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Much Ado About Nothing? – Smoking Bans and Germany's Hospitality Industry

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Michael Kvasnicka and Harald Tauchmann[1]

Much Ado About Nothing? – Smoking Bans and Germany's Hospitality Industry

Abstract

Over the last years, public smoking bans have been introduced in most European countries. Unlike elsewhere, in Germany such bans were introduced at state level at different points in time, which provides important intra-country regional variation that can be exploited to identify the effects of such bans on the hospitality industry. Using monthly data from a compulsory survey carried out by the German Federal Statistical Office, we study the short-run effects that these bans had on establishments' sales. In contrast to the largely US-based literature, we find that smoke-free policies had a negative (yet moderate) effect on establishment sales. Closure rates of businesses in the hospitality industry, however, were not significantly affected by the introduction of state smoking bans.

JEL Classification: L51; I12

Keywords: smoking bans; sales; intra-country regional variation

March 2010

1 *Both RWI. – The authors are grateful to the German Federal Statistical Office (Statistisches Bundesamt) for providing the data used in the empirical analysis and to Silke Anger, Thomas Bauer, and Christoph M. Schmidt for valuable comments. – All correspondence to Harald Tauchmann, Rheinisch-Westfälisches Institut für Wirtschaftsforschung (RWI), Hohenzollernstr. 1-3, 45128 Essen, Germany, e-mail: harald.tauchmann@rwi-essen.de.*

1 Introduction

Tobacco consumption represents the single greatest preventable cause of death in industrialized countries (World Health Organization, 2008). In recent years, most countries have enacted tobacco control policies to reduce smoking prevalence and to protect non-smokers against second-hand-smoke. Among these control policies, public smoking bans are widely considered the key measure for averting health damage caused by second-hand-smoking. Yet, such bans remain heavily disputed, particularly in the hospitality industry, as owners of bars, pubs, and restaurants fear to suffer significant losses in their sales and revenues.

Knowledge of the costs of smoking bans to businesses in the hospitality industry is important for public policy. They determine in part the political and public support that can be amassed for this tobacco control measure. And they are indispensable for an objective assessment of the overall net economic effects of this instrument. Indeed, recent evidence suggests that the costs of smoking bans may be more important in such an assessment than long thought, as the main benefit of smoking bans, that is a reduction in the overall exposure of non-smokers to passive smoking, may be significantly smaller than hitherto assumed (Adda and Cornaglia, 2006).[1]

The effects of smoking bans on businesses in the hospitality industry have been studied extensively; see Goel and Nelson (2006) and in particular Scollo and Lal (2008) for the most comprehensive literature reviews to date. Most studies do not find any evidence for a negative effect on businesses. However, previous research has focused on countries such as Australia (e.g. Wakefield et al., 2002; Lal et al.,

[1] Exploiting data on a metabolite of nicotine, Adda and Cornaglia (2006) in a cross state analysis for the US provide evidence that public smoking bans can increase rather than decrease the exposure of non-smokers to tobacco smoke by displacing smokers to private localities where they contaminate non-smokers, in particular children.

2004), Canada (e.g. Stanwick et al., 1988; Luk et al., 2006), and in particular the United States (e.g. Cowling and Bond, 2005; Adams and Cotti, 2007), where smoking prevalence – and hence also the likely effects on the hospitality industry – is significantly lower than in many European countries; see the country profiles provided in (World Health Organization, 2008)[2]. Yet, as of now, there is still little evidence for European countries, which only recently enacted public smoking bans. Two notable exceptions are Adda et al. (2007)[3] and Ahlfeldt and Maenning (2009). The latter study is closely related to the present analysis, as it also focuses on Germany and uses similar data to ours. However, the scope of the present paper goes beyond the work of Ahlfeldt and Maenning (2009), as we also consider various exemptions to state smoking bans, the importance of pre-announced delayed enforcements of bans, as well as potential effects not just on sales but also on business openings and start ups.

Furthermore, inadequate data and empirical methods often limit the explanatory power of existing studies, as subjective (survey-based) rather than objective outcome measures are used, pre-policy data is not available, or changes in economic conditions are insufficiently controlled for (see Scollo and Lal, 2008). Last but not least, most non-US analyses focus on the effects of country-wide smoking bans, as most countries have opted for this type of ban. Country-wide bans, however, provide variation in government policy only across time. This makes identification of causality difficult, as the effects of such country-wide bans can effectively only be studied by a before-after type of comparison of outcome measures of interest. In such a setting, the risk of neglecting potentially confounding time trends is great (Fleck and Hanssen, 2008), particularly if the period of analysis spans several years.

[2]Prevalence figures for Canada are missing in World Health Organization (2008). Yet, *Health Canada* reports rather small prevalence rates; see www.hc-sc.gc.ca.

[3]Estimating the short-run economic impacts of the March 2006 Scottish smoking ban on public houses, Adda et al. (2007) find the ban to have decreased sales by 10 percent and customers by 14 percent.

In this paper, we investigate whether state-level public smoking bans enacted in Germany between August 2007 and July 2008[4] had any discernable adverse effect on sales in the hospitality industry.[5] Following a nation-wide agreement in early 2007 to ban smoking from bars, pubs, restaurants, and discotheques, states enacted state-specific regulations at different points in time (see Figure 1 for a time-line of the sixteen state bans). This state-level variation in the timing of enactment of smoking bans allows us to disentangle genuine effects of tobacco control policies from potential confounding time-varying factors that might affect sales in the hospitality industry.

In our analysis, we also exploit the fact that state-level smoking bans varied not only in their date of enactment, but also in the pre-announced date from which any violations would be fined (enforcement) by state authorities (see Figure 1), and to some degree also in their scope and strictness (e.g. permission of separate smoking rooms in multiple-room establishments; exemptions for small single-room establishments; exemptions for smokers clubs).[6] The differential treatment of small and large establishments in state smoking ban regulations led to a constitutional complaint of several owners of single-room discotheques and bars, and a ruling of the Federal Constitutional Court on July 30th 2008 that smoking in single-room busi-

[4]Ahlfeldt and Maenning (2009) wrongly claim that in January 2008 smoking bans were in operation in all states and drop the period after this date in major parts of their empirical analysis. In the state of North Rhine-Westphalia, for instance, the relevant bill ('Gesetz zur Verbesserung des Nichtraucherschutzes in Nordrhein-Westfalen') was passed by the state parliament as early as December 19th 2007. However, § 7 of the bill explicitly states that in the hospitality industry smoking bans come into force only in July 2008. Moreover, as documented in Figure 1, in Rhineland-Palatinate, Saarland, Saxony, and Thuringia state smoking bans also were introduced later than January 2008.

[5]Public perception in Germany at the time and – arguably non-representative – opinion polls among German innkeepers (CHD Expert, 2008; Kvasnicka and Tauchmann, 2010) strongly conveyed the impression that smoking bans were harmful to businesses.

[6]In all states except Bavaria, state smoking ban regulations allowed bars and restaurants to operate separate smoking rooms; in ten of the sixteen German states, state bans also permitted dance clubs to operate such smoking rooms. In Saarland, as the only state to consider such exemptions, the state smoking ban – under certain conditions – did not concern small single-room pubs. Certain exemptions were granted to private clubs in several states. Yet, only in Bavaria and North Rhine-Westphalia did the relabelling of ordinary pubs to 'smokers clubs' develop into a major loophole of smoking bans.

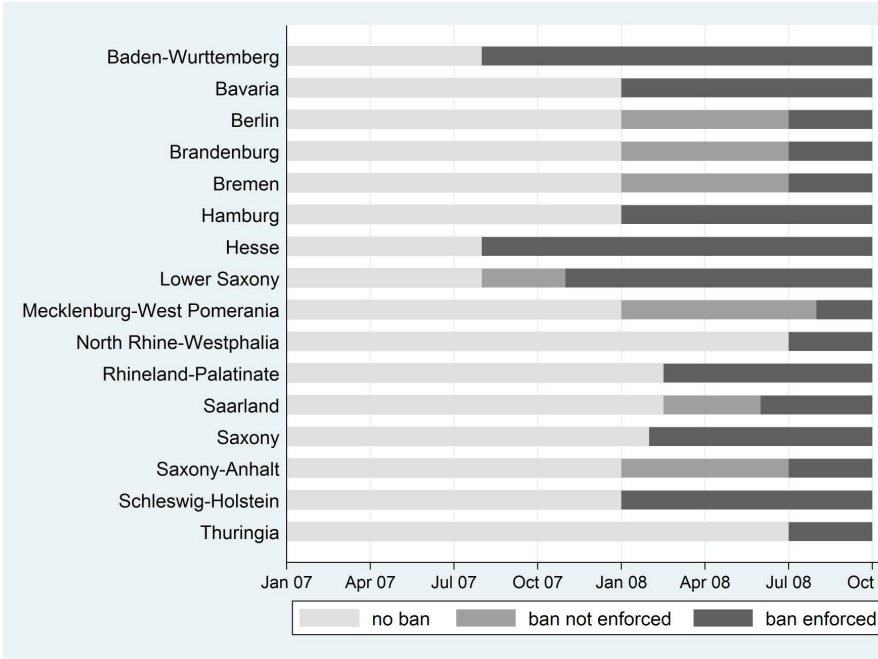

Figure 1: Time schedule for the introduction of state smoking bans

nesses is to be allowed[7] until December 31st 2009, a deadline by which states must have revised their legislation. Since then, several states relaxed their regulations, most notably Bavaria, although its state smoking ban did not conflict with the ruling of the federal Constitutional Court. This recent development is not addressed in our empirical analysis, which is confined to the period from January 2007 to September 2008 and, hence, focuses only on the short-run effects of the smoke-free policies that were initially enacted by Germany's sixteen federal states.

[7]The court's decision was directly concerned with the smoke-free legislation of only two states, i.e. Baden-Wurttemberg and Berlin. Yet, the court implicitly pointed out that its concerns would analogously apply to the smoking bans in any other state, except – for different reasons – Bavaria and Saarland. For the latter, state legislation already considered exemptions for small establishments. In Bavaria smoking rooms were generally not permitted and, hence, single-room pubs were not discriminated. Moreover, in the state of Rhineland-Palatinate, the State Constitutional Court came to a (provisional) decision similar to the later Federal Constitutional Court's one as early as February 2008. Thus in this state, smoking bans for small single-room establishments came never into force.

Our results show that state-level smoking bans in Germany had a negative but moderate effect on sales in the hospitality industry. Delayed enforcement (fining of violations) and exemption for single room pubs in some states proved immaterial for business sales. This does not hold for the option to open smokers clubs. In states that did allow for such clubs, no adverse effect of smoking bans on sales is found. We also find no evidence that business closures and start-ups were affected by the introduction of smoking bans. In contrast to the widespread public perception in Germany at the time and numerous – yet largely non-representative – opinion polls among German innkeepers (CHD Expert, 2008; Kvasnicka and Tauchmann, 2010), our results hence suggest that smoking bans, at least in the short run, proved to be of only moderate harm to businesses. Nevertheless, and in contrast to the bulk of the (largely US-focused) literature in this area, we do find evidence of adverse effects on businesses' sales.

The remainder of this paper is organized as follows. The subsequent section introduces the data, section 3 discusses the empirical approach, and section 4 presents the estimation results. Section 5 summarizes our main findings and concludes.

2 The Data

Our empirical analysis is based on data of year-to-year percentage changes[8] in monthly sales at state level. This sales data – hitherto unpublished – was provided upon request by the German the Federal Statistical Office (Statistisches Bundesamt).[9] It covers the period January 2007 through September 2008 and hence consists of 336 state-month observations (16 states $\times$ 21 months). Information is

[8] Absolute sales (in € or normalized to a reference date) are also available but judged as less comparable across time by the data provider (Federal Statistical Office) because of state differences in the sampling design with respect to the handling of panel mortality and the use of refreshment samples. Our rate of change data accounts for such differences.

[9] Data provision is gratefully acknowledged.

available on nominal as well as real sales. Our analysis is based on the latter.[10] Sales figures are available for the entire hospitality industry (industry classification WZ03 Code 55) as well as separately for bars, pubs and discotheques (WZ03 Code 55.4), and restaurants, coffee bars, etc. (WZ03 Code 55.3).

The original establishment-level sample (not available to us), from which state-level change rates in sales are calculated, consists of roughly 10,000 businesses per month, a panel randomly sampled from the industry register.[11] Sampled establishments are obliged by law to take part in the survey. Hence, our data does not suffer from self-selection that is likely to bias results obtained from surveys where participation is voluntary.[12]

The sample size substantially varies across states. While about 2,000 establishments are located in North Rhine-Westphalia the corresponding number for Saarland is less than 200. Overall, 14 percent of the surveyed establishments belong to the category 'bars and pubs'. Thus, if the focus of the analysis were on this category, the size of the underlying establishment-sample would be rather small, particularly in smaller federal states. In the following, we confine therefore the analysis to the entire hospitality industry (WZ03 Code 55).[13]

2.1 Descriptives

Figure 2 displays average (minimum and maximum) percentage changes in year-to-year monthly sales for the entire hospitality industry. On average, sales seem to

[10] Results remain largely unaffected if nominal sales are used instead.

[11] The industry register only covers establishment whose annual sales exceed 50,000 €.

[12] In the case of voluntary participation, innkeepers who regard themselves to be detrimentally affected by tobacco control policies might be more inclined to complain and hence to report – presumably lower – sales figures compared to those who do not notice any effect.

[13] Regressions considering only bars, pubs, and discotheques do not yield any significant results. This is most likely due to the small number of observations that underlie the aggregated state sales figures.

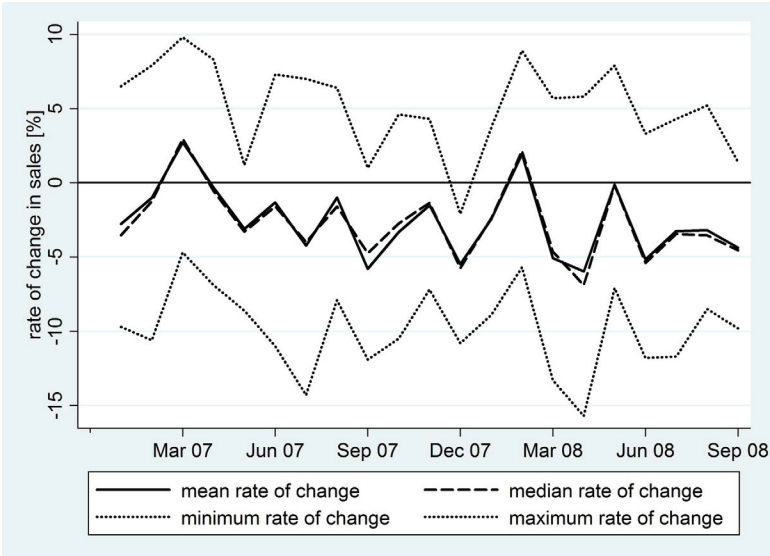

Figure 2: Rates of change in sales across states

decline at an (in absolute terms) slightly increasing rate. However, rates of change exhibit substantial heterogeneity across states. Figure 3 displays rates of change in sales using a relative time scale.[14] Specifically, month 'zero' represents (is normalized to) the first month that a smoking ban has come into force which varies across states in calendar time. As is evident, Figure 3 does not disclose any visible effect of state smoking bans on state sales in the hospitality industry.

2.2 Closures and Start Ups

Reporting rates of change in sales rather than absolute sale levels is related to the Statistical Office's approach to address panel mortality.[15] Specifically, changes in sales are calculated exclusively for those establishments that have already appeared

[14] On this relative scale, we only observe ten months simultaneously for all states.

[15] The hospitality industry is characterized by a rather large rate of market entries and exits, particularly among bars and pubs.

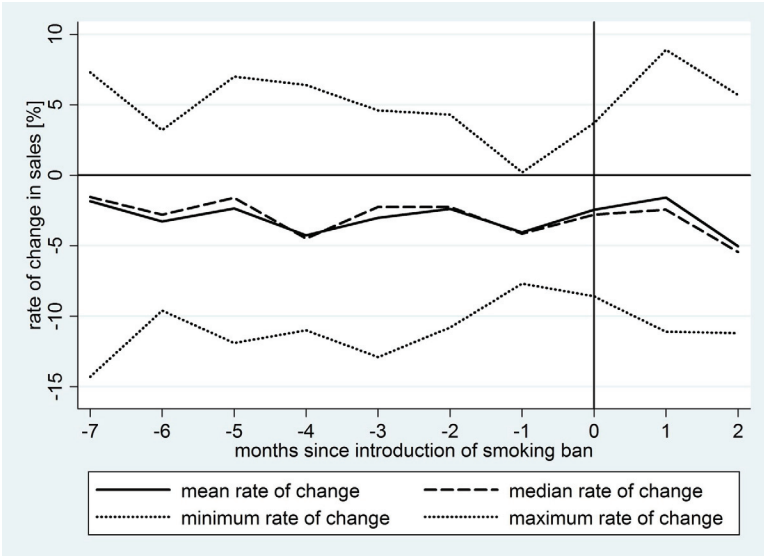

Figure 3: Rates of change in sales across states, normalized time scale

in the sample for the corresponding month of the previous year. As a consequence, our analysis can only detect potential effects on sales of pubs and restaurants that do survive from one year to the other, that is establishments that do not close down. To check whether potential closure-driven sample attrition is likely to downward bias the results of our exclusively sales-based analysis, we analyze data on all market entries and exists (closures and start ups) in the hospitality industry using again official, but different, data from the German Federal Statistical Office (Statistisches Bundesamt, 2006-2008a).[16] The data contains monthly state-level statistics for the period January 2006 through September 2008 on the total number of start ups and closures in the hospitality industry. In the following, we consider only genuine start ups and closures, that is we ignore events that in a legal but not in an economic sense represent market entries and exists, e.g. changes in the legal form of an establishment.

[16]Our sales data does not contain information on closure-induced attrition.

To ease comparability to Figure 3, which documents the evolution of sales on a relative time scale (normalized to the value of zero in the month a smoking ban was enacted), Figures 4 and 5 (see Appendix A.1) plot the respective year-to-year average percentage changes in monthly state closure and start-up rates around the time state smoking bans were enacted. As is evident, both the series for closures and start ups exhibit a distinct peak at the time that smoking bans came into force, that is turnover appears to have increased.

Multivariate econometric analyses, however, firmly negate this descriptive finding. We estimated several models, e.g. linear, log-linear, and count-data, to explain the number of openings and closures, and employed numerous different specifications, e.g. one- and two-way fixed effects, and different approaches to parameterize the potential effect of smoking bans (see Appendix A.2 for some selected results).[17] The vast majority of models does not yield any significant effect of smoking bans on either the number of closures or on the number of start ups. Interestingly, however, there appears to be a positive effect on both start ups and closures if state regulations did allow for smokers clubs. This finding is most likely explained by the conversion of existing pubs to smokers clubs that were not identified as changes in the legal form.

Of course, effects on genuine closures and start ups might only materialize with a substantial time lag. Nevertheless, for the short period under investigation in our sales analysis, the data does not point to any effects on closures or openings in the hospitality industry and hence does not give any reason to believe that sales estimates for this period will be severely biased because of sample attrition.

[17] See the model variants discussed in section 4.

3 The Econometric Specification

Consider a log-linear model that links sales y_{lt} in bars, pubs, and restaurants – aggregated at the state (Bundesland) level – to some exogenous variables x_{lt}:

$$\log(y_{lt}) = \beta' x_{lt} + v_{lt}. \tag{1}$$

Here $l = 1, \ldots, L$ indicates individual states, $t = 1, \ldots, T$ denotes months, and v_{lt} represents a random error term. However, in our sales data we do not observe absolute sales but percentage changes compared to the corresponding month of the previous year. As log-differences approximate percentage changes, the operational regression equation that corresponds to (1) can be specified as follows:

$$\frac{y_{lt} - y_{l(t-12)}}{y_{l(t-12)}} = \beta' \left(x_{lt} - x_{l(t-12)} \right) + \left(v_{lt} - v_{l(t-12)} \right). \tag{2}$$

Thus, any explanatory variable, including those indicators that capture the current status of smoking ban legislation, enter the regression model in terms of twelve-months-differences, i.e. $x_{lt} - x_{l(t-12)}$. The constant term, for this reason, captures a common trend in sales, and state fixed effects capture state-specific trends rather than time-invariant state differentials in levels.

Implicitly differencing the regression equation matters for the statistical properties of the regression model. Even if we assume serially uncorrelated errors, i.e.:

$$\operatorname{var}(v_{lt}) = \sigma_l^2 \quad \forall \quad t \qquad \text{and} \tag{3}$$

$$\operatorname{cov}(v_{lt}, v_{l\tau}) = 0 \quad \forall \quad \tau \neq t, \tag{4}$$

the structure of (2) results in a special case of error-auto-correlation in the opera-

tional error-terms $\varepsilon_{lt} \equiv v_{lt} - v_{l(t-12)}$, since:

$$\operatorname{cov}(\varepsilon_{lt}, \varepsilon_{l\tau}) = \begin{cases} -\sigma_l^2 & \text{if} \quad \tau = t - 12 \\ 0 & \text{if} \quad \tau \neq t - 12 \end{cases} \tag{5}$$

$$\operatorname{var}(\varepsilon_{lt}) = 2\sigma_l^2 \tag{6}$$

holds.

Equation (1) is formulated in terms of aggregated data. Thus, the errors v_{lt} are natural candidates for the presence of group (state) wise heteroscedasticity, since the size of the original samples, from which the aggregated variables are calculated, varies across the considered states. Group-specific variances $2\sigma_l^2$ can be estimated in a Goldfeld and Quandt (1965) fashion as:

$$2\widehat{\sigma_l^2} = \frac{L \times T}{L \times T - k} \times \frac{1}{T} \sum_{t=1}^{T} e_{lt}^2, \tag{7}$$

where $L \times T - k$ denotes the degrees of freedom and e_{lt} denotes regression residuals.[18]

One may account for the error variance-covariance structure (5) and (6) within a feasible generalized least squares (FGLS) framework. Yet, due to the relatively small number of observations, FGLS is likely to suffer from small-sample bias and may perform poorly in practice (e.g. Greene, 2000, 470). Thus, rather than using FGLS we apply ordinary least squares and correct the estimator's covariance matrix according to (5) through (7).[19]

[18] Alternatively one may specify the variance as a parametric function of the size N_l of the original sample from which the state-specific variables were calculated.

[19] In oder to check for the robustness of the estimation results, several alternative methods for calculating standard errors for the OLS estimates where tried along with FGLS for estimation.

4 Results

We start our analysis with a simple unconditional model that – besides the constant term – explains sales exclusively by a single dummy variable which takes the value one if a state public smoking ban is in force. As shown in column 1 of Table 1 (model i), smoking bans in this specification exert a significant and negative effect on sales in the hospitality industry.

Next, we add time- and state-specific effects (model ii), where the latter represent state-specific trends in sales. Both groups of dummy variables are highly statistically significant. As the model is already formulated in terms of year-to-year differences, this two-way fixed effects model extracts quite a lot of variation non-parametrically, and is hence rather unlikely to incorrectly attribute changes in sales to smoking bans that originate from unobserved confounding factors. Including time and state effects does not change our earlier qualitative result. The estimated magnitude of the adverse effect of smoking bans on sales even increases in absolute terms. However, the point estimate of -1.7 percent is still small and speaks for a rather moderate effect of public smoking bans. Indeed, taking standard errors into account, average losses in sales exceeding 3.5 percent can be ruled out at the 0.05-level of significance.

These findings of an adverse but moderate effect clearly do not support the worries often uttered in the public debate in Germany that smoking bans would be of serious harm to businesses in the hospitality industry (cf. CHD Expert, 2008). Our results are roughly in line with descriptive evidence of the Federal Statistical Office (Statistisches Bundesamt, 2008b), but deviate from those of Ahlfeldt and Maenning (2009) who do not find a statistically significant negative effect on sales in bars and restaurants. Apart from differences in model specification and time period considered, this divergence in findings is most likely attributable to the fact that Ahlfeldt

Table 1: **Effects on sales in the hospitality industry (percent)**

model variant	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
smoking ban	−1.224**	−1.685**	−1.584**	−0.862	−1.924**	−2.005**	−1.556**	−2.251***
	(0.624)	(0.785)	(0.782)	(0.811)	(0.757)	(0.816)	(0.789)	(0.856)
not enforced	–	–	–	−1.779*	–	–	–	−0.867
				(0.967)				(0.968)
month since introduction	–	–	–	–	0.364**	–	–	0.497***
					(0.173)			(0.183)
smokers clubs permitted	–	–	–	–	–	4.346***	–	5.278***
						(1.267)		(1.348)
except for single-room pubs	–	–	–	–	–	–	−0.559	0.899
							(1.134)	(1.126)
rainfall	–	–	0.022**	0.021**	0.020**	0.022**	0.022**	0.019*
			(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
unemployment	–	–	1.688	1.662	1.763	1.600	1.689	1.670
			(1.133)	(1.142)	(1.138)	(1.126)	(1.132)	(1.132)
constant	−2.120***	−1.097	−0.293	−0.670	−0.965	−0.202	−0.354	−1.156
	(0.327)	(0.969)	(2.030)	(2.035)	(2.032)	(2.014)	(2.041)	(2.014)
time-fixed-effects	no	yes***	yes***	yes***	yes***	yes***	yes***	yes***
state-fixed-effects	no	yes***	yes***	yes***	yes***	yes***	yes***	yes***
number of observations	336	336	336	336	336	336	336	336
degrees of freedom	334	299	297	296	296	296	296	293

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%; standard errors (in parentheses) adjusted for group-wise heteroscedasticity and year-to-year auto-correlation.

and Maenning (2009) apparently misspecify the dates for several states at which state smoking bans came into force (see footnote 4 above).

Our finding of a negative effect of smoking bans on sales is also robust to the inclusion of additional controls, such as state unemployment rates and weather conditions in the individual states (temperature and rainfall), which might affect state sales in the hospitality industry (model iii). Here, the coefficient attached to the monthly unemployment rate is positive, yet never significant. Temperature (excluded from the reported specifications) does not exert any significant effect on sales. In contrast, rain does matter and bears the expected positive sign. That is, people spend more money – and presumably more time – in bars pubs and restaurants if the weather is bad. One additional liter in rainfall per square-meter and month results in an increase in sales of 0.02 percent. This appears to be a rather moderate effect compared to average monthly rainfall, e.g. 48 l/m^2 in Berlin. Yet, it is still remarkable that the effect of rainfall on sales can such clearly be detected

even in aggregated data. Moreover, the positive effect of rainfall turns out to be very robust to various specifications of the regression model.

Smoking ban regulations varied in content across states and time (see section 1). To capture any potential effects of such variations, we augmented the model with several indicators that enter the regression as interaction terms with the smoking ban dummy. First (model iv), we examine whether smoking bans per se matter, or whether enforcement, i.e. fines in the case of infringement against these bans, is what really matters. Here, the model includes a dummy indicating that infringements against smoking bans are generally not fined (see Figure 1 for the states and months without enforcement). The relevant coefficient's estimate is somewhat puzzling. The interaction term – though just marginally significant – is negative, indicating that bans exert a stronger adverse effect on sales if not enforced. Moreover, in this specification, the estimated base-line effect becomes statistically insignificant. Jointly, however, both coefficients are significant. This result might be explained by the 'non enforced' dummy capturing an effect of the recent introduction of a ban rather than its genuine enforcement. For any state that allowed for a period without fines, this period only covers a few months following the introduction of a state smoking ban.

To check for this, model (v) includes the number of months for which a smoking ban has been in force. As it turns out, the estimated coefficient is indeed positive and significant, indicating that smoking bans had particularly strong effects immediately after their introduction. If both the 'no enforcement' indicator and 'time since introduction' are included in the regression, the former becomes insignificant, which supports our earlier reasoning. Yet, the role enforcement plays remains somewhat unclear. A significant and positive coefficient of 'time since introduction' is, however, relevant in its own right. It indicates that the instantaneous effect on

sales fades out as smoking bans are in effect for a longer period of time.[20] Smokers might be strongly disgruntled by smoking bans at first, and reduce their going out accordingly. As time proceeds, however, they may adapt and return to their previous going-out habits.

We also examined the role that 'smokers clubs' played for the impact of bans on sales. Specifically, we included another interaction that takes the value of one only if pubs and bars in a state could avoid becoming smoke free by converting themselves to a smokers club (model vi). This option did exist for establishments located in the states of Bavaria und North Rhine-Westfalia.[21] The estimated coefficients exhibit the expected pattern. While the base effect of smoking bans is still negative and significant – in absolute terms it even takes a larger value – the interaction term bears a positive sign. In absolute terms, its size even exceeds the size of the estimated base effect. Hence, a negative effect on sales is only found for those states that did not permit smokers clubs. The point estimates even argue for increasing sales in Bavaria und North Rhine-Westfalia after smoking bans – that permit smokers clubs – have been introduced. Yet, in statistical terms, this finding is not significant at the 0.05-level.

Finally, we analyzed the effect of exemptions for small single-room pubs (model vii). In the states of Rhineland-Palatinate and Saarland, such exemptions existed as soon as state bans were introduced. For any other state – except Bavaria – the Federal Constitutional Court virtually introduced such exemptions late in July 2008 (see section 1 for details). Yet, the corresponding coefficient is insignificant and the

[20] Given the rather short period of time considered and the limited number of observations for which bans have in been force for a substantial number of months, interpreting the estimated coefficient as a parametric time trend that allows for forecasting future effects does not make much sense.

[21] Several other state bans also considered certain exemptions for private clubs. Yet, these exemptions did not develop into a major loophole as simply relabelling ordinary pubs to smokers clubs was no option.

estimated base effect differs just marginally from our reference specification (model ii). Hence, we do find evidence for an effect of single-room pubs being exempted from smoking bans. This might be due to the fact that the relevant court ruling concerned the majority of states from the same point in time, which renders the identification of such an effect difficult.

If all four aforementioned interaction terms are simultaneously considered (model viii), we get a similar picture to that where we included these regressors individually. That is, the base effect of smoking bans is negativ and significant. The estimated coefficients for 'time since introduction' and 'smokers clubs' are both positive and statistically significant, while those for 'no enforcement' and exemptions for single-room pubs are insignificant.

In sum, our analysis suggests that smoking bans have harmed sales in the hospitality industry in Germany. In qualitative terms, this result is robust to wide range of variations to the model. As our empirical approach accounts for level-effects by considering year-to-year differences and in addition considers time-effects and state-specific trends, it is rather unlikely that unobserved heterogeneity drives our key result. In quantitative terms, the (maximum) point estimate indicates losses in sales of 2.3 percent with a corresponding (standard-error based) upper bound of 3.7 percent. Hence, the estimated effect is rather moderate. Yet, one has to keep in mind that this result represent an average effect across the entire hospitality industry. At the individual establishment level, losses in sales are likely to vary across establishments. Moreover, very small establishments, which often are assumed to be hit particularly hard by smoking bans, are not considered in the data. Hence, certain individual – and supposedly certain groups of – establishments might have suffered substantially larger decreases in sales than indicated by our estimates.

5 Conclusions

In this paper, we studied whether state-level smoking bans in Germany affected sales in the German hospitality industry in the short run. Identification of such effects rests on the fact that German states introduced smoking bans at different points in time within the period from August 2007 to July 2008. Our analysis hence does not rely on a simple before-after comparison, but exploits intra-country regional variation in regulations. Regression analyses yielded a statistically significant and negative average effect on sales of roughly two percent. This result is robust to a wide range of model specifications and to the inclusion of state- and time-specific level effects as well as state-specific trends in sales. There is some evidence, however, that adverse effects do fade out if smoking bans have been in force for some time. Our key finding of a significant and adverse effect of smoking bans on sales in the hospitality industry stands in contrast to the main body of the existing – largely US-based – literature, which in the majority does not find such an effect. One may explain this discrepancy by the fact that smoking prevalence is much smaller in the US than in Germany, which limits the magnitude of any adverse effect that smoking bans may exert on business sales. Although our results show that innkeepers were harmed by smoking bans, this does not render such bans unjustified. Rather, it contributes another piece of evidence for an objective overall comparison of the costs and benefits of anti-smoking regulation.

A Appendix

A.1 Descriptive analysis of closures and start ups

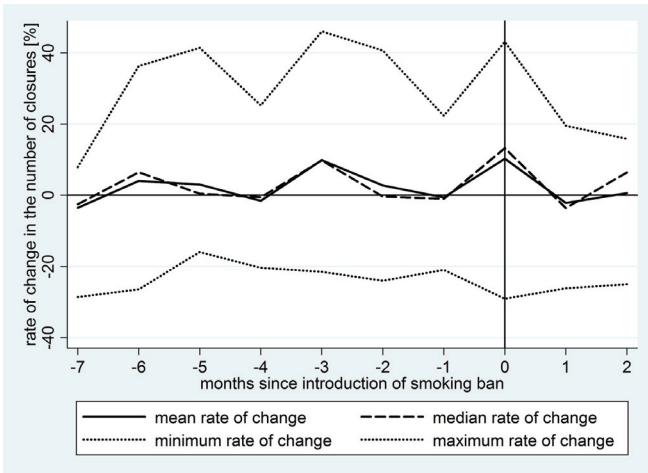

Figure 4: Rate of change in business closures, normalized time scale

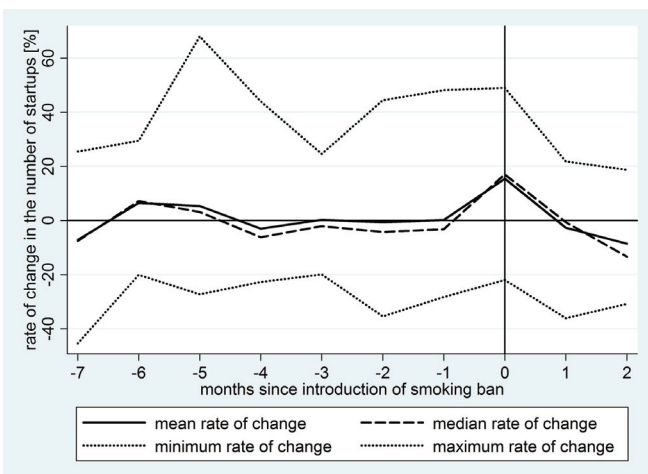

Figure 5: Rate of change in business openings, normalized time scale

A.2 Econometric analysis of closures and start ups

Table 2: **Effects on the number of business closures (OLS regression)**

specification	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)
smoking ban	−3.096	−7.627	−2.936	−13.477	−8.904	−6.716	−11.668
	(20.699)	(9.052)	(9.706)	(8.766)	(9.170)	(9.225)	(10.910)
not enforced	–	–	−12.299**	–	–	–	−7.532
			(5.136)				(4.848)
month since introduction	–	–	–	2.562*	–	–	2.638*
				(1.417)			(1.293)
smokers clubs permitted	–	–	–	–	16.486***	–	18.162**
					(3.964)		(6.506)
except for single-room pubs	–	–	–	–	–	−15.252	−8.653
						(9.554)	(8.945)
constant	254.577***	290.938***	290.938***	290.938***	290.938***	290.938***	290.938***
	(10.060)	(12.167)	(12.316)	(12.171)	(12.287)	(12.185)	(12.414)
time-fixed-effects	no	yes	yes	yes	yes	yes	yes
state-fixed-effects	no	yes	yes	yes	yes	yes	yes
number of observations	528	528	528	528	528	528	528
degrees of freedom	526	479	478	478	478	478	475

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%; absolute number of closures at the left-hand-side; months from January 2006 to September 2008 considered; robust standard errors in parentheses.

Table 3: **Effects on the number of start ups (OLS regression)**

specification	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)
smoking ban	−11.415	−8.593	−5.537	−9.443	−9.440	−8.438	−7.860
	(16.109)	(8.023)	(9.075)	(9.052)	(7.765)	(8.069)	(11.153)
not enforced	–	–	−8.012	–	–	–	−6.467
			(5.006)				(5.501)
month since introduction	–	–	–	0.372	–	–	0.420
				(1.583)			(1.920)
smokers clubs permitted	–	–	–	–	10.930***	–	10.222
					(2.803)		(7.474)
except for single-room pubs	–	–	–	–	–	−2.586	−0.291
						(6.821)	(7.696)
constant	208.545***	215.125***	215.125***	215.125***	215.125***	215.125***	215.125***
	(10.060)	(6.332)	(6.371)	(6.340)	(6.380)	(6.337)	(6.424)
time-fixed-effects	no	yes	yes	yes	yes	yes	yes
state-fixed-effects	no	yes	yes	yes	yes	yes	yes
number of observations	528	528	528	528	528	528	528
degrees of freedom	526	479	478	478	478	478	475

Notes: *** significant at 1%; ** significant at 5%; * significant at 10%; absolute number of start ups at the left-hand-side; months from January 2006 to September 2008 considered; robust standard errors in parentheses.

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