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The employment effect of deregulating shopping hours:
Evidence from German retailing

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The employment effect of deregulating shopping hours:
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Abstract: We provide difference-in-differences evidence from Germany on the effect of deregulating weekday shop opening hours on employment in food retailing. Using data on the universe of German shops, we find that relaxing restrictions on business hours increased employment by 0.4 workers per shop corresponding to an aggregate employment effect of 3 to 4 per cent. The effect was driven by an increase in part-time employment while full-time employment was not affected. The statistical significance of these results hinges on assumptions on error correlation, and we hence report inference robust to clustering at different levels. A back-of-the-envelope calculation gives an employment increase by 0.1 workers per additional actual weekly opening hour.

Zusammenfassung: Wir untersuchen den Effekt der Freigabe der Ladenöffnungszeiten auf die Beschäftigung im deutschen Lebensmitteleinzelhandel mit der Differenz-von-Differenzen-Methode. Unter Verwendung von Daten über alle deutschen Einzelhändler finden wir, dass die Ausweitung der gesetzlich erlaubten Ladenöffnungszeiten die Beschäftigung um durchschnittlich 0,4 Arbeitnehmer pro Laden erhöht hat, was einer Zunahme der Beschäftigung um 3 bis 4 Prozent entspricht. Getrieben wird dieser Effekt von einer Zunahme der Teilzeitbeschäftigung, wohingegen die Vollzeitbeschäftigung nicht betroffen war. Die statistische Signifikanz der gefundenen Effekte hängt von den Annahmen an die Störtermkorrelation ab und wir berichten daher Ergebnisse bei Clustering auf verschiedenen Ebenen. Eine Überschlagsrechnung zeigt eine Zunahme der Beschäftigung um 0,1 Arbeitnehmer pro Erhöhung der tatsächlichen wöchentlichen Öffnungszeiten um eine Stunde.

Keywords: Shop opening regulations, employment, retail sector, Germany

JEL-Classification: J23, L51, L81

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1. INTRODUCTION

Despite a general trend towards deregulation, weekday shop opening hours are still restricted in many places. For instance, Euro Commerce (2012) mentions restrictions in 10 out of 30 European countries.[1] When deregulations of such restrictions are debated, proponents typically claim that relaxing such restrictions increases employment whereas opponents insist that it reduces employment (see for an example from Italy Financial Times, 2012; or the German trade union Verdi, 2006). Some opponents of deregulations further argue that unrestricted business hours would not raise employment but create part-time jobs at the expense of full-time jobs (see again Verdi, 2006). We let the data speak and provide difference-in-differences evidence from West Germany on the effect of deregulating weekday opening hours on employment in food retailing.

Previous research neither offers unambiguous theoretical predictions nor empirical evidence on the employment effect of deregulating weekday opening hours. From a theoretical point of view, Gradus (1996) argues that the consequences of longer opening hours at the shop level are unclear since several opposing effects can be at work. Longer opening hours could increase sales and hence the derived labour demand. Another positive effect on employment could stem from the minimum number of workers necessary to keep a shop open. In contrast, if there are congestion costs that decrease labour productivity, the smoothing of sales over more hours could decrease labour demand, ceteris paribus. For instance, fewer cashiers might be necessary to serve customers if these are more equally distributed over a longer period. Additionally, effects on the market structure as modelled by Wenzel (2010; 2011) could also affect employment. Turning to empirical studies, there is evidence for a positive employment effect of lifting Sunday opening restrictions for Canada (Skuterud, 2005) and the USA (Burda and Weil, 2005; Goos, 2004). It is though unclear whether these findings generalise to (i) restrictions of opening hours on weekdays and (ii) to a European setting.

In this article, we provide the first quasi-experimental evidence on the effects of deregulating daily opening hours on employment. Our identification strategy relies on differences in the regulation of opening hours across German states (*Bundesländer*) after a reform of federalism in 2006. While regulations were identical before the reform, states could implement their own law afterwards. This creates exogenous variation in the restrictions faced by retailers, which we use to estimate the effect of these restrictions on total employment as well as on part-time and full-time employment. Furthermore, this is the first quasi-experimental study on

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[1]   The summary includes the 28 member states of the European Union as well as Iceland and Norway.

a deregulation of opening hours in a European country. We also contribute to the literature by providing the first ballpark estimate of the effect of changes in actual opening hours on employment.

To investigate the deregulation's employment effect, we use administrative panel data stemming from the social insurance system on all shops in Germany, i.e. the 100 per cent sample of the IAB Establishment History Panel. Looking at shops with similar employment trends before the reform, we identify the employment effect from this panel data with a difference-in-differences approach. In contrast, there is no systematic data on actual opening hours over time. To get at the effect of actual opening hours on employment, we hence mimic a two sample instrumental variable approach and calculate the deregulation's effect on actual opening hours from recent cross-sectional information under different assumptions. Combining the deregulation's effects on opening hours and employment yields the effect of actual opening hours on employment.

The article proceeds as follows. Section 2 describes the institutional setting of the regulation of shop opening hours in Germany and the source of the variation that we exploit. Section 3 provides information on the dataset and section 4 on the sample construction for the analysis. Section 5 presents the empirical analysis of the deregulation's effect on employment, including robustness checks. Section 6 gives the back-of-the-envelope-calculation of the effect of actual opening hours on employment. Finally, section 7 concludes.

2. THE (DE)REGULATION OF SHOP OPENING HOURS IN GERMANY

The 2006 reform of German federalism moved legislation on shop opening hours from the federal domain to the state domain. Before the reform, a federal law (*Ladenschlussgesetz*) prohibited shops to open on Sundays and holidays as well as Monday to Saturday before 6am and after 8pm, with few exceptions, such as for petrol stations, shops in train stations and in airports. Unlike in other countries, e.g. the United Kingdom, there were no additional exemptions for small stores or convenience stores. After the reform of federalism, which was passed in July 2006 and came into force on 1 September 2006, the federal law stayed in place unless a state issued a law of its own.[2]

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[2] Though the reform came into force soon after its formal approval, there could be anticipatory effects. The draft of the reform bill was discussed for the first time in both chambers of parliament in March 2006. By then, it was already informally approved by major political actors: the parliamentary groups of the governing coalition, the conference of the federal states' prime ministers, and the federal cabinet. The final bill was very similar to this draft, and the reform could hence be anticipated already in early 2006.

Between 15 November 2006 and 1 April 2007, new regulations of shop opening hours came into force in all West German states but Bavaria. Table 1 gives an overview of the regulations after April 2007. In seven out of these nine states, all restrictions on opening hours from Monday to Saturday were lifted. One state, Rhineland-Palatinate, allowed for longer opening hours in the evening, until 10 pm, and another state, the Saarland, adopted a law with only minor changes in comparison to the federal law. The restrictions of opening hours were hence changed for shops in eight states in a very similar way. Restrictions for Sunday openings were by and large unaffected and are therefore not further discussed.

Looking at the two states without major changes, Bavarian shops are more suitable as control group for two reasons. First, Bavaria did not deregulate more or less accidentally. In Bavaria, the parliamentary group of the governing party voted on lifting restrictions on shop opening hours and because of a tie in votes decided not to do so. The tie occurred because one supporter of the deregulation left early for another appointment and could not vote (a detailed account of the events can be found in FAZ, 2006). In contrast, the Saarland adopted a law of its own without major deregulations. What is more, there were some minor changes in the Saarland, for instance shops are now allowed to open for 24 hours once per year. Including the Saarland in the control group would thus lead to a control group that includes slightly treated shops as well as untreated shops. At the same, time shops in the Saarland were differently treated than shops in other states that changed the regulation making it infeasible to include these shops in the treatment group. Hence, we exclude shops in the Saarland from our sample.

A meaningful analysis of the employment effects of such a reform requires that shops changed behaviour in response to the deregulation; otherwise, any employment effects would be implausible. In our case, a substantial share of shops extended opening hours beyond the previous constraints. In early 2007, shoppers across several German cities were asked whether the shops at which they typically buy certain goods are open after 8pm. Depending on the good, between 9 per cent (leather goods) and 47 per cent (food) of shoppers said that these shops are open after 8pm (Kaapke et al., 2007). Since shops had to close at 8pm prior to the reform, these numbers show that the previous restrictions on opening hours were a binding constraint for a substantial portion of the retail industry and that the reform affected shops' behaviour. Changes in shop behaviour are also confirmed by other sources and our own data collection (see sections 4 and 7). It is hence plausible that the deregulation could have effects on employment.

3. EMPLOYMENT DATA

In our empirical analysis, we use the 100 per cent sample of the Establishment History Panel (BHP) of the Institute for Employment Research (for an overview of the dataset see Spengler, 2008; a detailed description of the latest version can be found in Gruhl et al., 2012). The dataset is based on information from the social insurance system, where every establishment is obligated to report information on all employees who are subject to the social security system. While this comprises all regular and marginal employees, self-employed persons and unpaid family members are not included in the data. The individual information as of 30 June is then aggregated at the establishment level in each year. This procedure yields a panel of annual data comprising all establishments with at least one employee.

The establishment level characteristics in the dataset include the total number of employees, the main outcome variable for our analysis, as well as information on the numbers of part-time and full-time employees, but no detailed information on working hours.[3] Furthermore, the dataset includes information on establishment location such that each establishment can be assigned to a federal state, regional district, and municipality. This is of particular importance since the estimation and identification of the present analysis relies on a variation in the deregulation of opening hours on the state level. The dataset also includes a 5-digit industry identifier. Since the industry classification changes in 2003 and 2009, we restrict our analysis to the years 2003 to 2008.

The data allows us to follow establishments over time. An establishment is defined as a worksite of a firm in a certain municipality, which is either the whole firm or a branch. If a firm has several worksites, here stores, in the same municipality, it may use the same establishment number for all notifications from these sites or it can use a different number for each site (and it may change this decision). Hence, an establishment comprises information on a firm, all branches of a chain in one municipality, or a single branch. In the dataset, each establishment has a unique identifier. This identifier is normally time constant and changes only in case of events such as a change of ownership or the legal form. Thus, the identifier allows us to track establishments. For ease of reading, we will from here on use the term shop to refer to establishments.

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[3] While we calculate employment as the sum over part- and full-time employment, the dataset also includes direct information on the total number of employees. The original employment variable from the dataset comprises also apprentices and home workers, who are not classified by working hours and hence not included in our employment variable. We ran all regressions also using the alternative measure, which does not alter our results, see Online Appendix A. However, for our main analysis, we use our employment variable so that the effects for full-time and part-time employment add up to the total effect.

The deregulation can potentially affect employment via three channels: employment in surviving shops, market entry, and market exit. Ideally, we would be able to identify newly-founded and exiting shops in our data via the shop identifier. However, shops can also be missing in the original data for other reasons. First, shops may be missing in some years because of changes in the identifier or the splitting up and consolidation of identifiers. These events would simply reallocate employment across identifiers. A change of identifiers would for instance show up as a new shop entering and the old shop dropping out of the sample, while no substantial change in employment has occurred. Second, shops may only be employing in some years while in other years only the owner and family members work in the shop. Since owners and their families are typically not liable to the social security system, such shops are not observed though they still exist. Unfortunately, there is no reliable procedure to identify such events in our data and we can therefore not take a closer look into the three channels through which employment could be affected.[4]

Notwithstanding these data limitations, we can obtain the shop-level effect of the deregulation on employment from a shop-level regression. To estimate the effect, we use a panel of all shops that have at least one employee in at least one year in our observation period and balance this panel by defining employment as zero if a shop is not observed in some year. If missing observations reflect only existing shops without employees, all observations actually exist and as the balanced panel contains their true employment, the estimated treatment effect is the average treatment effect on the treated. This balancing appears also appropriate for shops that enter or exit the market because of the treatment. If entries and exits are affected by the treatment, the coding ensures that the estimation captures these employment changes. However, to the extent that missing observations reflect changing identifiers or shop entries and exits independent of the treatment, the coding adds shops to our sample that do not exist and hence have a treatment effect of zero. Since our estimate averages the treatment effect also over these inexistent shops, it is biased towards zero. The ratio of the treatment effect to the average shop size is however unaffected since the average shop size in the sample is equally biased towards zero by inexistent shops.[5] By the construction of our analysis sample, our estimate of the treatment effect is thus a lower bound of the

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[4] Hethey-Maier and Schmieder (forthcoming) provide a procedure to identify market entries and exits. However, this procedure is based on worker flows and is not applicable for small establishments, which make up a major portion of our sample. Therefore, we do not apply the procedure.

[5] The average treatment effect over N observations is $\delta = N^{-1} \sum_{i=1}^{N} \delta_i$ and the average shop size is $\mu = N^{-1} \sum_{i=1}^{N} \mu_i$. Adding k inexistent observations with zero treatment effect and employment gives $\delta^k = (N+k)^{-1}(\sum_{i=1}^{N} \delta_i + k*0)$ and $\mu^k = (N+k)^{-1}(\sum_{i=1}^{N} \mu_i + k*0)$. Relating both yields $\delta^k/\mu^k = ((N+k)^{-1} \sum_{i=1}^{N} \delta_i)/((N+k)^{-1} \sum_{i=1}^{N} \mu_i) = \delta/\mu$.

shop-level effect, but it still gives the relative size of the treatment effect in comparison to the average shop size.

4. CONSTRUCTION OF THE ANALYSIS SAMPLE

To make the parallel trends assumption and the unaffectedness of the control group plausible, we confine our analysis by industry and location. Based on the industry identifier, we restrict the sample to food retailing for three reasons. First, food retailers reacted to the deregulation by extending opening hours. The survey by Kaapke et al. (2007) mentioned above points towards a relatively large impact of the deregulation. Furthermore, 3,000 out of 5,700 stores of the second largest German food retailer, Rewe, were open after 8pm in early 2012 (Welt, 2012), which is fairly similar to regionally limited numbers for the same retail chain from spring 2007 (FAZ, 2007). Finally, we collected data on opening hours of Edeka stores, the largest German food retail chain (for details see section 6). 203 out of 410 stores in states that deregulated are open after 8pm. Hence, the restriction on opening hours was binding for a substantial portion of food retailers making plausible that lifting the constraint affected shop behaviour. Second, food retailers are least likely to be affected by cross-border shopping caused by the change in opening hours. While shoppers might be willing to drive some distance to enjoy longer opening hours for event-like shopping, this appears to be less relevant for day-to-day groceries. Third, food retailers are least affected by the rise of online retailing. It is plausible that the effects of online retailing depend on regional characteristics, such as income and population density, which would violate the common trends assumption. Focusing on food retailers hence strengthens both the unaffectedness of the control group and the common trends assumption.

Regarding shop location, we restrict the sample to West Germany. The parallel trends assumption in absence of the treatment is more plausible for West German states since East Germany is still affected by lower economic development, high unemployment rates, substantial migration to the West, and finally East German states have a differing industrial structure and are still heavily subsidised. All of these issues point towards differences between East and West German states making the parallel trends assumption less plausible than for the more homogeneous group of West German states.

Finally, the FIFA World Cup took place in several West German cities and states between 9 June and 9 July 2006 making 2006 unsuitable as the last pre-treatment period. During the World Cup, some of the federal states relaxed the restrictions on opening hours, while others allowed municipalities to decide on the opening hours for that period. On the political side, the willingness to relax the restriction

temporarily is likely to correlate with the willingness to relax it permanently. On the firm's side, shops could already anticipate the reform and they could be more likely to react to a temporary change when it will soon become permanent. Thus, employment in the treatment and the control group could be affected differently in 2006, and we exclude this year from the analysis sample. Hence, we use 2003 to 2005 as pre-treatment period and 2007 to 2008 as treatment period.[6]

Panel A of Figure 1 yields a graphical illustration of the analysis when using shops in Bavaria as the control group and shops in the eight West German states that deregulated as the treatment group. The lines present the average employment per shop by treatment status over the period 2003 to 2008 centred on the values in 2005. Before the treatment, employment rose faster in the states that deregulated than in Bavaria, making a difference-in-differences analysis for these groups infeasible. We therefore take a closer look at the single states to find comparable treatment and control groups. Panel B shows the evolution of employment by state until 2005. Two features stand out in this graph. In all states but Bremen shops experienced increasing employment between 2003 and 2005. Furthermore, employment decreased between 2004 and 2005 in shops in all states but North Rhine-Westphalia and Schleswig-Holstein. Shops in the remaining six states have very similar employment trends, making a difference-in-difference analysis feasible, and we focus on these in the remaining paper.

5. THE DEREGULATION'S EMPLOYMENT EFFECT

Graphical and Descriptive Evidence

Panel A of Figure 2 gives the employment patterns for the treatment group (shops in Baden-Wuerttemberg, Hamburg, Hesse, Lower Saxony, and Rhineland-Palatinate) and the control group (shops in Bavaria) over the years 2003 to 2008. Looking at the pre-treatment period, employment trends are indeed very similar in both groups making the parallel trends assumption in the absence of the treatment plausible. Looking at 2007, the first post-treatment period, we observe that employment increased more in deregulated shops than in Bavarian shops. This difference amounts to about 0.4 employees per shop, which remains stable in 2008. This first result indicates a modest positive effect of the deregulation on employment.

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[6]   In fact, we observe a small placebo effect in 2006. Figure A1 in the appendix gives employment patterns on a monthly basis for our final sample. It shows that the placebo effect for 2006 in the annual data is only temporary. To scrutinize our analysis, we redid all regressions including 2006. The results are almost identical as can be seen from Online Appendix B.

Extending our sample over both reclassifications of the industry identifiers does not affect this pattern. Panel B replicates the graphical analysis for the years 1999 to 2010. As before, we see parallel movements in employment until 2005 and an increase in employment in treated shops after the deregulation that remains stable until the end of the observed period. Hence, using this extended period does not promise additional insights but adds noise to the analysis and we only look at the years 2003 to 2008.

Figure 3 shows the development of part-time employment (Panel A) and full-time employment (Panel B) by treatment status. For both types of employment, pre-treatment trends are very similar across both groups, which again suggests parallel trends in the absence of treatment. In the treatment period, we see a more pronounced increase in part-time employment for the treatment group than for Bavarian shops. For full-time employment, we observe only minor differences in the treatment period with a slightly stronger increase in the treated shops. These patterns suggest that differences in total employment are driven by the change in part-time employment.

Table 2 presents descriptive statistics by treatment status for the years 2005 and 2007. The sample consists of 20,788 shops in deregulating states and 8,795 shops in Bavaria. By construction of our sample, the number of shops does not change over time. In 2005, shops in deregulating states had on average 10.72 employees and were thus slightly larger than shops in Bavaria that had on average 10.19 employees. Between 2005 and 2007, the average difference in shop size by treatment status increases from 0.53 employees in 2005 to 0.94 employees in 2007. This implies a descriptive difference-in-differences estimate of the treatment effect of 0.41 employees per shop. In addition, Table 2 provides information by working time. In 2005, shops in deregulating states employed on average 7.86 part-time workers compared with 7.03 at Bavarian shops. The difference in part-time employment by treatment status increases after the deregulation yielding a descriptive difference-in-difference estimate of 0.33 employees. Turning to full-time employment, we see that shops in deregulating states employed on average 2.86 full-time workers compared with 3.16 at Bavarian shops. This difference remains almost constant after the deregulation.

Regression Analysis

In the regression analysis the treatment effect of interest is obtained from a standard difference-in-differences specification at the shop-level:

$$employment_{it} = \delta\, deregulated_{it} + \gamma_t + \alpha_i + \varepsilon_{it} \qquad (1)$$

The dependent variable in this baseline specification is the total number of employees in shop *i* at time *t*. *Deregulated$_{it}$* is the treatment dummy of interest, which is 1 for shops in the treatment group during the treatment period, and its coefficient δ is the average treatment effect on the treated shops. The baseline specification also includes year dummies, γ_t, as well as shop fixed effects, α_i. ε_{it} is the error component.

Turning to statistical inference on δ, we report results from two different approaches. First, we allow for arbitrary error correlation within districts, but not across districts. This captures not only local common shocks, but also takes into account data limitations like changing shop identifiers. Since shops are located across 248 districts ("Landkreise"), we report Huber-White cluster robust standard errors. While this assumption on the error distribution is rather restrictive, this approach should be reasonably powerful to detect moderate effects. Second, we relax the assumption of independent errors across districts and instead cluster at the state level, which is also the level of the identifying variation. Since this leaves us with only six clusters, we do not rely on analytical standard errors but bootstrap p-values using the wild cluster bootstrap procedure imposing the null hypothesis as proposed by Cameron et al. (2008). The second approach is robust to correlated state-level shocks and appears to provide valid p-values even with as few as six clusters. However, it is very conservative (probably more conservative than necessary) and has little power to detect effects of moderate size (see the simulation results of Cameron et al., 2008). Given these strengths and weaknesses, we report results from both approaches.

The first Column of Table 3 reports the results from the baseline specification. We find an increase of 0.42 employees after deregulating opening hours. Compared with the average shop size, this effect amounts to an employment increase by 3.9 per cent. Using standard errors clustered at the district level, we get a p-value below 0.01 and would hence reject the null of no effect. Clustering at the state-level gives a p-value of 0.19 and we would thus not reject the null.[7]

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[7]   Running the baseline regression using shops in all West German states but the Saarland, we obtain very similar point estimates and a bootstrapped p-value of 0.09. However, we do not pursue this line of investigation further since the parallel trends assumption is more plausible for the smaller sample.

Columns 2 and 3 of Table 3 show results from robustness checks. First, we add state-specific linear and quadratic time trends to our regression. Second, we add the logged disposable income and logged population at the district level as control variables. In both specifications, we get almost identical point estimates for the treatment effect of 0.38 and 0.41, which are again both statistically significant when clustering at the district level. When clustering at the state level, the p-value remains almost constant with trends included but drops to 0.08 if control variables are added. These robustness checks hence support our identification strategy.

Finally, we do a standard placebo test using pre-treatment observations only and assign the treatment status to shops in the treatment group in the last pre-treatment period. This estimation tests whether an anticipation of the treatment and therefore a reaction before the actual treatment period is observed. Furthermore, it is a regression-based test for a deviation from the parallel trends assumption in the last pre-treatment period. The estimated placebo effect is practically zero, supporting the parallel trends assumption.

The results so far show that the deregulation increased the number of employees, but we do neither know whether full-time or part-time jobs were created nor whether this reflects a crowding out of full-time jobs by part-time jobs. The Establishment History Panel provides us with the numbers of part-time and full-time employees in each shop, and we use these variables as dependent variables in separate regressions. Table 4 presents the treatment effect for both outcomes. As suggested by the graphical analysis we find an increase in part-time employment by 0.35 workers, but only an effect of 0.07 additional full-time workers. The effect on part-time employment is statistically significant when clustering at the district level as well as when clustering at the state level, with a p-value of 0.04. The small effect on the number of full-time workers is insignificant for both modes of inference. Doing the same robustness checks as before confirms the baseline estimates. These results show that the deregulation increased the number of part-time jobs, but did not affect the number of full-time jobs.

Further Robustness Checks

Before turning to the effect of actual opening hours on employment, we check the robustness of the deregulation's employment effect. As discussed above, the treatment group consists of shops in states with similar employment trends prior to the deregulation. To check robustness against a different selection of deregulating states and rule out that our results are driven by shops in a single state, we redo our baseline estimation dropping shops from each deregulating state at a time. Table 5 presents the results showing a treatment effect close to 0.4 irrespective of

the excluded state. The point estimates range from 0.35 to 0.51. All estimated effects are significant when clustering standard errors at the district level. When clustering at the state level, p-values range from 0.07 to 0.28. Hence, excluding shops from one deregulating state at a time does not alter our results.

Next, we address cross-border shopping as a channel through which the treatment could also affect shops in the control group. If this were the case, the estimated effect would reflect a redistribution of employment across states rather than the creation of additional employment. As argued above, the extent of cross-border shopping should be rather limited in food retailing. What is more, Column 1 of Table 6 shows that results hardly change when we drop shops from all districts which flank the border between a deregulating state and Bavaria.

Finally, we test whether the 2004 enlargement of the European Union (EU) violates the parallel trends assumption. Bavaria is the only state in our sample neighbouring a new member state, namely the Czech Republic. Hence, shops close to the border could be affected by the expansion while shops in the treatment group were not. We hence drop all shops in districts located at the border to the Czech Republic from the sample. As shown in Column 2 of Table 6, the results are again very similar and also hold when combining both restrictions as can be seen in the third Column.

6. BACK-OF-THE-ENVELOPE CALCULATION OF THE EFFECT OF ACTUAL OPENING HOURS ON EMPLOYMENT

So far, we have estimated the effect of relaxing restrictions on opening hours. The results are however not informative about the effect of actual opening hours on employment. This effect could in principle be obtained by running a regression of employment on opening hours and instrumenting opening hours with a dummy for the deregulation. Since we cannot match actual opening hours to shops in the Establishment History Panel, we pursue an indirect approach along the lines of a two sample instrumental variable estimation to obtain this effect.

Using two sample instrumental variable estimation, one can identify causal effects with moments from different samples. The conceptual starting point is the Wald estimator for one sample with a dummy instrument, as is our deregulation indicator. The instrumental variable estimate of the treatment effect in such a setting is the sample analogue of

$$\frac{E(employment_i|deregulation_i = 1) - E(employment_i|deregulation_i = 0)}{E(opening\ hours_i|deregulation_i = 1) - E(opening\ hours_i|deregulation_i = 0)}$$

To get the effect of interest, one can hence divide the reduced form by the first stage, i.e. we get the effect of opening hours on employment by dividing the deregulation's employment effect by the effect on opening hours. As derived by Angrist and Krueger (1992), the two sample moments can also be estimated from two different samples.

Along these lines, we combine the deregulation's effect on employment with an estimate of the effect on actual opening hours from a different sample to uncover the effect of actual opening hours on employment.[8] As there is no systematic data on opening hours over the period of analysis, we use recent data on opening hours for the largest German food retail chain, Edeka, to get an estimate of the first stage.[9]

Being aware that we pursue a rather indirect approach, we compute the effect of the deregulation on opening hours under different assumptions to get a range of possible results to see how stable the estimated effect is. For one thing, we vary our assumptions regarding the representativeness of the stores for which we have data on opening hours. We start with presuming that the opening hours of our sample of Edeka stores are representative for retailing over all cities and hence compute the average opening hours without weighting. However, comparing the numbers of Edeka stores with our employment sample by cities shows that the prevalence of Edeka stores differs across cities. Therefore, we alternatively assume that the stores' opening hours are representative at the level of each city and weight the city level averages by the number of shops according to the Establishment History Panel. We furthermore use different points of comparison to calculate the effect on opening hours. First, we compare shopping hours across states starting from the premise that shops would choose opening hours of equal length if they were subject to the same constraints. Second, we assume that shops only extended opening hours beyond the previous constraint but otherwise did not change their behaviour and look at opening hours of shops in deregulated states that were not permitted under the federal regulation. Combining these variants with the different weighting schemes gives four estimates of the change in opening hours.

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[8] A more detailed exposition of two sample instrumental variable regression can be found in Angrist and Pischke (2009, 147–150). Getting correct standard errors is difficult in our setting. However, since we only provide a rough estimate, we refrain from doing statistical inference and report point estimates only.

[9] Edeka is the largest food retailer by sales in Germany (see Lebensmittelzeitung, various years). It comprises shops owned and run by the cooperative as wells as shops of more than 4,000 individual shop owners. We collected the opening hours of all Edeka stores in all cities with more than 100,000 inhabitants in our sample using the online store locator. This data is available on request.

Table 7 presents descriptive statistics on opening hours by treatment status and the results from the computations outlined above. Shops are open on average for about 75 hours per week and opening hours are longer for shops in states that lifted restrictions. Without weighting, deregulated stores are on average 3.6 hours longer open than shops in Bavaria and are open 4.0 hours beyond previous constraints. Weighting by the number of shops in cities according to the Establishment History Panel increases our estimates of the effect of the deregulation on opening hours. We now find that shops in deregulating states are open for 4.7 hours more per week than shops in Bavaria and 4.9 hours after 8pm. Hence, our first stage estimates range from 3.6 to 4.9 hours per week, which corresponds to an increase in weekly opening hours by 4.9 to 6.7 per cent.

Combining these first stage results with the reduced form, which is given by the employment effect of the deregulation, yields the effect of actual opening hours on employment. Extending opening hours by one hour per week hence raises employment by 0.09 to 0.12 workers or 0.7 to 1.1 per cent compared with the average shop size in deregulating states in 2005.[10] We emphasize that this is only a back-of-the envelope calculation. First, we have to infer the effect on opening hours from more recent cross-sectional data. Second, we attribute the complete treatment effect to changes in opening hours ignoring potential employment effect via market entry and exit as we cannot identify these. Both limitations may lead to an over- or underestimation of the effect of actual opening hours on employment.

7. CONCLUSIONS

We study the effect of deregulating weekday shop opening hours on employment in food retailing in Germany. The identification relies on variation in the deregulation of opening hours across German states after the reform of federalism in 2006, which allows for a difference-in-differences approach. We find that lifting restrictions on weekday opening hours increased employment by 0.4 employees per shop, equivalent to an increase by 3.9 per cent. The deregulation affected part-time employment positively while it did not affect full-time employment. Most of the estimated effects are statistically significant when clustering standard errors at the district level, but not when clustering at the state level. Given the small number of states, it is plausible that this reflects low power of the tests when clustering at the state level rather than the absence of a substantial effect.

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[10] Estimating the reduced form only for cities for which we have information on opening hours yields a very similar employment increase ranging from 0.09 to 0.13 additional employees per additional opening hour.

Our results contradict claims that deregulating weekday shop opening hours has adverse employment effects in the retail sector, but point towards positive effects. In total, our estimates imply a rise in employment by about 8,700 employees in the deregulating states in food retailing. Deregulating shop opening hours hence appears to be a promising policy to create jobs. However, other costs are attached to the deregulation, for instance losses in coordinated leisure, such that it is open to debate whether the number of jobs created compensates for such costs. Furthermore, one has to keep in mind that we do not know about effects on other types of retailers and effects on other businesses, such as restaurants.

Combining our estimated employment effect with recent cross-sectional data on opening hours, we also provide ballpark estimates of the effect of actual opening hours on employment. We find that one additional opening hour per week raises employment by around 0.1 employees. Since mostly part-time jobs were created, an effect of one additional worker per ten weekly opening hours appears of plausible size.

We emphasize several limitations of our analysis. Absent more detailed information on working hours, we cannot say with finality that employment measured in hours increased, though the observed pattern suggests that it did. Moreover, we neither observe effects on self-employed individuals nor is an investigation of effects on market structure suitable with our data. Besides addressing these limitations, future research should also broaden the focus and look for instance at the effects of a deregulation on other outcomes such as time allocation. Finally, it remains unclear whether our results on the deregulation's effect as well as on the effect of additional actual opening hours generalise to other settings, which calls for replications.

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Table 1:   Opening hours regulations by state

Federal state	Regular potential opening hours in treatment period	State law came into force in
Baden-Wuerttemberg	no restrictions	March 2007
Bavaria	6am to 8pm	No state law
Bremen	no restrictions	April 2007
Hamburg	no restrictions	January 2007
Hesse	no restrictions	December 2006
Lower Saxony	no restrictions	April 2007
North Rhine-Westphalia[*]	no restrictions	November 2006
Rhineland-Palatinate	6am to 10pm	November 2006
Saarland[**]	6am to 8pm	November 2006
Schleswig-Holstein	no restrictions	December 2006

The restrictions apply Monday to Saturday. Sunday restrictions have practically not changed and are therefore not listed.
[*]) A stricter regulation came into force in May 2013, several years after our observation period.
[**]) The state law in Saarland implemented some changes compared to the old federal law like allowing for longer opening hours once per year at the municipality level.
Source: Federal law and various state laws.

Table 2:   Descriptive statistics

Shops located in	Number of Shops	Average number of employees					
		all		part-time only		full-time only	
		2005	2007	2005	2007	2005	2007
Deregulating states	20,788	10.72	11.50	7.86	8.60	2.86	2.90
Bavaria	8,795	10.19	10.56	7.03	7.44	3.16	3.12
Difference		0.53	0.94	0.83	1.16	-0.30	-0.22
Difference-in-Differences			0.41		0.33		0.08

Notes: Deregulating states are Baden-Wuerttemberg, Hamburg, Hesse, Lower Saxony, and Rhineland-Palatinate.
Source: Establishment History Panel.

Table 3: Deregulation effect on total employment

	(1) Baseline	(2) State-specific trends	(3) Control variables	(4) Placebo
Treatment effect	0.418	0.383	0.409	0.033
	(0.157)	(0.169)	(0.170)	(0.077)
	[0.19]	[0.21]	[0.08]	[0.44]
Observations		147,915		88,749

Note: Dependent variable is the number of employees at a shop. Standard errors clustered at the district level in parentheses. Two-sided p-values from a wild cluster bootstrap (state level clusters) with 1,000 iterations in brackets. All regressions include shop as well as year fixed effects. Column (2) includes linear and quadratic state-specific trends, Column (3) includes log(disposable income) and log(population) at the district level as controls. Analysis is based on artificially balanced panel, 2003 to 2008. In Column (4), only data until 2005 is used and 2005 is coded as treatment year.
Source: Establishment History Panel.

Table 4: Treatment effects on part-time and full-time employment

Dependent variable	(1) Baseline	(2) State-specific trends	(3) Control variables	(4) Placebo
Number of part-time	0.347	0.281	0.343	0.031
employees	(0.089)	(0.118)	(0.093)	(0.052)
	[0.04]	[0.32]	[0.03]	[0.68]
Number of full-time	0.072	0.102	0.067	0.002
employees	(0.100)	(0.089)	(0.109)	(0.042)
	[0.36]	[0.12]	[0.29]	[0.84]
Observations		147,915		88,749

Notes: See Table 3.

Table 5: Regression results excluding each deregulating state at a time

	(1)	(2)	(3)	(4)	(5)
			Excluded state		
	Hesse	Baden-Wuerttemberg	Lower Saxony	Hamburg	Rhineland-Palatinate
Treatment effect	0.349	0.453	0.513	0.399	0.402
	(0.151)	(0.180)	(0.173)	(0.159)	(0.164)
	[0.13]	[0.23]	[0.07]	[0.28]	[0.17]
Observations	126,470	115,235	116,130	143,300	134,500

Notes: Dependent variable is the number of employees at a shop. See further notes to baseline regression from Table 3.

Table 6: Regression results excluding border regions

	(1)	(2)	(3)
		Excluded border regions	
	Bavaria–deregulating states	Bavaria–Czech Republic	Both
Treatment effect	0.387	0.455	0.426
	(0.180)	(0.163)	(0.189)
	[0.25]	[0.20]	[0.25]
Observations	128,655	144,370	125,115

Notes: Dependent variable is the number of employees at a shop. See further notes to baseline regression from Table 3.
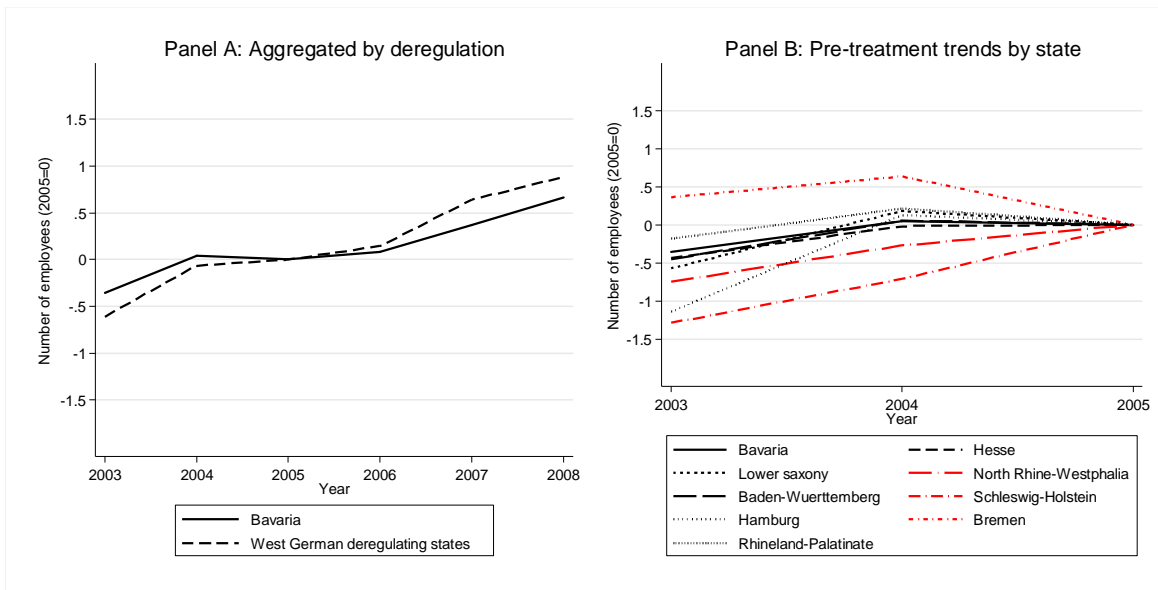
Table 7: Opening hours by treatment status

	Cities	Edeka stores	Avg. over Edeka stores		Avg. weighted by BHP establishments	
			opening hours per week	hours after 8 pm per week	opening hours per week	hours after 8 pm per week
Bavaria	8	141	73.1	0	73.1	0
Deregulating states	15	410	76.7	4.0	77.8	4.9
Difference			3.6	4.0	4.7	4.9

Notes: Bavarian cities are Augsburg, Erlangen, Fuerth, Ingolstadt, Munich, Nuremberg, Regensburg and Wuerzburg. Cities in deregulating states are Braunschweig, Darmstadt, Frankfurt, Freiburg, Hamburg, Hannover, Karlsruhe, Kassel, Ludwigshafen, Mainz, Mannheim, Oldenburg, Osnabrueck, Stuttgart, and Wiesbaden.
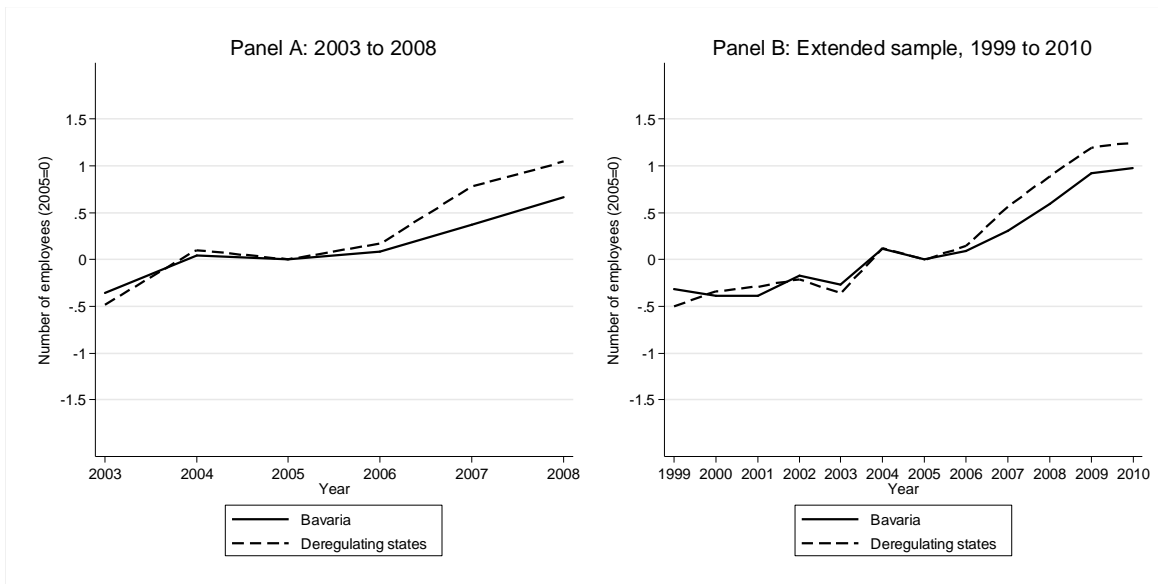*Source:* Own data collection using Edeka's online store locator.

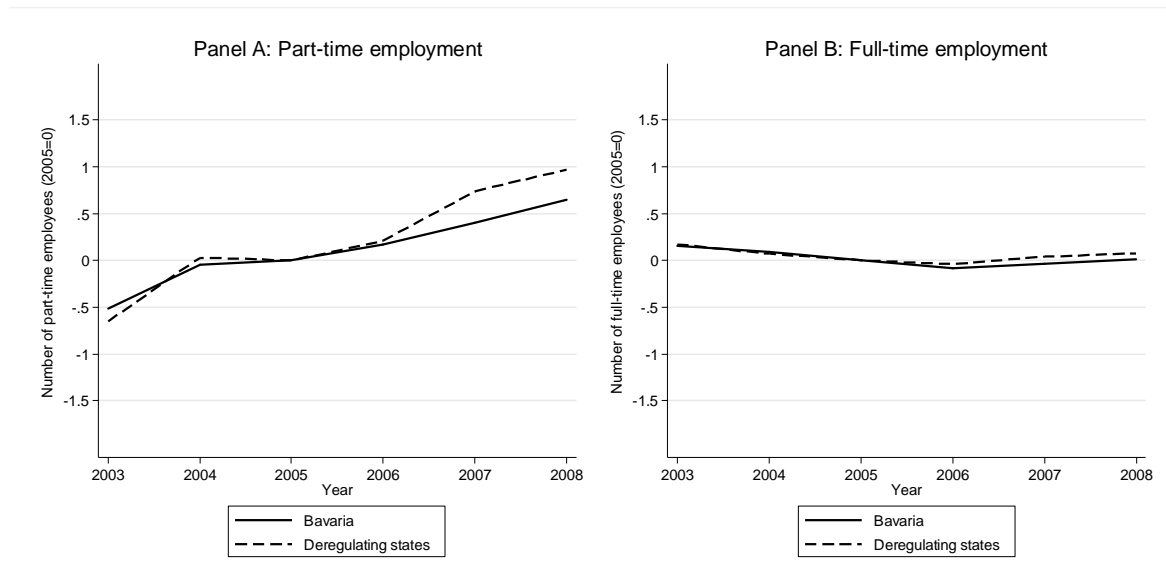**Figure 1:** Average employment per shop in West-Germany

Notes: The graphs show the average numbers of employees per shop centred at the values of 2005. Deregulating states in Panel A comprise all West German states but Bavaria and the Saarland.

Figure 2: Average employment per shop by treatment status, analysis sample only

Notes: The graphs show the average numbers of employees per shop centred at the values of 2005. Deregulating states are Baden-Wuerttemberg, Hamburg, Hesse, Lower Saxony, and Rhineland-Palatinate.
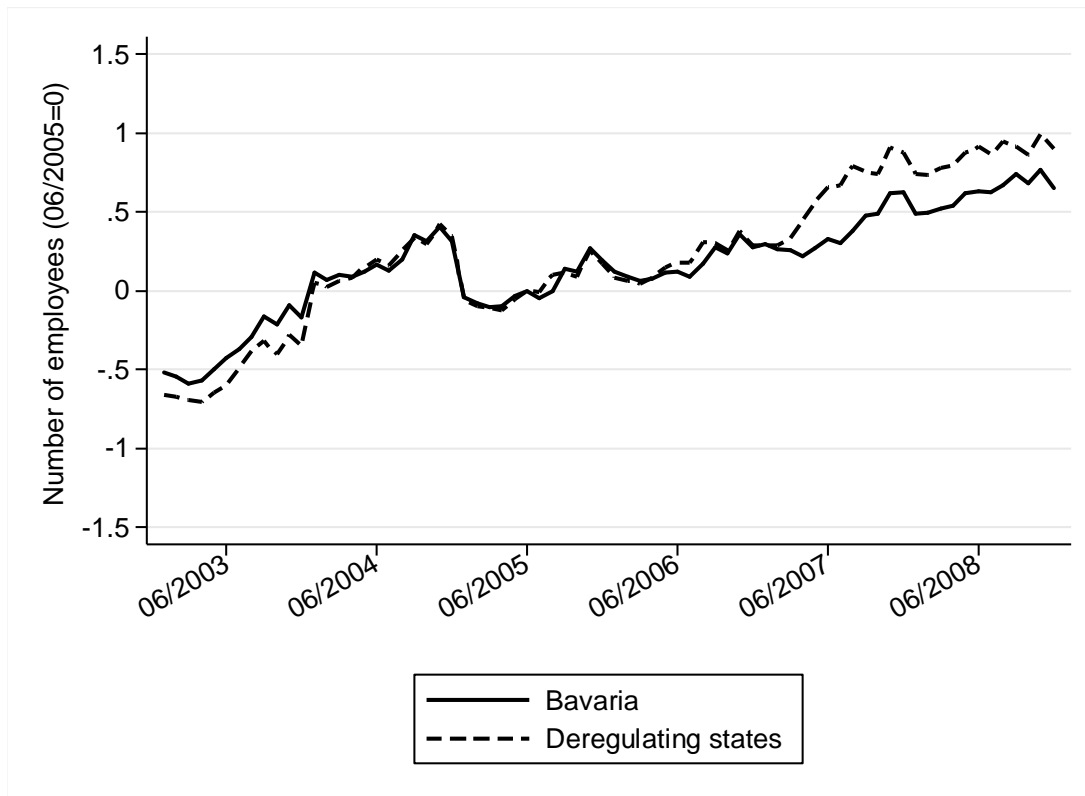
Figure 3: Average full-time and part-time employment per shop by treatment status, analysis sample only

Notes: The graphs show the average numbers of part-time and full-time employees per shop centered at the values of 2005. See further notes to Figure 2.

APPENDIX

Figure A1:   Monthly average employment per shop by treatment status, analysis sample only

Notes: The graph shows the average numbers of employees per shop by treatment status to the 15th of each month from January 2003 to December 2008. Values are centred at the values of 15 June 2005. Deregulating states are Baden-Wuerttemberg, Hamburg, Hesse, Lower Saxony, and Rhineland-Palatinate.
Source: Data source are the Integrated Employment Biographies of the Federal Employment Agency. We thank the Institute for Employment Research for provision of this special data product. The sample consists of the individual social security information of each individual employed at one of the establishments included in our analysis sample at the respective point in time. We aggregate the total number of employees at the establishment level to the 15th of each month.

Online Appendix A: Alternative measure for total employment

In this Online Appendix, we replicate our regression results for total employment using an alternative measure of total employment. While total employment in the paper is defined as the sum over part-time and full-time employment, we here use the total employment variable given in the BHP. This alternative measure of employment additionally includes apprentices and home workers. However, the results are robust to the choice of the employment variable.

Appendix Table A1: Deregulation effect on total employment (replication of Table 3)

	Baseline	State-specific trends	Control variables	Placebo
Treatment effect	0.415	0.381	0.406	0.060
	(0.165)	(0.175)	(0.178)	(0.084)
	[0.14]	[0.17]	[0.07]	[0.24]
Observations		147,915		88,749

Note: Dependent variable is the number of employees at a shop as given in the dataset. Standard errors clustered at the district level in parentheses. Two-sided p-values using a wild cluster bootstrap (state level clusters) with 1,000 iterations in brackets. All regressions include shop as well as year fixed effects. Column (2) includes linear and quadratic state-specific trends, Column (3) includes log(disposable income) and log(population) at the district level as controls. Analysis is based on artificially balanced panel, 2003 to 2008. In Column (4), only data until 2005 is used and 2005 is coded as treatment year.
Source: Establishment History Panel.

Appendix Table A2: Regression results excluding each deregulating state at a time (replication of Table 5)

	Excluded state				
	Hesse	Baden-Wuerttemberg	Lower Saxony	Hamburg	Rhineland-Palatinate
Treatment effect	0.363	0.423	0.504	0.394	0.405
	(0.158)	(0.187)	(0.182)	(0.167)	(0.172)
	[0.09]	[0.17]	[0.05]	[0.18]	[0.17]
Observations	126,470	115,235	116,130	143,300	134,500

Notes: Dependent variable is the number of employees at a shop. See further notes to baseline regression from Table A1.

Appendix Table A3: Regression results with exclusion of border regions (replication of Table 6)

	Excluded border regions		
	Bavaria–deregulating states	Bavaria–Czech Republic	Both
Treatment effect	0.387	0.454	0.431
	(0.189)	(0.170)	(0.198)
	[0.23]	[0.11]	[0.25]
Observations	128,655	144,370	125,115

Notes: Dependent variable is the number of employees at a shop. See further notes to baseline regression from Table A1.

Online Appendix B: Including 2006 to the analysis

In this Online Appendix, we replicate all regression results of the paper including the observations from 2006 to our analysis sample. 2006 is now defined as the last period before treatment. The results presented below show that, besides a slightly larger placebo effect, the treatment effects are robust to the choice of the last treatment period.

Appendix Table B1:   Deregulation effect on total employment (replication of Table 3)

	Baseline	State-specific trends	Control variables	Placebo
Treatment effect	0.392	0.348	0.381	0.109
	(0.148)	(0.120)	(0.157)	(0.083)
	[0.21]	[0.07]	[0.09]	[0.27]
Observations		177,498		118,332

Note: Dependent variable is the number of employees at a shop as given in the dataset. Standard errors clustered at the district level in parentheses. Two-sided p-values using a wild cluster bootstrap (state level clusters) with 1,000 iterations in brackets. All regressions include shop as well as year fixed effects. Column (2) includes linear and quadratic state-specific trends, Column (3) includes log(disposable income) and log(population) at the district level as controls. Analysis is based on artificially balanced panel, 2003 to 2008. In Column (4), only data until 2005 is used and 2005 is coded as treatment year.
Source: Establishment History Panel.

Appendix Table B2:   Treatment effects on part-time and full-time employment (replication of Table 4)

Dependent variable	Baseline	State-specific trends	Control variables	Placebo
Number of part-time	0.332	0.321	0.326	0.059
employees	(0.083)	(0.078)	(0.086)	(0.055)
	[0.03]	[0.05]	[0.03]	[0.16]
Number of full-time	0.060	0.027	0.055	0.050
employees	(0.094)	(0.070)	(0.100)	(0.051)
	[0.37]	[0.45]	[0.35]	[0.45]
Observations		177,498		118,332

Notes: See Table B1

Appendix Table B3: Regression results excluding each deregulating state at a time (replication of Table 5)

	Excluded border regions				
	Hesse	Baden-Wuerttemberg	Lower Saxony	Hamburg	Rhineland-Palatinate
Treatment effect	0.318	0.433	0.479	0.372	0.378
	(0.141)	(0.169)	(0.163)	(0.149)	(0.155)
	[0.13]	[0.23]	[0.07]	[0.24]	[0.22]
Observations	151,764	138,282	139,356	171,960	161,400

Notes: Dependent variable is the number of employees at a shop. See further notes to baseline regression from Table B1.

Appendix Table B4: Regression results excluding border regions (replication of Table 6)

	Excluded border regions		
	Bavaria–deregulating states	Bavaria–Czech Republic	Both
Treatment effect	0.359	0.426	0.396
	(0.170)	(0.153)	(0.179)
	[0.30]	[0.18]	[0.24]
Observations	154,390	173,245	150,137

Notes: Dependent variable is the number of employees at a shop. See further notes to baseline regression from Table B4.

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