DOES ENTRY REGULATION HINDER JOB CREATION? EVIDENCE FROM THE FRENCH RETAIL INDUSTRY*

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Are product market and entry regulation key sources of low employment growth in many European countries? We investigate this question in the context of the French retail trade industry. Since 1974, approval by regional zoning boards has been required for the creation or extension of any large retain store in France. We exploit a unique database that provides time- and region-specific variation in boards' approval decisions. We show that stronger deterrence of entry by the boards increased retailer concentration and slowed down employment growth in France.

I. INTRODUCTION

Labor market institutions are the most widely accepted explanation for the employment crisis that has plagued Europe over the last 30 years. Most observers blame strong unions, rigid employment protection systems, generous levels of unemployment benefits, high minimum wages, and high tax wedges for the low employment rates in many European countries. A vast body of empirical research has confirmed that labor market institutions, either directly or in combination with various macroeconomic and technological shocks, have played an important role in the lengthy depression of European labor markets.[1]

Another, increasingly discussed but much less widely re-

1. See Nickell and Layard [1999] for a review of the direct effects of labor market institutions on economic performance across OECD countries. In a recent study, Blanchard and Wolfers [2000] argue that most of the labor market constraints in Europe predate the rise in unemployment and that these constraints have, if anything, become less binding over a period of steady unemployment rise. These authors argue that the interaction of these preexisting institutions with macroeconomic shocks can better explain unemployment patterns across Europe. The existing empirical evidence on a possible interaction between labor market institutions and skill-biased technological shocks is more mixed; see Nickell and Bell [1995, 1996], Card, Kramarz, and Lemieux [1999], Krueger and Pischke [1997], Blau and Kahn [1996], and Kahn [2000].

searched, hypothesis is that product market and entry regulations have been other key factors in the slow rate of job creation in Europe.[2] Recent work has lent credence to this hypothesis by documenting that Europe distinguishes itself from the United States not only through its regulatory practices on the labor market side but also through many more constraints on entry and competition on the product market side. In multiple reports on Europe, the McKinsey Global Institute [1994, 1997] has emphasized the high levels of product market regulation in sectors such as services, construction or banking. In another recent study, Djankov, La Porta, Lopez-de-Silanes, and Shleifer [2002] document the multiple, costly and time-consuming requirements to starting a new business in many European countries. For example, they show that the startup process may take up to 66 days and 16 different legal and administrative steps in France compared with only 7 days and 4 steps in the United States.

The theoretical relevance of this hypothesis has also been established. Product market regulation has been causally linked to employment outcomes through its effect on market power and markups.[3] One recent macroeconomic paper highlighting this mechanism is Blanchard and Giavazzi [2001].[4] These authors show that tougher product market regulation, which they model as a higher cost of entry for new firms, increases equilibrium markups and rents for incumbent firms, and lowers equilibrium sales and employment. They also study how product market regulation interacts with labor market regulation, which they model as a bargaining over rents between firms and workers. They show that the negative employment effect of product market regulation gets reinforced when labor market regulation is also present.[5] Similar implications have also been worked out in the context of (maybe somewhat less fashionable) disequilibrium

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2. Krueger and Pischke [1997] were among the first to make this conjecture. Based on a comparative study of employment trends in the United States and various European countries, they concluded that labor market rigidities cannot fully account for the slow rate of *skilled* employment growth in Europe. The relevance of a product market explanation for Europe's employment crisis has also been stressed by Gersbach [2000].

3. While this is the most obvious channel through which product market and entry regulation can be tied to employment, other channels may also exist. We mention some that might have been especially relevant in the retail trade sector in Section V.

4. See also Gersbach and Schniewind [1998].

5. Blanchard and Giavazzi [2001] also examine the dynamic employment effects of product and labor market deregulation as well as the political economy interactions between the two types of deregulation.

models. Barro and Grossman [1971] present a model where the prices of goods are exogenously fixed above equilibrium levels. Output and employment in their model are both determined by the constrained product demand and fall below competitive levels.

Despite the factual and theoretical relevance of the product market hypothesis, there has been so far a dearth of systematic empirical studies on the question.[6] In this paper we propose to undertake such a study for France. We focus on what might have been the most important governmental rule to restrict entry in the French retail trade sector: commercial zoning regulation.

Introduced at the end of 1973, the "Loi D'Orientation du Commerce et de l'Artisanat," also known as the "Loi Royer," was originally motivated by a desire to protect small retail stores from the "unruly growth of new forms of distribution" [Ministère de l'Industrie, du Commerce et de l'Artisanat 1974]. Prior to the introduction of the Loi Royer, the major requirement before starting a new retail business in France was to obtain a building permit. Since 1974, another important step has been added to the startup process. The creation or extension of any new large retail establishment has first to be approved by *a regional* zoning board composed of store owners, consumer representatives, and regionally elected politicians. Between 1974 and 1998, the regional zoning boards approved only about 40 percent of the applications submitted to them each year.[7]

As a preliminary piece of evidence, Table I displays employment growth rates in the retail trade and hotels and restaurants sectors before and after the introduction of the Loi Royer. We compute average annual employment growth over the periods 1967 to 1978 and 1979 to 1998. As we discuss at greater length in

6. A few papers have documented a negative correlation between product market regulation and employment performance across OECD countries. See, for example, Boeri, Nicoletti, and Scarpetta [1999]. While these cross-country patterns are informative, they leave open the possibility that other factors, correlated with product market regulation, are driving the relationship. A larger literature has studied the labor market effects of various episodes of deregulation in the United States. See Peoples [1998] for a review of this literature. The deregulation of the airline and trucking industries have been associated with substantial employment gains. Declines in unions' bargaining power and in prices postderegulation have been identified as two likely channels for these employment gains. Employment gains were more modest following the deregulation of railroads, possibly due to higher natural barriers to entry in this industry.

7. While we focus on the French experience, commercial zoning laws are common in many other European countries. A study performed by the McKinsey Global Institute in 1994 shows that Germany, Italy, and Spain all have in place some form of zoning regulation.

TABLE I
AVERAGE ANNUAL EMPLOYMENT GROWTH RATES 1967–1998 (IN PERCENT)

Sector:	Retail trade	Hotels and restaurants
Period:		
1967–1978	.6	.8
1979–1998	.1	1.7

Source: Banques de Donnees Macroeconomiques (BDM), 1967 to 1998.

subsection III.A, construction delays justify the four-year lag we impose between the time the Loi Royer was introduced and the time we allow for its effect to kick in. Hotels and restaurants, which were not targeted by the zoning regulation, constitute a natural comparison group here. Both retail trade and hotels and restaurants employ a large fraction of low-wage workers and should have been rather similarly affected by a set of labor market rules, such as minimum wage laws, that have increased the cost of hiring workers from the bottom part of the wage distribution in France.[8]

Table I shows that both sectors experience about the same rate of employment growth in the preperiod (.6 percent in retail trade compared with .8 percent in hotels and restaurants). The two sectors, however, diverge in the postperiod. Employment growth in retail trade drops to less than .1 percent per year while it increases to 1.7 percent in hotels and restaurants. Note that the relatively faster growth of hotels and restaurants in the later period is not specific to that low-wage sector. Employment in other low-wage service sectors in France also grows relatively faster in the 1980s and 1990s than in the late 1960s and 1970s. This reflects in part the fact that low-wage sectors were hit by several large increases in the minimum wage in the first period, from which they might have somewhat recovered in the second period. Four minimum wage increases, each of more than 10 percent in real terms, occurred in France between 1968 and 1974. Retail trade, however, does not appear to recover. To the contrary, employment growth nearly comes to a halt in retail trade in the last two decades. While we find the patterns in Table I

    8. As Piketty [1997] already noted, the fraction of total employment in these two sectors in France (about 10 percent) is much smaller than it is in the United States. Piketty attributes this difference to the many binding labor market constraints in France.

informative, they far from establish the causal effect of the Loi Royer as they may reflect a variety of other industry-specific shocks.[9]

Fortunately, the specifics of the implementation of the Loi Royer allow us to develop an empirical methodology that nets out such other shocks and gets more closely at the causal effect of the regulation on retail trade employment. Specifically, the Loi Royer was administered at the level of the region, or "département."[10] In contrast with the decentralization of the zoning regulation, most other governmental and legal rules in France are set nationally. One can therefore hope to study the regulation of entry in the retail trade sector in France independently of other policies and general industry shocks that might also affect job creation.[11] Moreover, the availability of a unique database makes it possible for us to measure the strength of entry deterrence in each region. We were able to obtain detailed information on all the applications submitted to each regional board between 1975 and 1998. For each application, we know the size and specialty of the store, the brand name of the promoter, and most importantly, whether or not the application was approved by the zoning board.

In practice, we follow two different empirical approaches. In our basic estimation strategy, we use approval rates (or number of approvals conditional on number of applications) by département over time to form a measure of the stringency of entry deterrence. While this first approach is intuitive, it rests on the assumption that applications are exogenous to the entry regulation process, an assumption that is likely to be violated in practice.[12] In response, we propose a second empirical strategy that

9. It is worth mentioning that employment growth in the U. S. retail trade sector also slowed down over the last two decades.
10. In the rest of this paper we use the word "region" and "département" interchangeably. There are 95 different départements in metropolitan France. These administrative entities were created at the time of the French Revolution. Until 1982, each département was headed by a "préfet" who was appointed by the central government. After 1982, the executive power in a département moved to an elected "conseil général" and its president. In the 1990 Census the average French département had 600,000 inhabitants. The département at the 10th (respectively, 90th) decile of the size distribution had 250,000 (respectively, 1,000,000) inhabitants.
11. Opening hours restrictions, another form of product market regulation in the retail industry, exist in almost every European country [McKinsey Global Institute 1994]. However, the effect of this regulation is harder to evaluate as it is typically set nationally.
12. In subsection III.B we provide a detailed discussion as to why applications may be either an under- or an overestimate of the number of stores that would have been created in a laissez-faire situation.

does not rely directly on applications. This second strategy is motivated by the politics of the entry regulation process. Right-wing parties in France are known to receive the political support of the self-employed and are therefore more likely to oppose entry. Because an elected politician is often the median voter on the zoning boards, these political preferences can directly influence the number of approvals in a region. Therefore, in our second empirical approach, we use the distribution of electoral votes across political parties by département as an instrument for approvals. Both empirical approaches allow us to account for fixed differences across départements as well as for aggregate time shocks. Under both approaches, we find that more stringency in entry deterrence hinders job creation. We verify that these results are robust to a series of specification checks.

We also investigate in further detail the mechanisms by which entry regulation may slow down employment growth. More specifically, we analyze the effect of the Loi Royer on the concentration of large retail chains in France and on retail prices. For this part of the analysis, we focus on the food retail sector in order to isolate a more homogeneous set of goods. We show that stronger entry deterrence increases both retailer concentration and retail prices. Moreover, we show that the higher retailer concentration induced by entry deterrence has a direct negative effect on employment in the food retail sector.

The rest of this paper is organized as follows. Section II describes the Loi Royer in further detail and presents some descriptive statistics from the applications database. The next two sections contain our central analysis of the effect of the entry regulation on retail employment. Section III presents the OLS results. Section IV presents the instrumental variables' approach and results. Section V studies the effect of the zoning regulation on retailer concentration and retail prices. Section VI investigates broader labor market effects of the zoning regulation. We summarize and discuss future extensions in Section VII.

II. ENTRY REGULATION

II.A. La Loi Royer

In December 1973 the French parliament adopted the "Loi d'Orientation du Commerce et de l'Artisanat," commonly known as the Loi Royer [Ministère de l'Industrie, du Commerce et de

l'Artisanat 1974]. The original spirit of this legislative change was to protect small shopkeepers and craftsmen in the face of a rapidly evolving large distribution market.[13] The law proposed to achieve this goal through various measures. Some of these measures were implemented at the national level. For example, one component of the law was to establish more fiscal equity between salaried and self-employed. The law also set up funds for financial assistance to old-aged shopkeepers and craftsmen. Such centralized reforms are difficult to study empirically as they cannot easily be separated from other policy or economic changes. In this paper we focus on the only component of the law that was decentralized: commercial zoning regulation.

Before the introduction of the Loi Royer, opening a large store in France only required obtaining a building permit. As it is the case in the United States, environmental and urban design considerations are the major criteria behind the granting of such a permit. Since 1974, however, the creation of any new large store also has first to be approved by a regional zoning board called the "Commission Départementale d'Urbanisme Commercial."[14] Each French département has its own zoning board.

The construction of any new store with a sales area of more than 1,500 square meters falls under the zoning regulation. In cities of less than 40,000 people, the regulation applies to any new store of more than 1,000 square meters. In addition, extensions of existing stores or conversions of existing buildings into retail space also have to be approved by the regional zoning boards if the resulting sales area exceeds the size thresholds above. In case of refusal by the board, a promoter has the opportunity to appeal the decision to the national Minister in charge of retail and craft. The Minister, advised by a national zoning commission called "Commission Nationale d'Urbanisme Commercial," has the power to overturn the decision of the regional board.[15]

The structure of the regional boards is also heavily regulated.

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13. In France, butchers and bakers are referred to as "craftsmen of the mouth."

14. It is important to stress that the zoning regulation did not replace the building permit requirement but instead introduced an extra step in the startup process.

15. While our data cover the whole universe of applications submitted to the regional boards from 1975 on, we have only limited information on the number of appeals occurring at the national level and on the outcome of these appeals. There is some anecdotal evidence that the number of appeals was high in early years but declined very rapidly over time. We know that over the period 1994 to 1998, 10 percent of all approved stores were approved at the national level.

Boards are composed of twenty voting members. Nine members are shopkeepers and craftsmen's representatives. Seven of these nine members must be self-employed. Two members represent consumers' interests. The last nine members are locally elected politicians, typically city mayors. A new retail establishment is rejected if a simple majority of board members vote against it. Because consumer representatives are anecdotally believed to almost always favor entry, the median voter on these boards tends to be an elected politician. We exploit this fact later on as part of our identification strategy.

The Loi Royer is still in place today. It has been amended in several ways, all tending toward increasing its stringency. This has been primarily achieved by reducing the size thresholds and strengthening the majority requirement for approval. Unfortunately, most of these amendments occurred toward the end of our sample period, making it difficult for us to incorporate them into the identification strategy.

II.B. Applications Database: Descriptive Statistics

The "Direction du Commerce de l'Artisanat" (DECAS), a branch of the Ministry of Economics and Finance, maintains an exhaustive listing of all *applications* submitted to the zoning boards since the inception of the Loi Royer. For almost every application, the DECAS records the brand name of the applicant, size of the store (in square meters), specialty of the store (food or nonfood with varying levels of detail depending on the year), location of the store (département and city), terms of the application (whether seeking to create a new store, expand a previously existing store, or convert an existing building into retail space), and outcome of the board vote (an indicator for whether the store was approved or rejected, as well as the number of "yes" votes, "no" votes, and abstentions). The DECAS kindly gave us access to a copy of this listing for all applications submitted between January 1975 and December 1998.[16]

Simple tabulations from the DECAS data show that a little more than 16,000 applications were submitted to the zoning boards between 1975 and 1998. These applications are roughly equally divided between food and nonfood retail. In terms of square meters approved, the mean annual approval rate in a

16. The applications database is described in further detail in the Data Appendix.

TABLE II
DETERMINANTS OF APPROVAL RATES ACROSS DEPARTEMENTS

	Dependent variable: mean annual approval rate by département (1975–1998)					
	(1)	(2)	(3)	(4)	(5)	(6)
Determinants:	Log total population	Average age	Fraction over 60 years old	Fraction working in agriculture	Fraction living in rural areas	Average log weekly earnings
	.119	−.022	−.997	−.568	−.304	.431
	(.014)	(.004)	(.198)	(.104)	(.059)	(.120)
Adjusted R^2	.431	.220	.206	.235	.212	.112

Sources: French LFS, 1975 and 1982; DECAS Applications Database, 1975 to 1998.
a. Each column corresponds to a different regression. All regressions are estimated using OLS. Standard errors are in parentheses.
b. All the département characteristics are measured in 1975 except for weekly earnings which are only available in the LFS starting in 1982 and are measured in that year.
c. Sample size in all regressions is 95.

département is 42 percent, with a standard deviation of 37 percent. In terms of numbers of stores, the equivalent statistics are 45 and 35 percent, respectively. This suggests that larger stores face a slightly lower probability of approval. The mean annual approval rate (in square meters) in food retail is about 30 percent, compared with 45 percent in nonfood retail. The DECAS data also reveal that approval rates have been trending up over time, at an average rate of about 1.5 percent per year. A closer examination shows that most of the increase in the approval rate was concentrated in the 1990s. Average annual approval rate was about 30 percent prior to 1990 compared with about 55 percent in the later period.

Mean approval rates differ widely across départements, from a minimum of about 10 percent to a maximum of about 70 percent. It is interesting to ask whether these differences are related to specific département characteristics. We perform such a cross-sectional analysis in Table II. We regress mean annual approval rates by département over the period 1975 to 1998 on a set of base-year (1975) département characteristics. Column (1) shows that an important cross-sectional determinant of approval rate is a département's total population. Larger départements approve a larger share of applications. Age demographics also affect approval rates. Older départements, which we measure either as

average age (column (2)) or as the fraction of people over 60 years old (column (3)), approve less stores on average. We also find that a more rural setting, measured either by the fraction of people living in rural areas (column (4)) or by the fraction of people employed in agriculture (column (5)), is associated with a lower fraction of approvals. Finally, column (6) shows that richer départements (those with higher average log weekly earnings) typically accept a higher share of applications.

While approval rates appear to be related to département characteristics and time effects, it is important to note that they still vary a lot within département and year. A regression of annual département approval rates on département and year fixed effects has an adjusted R^2 of only 0.29. About two-thirds of the explained variance can be attributed to the time effects.[17]

III. RETAIL EMPLOYMENT EFFECTS: BASIC RESULTS

III.A. Empirical Methodology

Our basic empirical approach consists in using observed approvals and approval rates as measures of the stringency of the entry regulation process. This first approach relies on the assumption that applications are exogenous to the entry regulation process itself. In other words, we start by assuming that applications represent the number of stores that would have been created in the absence of entry deterrence. There are several reasons why this assumption might be violated in practice. In subsection III.B we discuss these reasons in detail as well as their implications for the validity and interpretation of this basic model. In Section IV we propose an instrumental variable approach that attempts to isolate a more exogenous source of variation in the stringency of entry deterrence.

We propose to estimate the two following panel data regressions:

$$(1) \quad \log(\text{retail emp}_{it}) = \theta \log(\text{approvals})_{i(t-4)}$$
$$+ \psi \log(\text{applications})_{i(t-4)} + \alpha_i + \beta_t + \gamma X_{it} + \epsilon_{it}$$

17. The rather low approval rates summarized above suggest that the zoning boards did exert some power to deter entry. However, these descriptive statistics only offer a partial and imperfect insight as to how much the zoning regulation distorted entry away from a laissez-faire equilibrium. See subsection III.B for a detailed discussion.

(2)

$$\log(\text{retail emp}_{it}) = \theta \text{ share approved}_{i(t-4)} + \alpha_i + \beta_t + \gamma X_{it} + \epsilon_{it},$$

where "retail emp$_{it}$" is retail trade employment in département i and year t, "approvals$_{i(t-4)}$" is the stock of approvals (in square meters) in département i up to year $(t-4)$, "share approved$_{i(t-4)}$" is the share of applications approved in département i up to year $(t-4)$, "applications$_{i(t-4)}$" is the stock of applications (in square meters) submitted in département i up to year $(t-4)$, α_i are département fixed effects, β_t are year fixed effects, X_{it} is a vector of time-varying département characteristics, and ϵ_{it} is an error term.

The key variables in equations (1) and (2) are "log(approvals)$_{i(t-4)}$" and "share approved$_{i(t-4)}$," respectively. These variables represent our measures of entry regulation. We define a more lenient board as a board that approves more stores, conditional on the number of applications, or alternatively, as a board that approves a larger share of applications. We ask whether increases in the stock or in the fraction of approvals affect retail employment.

Based on conversations with members of DECAS, we lag both of the entry regulation variables by four years in order to allow for a reasonable construction lag.[18] This rather long lag results in part from the fact that a building permit must still be granted for any project newly approved by a zoning board before actual construction can begin.[19]

Equations (1) and (2) include year dummies as regressors to account for aggregate time shocks. These year dummies will capture any aggregate economic shock but also any policy change at the central level that might affect retail employment. As we mentioned earlier, French départements have little discretion in setting their own policies.[20] Importantly, départements have

18. For example, we use the stock of applications that was approved as of January 1, 1976, to explain retail employment in 1980. More detail on the exact construction of the entry regulation variables are provided in the Data Appendix.
19. Our results are robust to using slightly shorter construction lags.
20. One decentralized policy that could potentially affect retail employment are local taxes. Most important for our purpose are business taxes ("taxe professionnelle"), taxes paid by building owners ("taxe foncière sur le bâti") and taxes paid by landowners ("taxe foncière sur le non-bâti"). Until 1982, départements had very little latitude in deciding the level of these local taxes. Both rates and bases for these taxes were set at the national level. After 1982, local tax rates were decentralized but only to a certain degree as they are still subject to a national floor and a national ceiling. Moreover, the bases for each local tax are still set at the national level.

close to no autonomy in adopting their own labor market rules. Hence, the year dummies should adequately control for most of the other policy changes that may have occurred over the period under study. For example, any change in the minimum wage, which is set in Paris and applies uniformly to all départements, will be captured by the year dummies.[21]

Département fixed effects are also added as regressors in equations (1) and (2). They control for any fixed factors that might affect the level of retail employment in a given département. Such factors include the size of the département and the stock of retail capital (both large and small stores) that was in place prior to the introduction of the zoning regulation.

We will investigate the robustness of our results to the inclusion of various time-varying département characteristics X_{it}. Our preferred specification will control for the age composition in a département (fraction below 21, fraction between 21 and 40, fraction between 41 and 60, and fraction above 60 years old), as well as the urban/rural structure in a département (fraction living in rural areas, fraction living in small towns, fraction living in midsize cities, and fraction living in large cities).

Finally, to make our results representative for the average French citizen, each observation in equations (1) and (2) is weighted by the share of each year's total population in the département.

The demographic and employment variables used in equation (1) and (2) are computed from the Labor Force Survey (LFS), a survey of about 60,000 households conducted annually by the French Statistical Institute (INSEE). This survey is described in further detail in the Data Appendix. We also provide summary statistics for the main variables of interest from the LFS in Appendix 1. It is important to note that retail employment is defined to include all individuals that report working in the retail sector, whether or not they receive strictly positive earnings. For example, the wife of a self-employed retailer who "helps" in the store is regarded as employed in the retail trade sector.

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21. One could argue that even though the minimum wage is set nationally, it does not have the same impact across all départements, for example due to differential industrial composition. We will show below that our findings are robust to this critique.

III.B. Discussion

While the empirical approach presented above is simple and intuitive, its validity and interpretation are subject to the important assumption that applications are exogenous to the entry regulation process and truly measure the number of stores that would have entered in the absence of deterrence. We can see two major reasons why this assumption might be violated in practice.

First, if the application process is costly, one might expect the number of applications to be low when the expected stringency of the zoning is high. A high application cost combined with a low probability of success may discourage some promoters from applying, even though these promoters would have entered in a free entry situation. If this discouragement effect is large, it will induce measurement error in the two variables used to proxy for the stringency of entry regulation. Observed approval rates will underestimate the true level of deterrence.

Anecdotal evidence we collected from members of DECAS suggests that the application process is costly and that this cost has trended up over time. In the earlier years, the main cost of applying was a time cost. The average delay between the submission of an application and its consideration by the zoning board is about four months. As time went on, additional costs have emerged. Specifically, corruption became an important factor in the 1980s. For example, promoters were often "encouraged" to subsidize some public work, such as the construction of a new road or of a municipal swimming pool, in order to grease the wheels of the approval process.[22] Finally, administrative costs increased substantially in the 1990s as an impact study then became mandatory for every application.

Second, because entry regulation by its very nature protects the rents of incumbent stores, it can also be argued that more applications will be submitted than the number of stores that would otherwise have been created in a laissez-faire situation. This can more easily be seen with the following example. Suppose that a city can economically sustain just one additional store and that the board rejected the first application submitted for that specific opportunity. This will encourage subsequent applications from other promoters hoping to share in the rents still available in that city. Under this scenario, our proposed measures of entry

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22. A new law was adopted in 1993 (Loi Sapin) to fight this kind of practice.

deterrence will exaggerate the true deviation from free entry in that city. If there are repeated applications for the same investment opportunity, observed approval rates will overestimate the true level of deterrence.

It is clearly difficult for us to assess the practical importance of this last issue as we do not know the number of viable stores in each local market. However, in order to provide some suggestive evidence, we tabulated the number of applications by city over the entire period under study (from 1975 to 1998).[23] We focused on food retailing in order to isolate a more homogeneous set of desired investments. Applications for food retail stores were submitted in about 2,300 different cities. We found that the total number of applications per city was relatively low. Only one application was submitted in about 900 cities. Another 500 cities saw only two applications over the period. Three-quarters of the cities recorded *at most* three applications.

As we mentioned earlier, we propose in Section IV an alternative empirical approach that does not directly rely on applications and therefore deals with the two potential sources of bias outlined above. As we will see, our results are qualitatively similar under both approaches.[24]

The two issues we just discussed also have important implications for any attempt to extrapolate the long-run economic effect of entry deterrence. They make it clear that we may not dispose of a good counterfactual for what the equilibrium stock of stores would have been in a laissez-faire situation. This will make tricky any attempt to quantify the long-run economic impact of entry regulation on employment. In subsection IV.E we present several attempts at such quantification under more and less conservative scenarios.

III.C. Results

The results of the estimation of the two basic regressions (1) and (2) are reported in Table III. The data cover 95 départements.[25] Each coefficient in Table III is the estimated coefficient

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23. This exercise assumes that cities can be regarded as independent retail markets.

24. While we have focused here on potential problems due to the endogeneity of applications, we discuss in Section IV another possible source of bias, namely that approvals and approval rates may be correlated with unobserved shocks to retail demand.

25. Note that the panel is not balanced because it took several years for some smaller départements to have a strictly positive stock of applications and approv-

TABLE III
EFFECT OF ENTRY REGULATION ON RETAIL EMPLOYMENT BASIC RESULTS

Additional controls:	Dependent variable: log(retail employment)		
	(1) None	(2) Age demographics Rural/urban demographics	(3) Age demographics Rural/urban demographics Unemployment rate
log(approvals)$_{(t-4)}$	.085	.080	.080
(in square meters)	(.018)	(.018)	(.017)
	[.026]	[.025]	[.025]
share approved$_{(t-4)}$	.182	.169	.163
	(.049)	(.047)	(.047)
	[.064]	[.061]	[.060]
log(15-year approvals)$_{(t-4)}$	.088	.080	.080
(in square meters)	(.017)	(.017)	(.017)
	[.026]	[.025]	[.025]
15-year share	.191	.175	.166
approved$_{(t-4)}$	(.047)	(.046)	(.045)
	[.064]	[.061]	[.061]
log(10-year approvals)$_{(t-4)}$	.070	.063	.062
(in square meters)	(.015)	(.015)	(.014)
	[.021]	[.021]	[.021]
10-year share	.167	.151	.141
approved$_{(t-4)}$	(.041)	(.040)	(.040)
	[.057]	[.054]	[.055]

Source: French LFS, 1980 to 1998; DECAS Applications Database, 1975 to 1998.
a. Each coefficient corresponds to a separate regression. All regressions include year and département fixed effects. Rows 1, 3, and 5 also include as a control the logarithm of the stock of applications up to year $(t-4)$ (all years, 15-year and 10-year, respectively).
b. "Age demographics" include fraction under 20, between 21 and 40, between 41 and 60, and above 60 years old in the département in year t. "Urban/rural demographics" include fraction living in rural areas, in cities of less than 20,000 people, in cities of between 20,000 and 200,000 people, and in cities of more than 200,000 people in the département in year t. "Unemployment rate" is the unemployment rate in the département in year t.
c. Observations are weighted by the share of each year's total population in the département.
d. OLS Standard errors are in parentheses. Standard errors that allow for clustering of the observations at the département level are in brackets.
e. Sample size in all regressions is 1,683.

on either "log(approvals)" or "share approved" and results from a separate regression. Each column corresponds to a different set of time-varying département characteristics, X_{it}. One issue when

als. Our results are qualitatively unchanged when we limit the sample to a balanced panel of the largest départements.

controlling for time-varying département characteristics is that these may themselves be endogenous to the entry regulation process. Column (1) presents a benchmark specification where we do not include any X_{it}. In column (2) we control for the age and urban/rural composition in the département/year cell. Column (3) also includes the unemployment rate in the département as an additional regressor.

We report two sets of standard errors. OLS standard errors are in parentheses. To account for serial correlation in the error term, we also report in brackets standard errors that allow for clustering of the observations by département (see Bertrand, Duflo, and Mullainathan [2001] and Kezdi [2001]).

Rows 1 and 2 present results from the estimation of equations (1) and (2), respectively. Under both specifications, we find that fewer approvals are associated with lower retail employment. In row 1, we find that a 1 percent increase in the stock of authorized retail space leads to a .080 to .085 percent increase in retail employment, depending on the set of time-varying controls X_{it}. In row 2, we find that a 1 percentage point increase in the approval rate leads to a .16 to .18 percent increase in retail employment.

Rows 3 to 6 replicate rows 1 and 2 but try to account for the possible depreciation of the retail capital stock. In rows 3 and 4 we assume that stores approved more than fifteen years prior to $(t-4)$ have fully depreciated by time t. In rows 5 and 6 we assume that stores approved more than ten years prior to $(t-4)$ have fully depreciated by time t. We then compute the stock of approvals and approval rates based on these assumptions. In all cases, we find that fewer approvals and a lower approval rate are associated with lower retail employment.

We considered two alternative specifications of equations (1) and (2). First, to account for the possibility of sampling errors in the employment and demographic variables due to the relatively small size of the LFS survey, we reestimated equations (1) and (2) using three-year averages instead of yearly data. The results were unaffected. Second, we reestimated equation (1) in first-differences rather than in deviation from the mean. This allowed us to investigate some additional timing issues. While we assume in equations (1) and (2) that it takes exactly four years between the approval and the actual opening of a store, we might expect in practice a more gradual effect, with some stores opening faster

and some stores experiencing even longer delays. More specifically, we estimated the following regression using OLS:

$$\Delta \log(\text{retail emp}_{it}) = \theta_1 \Delta \log(\text{approvals}_{i(t-5)})$$
$$+ \theta_2 \Delta \log(\text{approvals}_{i(t-4)})$$
$$+ \theta_3 \Delta \log(\text{approvals}_{i(t-3)})$$
$$+ \psi_1 \Delta \log(\text{applications}_{i(t-5)})$$
$$+ \psi_2 \Delta \log(\text{applications}_{i(t-4)})$$
$$+ \psi_3 \Delta \log(\text{applications}_{i(t-3)}) + \beta_t + \epsilon_{it},$$

where Δ represents a first-difference operator and all the other variables are defined as before. The estimated coefficient on Δ $\log(\text{approvals}_{i(t-4)})$ was .072 and similar in magnitude to the estimated coefficient on $\log(\text{approvals}_{i(t-4)})$ in the fixed effects estimation. That coefficient was statistically significant. The estimated coefficients on $\Delta \log(\text{approvals}_{i(t-3)})$ and $\Delta \log(\text{approvals}_{i(t-5)})$ were, as expected, positive (.014 and .003, respectively) but statistically insignificant.

Table IV replicates the basic specifications of Table III but breaks down the retail trade sector into food (column (1)) and nonfood (column (2)). Note that in column 1 we use applications and approvals only in the food retail sector to construct the measures of entry regulation. Similarly, in column (2) we use applications and approvals only in the nonfood retail sector. Because one can reasonably argue that the unemployment rate is likely to be endogenous to the zoning process, we limit the set controls X_{it} to the age and rural/urban demographic controls. Our basic finding of a negative relationship between retail employment and approvals or approval rates holds true in both the food and nonfood branches of retail trade.

Table V investigates the robustness of our findings in the light of various alternative economic interpretations. We focus on equation (1).[26] In column (1) we address a basic causality concern by asking whether *future* approvals affect current retail employment. To do so, we add the stock of approvals as of $(t + 1)$ as a regressor. The impact of future approvals on current employment

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26. We have verified that the results from equation (2) are also robust to all the specification checks below.

TABLE IV
EFFECT OF ENTRY REGULATION ON RETAIL EMPLOYMENT:
FOOD RETAIL AND NONFOOD RETAIL

Dependent variable:	(1) *log (food retail employment)*	(2) *log (nonfood retail employment)*
log(approvals)$_{(t-4)}$	.106	.061
(in square meters)	(.018)	(.023)
	[.040]	[.034]
share approved$_{(t-4)}$	.125	.163
	(.067)	(.057)
	[.125]	[.079]

Source: French LFS, 1980 to 1998; DECAS "Applications Database," 1975 to 1998.

a. Each coefficient corresponds to a separate regression. All regressions include year fixed effects, département fixed effects, and age and urban/rural demographic controls. "Age demographics" include fraction under 20, between 21 and 40, between 41 and 60, and above 60 years old in the département in year t. "Urban/rural demographics" include fraction living in rural areas, in cities of less than 20,000 people, in cities of between 20,000 and 200,000 people, and in cities of more than 200,000 people in the département in year t. Row 1 also includes as a control the logarithm of the stock of applications up to year $(t-4)$.

b. Approvals, applications, and share approved are for food retail in all regressions in column 1; nonfood retail in column 2.

c. Observations are weighted by the share of each year's total population in the département.

d. OLS Standard errors are in parentheses. Standard errors that allow for clustering of the observations at the département level are in brackets.

is both economically and statistically insignificant, weakening any concern of reverse causation.[27]

Another important question is whether the positive effect of approvals on employment persists over time. One could argue that the entry of a new megastore will create jobs in the short run but will eventually drive out smaller retailers, say due to lower average prices in the megastore. This could result in either no or even a negative effect on employment in the longer run. Column (2) provides some evidence against this argument. In column (2) we break down the stock of approvals as of $(t-4)$ into two components: the stock of approvals as of $(t-7)$ and the stock of approvals between $(t-4)$ and $(t-7)$. If the estimated employment effects are only short term, one would expect that stores approved earlier in time would have a weaker positive effect on current employment than stores approved more recently. In fact, we find that earlier approvals have a larger effect on retail em-

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27. We have also investigated these dynamic issues by adding as a control one-year lagged log(retail employment). The results were qualitatively unchanged.

TABLE V
EFFECT OF ENTRY REGULATION ON RETAIL EMPLOYMENT: ROBUSTNESS RESULTS

Dependent variable:	(1)	(2)	(3)	(4) log(hotels & restaurants employment)	(5) log (retail employment)
	log (retail employment)				
log(approvals)$_{(t-4)}$	.086	—	.053	.005	.054
(in square meters)	(.018)		(.028)	(.023)	(.022)
	[.027]		[.029]	[.044]	[.029]
log(approvals)$_{(t+1)}$	−.003	—	—	—	—
(in square meters)	(.022)				
	[.034]				
log(approvals)$_{(t-7)}$	—	.105	—	—	—
(in square meters)		(.028)			
		[.044]			
log(approvals)$_{(t-4)}$ −	—	.076	—	—	—
log(approvals)$_{(t-7)}$		(.028)			
		[.046]			
département F.E. * log(minimum wage)	no	no	yes	no	no
département F.E. * year	no	no	no	no	yes
Adjusted R^2	.936	.935	.943	.869	.945
N	1,588	1,403	1,604	1,683	1,683

Source: French LFS, 1980 to 1998; DECAS Applications Database, 1975 to 1998.
a. Each coefficient corresponds to a separate regression. All regressions include year fixed effects, département fixed effects, the logarithm of the stock of applications up to year $(t-4)$, and age and urban/rural demographic controls. "Age demographics" include fraction under 20, between 21 and 40, between 41 and 60, and above 60 years old in the département in year t. "Urban/rural demographics" include fraction living in rural areas, in cities of less than 20,000 people, in cities of between 20,000 and 200,000 people, and in cities of more than 200,000 people in the département in year t.
b. Observations are weighted by the share of each year's total population in the département.
c. OLS Standard errors are in parentheses. Standard errors that allow for clustering of the observations at the département level are in brackets.

ployment than later approvals. The difference between the two coefficients, however, is not statistically significant.

As we mention above, the year dummies included in equations (1) and (2) account for any aggregate economic and policy shocks that might affect retail employment. Among such aggregate shocks, changes in the minimum wage are especially relevant here. The retail trade industry has a disproportionately large share of low-wage workers and any change in the minimum wage is likely to have a substantial impact on employment in this

sector.[28] Because the minimum wage is set nationally, its effect on employment should be adequately captured by the year dummies as long as this effect is the same across départements. It is possible, however, that some départements are more affected than others by a given increase or decrease in the minimum wage. Specifically, one might expect the effect of the minimum wage to be stronger in a low-wage than in a high-wage département. Column (3) accounts for this concern by adding to equation (1) interactions between département fixed effects and the real minimum wage. The results are qualitatively unaffected.

Another way to verify that our entry regulation variables are not proxying for other policy or economic shocks targeted toward the less skilled is to ask whether other economic sectors with a disproportionately large fraction of low-wage workers also appear to be "affected" by the zoning regulation. We do this in column (4), where we reestimate equation (1) using the logarithm of employment in hotels and restaurants as the dependent variable. The effect of approvals on employment in the hotels and restaurants sector is neither economically nor statistically significant.

Finally, we saw in Table II that annual approval rates differ systematically along a set of département characteristics. For example, we saw that larger départements accept a larger fraction of applications. This raises the concern that retail employment may have evolved differentially over time across départements (for example, growing faster in larger départements) and that our entry regulation variables may in part be capturing such differential trends. We address this concern in column (5), where we allow retail employment to grow at a different average annual rate in each département. The estimated coefficient on approvals is statistically unchanged.

IV. RETAIL EMPLOYMENT EFFECTS: IV RESULTS

IV.A. Motivation

Our empirical strategy so far has relied on the assumption that applications are exogenous to the entry regulation and represent the number of stores that would have entered in a laissez-faire situation. Based on that assumption, we have defined a lower approval rate, or a lower stock of approvals conditional on

28. See, for example, Kramarz and Philippon [2001].

the stock of applications, as proxies for the stringency of entry deterrence. We identified two reasons why this assumption might be violated in practice and why the OLS results above might be biased. First, discouragement effect due to a positive cost of applying and a low probability of success may bias the stock of applications downward compared to the free entry number of entrants. On the other hand, the stock of applications may be an overestimate of the number of entrants in the absence of deterrence because of the rents created by the deterrence process itself.

Abstracting away from issues surrounding the endogeneity of applications, another possible source of concern with the OLS estimation of equations (1) and (2) is that approvals and approval rates might be correlated with unobserved shocks to retail employment. This could occur if zoning boards change their approval policy as a function of economic conditions in the retail industry. For example, zoning boards may authorize more stores when they expect retail demand to grow for some exogenous reasons and fewer stores in the opposite case. If zoning boards behave as such, this will bias our estimates in equations (1) and (2) upward. Alternatively, one could imagine that the quality of applications vary with unobserved shocks to retail employment. An expected positive shock to retail demand may induce promoters with lower quality or more marginal projects to apply. If zoning boards reject a larger fraction of such applicants, this will bias our estimates in equations (1) and (2) downwards.

The discussion above makes it clear that we need to assess the robustness of our results to a more exogenous source of variation in the stringency of entry regulation, one that does not directly rely on applications and accounts for the possible correlation between approvals and unobserved shocks to retail demand. To do so, we propose an instrumentation strategy that is based on the politics of entry regulation, a process of interest in its own right.

IV.B. The Politics of Entry Regulation

As we mentioned in Section II, the membership composition of zoning boards is such that the median voter on these boards is likely to be an elected politician. Recall that out of twenty voting members on a zoning board, nine are shopkeepers and craftsmen's representatives (and seven of those self-employed), two are consumer representatives, and the rest are locally elected politicians. Given that consumer representatives are anecdotally

known to favor entry, incumbent shopkeepers will need the support of at least some politicians to prevent entry. One might therefore expect political representation in a given département and a given year to affect the strength of entry deterrence in that département and year.

In fact, some further institutional knowledge might help us hypothesize as to which political parties might be most opposed to the entry of large stores. In its original spirit, the Loi Royer was introduced as a way to protect small shopkeepers from larger competitors. One might thus expect the political parties who get a disproportionate share of small shopkeepers' votes to be the strongest opponents to the creation of new large stores. As these parties try to maintain the political support of this group of electors, they may use their influence on the boards to limit entry.

In a book called *La Boutique Contre La Gauche* (*The Boutique Against the Left*), Mayer [1986] undertook a thorough investigation of the social and political behavior of small shopkeepers in France. She documents a strong political bias among small shopkeepers toward nonextremist right-wing parties. More specifically, she shows that the UDF ("Union pour la Démocratie Française") and the RPR ("Rassemblement Pour la République"), the two largest right-wing parties in France, receive a disproportionate fraction of shopkeepers' votes. Such a preference for right-wing parties, Mayer comments, is rather typical among the self-employed. Self-employed typically assume that left-wing parties in France, the most prominent of which being the PS ("Parti Socialiste"), will rather protect the interest of salaried workers.

Hence, in theory at least, a good predictor of the approval of new retail space in a given département and given year is the political representation in that département and year. More elected politicians from the right of the political spectrum would imply a median voter more likely to oppose entry; more elected politicians coming from the left of the political spectrum would imply a median voter more likely to favor entry. In practice, we will use the results of national elections to the Assemblée Nationale, France's main legislative body, to measure the representation of political parties by département.[29] We will use the cumulative representation of each political party in a département up to $(t-4)$ as instrument for the stock of approvals in that département up to $(t-4)$.

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29. The election data are presented in the Data Appendix.

While we have just argued that political representation may generate variation in entry by affecting the preferences of the median voter on the zoning boards, we further need to justify why instrumenting with political representation will solve the various endogeneity problems outlined above. First, and most importantly, the use of political representation as an instrument allows us to analyze the stringency of entry regulation *without* directly relying on applications in our empirical model. We measure the stringency of entry deterrence by the politically induced variation in the stock of approvals. This should eliminate any bias in the OLS models due to deviations of applications from the laissez-faire number of stores.

We further need to argue why political representation in a département may not be correlated with unobserved shocks to retail demand in that département. There are two specific sources of concern here. First, political business cycles models suggest that election outcomes might be driven by economic conditions. If shocks to retail employment correlate with general employment shocks in a département and election results correlate with these general employment shocks, this might induce a correlation between retail employment and political representation. One simple way to account for this empirically is to verify that our results are robust to controlling for the unemployment rate in the département. The relevance of this concern is further weakened by the fact that current political representation is not part of the identification strategy; only representation up to year $(t-4)$ is used as an instrument. For political business cycles to drive our results, one would require rather sophisticated voters who would be able to predict shocks to retail demand four years in advance and consequently adjust their voting behavior. In practice, we will also control for political representation in years t to $(t-3)$ to further alleviate any political business cycles concern.

Another source of concern is that political representation might capture local policies other than zoning. Fortunately, as we already stressed several times above, the decentralization of the zoning regulation is rather exceptional in France where local politicians have only very limited policy discretion. Also, the additional controls for political representation in years t to $(t-3)$ should also account for the possibility of other more recent local policy changes. More generally, we will test the validity of the political instruments by performing an overidentification test.

TABLE VI
FIRST-STAGE REGRESSIONS: EFFECT OF POLITICAL REPRESENTATION ON APPROVALS

Dependent variable:	(1)	(2)	(3)	(4)
	$log(approvals)_{t-4}$		$log(10$-year approvals$)_{t-4}$	
cumulated fraction RPR−UDF$_{(t-4)}$	−.047	.083	−.222	.147
	(.026)	(.101)	(.046)	(.126)
	[.110]	[.279]	[.189]	[.338]
cumulated fraction PS$_{(t-4)}$	.318	.447	.522	.909
	(.037)	(.116)	(.064)	(.158)
	[.142]	[.340]	[.246]	[.530]
cumulated fraction various right$_{(t-4)}$	—	.502	—	.555
		(.127)		(.144)
		[.323]		[.376]
cumulated fraction various left$_{(t-4)}$	—	−.545	—	.658
		(.295)		(.234)
		[.622]		[.642]
cumulated fraction extreme-right$_{(t-4)}$	—	.329	—	.472
		(.162)		(.209)
		[.387]		[.558]
cumulated fraction extreme-left$_{(t-4)}$	—	.101	—	.360
		(.104)		(.135)
		[.266]		[.450]
cumulated fraction green party$_{(t-4)}$	—	−.544	—	−.152
		(.258)		(.327)
		[.783]		[1.023]
Adjusted R^2	.912	.913	.853	.855
F-statistic for the nullity of instruments (p-value)	(.000)	(.000)	(.000)	(.000)
F-statistic for RPR−UDF = PS (p-value)	(.000)	(.000)	(.000)	(.000)

Source: French LFS, 1980 to 1998; DECAS Applications Database, 1975 to 1998; CEVIPOF Election Files, 1975 to 1998.

a. All regressions also include year fixed effects, département fixed effects, the logarithm of the stock of applications up to year $(t-4)$, and age and urban/rural demographic controls. "Age demographics" include fraction under 20, between 21 and 40, between 41 and 60, and above 60 years old in the département in year t. "Urban/rural demographics" include fraction living in rural areas, in cities of less than 20,000 people, in cities of between 20,000 and 200,000 people, and in cities of more than 200,000 people in the département in year t.

b. "cumulated fraction PS$_{(t-4)}$" in columns (1) and (2) is the cumulated fraction of votes going to the PS party from 1975 up to year $(t-4)$. "cumulated fraction PS$_{(t-4)}$" in columns (3) and (4) is the 10-year cumulated fraction of votes going to the PS party from 1975 to up to year $(t-4)$. All the other political variables are constructed accordingly. See the Data Appendix for further detail.

c. Observations are weighted by the share of each year's total population in the département.

d. OLS Standard errors are in parentheses. Standard errors that allow for clustering of the observations at the département level are in brackets.

e. Sample size in all regressions is 1,683.

IV.C. First-Stage Regressions

Table VI presents the first-stage regressions. We propose two different sets of instruments. The first set (columns (1) and (3))

focuses on the political representation of the major political parties only, i.e., the RPR and the UDF on the right wing and the PS on the left wing. Note that because the UDF and the RPR formed a coalition during some of the elections in the 1980s, we cannot systematically separate them in the empirical analysis. The second set (columns (2) and (4)) includes the political representation of all parties, major and minor. The dependent variable in columns (1) and (2) are the logarithm of the total stock of approvals up to year $(t-4)$. The dependent variable in columns (3) and (4) is the logarithm of the ten-year stock of approvals up to year $(t-4)$. We construct the political instruments accordingly. For example, Fraction PS in column (1) is the cumulated fraction of votes going to the Socialist Party from 1975 until $(t-4)$.[30] All regressions include, in addition to the political variables, year fixed effects, département fixed effects as well as controls for the age and rural/urban demographics in the département. The missing category among the political variables in columns (1) and (3) is the fraction of votes going to all minor parties, abstentions, and null votes. The missing category in columns (2) and (4) is the fraction of abstentions and null votes.

Table VI verifies that political representation in a département is an important determinant of approvals. More specifically, the first-stage regressions confirm the expected divide between RPR/UDF and PS. Consider column (1), for example. A 1 percent increase in the political representation of the RPR and UDF decreases the stock of approvals by .05 percent. A 1 percent increase in the PS representation increases the stock of approvals by .31 percent. The difference between the estimated effects for PS and RPR-UDF representation is statistically significant in all specifications. This sharp contrast between the RPR-UDF and PS is stable across all specifications. The picture is a little murkier and less stable when it comes to the influence of the minor political parties. One interesting finding is that increased representation by the green party seems to be associated with a stronger deterrence of entry, even though the coefficient is imprecisely estimated. Finally, note that the political variables are jointly significant for each of the first-stage regressions in Table VI.

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30. See the Data Appendix for further detail on the construction of the political variables.

TABLE VII

EFFECT OF ENTRY REGULATION ON RETAIL EMPLOYMENT: IV RESULTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
							log(hotels & restaurants employment)
			log(retail employment)				
Dependent variable:	IV1	IV2	IV1	IV2	IV2	IV2	IV2
log(approvals)$_{(t-4)}$							
(in square meters)	.132	.134	—	—	.127	.104	.000
	(.054)	(.047)			(.046)	(.038)	(.063)
	[.057]	[.049]			[.047]	[.039]	[.068]
log(10-year approvals)$_{(t-4)}$							
(in square meters)	—	—	.122	.123	—	—	—
			(.043)	(.039)			
			[.047]	[.043]			
political composition (t, $t-1$, $t-2$, and $t-3$)	no	no	no	no	no	yes	yes
unemployment rate	no	no	no	no	yes	yes	no
χ^2 Overidentification test (p-value)	(.477)	(.901)	(1.00)	(.626)	(.694)	(.969)	(.143)

Source: French LFS, 1980 to 1998; DECAS Applications Database, 1975 to 1998; CEVIPOF Election Files, 1975 to 1998.

a. Each regression is estimated using IV. In IV1, the instruments are "cumulated fraction RPR−UDF$_{(t-4)}$" and "cumulated fraction PS$_{(t-4)}$." In IV2, the instruments are "cumulated fraction RPR−UDF$_{(t-4)}$," "cumulated fraction PS$_{(t-4)}$," "cumulated fraction various right$_{(t-4)}$," "cumulated fraction various left$_{(t-4)}$," "cumulated fraction extreme-right$_{(t-4)}$," "cumulated fraction extreme-left$_{(t-4)}$" and "cumulated fraction green party$_{(t-4)}$." "cumulated fraction PS$_{(t-4)}$" is the cumulated fraction of votes going to the PS party from 1975 up to year ($t-4$). All the other political variables are constructed accordingly. See the Data Appendix for further detail.

b. All regressions also include year fixed effects, département fixed effects, the logarithm of the stock of applications up to year ($t-4$), and age and urban/rural demographic controls. "Age demographics" include fraction under 20, between 21 and 40, between 41 and 60, and above 60 years old in the département in year t. "Urban/rural demographics" include fraction living in rural areas, in cities of less than 20,000 people, in cities of between 20,000 and 200,000 people, and in cities of more than 200,000 people in the département in year t. "Unemployment rate" is the unemployment rate in the département in year t. "Political composition (t, $t-1$, $t-2$, and $t-3$)" is a vector of representations for each political party in the département in year t, ($t-1$), ($t-2$), and ($t-3$).

c. Observations are weighted by the share of each year's total population in the département.

d. Standard errors are in parentheses. Robust standard errors are in brackets.

e. Sample size in all regressions is 1,683.

IV.D. Second-Stage Regressions

Table VII displays the effect of entry regulation on retail employment in the IV model. All regressions include year fixed effects, département fixed effects, and the usual vector of age and rural/urban demographic variables. Robust standard errors are in brackets. Columns (1) and (4) display the basic specifications,

the first-stages of which are columns (1) and (4) of Table VI respectively. In all cases, we find a positive relationship between retail employment and the stock of approvals. The results are very similar under both sets of instruments and qualitatively similar to the OLS results in Table III. The estimated effects of entry regulation on employment are about 50 (columns (1) and (2)) to 100 (columns (3) and (4)) percent larger here than under the OLS models. The χ^2 overidentification test at the bottom of Table VII does not reject the null hypothesis of orthogonality of the IV residuals to the instruments.

In column (5) we replicate column (2) but add a control for the current unemployment rate in the département. The point estimate is unchanged. In column (6) we add, in addition to the unemployment rate, controls for the current, one-year, two-year, and three-year lagged political representation in the département. The introduction of these controls reduces the estimated coefficient on log(approvals) (.104 versus .127). Column (7) replicates column (6) but uses the logarithm of employment in hotels and restaurants as a dependent variable. We find no effect of entry regulation, as predicted by political representation, on employment in hotels and restaurants. This last result suggests that the instruments are unlikely to capture other policy changes targeted toward the less skilled.

IV.E. Magnitude of Effect

How important are these effects? How much larger would retail employment be today if free entry had occurred? As we discussed earlier, there is no foolproof way for us to answer these questions. Unfortunately, the applications database, while very rich, does not provide an obvious counterfactual for what the number of stores would have been free of regulation. However, we try below to give possible orders of magnitude under more and less conservative sets of assumptions.

If one is willing to assume that cities represent independent retail markets, one can use the distribution of applications and approvals by city to provide a lower bound on the size of the effect. As we mention earlier, the main reason why we might suspect the stock of applications to be an overestimate of the laissez-faire number of stores is if multiple applications are submitted over time for the same investment opportunity. One could therefore estimate a lower bound of the long-run employment effect of deterrence by assuming that, had free entry occurred:

(1) exactly one extra store would have been constructed in all the cities where applications were submitted but none was successful, and (2) exactly one extra store would have been constructed in all the cities where additional applications were submitted following the last approval. While this exercise is difficult to carry out for nonfood retail (given the diversity of investment projects represented there), one can perform this exercise in the food sector. About 500 of the 2,300 cities where applications were recorded in the food retail sector ended up without any approval by the end of the sample period. In addition, we computed that new applications occurred in about 200 extra cities following the last approval. Hence, under that conservative scenario, about 700 extra stores would have been constructed free of deterrence. Assuming that all stores are the same size, this would correspond to about a 30 percent increase in the stock of approvals in food retailing.[31] Given an elasticity of employment to approvals of about .1 (see Table IV), this would imply a 3 percent increase in food retail employment.

A second, somewhat less conservative, quantification exercise can be carried out for the entire retail sector. We assess the effect of moving a département from the first quartile of the approval rate distribution (about 30 percent) to the third quartile of that distribution (about 50 percent). Such a move corresponds to about a two-thirds increase in the stock of approvals. Given an elasticity of employment to approvals of about .1 (a conservative average of the OLS and IV estimates), this would imply about a 7 percent increase in retail employment.

Finally, one could assume at the other extreme that all applications truly represent stores that would have been constructed if free entry had occurred. With an average approval rate of 40 percent, this would imply a 150 percent in the stock of approvals and a 15 percent increase in retail employment. It is important to note that even this scenario is not an absolute upper bound for the impact of entry regulation as it assumes away any discouragement effects.

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31. About 8,000 applications were submitted in food retailing, and the approval rate in that sector was a little less than 30 percent.

V. RETAILER CONCENTRATION AND RETAIL PRICES

In this section we investigate some specific mechanisms by which the Loi Royer may have hindered employment. One plausible causal channel is through concentration and prices. Fewer approvals by the zoning boards may have increased large retailers' market power, leading to higher markups, lower sales, and lower employment. As we discussed in the introduction, this mechanism is central to most of the theoretical work on the link between product market regulation and employment. However, there might be other, more industry-specific, mechanisms. First, high concentration may hurt employment through other channels than its impact on prices and sales. Concentration may also slow product differentiation among large retailers. In the retail industry, more product differentiation may lead to the development of higher value, more labor-intensive stores [McKinsey Global Institute 1997]. In the U. S. food retail sector, for example, high quality chains have developed in parallel with lower quality ones. Because the high quality chains compete less on prices but more on the multiple services that they provide to customers, they are typically more labor intensive.

Also, the Loi Royer may have hindered employment growth independently of any impact on retailer concentration. For example, some authors have noted the importance of positive spillover effects in retail trade, whereby the creation of a large anchor store induces enough customer traffic to encourage the creation of additional stores (and jobs). Using evidence on rents paid by anchors and nonanchor stores in U. S. shopping malls, Pashigian [1998] showed that the positive externalities created by anchor stores can be quite substantial. The Loi Royer, by restraining the creation of large anchor stores, may have prevented such positive spillover effects from occurring.[32] Finally, the regulation of entry in a service sector such as retail trade may lead to trading off employment for customers' time. Limiting the number of stores forces customers to face longer commuting time to the closest supermarket and longer waiting times once inside the store. In other words, with fewer large stores available to shop at, customers' time becomes a more important input in the retail industry

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32. It is interesting to note that while restaurants may also benefit from the development of anchor stores and malls, we did not find any apparent spillover of the zoning regulation into the hotels and restaurants sector.

production function and operates as a substitute for retail employment.

While all the channels above would deserve empirical investigation, data limitations seriously limit the scope of this exercise. Instead, we focus in this section on the effect of the Loi Royer on the concentration of large retail chains in France and on retail prices.

Supermarkets and megastores in France operate in a rather concentrated market environment.[33] In a recent study, Dobson and Waterson [1999] report that, in 1997, sales of the top five chains in France represented 31 percent of total sales in the retail industry. In food retail, the market share of the top five retailers increased from 42 to 52 percent between 1988 and 1996. As a comparison, the market share of the top five food retailers in the United Kingdom, which has the most concentrated retail industry in Europe, went from 53 to 64 percent over the same time period. Dobson and Waterson also show that retailer concentration across European countries is positively correlated with price indices.

It has been anecdotally suggested that the Loi Royer, under the guise of protecting small shopkeepers, was de facto used to limit product market competition among large retail chains and might thus have played a nonnegligible role in the development of such a concentrated retail environment in France. More specifically, incumbent stores are said to exert influence on the zoning boards to restrain the entry of competing chains and thereby protect their local market power.[34] We used the applications database to more formally test this story. We again focused on food retailing in order to isolate a more uniform industrial sector. Using city-level data, we showed that the probability of approval for a new applicant was about 5 percent lower when the applicant had a different brand name than a store previously approved in that city.[35]

More generally, we show in Table VIII that the stringency of deterrence is positively related to the concentration of large retail chains and to retail prices. Again, we concentrate on the food

33. A megastore is defined as a store with over 2,500 square meters of retail space. A supermarket is defined as a store with between 400 and 2,500 square meters of retail space.
34. Personal conversion, DECAS. Remember that representatives for the local supermarkets and megastores are also appointed to the zoning boards.
35. These results are available from the authors upon request.

TABLE VIII

EFFECT OF ENTRY REGULATION ON CONCENTRATION AND PRICES:
EVIDENCE FROM FOOD RETAIL

Dependent variable:	(1) 1-firm concentration ratio	(2) 2-firm concentration ratio	(3) Herfindahl index	(4) log(price)
log(approvals)	−.071	−.054	−.075	−.017
(in square meters)	(.008)	(.007)	(.009)	(.004)
	[.022]	[.017]	[.025]	[.005]
share approved	−.139	−.078	−.146	−.045
	(.032)	(.026)	(.035)	(.020)
	[.082]	[.063]	[.089]	[.021]
N	1890	1890	1890	90344

Source: French LFS, 1975 to 1998; DECAS Applications Database, 1975 to 1998; Price Database, 1994 to 1998.

a. "log(approvals)" and "share approved" are measured in year t in columns (1) to (3), year $(t-4)$ in column (4).

b. "1-firm concentration ratio" is the largest market share in the département in year t. "2-firm concentration ratio" is the sum of the two largest market shares in the département in year t. "Herfindahl index" is the sum of the squares of the market shares in the département in year t. See the Data Appendix for further detail.

c. Each coefficient corresponds to a separate regression. All regressions include year fixed effects, département fixed effects, and age and urban/rural demographic controls. "Age demographics" include fraction under 20, between 21 and 40, between 41 and 60, and above 60 years old in the département in year t. "Urban/rural demographics" include fraction living in rural areas, in cities of less than 20,000 people, in cities of between 20,000 and 200,000 people, and in cities of more than 200,000 people in the département in year t. Regressions in row 1 also include the logarithm of the stock of applications up to year t (columns (1) to (3)) or $(t-4)$ (column (4)). Regressions in column (4) also include dummies for specific food items and store types.

d. OLS Standard errors are in parentheses in columns (1) to (3). Standard errors that allow for clustering of the observations at the département-year level are in parentheses in column (4). Standard errors that allow for clustering of the observations at the département level are in brackets.

retail sector. We start with our analysis of concentration effects (columns (1) to (3)). We use the brand names of approved food stores in the applications database to construct three different measures of retailer concentration at the département/year level: the market share of the largest retailer (one-firm concentration ratio), the market share of the two largest retailers (two-firm concentration ratio), and the Herfindahl index. One substantial measurement problem with these concentration measures is that they do not account for supermarkets and megastores' presence prior to the inception of the zoning regulation, a piece of information that is not available to us.[36] However, these concentration

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36. This measurement problem is somewhat minimized by the fact that supermarkets and megastores' market shares were rather small in the early 1970s. In 1970, megastores' market share was 3.6 percent; supermarkets' market

measures should at least inform us as to how the zoning regulation shaped the structure of competition among approved stores. All regressions in Table VIII include year and département fixed effects as well as the usual vector of demographic characteristics. We correlate the three concentration measures with the contemporaneous stock of approvals (row 1) and with the contemporaneous share approved (row 2). We find that all three measures of concentration are strongly positively related to the strength of entry deterrence. Increases in either the number or the fraction of approvals reduce the concentration of large retail chains.

In column (4) we study the effect of the Loi Royer on retail prices in the food sector. This study is unfortunately subject to serious data limitations. Consumer price indices do not exist at the département level in France. However, we were able to obtain from INSEE, the French Statistical Institute, micro-level price data from 1994 to 1998. These are the data collected to compute the CPI in France.[37] Each data point corresponds to the retail price of a specific product in a specific store at a given point in time. From the full sample of products represented, we selected approximately a hundred food products labeled "homogeneous" by INSEE, i.e., products for which there is little scope for quality differentiation.[38]

We regress log(price) on the two four-year lagged measures of entry regulation. Each regression includes dummy variables for the specific products, dummy variables for store types, year fixed effects, département fixed effects, and the usual set of age and urban/rural demographic controls. The standard errors reported in parentheses allow for group effects at the département-year level; those reported in brackets allow for group effects at the département level. We find that food prices go down when zoning boards authorize more entry.[39]

It is interesting to compare the estimated elasticity of prices to approvals with that of employment (Table IV, columns (1) and

share was 9 percent. These shares, respectively, were up to 33 percent and 28.3 percent in 1997 [Allain and Chambolle 1999].

37. See the Data Appendix for a more detailed description for the price database.

38. For confidentiality reasons, INSEE does not allow us to reveal a list of the sampled products.

39. Bertrand, Biscourp, Haller, and Kramarz [2000] present complementary evidence on retail prices and entry deterrence at the city level. In that paper we also analyze the differential effect of megastores and supermarkets entry for retail prices.

(2)). The elasticity of price appears smaller. The estimated coefficients in the price regressions are about a fifth to a third of the estimated coefficients in the employment regressions. This would imply unreasonably high price elasticity of food demand for prices to be the sole causal channel here. This suggests that some of the other mechanisms outlined above may also be at play. Concentration may hurt employment through other channels than price, and entry regulation may hurt employment through other mechanisms than its impact on retailer concentration. One important caveat, though, is that the price and employment sensitivities are computed over different sample periods. Because the panel length for the price data is much shorter than for the employment data, one may not want to push this inference exercise too far.[40]

In Table IX we directly assess the impact of retailer concentration on employment. We correlate employment in food retailing with the three concentration measures described above. Each regression includes the usual set of controls. As expected, we find that employment is strongly negatively correlated with each concentration measure (columns (2), (4), and (6)). We also try to separate the effect of entry regulation and retailer concentration. In columns (3), (5), and (7) we allow for both a measure of entry deterrence ($log(approvals_{(t-4)})$ and a measure of retailer concentration in the same regression. We find that both measures are negatively related to employment.

It is interesting to note that the direct effect of entry deterrence on employment (row 1) drops by about 20 percent when we include a concentration measure in the regression but that this effect stays economically and statistically significant. Again, this suggests that the impact of entry deterrence on retailer concentration, while it does appear to be an important channel for the employment effects, might not be the only one. There is, however, one important caveat to this statement. Départements might not be the right geographical unit to study the impact of concentration. Instead, the analysis above should best be carried out in smaller geographic units. Unfortunately, the LFS survey is too small for us to compute employment levels, say, at the city level.

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40. We reestimated the employment elasticities over the shorter time period (1994 to 1998). They did appear smaller in magnitude but were too imprecisely estimated to justify any inference based on them.

TABLE IX

EFFECT OF CONCENTRATION ON EMPLOYMENT: EVIDENCE FROM FOOD RETAIL

	Dependent variable: log(food retail employment)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log(approvals)$_{(t-4)}$	.106	—	.087	—	.085	—	.086
(in square meters)	(.018)		(.019)		(.019)		(.038)
	[.040]		[.037]		[.037]		[.038]
1-firm concentration	—	−.278	−.168	—	—	—	—
ratio$_{(t-4)}$		(.060)	(.062)				
		[.129]	[.097]				
2-firm concentration	—	—	—	−.376	−.258	—	—
ratio$_{(t-4)}$				(.072)	(.075)		
				[.166]	[.130]		
Herfindahl index$_{(t-4)}$	—	—	—	—	—	−.250	−.148
						(.054)	(.057)
						[.117]	[.090]
Adjusted R^2	.854	.848	.851	.848	.852	.848	.851

Source: French LFS, 1980 to 1998; DECAS Applications Database, 1975 to 1998.

a. "1-firm concentration ratio" is the largest market share in the département in year t. "2-firm concentration ratio" is the sum of the two largest market shares in the département in year t. "Herfindahl index" is the sum of the squares of the market shares in the département in year t.

b. All regressions include year fixed effects, département fixed effects, and age and urban/rural demographic controls. "Age demographics" include fraction under 20, between 21 and 40, between 41 and 60, and above 60 years old in the département in year t. "Urban/rural demographics" include fraction living in rural areas, in cities of less than 20,000 people, in cities of between 20,000 and 200,000 people, and in cities of more than 200,000 people in the département in year t. Regressions in columns (1), (3), (5), and (7) also include the logarithm of the stock of applications up to year $t-4$.

c. In column (2) observations are weighted by the share of each year's total population in the département.

d. OLS Standard errors are in parentheses. Standard errors that allow for clustering of the observations at the département level are in brackets.

VI. OTHER LABOR MARKET OUTCOMES

In Table X we study the effect of the entry regulation on a broader set of labor market outcomes. Each coefficient in Table X corresponds to a separate regression. We estimate the basic empirical model (equation (1) in column (1) and the IV model in column (2)). In the IV model we also control for the current, one-year, two-year, and three-lagged political representation in the département.

We start by assessing the effect of the Loi Royer on overall employment in the département (row 1). The specific question we want to address here is whether the apparent effect on retail employment represents simply a redistribution of employment across industrial sectors or whether overall employment did indeed change as a result of the zoning regulation. The estimated coefficient on "log(approvals)$_{i(t-4)}$" in column (1) is positive but

TABLE X
EFFECT OF ENTRY REGULATION ON OTHER LABOR MARKET OUTCOMES:
COEFFICIENT ON LOG(APPROVALS)$_{(t-4)}$ i.o. $(r-4)$

	(1)	(2)
	Basic	IV2
Dependent variable:		
log(overall employment)	.004	.030
	(.008)	(.017)
	[.016]	[.022]
log(weekly earnings) (retail)	−.009	−.021
	(.009)	(.022)
	[.012]	[.025]
fraction male workers (retail)	−.012	−.011
	(.006)	(.012)
	[.008]	[.013]
fraction male workers (overall)	−.002	−.010
	(.001)	(.003)
	[.002]	[.005]
fraction young workers (retail)	−.000	.000
	(.002)	(.006)
	[.003]	[.007]
fraction young workers (overall)	.000	−.003
	(.001)	(.001)
	[.001]	[.002]
fraction in small firms (retail)	−.015	−.150
	(.008)	(.020)
	[.014]	[.026]

Source: French LFS, 1980 to 1998; DECAS Applications Database, 1975 to 1998; CEVIPOF Election Files, 1975 to 1998.

a. In column (1) we estimates equation (1) for the different dependent variables. The instruments included in the IV regressions (column (2)) are "cumulated fraction RPR−UDF$_{(t-4)}$," "cumulated fraction PS$_{(t-4)}$," "cumulated fraction various right$_{(t-4)}$," "cumulated fraction various left$_{(t-4)}$," "cumulated fraction extreme-right$_{(t-4)}$," "cumulated fraction extreme-left$_{(t-4)}$" and "cumulated fraction green party$_{(t-4)}$." "cumulated fraction PS$_{(t-4)}$" is the cumulated fraction of votes going to the PS party from 1975 up to year $(t-4)$. All the other political variables are constructed accordingly. See the Data Appendix for further detail.

b. Each coefficient represents the result of a different regression. All regressions also include year fixed effects, département fixed effects, the representation of each political party in the département in year t, $(t-1)$, $(t-2)$ and $(t-3)$, and age and rural/urban demographics controls. Age demographics include fraction under 20, between 21 and 40, between 41 and 60, and above 60 years old in the département in year t. Urban/rural demographics include fraction living in rural areas, in cities of less than 20,000 people, in cities of between 20,000 and 200,000 people, and in cities of more than 200,000 people in the département in year t. Each regression in column (1) also includes the logarithm of the stock of applications up to year $(t-4)$. Each regression in column (2) also includes a vector of representations for each political party in year t, $(t-1)$, $(t-2)$, and $(t-4)$.

c. Observations are weighted by the share of each year's total population in the département.

d. Standard errors are in parentheses. Standard errors that allow for clustering of the observations at the département level are in brackets in column (1). Robust standard errors are in brackets in column (2).

statistically insignificant. Because retail employment constitutes about 7.5 percent of total employment in France, the point estimate (.004) suggests a slightly less than proportional increase in

total employment.[41] The IV estimate in column (2) is less noisy. The point estimate is also positive and suggests a more than proportional increase in overall employment.[42]

The second dependent variable we consider is weekly earnings for salaried workers in the retail industry (row 2).[43] There is a large U. S. literature on the effect of product market deregulation on industry wages. Workers in the trucking industry experienced a substantial decline in earnings postderegulation [Rose 1987]. Earnings also declined after the deregulation of the airline industry [Card 1986, 1997] and of the banking industry [Black and Strahan 2001], although more modestly.[44] These negative earnings effects have been attributed in part to a decline in workers' bargaining power and in part to a decline in the size of the rents to be shared between firms and workers postderegulation. It is not clear how large these effects could be in the French retail sector. Indeed, because a large fraction of retail workers are paid at or around the minimum wage, there is little scope for downward wage flexibility following increases in approvals. In practice, we find a negative relationship between wages and approvals. The effect, however, is statistically insignificant. The point estimate suggests that the elasticity of wages with respect to approvals is about a fifth of the elasticity of employment in the IV model.

We next focus on the relative employment of two groups that are, a priori, most likely to be affected by changes in retail trade: women and youth. In row 3 we show that less entry deterrence reduces the fraction of male workers in the retail industry. The effect, however, is not statistically significant in the IV model and only marginally significant in the basic model. There are several possible interpretations for this negative sign. First, it is consis-

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41. Note that, in a regression not reported here, we broke down earlier and later approvals as we did in column (2) of Table V. Interestingly, we found a stronger, more than proportional, effect of earlier approvals on overall employment, consistent with possible spillovers of the entry regulation onto other industrial sectors. This estimate, however, was again very imprecise.

42. In regressions not reported here, we tried to assess the sensitivity of employment in specific industrial sectors to the zoning regulation. While the estimates were all very noisy, there was an apparent positive sensitivity to approvals in some manufacturing sectors, especially food processing and consumer goods manufacturing.

43. This variable is available in the LFS data starting in 1982.

44. In contrast, there was close to no earnings decline following the deregulation of the railroad industry. A more complete literature review is available in Peoples [1998]. See also Fortin and Lemieux [1997] for a review of the effect of deregulation (among other institutional changes) on the increase in income inequality in the United States.

tent with the presence of taste discrimination against women in the retail industry, a discrimination that is easier to sustain in the absence of intense competitive pressures [Becker 1957].[45] An alternative interpretation is that large stores offer more flexible work schedules and positions (such as cashier jobs) and may therefore attract a larger fraction of female workers.

In row 4 we assess the effect of entry regulation on the general labor market performance of women. Interestingly, we find that the fraction of men in overall employment also decreases as entry deterrence falls. The effect is only statistically significant in the IV model. The magnitude of this effect, compared with that for retail employment only (row 3), indicates that entry regulation might hurt female employment in sectors other than retail trade. As we suggested earlier, one plausible interpretation for this effect is that freer entry has reduced shopping time and eased the transition of some women into the labor force.

In rows 5 and 6 we investigate whether youth employment was also affected by the Loi Royer. We consider the effect of approvals on the fraction of young workers (less than twenty years old) both in retail employment (row 5) and in overall employment (row 6). Maybe somewhat surprisingly, we find no consistent effect of the zoning regulation on the relative employment of the youth.

Finally, row 7 displays the effect of the regulation on the fraction of retail workers who are employed in small establishments (fewer than ten workers). Recall that the original motivation behind the Loi Royer was to protect small mom-and-pop stores from the rapid growth of larger retail establishments. Not surprisingly, the findings in row 7, especially the IV results, suggest that entry regulation indeed helped protect smaller retailers' market share.

VII. SUMMARY AND CONCLUSION

Our findings suggest that the zoning regulation introduced in France in the early 1970s to restrain the development of large retail stores has had a negative impact on employment. The slow employment growth witnessed in the French retail sector over the

45. In a related paper Black and Strahan [2001] find that the deregulation of state-level restrictions on bank expansion in the United States led to a reduction in the male-female earnings gap in the banking industry.

last two or three decades may therefore not only be the result of labor market rigidities. Instead, barriers to entry and high levels of concentration among large retail chains may also have played a significant role.

Although the results in this paper are specific to the retail trade sector in France, we believe that the lesson we learned is of much more general interest. Specifically, countries fighting sluggish rates of job creation may be misguided in thinking that their employment problem will be fully solved once they reform their labor markets. Instead, elimination of entry regulation and other forms of product market restrictions may provide an additional mechanism to bolster employment growth.

This lesson is, we believe, especially relevant when one considers the political economy of reforms. Labor market deregulation has proved to be extremely difficult to implement in continental Europe. Labor market reforms are perceived as a direct threat by most incumbent workers and rarely receive the support of a majority of voters. Product market reforms, on the other hand, may not carry such strong negative connotation in the public opinion and may therefore be easier to introduce.

We hope to extend this paper in several directions in the future. First, a careful and systematic study of profitability margins in the retail industry in the United States and European countries would complement the evidence we have put together so far. Second, while we have focused on the retail industry, product market restrictions have been placed in other industrial sectors and deserve as much scrutiny. Finally, while we have shown that product market regulation affects job creation, it is not the only relevant form of nonlabor market distortions. For example, financial markets also operate under many more constraints in Europe than in the United States. Studying whether such capital market regulations have also hindered firm creation and employment growth is another priority for future research.

DATA APPENDIX

Labor Force Data. In March of every year the French statistical institute (INSEE) conducts a Labor Force Survey (LFS). The LFS is a household survey. Each individual in a household is asked a set of standard questions that are repeated in every LFS. Roughly 60,000 households are interviewed every year. We restrict our attention to all so-called "département de France Mét-

ropolitaine," i.e., we exclude the Antilles, La Réunion, and other maritime provinces. However, we include Corsica. There are 95 such départements. Although the structure of the questionnaire changed in 1975, 1982, and 1990, most variables of interest are available across survey years. The only exception is earnings information, which became available in the LFS only starting in 1982. The LFS includes a four-digit industry code (the Nomenclature d'Activités et de Produits—NAP—up to 1992 and the Nomenclature d'Activités Française—NAF—starting in 1993), which we use to measure employment in different industrial sectors. We use sampling weights to construct the variables of interest by département-year cell. Means and standard deviations for the main variables of interest are reported in Appendix 1.

Applications Database. The "Direction du Commerce et de l'Artisanat" (DECAS) at the Ministry of Economics and Finance has maintained an exhaustive listing of all applications submitted to any zoning board since the inception of the Loi Royer. DECAS has given us access to that listing for the period 1975 to 1998. The records were kept on paper from 1975 to 1986 and in an electronic format thereafter. We created an electronic version of the 1975 to 1986 files. During that period 16,278 different applications were submitted. For almost every application, the DECAS data record the brand name of the applicant, size of the store (in square meters), specialty of the store (food or nonfood with varying detail depending on the year), the location of the store (département and city), terms of the application (whether seeking to create or expand a previously existing store), and the outcome of the commission vote (an indicator for whether the store was authorized or rejected, as well as the number of "yes" votes, number of "no" votes, and number of abstentions).

We use the DECAS data to compute stocks of applications and approvals by département/year cell. More specifically, the different variables used in our empirical analysis are defined as follows.

- Log(approvals)$_t$ is the logarithm of the stock of approvals (in square meters) from January 1, 1975, up to January 1 of year t in a given département. For example, log(approvals)$_t$ when $t = 1990$ is the logarithm of the sum of all approvals between January 1, 1975, and January 1, 1990.
- Log(applications)$_t$ is the logarithm of the stock of applications (in square meters) from January 1, 1975, up to Janu-

ary 1 of year t in a given département. For example, log(applications)$_t$ when $t = 1990$ is the logarithm of the sum of all applications between January 1, 1975, and January 1, 1990.

- Share approved$_t$ is the ratio of stock of approvals (in square meters) from January 1, 1975, up to January 1 t to the stock of applications (in square meters) over the same period in a given département.

We also constructed ten-year and fifteen-year stocks to allow for possible depreciation of the retail capital stock. For example, log(10-year approvals)$_t$ is the logarithm of the stock of approvals (in square meters) from January 1 of year max $(1975, (t - 10))$ up to January 1 of year t in a given département. Hence, when $t = 1990$, log(10-year approvals)$_t$ is the logarithm of the sum of all approvals between January 1, 1980, and January 1, 1990. Finally, we also computed all the variables above separately for food and nonfood retailing.

We also used the DECAS data (and the information it provides about applicants' brand names) to compute three measures of retailer concentration in food retailing. These measures are defined as follows.

- 1-firm concentration ratio$_t$ is the share of the biggest chain in the stock of approvals up to year t in a given département.
- 2-firm concentration ratio$_t$ is the share of the two biggest chains in the stock of approvals up to year t in a given département.
- Herfindahl index is the sum of the squares of the market shares in the stock of approvals up to year t.

Means and standard deviations for these variables as well as for annual approval rates are presented in Appendix 2.

CEVIPOF Election Files. The "Centre d'Etudes de la Vie Politique Française" (CEVIPOF) is a research institute that focuses on the political practices of French citizens. It maintains a database on all national elections to the Assemblée Nationale, France's main legislative body. The CEVIPOF records the breakdown of votes by département for each of the major French political parties. From the right to the left of the political spectrum, these major political parties are the "extreme right," various small right-wing parties labeled as "various right," the "Rassemblement Pour la République" (RPR, and its other de-

nominations in various election years), the "Union pour la Démocratie Française" (UDF, and its other denominations in various election years), the "Parti Socialiste" (PS, and its other denominations in various election years), other small left-wing parties labeled as "various left," and the "extreme left," which mostly accounts for the communist party. Finally, the data also isolate the green party as an independent entity. In our empirical strategy, we group together the two major right-wing parties in France, UDF and RPR. These two parties formed a coalition during some of the elections of the 1980s and cannot be systematically separated over the period under study.

Legislative elections in France typically have two rounds. Under French voting law, only the subset of political parties that have performed well enough in the first round are allowed to stay on for the second round. In this paper we will focus on the results of the first round since they best reflect voters' preferences in a given year and département. Elections took place in 1973, 1978, 1981, 1986, 1988, 1993, and 1997.

We use the CEVIPOF data to construct our instruments. For example, cumulated fraction PS_t is the sum of the fraction of votes going to the PS each year from 1975 up to year t in a given département. For years in between elections, we use the results of the most recent election. All the other instruments are constructed accordingly.

Means and standard deviations for the instruments as well as annual representations of each party are reported in Appendix 3.

Price Database. INSEE releases every month a Consumer Price Index (CPI). This index is computed from a list of prices recorded in a sample of retail stores in metropolitan France. We obtained access to the full sample of prices observed between June 1994 to December 1998. From that sample, we selected approximately a hundred food products labeled "homogeneous" by INSEE; i.e., products for which there is little scope for quality differentiation. For each price, we know the date, the exact location of the retail store (city, département), the type of store (hypermarkets, supermarkets, discounters, "superettes," "magasins populaires," farmers markets, or traditional shops). For our analysis, we collapse the monthly data to the annual level. We use as dependent variable the average price of a specific product in a specific store in a given year. In order to preserve the index from possible manipulations, INSEE does not permit us to reveal

the exact nature of the products included in our analysis. Our sample contains 90,344 product/store/year observations.

APPENDIX 1: SUMMARY STATISTICS: LFS DATA

	Mean	Standard deviation
Sample: population		
fraction living in rural areas	.365	.186
fraction living in cities of less than 20,000 people	.197	.112
fraction living in cities of between 20,000 and 200,000 people	.233	.162
fraction living in cities of more than 200,000 people	.205	.308
fraction under 20 years old	.108	.022
fraction between 21 and 40 years old	.358	.047
fraction between 41 and 60 years old	.288	.025
fraction above 60 years old	.245	.058
log (overall employment)	12.05	.720
log (retail employment)	9.42	.796
log (food retail employment)	8.53	.806
log (nonfood retail employment)	8.84	.884
log (hotels and restaurants employment)	8.44	.898
Sample: all employed		
fraction in retail	.031	.011
fraction in food retail	.031	.011
fraction in nonfood retail	.043	.014
fraction in hotels and restaurants	.030	.016
fraction male workers	.573	.035
fraction young workers (less than 20 years old)	.027	.014
fraction in small firms (1 to 9 workers)	.405	.148
log (weekly earnings)	8.08	.114
Sample: all employed in retail sector		
fraction male workers	.403	.087
fraction young workers (less than 20 years old)	.048	.045
fraction in small firms (1 to 9 workers)	.544	.142
log (weekly earnings)	7.86	.152

Source: French LFS, 1980 to 1998. The unit of observation is a département-year cell.

APPENDIX 2: SUMMARY STATISTICS: APPLICATIONS DATABASE

	Mean	Standard deviation
Sample: all applications		
annual approval rate	.420	.369
log (approvals)	10.60	1.53
log(15-year approvals)	10.55	1.50
log(10-year approvals)	10.43	1.46
log(applications)	11.59	1.45
log(15-year applications)	11.54	1.42
log(10-year applications)	11.41	1.36
share approved	.379	.198
10-year share approved	.391	.207
15-year share approved	.382	.199
Sample: food retail applications		
annual approval rate	.306	.373
log(approvals)	9.68	1.30
log(applications)	10.82	1.30
share approved	.283	.198
1-firm concentration ratio	.538	.265
2-firm concentration ratio	.737	.220
Herfindahl index	.438	.297
Sample: nonfood retail applications		
annual approval rate	.450	.394
log(approvals)	10.19	1.54
log(applications)	11.01	1.47
share approved	.448	.207

Source: DECAS Applications Database, 1975 to 1998. The unit of observation is département-year cell. To compute shares, we measure applications and approvals in square meters.

APPENDIX 3: SUMMARY STATISTICS: CEVIPOF ELECTION FILES

	Mean	Standard deviation
fraction RPR−UDF	.388	.093
fraction PS	.300	.101
fraction various right	.050	.068
fraction various left	.017	.038
fraction extreme right	.059	.061
fraction extreme left	.148	.077
fraction green party	.032	.046

APPENDIX 3 (CONTINUED)

	Mean	Standard deviation
cumulated fraction RPR-UDF	4.89	2.92
cumulated fraction PS	3.79	2.40
cumulated fraction various right	.765	.454
cumulated fraction various left	.254	.218
cumulated fraction extreme right	.400	.516
cumulated fraction extreme left	2.14	1.32
cumulated fraction green party	.197	.254

Source: CEVIPOF Election Files, 1975 to 1998. The unit of observation is a département-year cell.

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