assume that firms enter as long as profits are positive and the entry restriction is not binding; this equilibrium condition allows me to estimate the profit function by maximum likelihood. I show that markets become more competitive with the entry of additional firms, the bulk of these changes takes place with the third and fourth entrants. The minimum number of customers that an average pharmacy needs to break even is around 30% of the current statutory

minimum number, which rationalizes my reduced-form results. I use the model to simulate outcomes under free entry and I find that full liberalization would increase the overall number of establishments by 60% and decrease the number of cities with only one pharmacist by more than 130%. Finally, I use *ex-post* data to assess the model's out-of-sample performance and show that it accurately predicts postreform outcomes in 50% of markets. My findings suggest that the model exaggerates profits and overstates the benefits of liberalizing entry.

My results contribute to the literature on the effects of entry regulation in Italy. Empirical evidence suggests that regulation of retail in Italy is distortionary, reducing both consumer surplus and productivity. Calzolari *et al.* (2018) show that entry restrictions enable pharmacists in small Italian towns to appropriate more surplus from inelastic consumers relative to markets with more competitors. Schivardi & Viviano (2011) show that entry barriers in the Italian retail sector lead to higher profit margins and lower productivity. My contribution is to provide evidence on the effects of regulation in a different market and to assess alternative regulatory schemes. Evidence on the distortionary effects of regulation, as noted by Pozzi & Schivardi (2016), is yet insufficient to comprehensively judge the current regulatory scheme. In particular, the potential benefits of regulation are largely unknown, mostly because they involve dimensions which are hard to quantify, such as quality of life or harmonic urban development.

My findings are also methodologically relevant since I evaluate the predictive accuracy of a widely used model in empirical studies of entry. The literature combining simulations from structural models with retrospective analysis of policy reforms is small, recent and focused on the analysis of models about mergers; the relevance of retrospective strategies has been stressed by both Nevo & Whinston (2010) and Angrist & Pischke (2010). My strategy is closest to the approach in Peters (2006), who uses structural models to predict postmerger prices and then confronts these predictions with observed data from airline mergers in the United States. Björnerstedt & Verboven (2016) have a similar strategy using detailed data on a large merger between all firms in a segment of the analgesics market in Sweden. My contribution is to apply this retrospective analysis to a standard entry model. In particular, I assess the predictive power of the model in Schaumans & Verboven (2008), an adaptation of Bresnahan & Reiss (1991) to a setting with entry restrictions.

Restricting entry in the Italian pharmacy market has substantial economic effects. It reduces the number of establishments by around 60% and more than doubles the number of monopoly markets. Although these results overstate the distortionary effects of regulation, the velocity at which markets become restricted again after increasing the number of allowed pharmacies suggests that many incumbent firms are effectively shielded from additional competition. I do not assess the potential benefits from regulation but my results imply that these can only justify the policy if they are sufficiently large to offset the policy's impact on market power.

These findings are relevant for policy given that many European countries are discussing reforms to their entry regulation policies. I provide estimates that emphasize the potential benefits from deregulation. My findings also are relevant to examine the consequences of new platforms that are close substitutes to highly regulated services, such as Uber vs. taxi services or Amazon vs. traditional retail stores. The development of unregulated alternatives to traditional regulated sectors is analogous to a reduction in entry restrictions.

My results are also methodologically relevant since I provide an assessment of a routinely used structural model to study entry and predict the effects of policy reforms, as in Schaumans & Verboven (2008) who study liberalization in the Belgian pharmacy market and Grant *et al.* (2019) who simulate reforms in the German long-term care market. I show that the model fits the data moderately in my application since it overestimates firms' profits. This simple model, however, is able to predict half of the postreform outcomes and its general prediction is largely in line with the postreform outcomes. The model's shortcomings might be overcome with additional data that improve the specification of the profit function. It is therefore important to retrospectively study other reforms to disentangle the weaknesses of my data from those of the model.

My analysis is limited by the lack of information at the pharmacy level. Incorporating micro-level data on prices and quantities would strengthen the reduced-form strategy significantly by expanding it to study the reform's effect on additional competitive outcomes. The structural model could also be extended to recover strategic interactions between pharmacies and parapharmacies or between pharmacies and physicians using the strategy in Schaumans & Verboven (2008). Additionally, micro-level data can be incorporated to the model to accomodate heterogeneity across firms following Berry (1992).