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Product Market Deregulation and Employment Outcomes: Evidence from the German Retail Sector*

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Abstract

This paper investigates the effects of the deregulation of business hours legislation on retail employment in Germany. In 2006, the legislative power was shifted from the federal to the state level, leading to a gradual deregulation of shop opening restrictions in most of Germany's sixteen federal states. The paper exploits intra-country regional variation in the liberalization of closing laws in order to identify the effect of product market deregulation on retail employment. I report evidence that the deregulation had moderately negative effects on retail employment, leading to a loss of 19,000 full-time equivalent jobs. These job losses are concentrated among small establishments and are almost exclusively borne by full-time employees. I further show that the number of small stores significantly reduced, implying that deregulation has induced structural changes within the retail sector. These results are robust to various checks, including placebo tests and variations in model specifications. Robust effects on sales or prices were not detected.

Key Words: Product market regulation; Employment; Retail trade

JEL Classification: J21, L51, L81

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1 Introduction

It is a well established fact in the economic literature that the regulatory environment of product markets has an impact on labor market outcomes. In this context, the deregulation of product markets is often mentioned as a promising means to foster employment growth. While the majority of existing empirical literature indeed finds positive labor market effects of deregulation (e.g. Bertrand and Kramarz (2002)), the present analysis shows that the post-liberalization path of employment can, as well, take an unfavorable course.

I study the deregulation of the retail sector resulting from a reform of shop closing legislation in Germany in 2006 and 2007. To uncover employment effects, this study exploits regional variation in trading provisions across the German states. Within the realm of the reform of federalism (*Föderalismusreform*), adopted by the Federal Parliament and the Federal Council in 2006, legislative power on shop closing laws was conferred upon the federal states. This initiative marked the beginning of a period of extensive deregulation, in which 14 of the 16 German federal states liberalized their trading provisions. The remaining two states, Bavaria and the Saarland, retained the previously effective federal law.[1] This policy reform represents a natural experiment that can be used to identify the causal effect of the liberalization of shop closing laws.

I present evidence that the deregulation of shop closing legislation had a negative effect on aggregate retail employment. In quantitative terms, the coefficient estimates suggest that liberalization is associated with a moderate loss of 19,000 full-time equivalent jobs. In addition, this study sheds light on the transmission channel of the reform. First of all, I show that losses were mainly borne by full-time employees, while part-time employment was unaffected. Secondly, deregulation induced a sweeping change in the market structure by significantly decreasing the number of small retail stores. Thirdly, evidence for increased revenues or significant declines in prices, as was hoped for by policymakers by the time, could not be found. In combination, these results explain why the aggregate effect on employment is negative: deregulation has not led to a post-liberalization output boom, but instead caused a redistribution of sales from small towards larger establishments, which are relatively less personnel-intensive than small formats.

While these results stand in contrast to the majority of existing literature in this field, it bears notice that, from a theoretical perspective, the sectoral employment effect of deregulation is am-

[1]While Bavaria didn't pass any state legislation at all, the Saarland adopted a state law concerning shop opening times which did not change provisions effective under federal law.

biguous (Blanchard, 2006). As deregulation increases productivity, less employment is needed for a given level of output. In particular, if a regulated environment facilitates the creation of X-inefficiencies with overstaffed operating levels, employment decreases after deregulation. Yet, if liberalization-induced productivity gains decrease output prices, then final demand and output rise, eventually increasing labor demand. Therefore, the question how deregulation affects employment is ultimately of empirical nature.

A number of theoretical studies is concerned with the question how deregulation affects market stuctures. Although shop closing regulations were most often designed for religious reasons and in order to protect employees in the retail sector, they tend to favor small retailing units. First of all, restrictive opening hours reduce returns on investment. As large retailers have higher investments in real estate and inventories, they are more heavily affected by regulation (Pilat, 1997). Secondly, in the presence of restrictive closing laws, consumers have less time to drive to larger stores, which are often located outside city centers, even if there are price differences between the formats (Tanguay et al., 1995). Thirdly, due to the need for threshold labor, i.e. the need for one person to be employed at all times a shop is open, it is more costly for small retailing units to extend opening hours than for large ones (Nooteboom, 1983). Wenzel (2011) generalizes these arguments and develops a theoretical model, where efficiency differences between large and small establishments result in asymmetric shopping hours and eventually harm small formats. In a recent study, Haskel and Sadun (2012) empirically analyze whether there is evidence for productivity differences between shops of different sizes. Indeed, they find a strong association between the shift towards smaller stores and decreases in productivity growth.

Given the intensity of the public debate on shop closing laws in Germany, the academic literature on this issue is relatively scarce and rather inconclusive. Täger et al. (2000) examine the implications of the federal reform of shop closing laws in 1996. The authors find that employment and turnover have developed positively, while competition among retailers has increased as a consequence of deregulation. In contrast, studies by Hilf and Jacobsen (1999; 2000) find that employment in the retail sector has not increased after the reform, but that working time arrangements of employees have worsened. Most importantly, the problem with existing studies is that they rely on a single source of variation in legal provisions to identify employment effects of deregulation. Thus, they lack an adequate control group that would help eliminate the impact of confounding factors on employment changes in retail. The present study overcomes this problem by exploiting regional variation in trading provisions. To my knowledge, the only study that uses a similar identification strategy is the paper by Bossler and Oberfichtner (2014) who focus on employment developments in a subset of overall retailing.

This paper is closely related to a number of empirical studies that have analyzed the impact of product market regulations in the retail sector on employment outcomes. One of the first analyses was conducted by Bertrand and Kramarz (2002), who examine zoning laws in France which regulate the entry of large firms in the market. They find that this policy had a sizable adverse effect on retail employment, estimating that in absence of these laws, employment could have been approximately 10% higher. Further, the authors find that less stringent entry regulation leads to a significant decrease of employment in small shops. In a similar vein, Viviano (2008) shows that lower entry barriers for large stores led to higher employment in Italy, where additional employment is almost exclusively created in large stores. Yet, at least in the medium term, she does not find a significant negative employment effect of deregulation on small shops.

Skuterud (2005) analyzes the employment effects of changes in shop closing legislation by exploiting differences in provisions on Sunday trading across Canadian provinces. At the aggregate level, he finds evidence of modest employment gains, and decomposes this effect into positive threshold labor and sales effects and a negative effect on employment resulting from increased labor productivity. In a similar approach, Goos (2004) examines the impact of shop closing hours on employment and product markets in the United States. Using a difference-in-difference strategy, he shows that deregulation increases employment by 4.4 to 6.4 percent. Burda and Weil (2005) use changes in regulatory regimes in the period 1969-1993 to identify the employment effect of opening restrictions in the US. They find that Sunday closing regulation significantly reduces employment inside and outside the retail sector, with part-time employment being particularly affected. Though, a robust effect of closing laws on wages, prices and labor productivity was not found in the study.

This paper is also related to the literature on the displacement effects of large "Big-Box" retail establishments on smaller "Mom-and-Pop" stores. Haltiwanger et al. (2010) find substantial negative effects of Big-Box establishments on single unit and local chain stores. In a similar vein, a number of studies analyze the competitive effects of Wal-Mart stores on local competitors in the United States. Basker (2005) finds that Wal-Mart increases retail employment right after market entry. This positive effect decreases considerably over time, when some small and medium retailers close. Neumark et al. (2008) even find a negative effect of Wal-Mart on total retail employment. This results is supported by findings in Jia (2008), who reports that the expansion of Wal-Mart explains 50 to 70% of the net change in the number of small discount retailers.

The remainder of the paper is organized as follows. In the subsequent section, I describe the institutional background of the recent liberalization of shop closing legislation in Germany. In section 3, I present the estimation strategy, discuss identification issues and provide an overview of the data used in the analysis. The econometric analysis is conducted in sections 4 and 5. Section 6 concludes.

2 Legislation

The German retail sector was highly regulated for decades.[2] Since 1956, legislative power regarding shop opening hours lay with the federal government. The "Law Concerning Shop Closing Time" (*Gesetz über den Ladenschluss*) restricted opening hours of retail stores from 7 am to 6:30 pm on weekdays and from 7 am to 2 pm on Saturdays. On public holidays and Sundays, shop opening was generally prohibited. Despite a lively debate on the usefulness of restrictions of shop opening hours, the law was not fundamentally changed for three decades.[3] With the introduction of the "service evening", allowing retail stores to open until 8:30 pm on Thursdays, the deregulation process began in 1989. Further relaxations followed in 1996 and 2003, following which shops could remain open between 6 am and 8 pm on all weekdays and Saturdays.

In June and July 2006, the Federal Parliament and the Federal Council adopted a reform of federalism, as part of which the legislative power on shop closing issues was conferred upon the federal states. This marked the beginning of a period of extensive deregulation, in which 14 of the 16 German federal states liberalized their trading provisions. Berlin was the first state to pass a law in November 2006, with 13 other federal states following soon. Mecklenburg-West Pomerania was the last of the states to liberalize closing laws, doing so in July 2007. Only Bavaria and the Saarland adhered to the initial regulation. Notably, the decision of the Bavarian government not to deregulate shop closing laws was made capriciously, a fact that is important for the following econometric

[2] For a comprehensive overview of the history of shop closing laws, see Täger et al. (1995) and Spiekermann (2004).

[3] Several amendments concerned the exemption of certain store types (gas stations), specific locations (train stations, airports) as well as specific dates (Saturdays in the advent season) from shop closing laws.

analysis. Before the reform, the Bavarian minister of economic affairs had emphasized Bavaria's pioneering role in the deregulation of shop closing laws (WaMS, 2006). Yet, the vote in the caucus which decided on the extensions resulted in a standoff because Prime Minister Edmund Stoiber had left the meeting early (SZ, 2006). As a consequence, the Bavarian government adhered to the restrictive closing laws that were effective under Federal Law and decided to observe experiences made by other states before taking further action.[4]

The new state laws vary not only at the regional level but also differ with respect to the scope of liberalization. While nine out of 14 states abolished all opening restrictions on weekdays and on Saturdays, the remaining five retained some provisions. Also, regulations on Sunday trading differ across states. Detailed information on the enforcement dates of the state laws as well as on the provisions on shop opening is given in Table 1.

The legislative changes were subject to contentious political and public debate. With respect to the reform's costs and benefits, the most controversial issue was its expected labor demand effect.[5] Proponents viewed the deregulation as a means to boost sales and to create more jobs. These expectations were backed by a report of the expert advisory board (Deutscher Bundestag, 1995) and a simulation by the ifo institute, according to which an extension of shop opening hours from 6.30 pm until 10 pm would create 50.000 additional full-time jobs (1995, p. 328). In contrast, opponents of the reform feared a reduction of employment and a shift towards more part-time and casual work.

In sum, two features of the legislative process lay the foundation for the identification strategy in the following empirical analysis. First of all, there exists regional variation in the deregulation process which allows me to compare employment outcomes in federal states which lifted restrictions with federal states that did not. Secondly, the decision of the state of Bavaria not to deregulate constituted a "shock" to the local economy and did not reflect socio-economic particularities of the Bavarians.

[4] A further particularity of the legislation process is that in some states, the deregulation decision was influenced by courts. Liberalization opponents have repeatedly made efforts to take legal actions against state-level closing laws. For instance, the Christian churches in Berlin filed suits against plans to liberalize Sunday shopping throughout December, pleading constitutionally guaranteed Sunday rest. The Federal Constitutional Court ruled in favor of the liberalization opponents (BVerfG, 2009).

[5] Further arguments relate to the coordination of leisure, the protection of small retailers from large outlets and the need to meet changing consumer demands.

Table 1: Deregulation of Shop Opening Hours Legislation

Federal State	Introduction	Weekday	Saturday	Sunday	Scope
Baden-Wurttemberg	06. March 2007	0 am - 12 pm	0 am - 12 pm	3 × 5 hrs	.71
Bavaria	-	6 am - 8 pm	6 am - 8 pm	4 × 5 hrs	-
Berlin	14. November 2006	0 am - 12 pm	0 am - 12 pm	8 × 7 hrs	.72
Brandenburg	29. November 2006	0 am - 12 pm	0 am - 12 pm	6 × 7 hrs	.72
Bremen	01. April 2007	0 am - 12 pm	0 am - 12 pm	4 × 5 hrs	.71
Hamburg	01. January 2007	0 am - 12 pm	0 am - 12 pm	4 × 5 hrs	.71
Hesse	30. November 2006	0 am - 12 pm	0 am - 12 pm	4 × 6 hrs	.71
Lower Saxony	01. April 2007	0 am - 12 pm	0 am - 12 pm	4 × 6 hrs	.71
Mecklenburg-West Pomerania	16. July 2007	0 am - 10 pm	0 am - 10 pm	4 × 5 hrs	.69
North Rhine-Westphalia	21. November 2006	0 am - 12 pm	0 am - 12 pm	4 × 5 hrs	.71
Rhineland-Palatinate	29. November 2006	6 am - 10 pm	6 am - 10 pm	4 × 5 hrs	.14
Saarland	15. November 2006	6 am - 8 pm	6 am - 8 pm	4 × 5 hrs	-
Saxony	16. March 2007	6 am - 10 pm	6 am - 10 pm	4 × 6 hrs	.14
Saxony-Anhalt	30. November 2006	0 am - 12 pm	0 am - 8 pm	4 × 5 hrs	.66
Schleswig-Holstein	01. December 2006	0 am - 12 pm	0 am - 12 pm	4 × 5 hrs	.65
Thurangia	29. November 2006	0 am - 12 pm	0 am - 20 pm	4 × 6 hrs	.66
Federal law before reform	01. June 2003	6 am - 20 pm	6 am - 20 pm	4 × 5 hrs	-

Notes: Information on legislation is compiled from law texts. The scope of deregulation is defined as the percentage change in hours which shops are allowed to additionally open according to new state legislation.

3 Empirical Strategy and Data Description

3.1 Empirical Strategy and Identification

The quasi-experimental setting described in the previous section allows me to use a difference-in-difference strategy in order to gauge the causal effect of deregulation on employment in the retail sector. While the majority of federal states passed laws to deregulate shop opening hour restrictions in the years 2006 and 2007, two states adhered to federal law. The first group comprises the treatment group and the latter the control group. In the analysis, I contrast employment outcomes before and after the deregulation in the treatment group. The control group of non-deregulating states is needed to extract employment trends in the retail sector common to all federal states, as they would otherwise falsely be attributed to the extension of shop opening hours. In order to identify the employment effect of deregulation, I fit empirical models of the following type:

$$\ln Y_{dst} = \alpha + \beta_1 Dereg_{st} + \mathbf{X'_{dt}}\beta_2 + \gamma_d + \delta_t + \epsilon_{dst}. \tag{1}$$

The main dependent variable Y_{dst} represents the fraction of retail sector employees in overall employment, calculated for each administrative district d located in state s at time t. In order to analyze effect heterogeneity, I further divide overall retail employment into different subsets bifurcated by establishment size, working-time arrangement, or gender. In addition, I generate a dependent variable which reflects the number of small, medium and large shops in district d at time t. All dependent variables are expressed as natural logarithms.

$Dereg_{st}$ denotes an indicator variable equal to one if a district is located in a state s which has deregulated its shop closing law at time t and zero otherwise. Thus, β_1 is the parameter of interest and reflects the differential employment effect due to the deregulation of shopping hours. All estimates include a vector of district dummies, γ_d, which control for mean differences in retail employment across districts. Furthermore, the regressions include year dummies, δ_t, that control for aggregate time shocks. In extensions to this, I augment the model by time-varying district characteristics, X_{dst}, which may independently influence employment in retail. Further, I estimate specifications where the model described by equation 1 is enriched by linear as well as quadratic district-specific time trends. This modification allows for deviations from the common trend assumption, such that the identification of the deregulation effect results from whether the law change led to deviations from pre-existing trends.

One concern for the identification strategy is that unobserved determinants of retail employment growth may be correlated with the decision to deregulate shop closing laws. If deregulation is endogenously determined by economic and social conditions, the estimates of β_1 will be biased. Yet, as discussed in the previous section, the most important control state of Bavaria was assigned to the control group capriciously, implying that the deregulation experience at hand does not suffer from endogeneity problems. Further, it bears notice that endogeneity would result in upward-biased estimates, as new policies are likely to be implemented where the gain from a law change is greatest. Hence, given that I find negative deregulation effects on employment, my results are still valid in the presence of endogeneity and would then have to be interpreted as lower bounds.

As the analysis employs multiple time periods, inference based on the traditional treatment of standard errors can be misleading due to serial correlation (Bertrand et al., 2004). Furthermore, the employment outcomes vary at the district level, while the regressor of interest varies only at group level, which results in downward-biased standard errors (Moulton, 1986). To address these concerns, I follow the proposition by Angrist and Pischke (2009) and use Huber-White robust standard errors clustered at state level. This allows for an arbitrary autocorrelation process of the error terms within the states over the years, reducing the bias in the standard errors. In a recent study, Brewer et al. (2013) show that even if the number of clusters is relatively small, tests of the correct size can be obtained. In particular, this is achieved by computing a t-statistic with cluster robust standard errors that use residuals scaled by $\sqrt{\frac{G(N-1)}{(G-1)(N-K)}}\sqrt{G/(G-1)}$ and using critical values from a t-distribution with $G-1$ degrees of freedom. The robustness of the results is additionally confirmed by implementing the two-way bootstrap clustering method suggested by Cameron et al. (2008).

3.2 Data and Descriptive Evidence

The primary data source employed for the analysis is the Establishment History Panel (*Betriebshistorikpanel*, BHP) for the period from 2003 to 2010. The BHP is a 50 percent sample of all establishments in Germany with at least one employee liable to social security as of the 30th June of a given year, stratified by establishment size (for details, see Gruhl et al., (2012)). While the dataset contains information on regular employees and marginal employees, the self-employed and unpaid family members are not included. In addition to the number of total employees, employment information is available at a more disaggregated level, i.e. by gender, working time arrangenment and education. The BHP further contains a 5-digit industry identifier, as well as information on the district in which an establishment is located.

To construct the sample, I first translate total employment into full-time equivalents (FTE) at the firm level.[6] Then, by aggregating the firm-level employment information at the level of 412 districts, I construct a panel with district-year observations. In order to enable analyses of effect heterogeneity, I additionally calculate district employment bifurcated by working-time (full-time vs. part-time), gender and establishment size. As the dataset does not include information on sales volume or floor size, stores are grouped according to their number of employees. I follow the classification of Viviano (2008) and define firms as small if they have up to five employees, as medium when there are more than five but less than sixteen employees, and as large when there are sixteen employees or more.

As discussed in section 3.1, the main dependent variable is the fraction of retail employment in overall employment. For the purpose of my analysis, it is reasonable to restrict the retail sector to the sale of new goods in stores (Sector Industry Code 521 to 524). Specifically, I exclude retail sale

[6]The data lacks exact information on hours worked. In order to calculate FTE employment, I follow Dauth (2010) and weigh employment according to a worker's employment status: Employees are assigned a weight of 1 if they work full-time, a weight of $\frac{24}{39}$ if they work in major part-time (between 19 and 39 hours) and $\frac{16}{39}$ if they work in minor part-time (up to 19 hours).

not in stores (SIC 526), which is not bound to opening restrictions, as well as sale of second-hand goods (SIC 525).[7]

In order to construct the time-varying district characteristics that are used as control variables, I match information on tourism, proxied by the number of overnight stays, as well as on disposable income in district d and year t. These time series are provided by the German Federal and State Statistical Offices. In the Establishment History Panel, districts are defined following a time-consistent definition of 412 administrative districts in West Germany according to the territorial status of 2008. To make the data from the Federal Statistical Offices consistent with this classification, six districts in Saxony-Anhalt have to be excluded from the analysis.[8]

Table 2 presents summary statistics on the variables employed in the analysis for the treatment and control districts in the baseline sample for the years 2003 and 2010. Columns 1 and 4 report the means for the control districts and columns 2 and 5 report those for the treatment districts in each year. Columns 3 and 6 include the respective differences and indicate the statistical significance from t-tests on the equality of means. The average fraction of district employment in retail amounts to 8.19% and 8.14% of the overall working population in the control and treatment states, respectively. This number also comprises those working in out-of-store retail environments (e.g. mail order business and markets) as well as employment in second hand stores. Without these categories, the retail employment share decreases to approximately 7.8%. Since part-time employees are overrepresented in the retail sector relative to the overall working-time structure, the retail employment share declines again when employment is expressed in full-time equivalents. Table 2 also displays employment shares disaggregated by establishment size and gender. Notably, when comparing treatment and control states, employment is similarly distributed within the respective groups.

In sum, an unconditional cross-sectional comparison of the dependent variables between the treatment and the control group reveals no significant differences in the structure of retail employment. Yet, it bears notice that the treatment districts have, on average, a larger population, receive more tourist stays and have a lower income level.

[7]The industry classification changes in 2009. In order to obtain a consistent classification of the industry, I used the industry crosswalk provided by the Federal Statistical Office. Fortunately, this crosswalk allows a 1:1 mapping of the industries at the 3-digit level for industry code 521 to 524.

[8]Within the realm of several district reforms, county boundaries were redrawn in some East German states. In most cases, this does not pose a problem, because districts were merged together. Yet, in Saxony-Anhalt, boundaries were redrawn in a way such that some former districts cannot be matched 1:1 to new ones.

Table 2: Summary Statistics of Variables Employed for 2003 and 2010

	2003			2010		
	Control (1)	Treatment (2)	Diff. (3)	Control (4)	Treatment (5)	Diff. (6)
Dependent Variables						
Fraction in retail	8.19	8.14	.05	8.13	8.06	.07
	(.21)	(.12)	(.24)	(.22)	(.10)	(.22)
Fraction in retail excl. 525 & 526	7.73	7.81	-.07	7.72	7.75	-.03
	(.18)	(.11)	(.22)	(.16)	(.10)	(.20)
Fraction in retail (FTE)	6.88	6.89	-.01	6.88	6.78	.10
	(.17)	(.11)	(.21)	(.16)	(.10)	(.19)
Fraction full-time	4.09	4.03	.06	3.74	3.55	.19
	(.12)	(.07)	(.14)	(.11)	(.06)	(.12)
Fraction part-time (FTE)	2.79	2.86	-.07	3.15	3.23	-.09
	(.04)	(.05)	(.09)	(.06)	(.05)	(.09)
Fraction in small estbl.	2.94	2.82	.12	2.80	2.69	.11
	(.07)	(.05)	(.99)	(.06)	(.04)	(.08)
Fraction in medium estbl.	1.02	1.04	-.02	1.08	1.03	.05
	(.03)	(.03)	(.05)	(.04)	(.02)	(.05)
Fraction in large estbl.	2.92	3.03	-.11	3.01	3.07	-.06
	(.17)	(.08)	(.18)	(.15)	(.08)	(.16)
Fraction female	4.71	4.75	-.04	4.78	4.71	.08
	(.12)	(.07)	(.14)	(.11)	(.01)	(.13)
Fraction male	2.18	2.15	.02	2.12	2.10	.01
	(.08)	(.05)	(.09)	(.07)	(.04)	(.08)
Control Variables						
Tourist stays (log)	12.82	13.01	-.19**	12.95	13.25	-.30***
	(.11)	(.06)	(.12)	(.10)	(.06)	(.11)
Disp. inc. PP (log)	9.78	9.69	.09***	9.92	9.83	.09***
	(.01)	(.01)	(.01)	(.10)	(.01)	(.01)
Working age pop.	88,490	149,361	-60,871***	88,446	114,952	-26,506***
	(9,007.32)	(9,726)	(17,736)	(9,516)	(144,982)	(17,733)
Population	132,203	221,904	-89,700***	132,904	329,985	-197,080***
	(12,785)	(13,872)	(25,284)	(13,751)	(14,209)	(26,011)

Notes: Number of observations: 412 for each year. Dependent variables are expressed as the fraction of FTE district employment. Standard deviations in parentheses. Sector 525 and 526 comprise out-of-store retail and second hand retail.

The validity of the diff-in-diff approach hinges critically on the assumption that in absence of treatment, employment in both groups would evolve identically. As a first descriptive test of the validity of this identifying assumption, I compare pre-deregulation trends in retail employment in the treatment and the control group. If retail employment has evolved similarly in both groups before the treatment, it is likely that any differences in the development after the treatment can be attributed solely to deregulation. Figure 1 depicts the average fraction of retail employment in overall employment in the treatment and the control group between 2003 and 2010, using a relative time scale. Specifically, year zero is normalized to the first year that shop closing laws were liberalized.[9] The data reveal a parallel increase in employment share at the beginning of the

[9]Because for all but one of the federal states this was in 2007, year-zero employment in the control states also represents the year 2007.

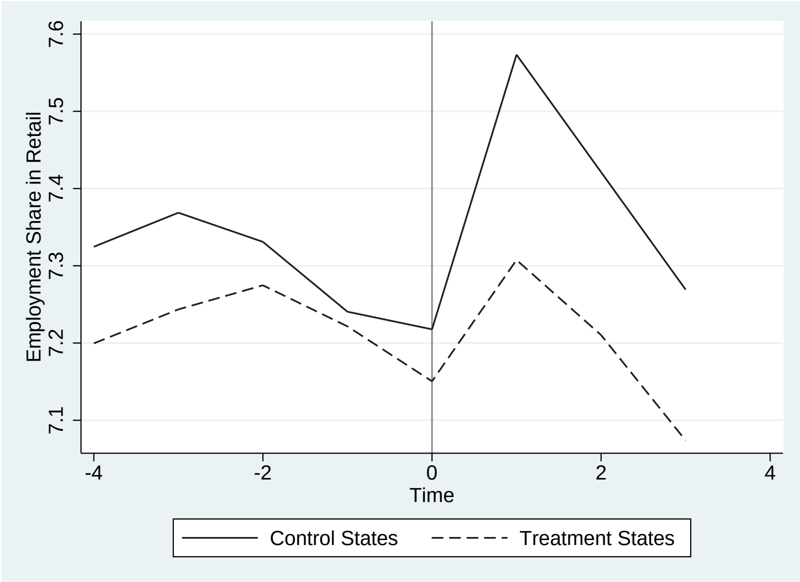

Figure 1: Employment Shares in Retail

Notes: Employment shares are calculated from the Establishment History Panel (BHP).

observation period, before retail employment decreases in both groups. After the deregulation of shop opening hours, marked by the vertical line in the figure, employment shares in both groups increase first and then decrease again. Although only descriptive, the figure presents evidence for a similar trend of retail employment shares in both groups. Yet, it bears notice that also in the post-treatment period, employment developments do not differ markedly. The validity of the identifying assumption is further tested in section 4.2, where I perform several placebo experiments.

4 Results

4.1 Overall Retail Employment

I start the econometric analysis by assessing the aggregate employment effect of deregulation. Table 3 shows the results for the regression of the log fraction of all workers employed in retail. The simplest specification reported in column 1 includes the deregulation dummy as well as year and district fixed effects, which are highly significant in most instances. Due to the large number of district fixed effects and time trends included, I only report the result on the variable of interest. The coefficient in column 1 is negative and statistically significant at the 5% level, suggesting that deregulation decreased employment in the retail sector.

In order to control for potentially confounding factors, I augment the model with additional covariates that might independently influence the development of employment in the retail sector. As discussed in section 3.2, six districts in Saxony-Anhalt have to be excluded once covariates are added to the model. To see whether the mere exclusion of these districts changes the result obtained so far, I repeat the regression with the restricted sample. As can be seen in column 2, the size of the coefficient decreases marginally and the standard error does not change. It is therefore reasonable to assume that any changes in the size or significance of the estimate on the deregulation dummy will stem from the inclusion of additional control variables rather than from the sample restriction itself.

Retail sector employment might be positively affected by tourists, as they create additional purchasing power in a region. I test this hypothesis by including the number of overnight stays of visiting foreigners in district d at time t. The positive, albeit insignificant, coefficient in column 3 verifies this conjecture. However, the inclusion only marginally alters the point estimate on the coefficient of interest. In the next column, I additionally augment the regression by a measure of average disposable income per person in district d in time t. The coefficient on the deregulation variable remains stable and the results indicate that disposable income is positively associated to employment in retail, although the coefficient is statistically not different from zero.

Table 3: Employment Effect of Deregulation, Baseline Results

Dependent Variable: FTE Retail Empl. Share (log)	(1)	(2)	(3)	(4)	(5)	(6)
Deregulation	-.019**	-.018**	-.019**	-.019**	-.017***	-.015***
	(.008)	(.008)	(.008)	(.008)	(.003)	(.005)
Tourism	no	no	.040	.044	.009	.020
			(.040)	(.042)	(.022)	(.014)
Disposable Income	no	no	no	.085	.171	.133
				(.083)	(.124)	(.117)
District × time trends	no	no	no	no	yes	yes
District × time^2 trends	no	no	no	no	no	yes
R^2	.900	.900	.901	.901	.954	.968
N	3,296	3,248	3,248	3,248	3,248	3,248

Notes: All regressions include district and year fixed effects. The explanatory variable "Tourism" is proxied by the log number of overnight stays in time t in district d. Robust standard errors in parentheses are clustered at the federal state level. *Significant at 10%, ** at 5%, *** at 1%.

In column 5, the model is augmented by a full set of district-specific linear time trends. The precision of the estimates is increased considerably, with the size of the standard error being more than halved. The absolute value of the coefficient decreases only slightly and becomes significant at the 1% level. Also the inclusion of quadratic trends (column 6) hardly alters the point estimate. The coefficient suggests that the share of retail employment decreased by 1.5% as a consequence of the reform. Evaluated at the pre-treatment sample mean for the fraction of FTE retail employment in overall employment (6.88%), the point estimate translates into an average loss of approximately .1 percentage points of overall employment or about 19,000 full-time equivalent jobs in the deregulating federal states.[10] Hence, the point estimate of -1.5% is quantitatively small and implies, on average, a rather moderate adverse employment effect of deregulation.

4.2 Robustness Checks

In this section, I perform several robustness checks of the main result, that the deregulation of shop closing laws significantly reduced retail employment in Germany. The results of these checks are depicted in Table 4, where the baseline estimate for the aggregate deregulation effect from Table 3 is reproduced in row 1 for comparison. One threat to the identification strategy applied in our analysis is that the negative employment effect observed in the deregulating states might be driven by policies other than the liberalization of shop closing laws, which indirectly affect retail employment. To test this hypothesis, I perform two "placebo experiments". First, I fit a model according to equation 1, where the dependent variable is the share of employment in the hotel sector, a service sector similar to retail trade (Bertrand and Kramarz, 2002). If the estimated coefficient for the hotel sector is negative, the differential development of retail employment in the treatment group would falsely be attributed to the deregulation. Instead, such a result would be suggestive of other state policies that exert an adverse effect on overall employment. Row 2 reports estimates for the hotel sector. The coefficients are positive and do not differ significantly from zero in the fully specified model, confirming that the results obtained in Table 3 are indeed specific to the retail sector.

As a second test, I prepone the timing of the liberalization by two years. Here, a coefficient significantly different from zero would indicate that the evolution of retail employment has evolved differently in the treatment and the control groups, but due to some other reason than the treatment. The placebo experiment is presented in row 3 and reveals that the estimated policy effect is not different from zero.

[10] Employment in the average treated district amounts to 60,762 FTE workers in the pre-liberalization period. A decrease of this population by .1 percentage points implies a reduction of 61 FTE jobs per district and adds up to 19,000 full-time equivalent jobs in the treated states.

Table 4: Robustness Checks

	(1)	(2)	(3)
1. Baseline estimates from Table 3	-.019**	-.017***	-.015***
	(.008)	(.003)	(.005)
2. Placebo: Estimates for the hotel sector	.038	.028*	.026
	(.025)	(.010)	(.018)
3. Placebo: Pre-ponement of timing	-.004	.020	.016
	(.005)	(.012)	(.012)
4. Treatment intensity	-.015	-.021**	-.018*
	(.019)	(.008)	(.009)
5. Log employment	-.036***	-.013**	-.015***
	(.011)	(.005)	(.005)
6. Retail as a fraction of working age population	-.026***	-.011**	-.014***
	(.008)	(.004)	(.004)
7. Weight by district population	-.002	-.015***	-.014**
	(.007)	(.004)	(.006)
8. Bootstrapped Standard Errors	-.019	-.017*	-.016**
	[.55]	[.070]	[.043]
Add. Controls	yes	yes	yes
District $\times$ time trends	no	yes	yes
District $\times$ time^2 trends	no	no	yes

Notes: N=3,248. Each cell reports the coefficient on the treatment variable for one regression. All regressions include district and year fixed effects. If not reported differently, standard errors in parentheses are clustered at the federal state level. p-values in brackets. * Significant at 10%, ** at 5%, *** at 1%.

I next consider the robustness with respect to an alternative definition of the treatment variable. So far, the treatment was reflected by a dummy variable which indicated whether a federal state had deregulated its shop closing laws or not. However, as discussed in section 2, there also exists variation in the scope of deregulation between the states. To incorporate this additional variation, I estimate a model, where the explanatory variable is a measure of deregulation intensity. Specifically, the variable reflects the percentage change in hours that shops are allowed to open under new state legislation. The treatment intensity takes values between zero (for non-deregulating states) and 0.72 for the states with the most liberal regulations (see Table 1). The coefficient estimates reported in row 4 are consistent with the baseline result.

So far, the retail employment variables used in my analysis were expressed as fractions of overall employment. Hence, also changes in the overall working population influence the dependent variable. Further, employment evolutions in retail influence both the nominator and the denominator. To address this potential concern, I re-estimate the model and express retail employment in levels instead of shares and as the fraction of the overall working age population, respectively. For the level of retail employment (row 5), the result is identical to the baseline estimate in the fully specified model. The coefficient for retail employment as the fraction of the working age population (row 6) is marginally smaller than the baseline but remains highly significant. In row 7, I present results where observations are weighted by the respective district population in order to account for differences in the district size and to make the results representative for the average German employee. Reassuringly, the estimated coefficient is only slightly smaller than the baseline estimate. Finally, in row 8, I confirm the validity of the results by implementing the two-way bootstrap clustering method suggested by Cameron et al. (2008).

I further test the robustness of my results by analyzing whether they are driven by a certain year or a specific federal state. To this end, I re-estimate the model, and consecutively exclude one year or state from the regression. The results from these estimations are depicted in Appendix Tables 8 and 9. As can be seen, they prove robust to the exclusion of particular years or federal states.

4.3 Effect Heterogeneity by Establishment Size

This section is devoted to the analysis of effect heterogeneity with respect to store size. As discussed in the introduction, the mechanism through which deregulation affects the distribution of employment among small and large shops bases on productivity differences between establishments of different size. Specifically, these may result from economies of scale, better organizational structure and more buyer power of large establishments (Haskel and Sadun, 2012). In the presence of such productivity differences, small retailers are not able to match longer shopping hours and eventually suffer from deregulation.[11] Figure 2 in the Appendix presents some descriptive evidence on the relationship between store size and productivity in the German retail sector. It displays average sales productivity for establishments of different size in the year 2005, where establishments are categorized by yearly sales and overall employees, respectively. As can be seen, sales productivity increases with establishment size. In shops with sales exceeding 10 million Euro, average sales per

[11] As the BHP does not contain information on actual opening hours, we cannot inspect the issue of heterogeneous opening hours extension. Yet, evidence from an earlier reform of the shop closing legislation in 1996 suggests that, indeed, size is an important determinant of whether stores actually use the leeway of extending opening hours beyond the existing level (Täger et al., 2000).

employee are almost three times larger than in shops with a sales volume of up to one million Euro.

Table 5: Deregulation Effects, Results by Establishment Size

	Panel A: Dep. Var.: Empl. Share (log)			Panel B: Dep. Var: Number of Shops (log)		
	(1)	(2)	(3)	(1)	(2)	(3)
			Small Establishments (≤ 5 empl.)			
Deregulation	-.014**	-.011*	-.013**	-.030***	-.016**	-.017**
	(.007)	(.006)	(.005)	(.008)	(.007)	(.007)
R^2	.962	.976	.981	.994	.997	.998
			Medium Establishments (6 to 15 empl.)			
Deregulation	.018	-.008	-.006	.008	-.004	-.004
	(.012)	(.008)	(.009)	(.012)	(.011)	(.011)
R^2	.886	.927	.938	.957	.970	.974
			Large Establishments (≥ 16 empl.)			
Deregulation	-.025***	-.005	-.006	-.028***	.004	-.002
	(.007)	(.005)	(.005)	(.008)	(.011)	(.012)
R^2	.958	.973	.977	.969	.980	.983
Add. Controls	yes	yes	yes	yes	yes	yes
District × time trends	no	yes	yes	no	yes	yes
District × time^2 trends	no	no	yes	no	no	yes

Notes: N=3,248. Each cell reports the coefficient on the treatment variable for one regression. All regressions include district and year fixed effects. Standard errors in parentheses are clustered at the federal state level. * Significant at 10%, ** at 5%, *** at 1%.

To analyze whether deregulation has had an impact on the structure of the retailing sector, I re-estimate the model described by equation 1 separately for small, medium and large establishments. The results of these estimations are presented in Panel A of Table 5. In line with the theoretical predictions, deregulation has heterogeneous effects on stores of different sizes. In particular, liberalization has led to a significant decrease of employment in small retail stores. The negative coefficient suggests that the share of employment in these establishments has decreased by 1.3%. Notably, employment losses have been accompanied by a significant decrease in the overall number of small shops (see top part of Panel B). In contrast, neither employment in medium and large establishments nor the number of these shops has been significantly affected by deregulation.

In sum, my results imply that deregulation has led to modest employment losses in the retail sector, which originate from employment decreases in small shops. One interpretation of the results is that the formerly regulated retail environment has facilitated the emergence of inefficient retailing structures with relatively low productivity and overstaffed operating levels. After deregulation, these less efficient formats disappear. This, in turn, results in a net decrease in employment, as the losses are not sufficiently compensated by employment creation in large establishments.

4.4 Further Employment Outcomes

In this section I analyze whether employment losses were concentrated among particular subsets of employees in the retail sector. To do so, I break down overall retail employment into different subsamples bifurcated by working time arrangement and gender and estimate the basic empirical model described by equation 1. The results are presented in Table 6, where each coefficient corresponds to a separate regression.

I start by analyzing whether deregulation has differentially affected full-time and part-time employment. The results show that the adverse effect of deregulation is exclusively borne by full-time employees. The estimated coefficient is highly significant and suggests that full-time employment has decreased by 2.5%. Evaluated at the average fraction of full-time employment in retail (3.92%), the point estimate suggests that full-time employment has decreased by .1 percentage points of the working population, which is equivalent to the aggregate effect. In contrast, the point estimates for part-time employment are close to zero and statistically insignificant.

Table 6: Deregulation Effects, Results by Employment Subset

	(1)	(2)	(3)
By working time arrangement			
Full-time employment	-.037***	-.029***	-.025**
	(.010)	(.006)	(.009)
Part-time employment	.002	-.006	-.003
	(.007)	(.005)	(.005)
By gender			
Female employees	-.024***	-.012***	-.010**
	(.008)	(.003)	(.004)
Male employees	-.015	-.032***	-.029***
	(.010)	(.007)	(.010)
Total employment (log)	-.017*	.004	-.000
	(.005)	(.003)	(.002)
Add. Controls	yes	yes	yes
District × time trends	no	yes	yes
District × time^2 trends	no	no	yes

Notes: N=3,248. Each cell reports the coefficient on the treatment variable for one regression. All regressions include district and year fixed effects. Standard errors in parentheses are clustered at the federal state level. * Significant at 10%, ** at 5%, *** at 1%.

In rows 3 and 4 I focus on the employment outcomes of male and female retail workers. While the coefficients for both genders are negative and statistically significant, it is worth noting that the point estimate for male employees is almost three times larger than its counterpart for the female subsample. Finally, I estimate the deregulation effect on overall district employment. This is to address the question whether the effect on employment in the retail sector represents a redistribution across sectors or whether overall district employment declined as a result of deregulation. The point estimate in column 1 of row 5 is negative and statistically significant at the 10% level, implying a decrease of overall employment by approximately .2 percent. Yet, once time trends are added to the model (column 2 and 3), the magnitude of the estimated coefficient decreases substantially and becomes statistically insignificant.

5 Sales and Prices

To put the employment results into a broader context, it is interesting to analyze whether the deregulation of shop closing laws has also affected sales and prices in the retail sector. Unfortunately, the study of sales and prices is subject to some data limitations, as neither sales data nor data on consumer price indices exist at the district level in Germany. However, I was able to collect monthly sales and price data at the federal state level from the Regional Statistical Offices between 2006 and 2008 and 2005 and 2010, respectively.[12]

I start by analyzing the deregulation effect on sales in the retail sector. From a theoretical perspective, the sales effect of deregulation is ambiguous. Stützel (1958) argues that changes in opening hours will not have first order effects on the demand for final goods, as consumers would respond to longer opening hours by making the same purchases in a longer time interval. In contrast, Gradus (1996) and Burda and Weil (2005) develop theoretical frameworks, where "Stützel's Paradox" does not hold in general, but where positive sales effects are possible. To analyze the deregulation effect on sales, I fit the following model:

$$Y_{st} = \alpha + \beta_1 Dereg_{st} + \mathbf{X}'_{\mathbf{st}}\beta_2 + \gamma_t + \delta_s + \epsilon_{st}. \tag{2}$$

The dependent variable reflects nominal or real retail sales in state s in time t, where revenues are

[12] See data Appendix for a detailed description of the sales and price databases.

normalized to the reference year 2005. The regression includes state and time fixed effects as well as the same control variables that were used in earlier specifications, aggregated at the state level. The results are presented in Panel A of Table 7. For both nominal and real sales, the coefficient on the deregulation dummy is positive in the fully specified model in column 3, implying that revenue increased after deregulation. Yet, in quantitative terms, the estimated effect is relatively moderate, suggesting revenue gains of .5 to .8 percent. Additionally, the standard errors are large, rendering the coefficients not statistically different from zero.

Table 7: Deregulation Effects on Sales and Prices

	(1)	(2)	(3)
		Panel A: Sales	
Real Sales	-1.650	.190	.787
	(2.843)	(2.088)	(1.354)
Nominal Sales	-2.224	-.112	.489
	(2.771)	(1.964)	(1.212)
		Panel B: Prices	
Food prices	.080	-.432	-.331
	(.334)	(.457)	(.292)
Apparel prices	2.929	-1.306	-.528
	(1.904)	(.968)	(.512)
Furniture prices	.842	.217	-.196
	(.488)	(.222)	(.266)
Add. Controls	yes	yes	yes
District $\times$ time trends	no	yes	yes
District $\times$ time^2 trends	no	no	yes

Notes: N=576 in Panel A, N=1008 in Panel B. Each cell reports the coefficient on the treatment variable for one regression. All regressions include state and month*year fixed effects. Standard errors in parentheses are clustered at the federal state level. * Significant at 10%, ** at 5%, *** at 1%.

The results obtained so far suggest that deregulation has not led to an increase in retail sales volume. Yet, another possible explanation for these findings is that deregulation may simultaneously affect retail sales and prices. In that case, the CPI based on all consumer goods, which is used to deflate the nominal sales data, is an imperfect indicator for price changes in the retail sector, resulting in an imprecise estimation of the sales effect.

To assess this possibility, I estimate the price effect of deregulation using CPI data from the Regional Statistical Offices. As consumer prices indices do not exist at the industry level but for different categories of goods, I obtain exemplary consumer price indices for food, apparel and furniture as well as the overall CPI at the level of federal states. I normalize the CPI of the three product groups by the CPI for all consumer goods to fit models as described by equation 2. The results from this analysis are presented in Panel B of Table 7. For all product groups, the coefficients are negative, suggesting that relative prices in the retail sector have decreased after deregulation. This implies that sales volume may indeed have been positively affected by deregulation, which remained unidentified in the upper part of Table 7 due to retail specific price decreases. Yet, the coefficients are imprecisely estimated. Unfortunately, neither the sales nor the price data could be split further into subsamples to analyze effect heterogeneity by establishment size.

6 Conclusion

This paper presents empirical evidence that product market regulation affects labor market outcomes. The case studied is the deregulation of shop closing laws, introduced in Germany in 2006 and 2007. This reform conferred the legislative power regarding shop opening issues upon the federal states. I exploit regional variation in trading provisions to identify the effect of deregulation on employment outcomes. I present evidence that the reform led to modest employment losses in the retail sector. In line with theoretical predictions, I show that these losses are concentrated among small retail establishments, while medium and large size establishments were unaffected by the law change. Further, I show that the decreases in employment were mainly borne by full-time employees and over-proportionally by male workers.

The key finding of an adverse employment effect stands in contrast to the main body of the existing - largely US based - literature, in which the majority of studies find that shop closing deregulation leads to significant employment gains. One may explain this discrepancy by a relatively high level of X-inefficiencies in the German retail sector prior to deregulation, associated with low productivity and excessive employment levels. Further reasons for the different findings involve high labor costs in Germany, which have the potential to suppress positive labor demand effects. Hence, my results suggest that in any debate on the employment consequences of deregulation, it is crucial to account for the conditions of the specific case at hand, as the post-liberalization path may vary considerably among sectors and countries (Blanchard, 2006; Boeri et al., 2006).

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Appendix

Data Appendix

Sales Data

The data on sales are collected from the Regional Statsistical Offices. The dataset consists of monthly observations of sales at the spatial unit of federal states, normalized to a reference level. Information is available on nominal as well as real sales, which are deflated by the the CPI based on all consumer goods. In each federal state, a panel of establishments is randomly sampled from the industry register, which covers establishments whose annual sales exceed 250,000 Euro. Because sampled establishments are obliged by law to take part in the survey, the data set does not suffer from self-selection.

The sample period is restricted to January 2006 to December 2008 for the following reasons. On the early end I am limited because as of 2006, refreshment samples were included in a number of states, leading to a structural break in the time series. After 2008, the industry classification change, and a one-to-one mapping between the two classifications is not possible due to the high level of aggregation. Information on sales volumes in Lower Saxony is only available from July 2007 onwards. Hence, the final dataset consists of 576 state-month-observations.

Data on Prices

The data on prices is obtained from the federal statistical offices, which publish state level consumer price indices on a monthly basis. Two federal states, namely Hamburg and Schleswig-Holstein, do not publish state level price indices and hence have to be excluded from the analysis. The CPI is calculated according to Laspeyre's formula, with the reference year for the entire time series being 2010. Overall, the dataset consist of 1008 state-month observations.

Apart from an overall price index, which is based on all consumer goods, price information is also consistently available for 12 main groups. From these, I restrict the analysis to the following: food and nonalcoholic beverages (group 1), apparel and shoes (group 3), and furniture (group 5).

Table Appendix

Table 8: Further Robustness Checks

	(1)	(2)	(3)
Schleswig-Holstein	-.018**	-.017***	-.015***
	(.008)	(.004)	(.005)
Hamburg	-.018**	-.017***	-.015***
	(.008)	(.003)	(.005)
Lower Saxony	-.015**	-.018***	-.015***
	(.007)	(.004)	(.005)
Bremen	-.018**	-.017***	-.015***
	(.008)	(.003)	(.005)
North Rhine-Westphalia	-.021**	-.018***	-.016***
	(.008)	(.004)	(.005)
Hesse	-.019**	-.017***	-.015***
	(.008)	(.003)	(.005)
Rhineland-Palatinate	-.017**	-.018***	-.016***
	(.009)	(.004)	(.006)
Baden-Wurttemberg	-.022**	-.016***	-.015***
	(.008)	(.004)	(.005)
Berlin	-.019**	-.017***	-.015***
	(.008)	(.003)	(.005)
Brandenburg	-.018**	-.016***	-.014**
	(.008)	(.003)	(.005)
Mecklenburg-West Pomerania	-.018**	-.017***	-.017***
	(.008)	(.003)	(.003)
Saxony	-.018**	-.017***	-.015***
	(.008)	(.003)	(.005)
Saxony-Anhalt	-.019**	-.017***	-.015***
	(.008)	(.003)	(.005)
Thurangia	-.019**	-.017***	-.015**
	(.008)	(.004)	(.005)
Add. Controls	yes	yes	yes
District $\times$ time trends	no	yes	yes
District $\times$ time^2 trends	no	no	yes

Notes: N=3,248. Each cell reports the coefficient on the treatment variable for one regression. Each row indicates, which federal state is excluded from the regression. All regressions include district and year fixed effects. Standard errors in parentheses are clustered at the federal state level. * Significant at 10%, ** at 5%, *** at 1%.

Table 9: Further Robustness Checks

	(1)	(2)	(3)
2003	-.021**	-.012***	-.014***
	(.008)	(.003)	(.005)
2004	-.021**	-.017***	-.015**
	(.008)	(.003)	(.006)
2005	-.017**	-.014***	-.013***
	(.008)	(.003)	(.004)
2006	-.0163*	-.011**	-.014**
	(.008)	(.004)	(.007)
2007	-.025**	-.047***	-.044***
	(.010)	(.009)	(.008)
2008	-.016*	-.013***	-.007**
	(.008)	(.004)	(.003)
2009	-.016**	-.016***	-.015***
	(.007)	(.003)	(.005)
2010	-.017**	-.017***	-.007**
	(.006)	(.004)	(.003)
Add. Controls	yes	yes	yes
District $\times$ time trends	no	yes	yes
District $\times$ time^2 trends	no	no	yes

Notes: N=3,248. Each cell reports the coefficient on the treatment variable for one regression. Each row indicates, which year is excluded from the regression. All regressions include district and year fixed effects. Standard errors in parentheses are clustered at the federal state level. * Significant at 10%, ** at 5%, *** at 1%.

Figure Appendix

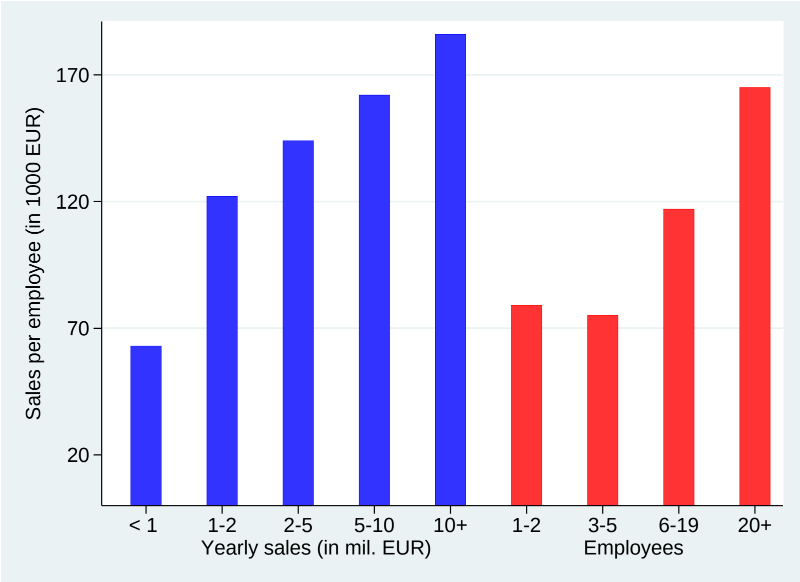

Figure 2: Sales per Employee in 2005, Differentiated by Establishment Size

Notes: Data source: Federal Statistical Office.