Schmollers Jahrbuch 131 (2011), 419 – 429
Duncker & Humblot, Berlin

Does a Smoking Ban Reduce Smoking? Evidence from Germany

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Abstract

In 2007 and 2008 the 16 German federal states introduced public smoking bans. The prime objective of the smoking bans was to reduce passive smoking. However, a welcomed side-effect of the smoking bans might have been to reduce active smoking. In this paper we investigate whether such a side-effect occurred. Using data from the German Socio-Economic Panel Study (SOEP), we investigate with fixed-effects models whether the introduction of smoking bans in the German states reduced the prevalence and the intensity of smoking. Our findings show no effects of public smoking bans on smoking behaviour.

JEL Classifications: I10, I12, I18, C33

1. Introduction

In 2007 and 2008 the 16 German federal states introduced public smoking bans. With these laws smoking has been banned in state buildings (administrations, schools, universities, hospitals, etc.) and in public places (restaurants, bars, clubs, etc.). Implementation and enforcement date differed by state. The first state to enforce a smoking ban was Baden-Württemberg in August 2007; the last one was Mecklenburg-West Pomerania in August 2008. Also the strictness of the laws differed. All states banned smoking completely in state buildings; concerning public places, however, several exemptions were allowed. Ten states allowed for separate smoking rooms in restaurants and clubs; five states allowed for separate smoking rooms only in clubs, and only one state (Bavaria) did not allow for any separate smoking rooms (for more institutional details see Anger / Kvasnicka / Siedler, 2010).

* For helpful suggestions we thank Thomas Siedler, two anonymous referees, and participants of the 9th International German Socio-Economic Panel User Conference at the Social Science Research Center Berlin (WZB) (July 2010).

The prime objective of the smoking bans certainly was to reduce passive smoking and thereby to improve the health of guests and employees. There is an ongoing debate whether this objective has been effective (Carpenter/Postolka/Warman, 2010). However, a welcomed side-effect of the smoking bans might have been to reduce active smoking, as earlier research suggests (Levy/Friend, 2003). Therefore, we will pose the following research question: Did the introduction of the smoking bans in the German states reduce active smoking? We will shed light on this question by using data from the German Socio-Economic Panel Study.

2. Theory: Why Should a Smoking Ban Reduce Smoking?

It is not obvious how banning smoking in restaurants might reduce active smoking. Smokers could still simply leave the restaurant to smoke (or move to a separate smoker room). However, within a straightforward rational choice approach one could argue that these options entail some inconvenience (i.e. costs), thereby reducing the utility derived from smoking. Following this argumentation, one would expect a smoking ban to reduce the number of cigarettes smoked per smoker (smoking intensity). The inconvenience hypothesis, however, does not predict a reduction in the number of smokers (smoking prevalence), because a public smoking ban does not affect smoking in private places (some researchers even argue that a public smoking ban increases smoking in private places, e.g. Adda/Cornaglia, 2010).

A second hypothesis, however, predicts a reduction also in smoking prevalence. The general anti-smoking climate expressed in the smoking bans, along with the fact that after the ban smokers were forced to gather in special places (before the front door of the restaurant or in a smoker room), might impose social costs on smoking generally. Therefore, the introduction of a smoking ban might increase the social costs of smoking and thereby induce some smokers to quit smoking (ostracism hypothesis). Together, these hypotheses predict that the introduction of public smoking bans reduces the prevalence and the intensity of smoking (ban-effect, H 1).

Further, the inconvenience hypothesis predicts several interaction effects. First, inconvenience is certainly greater in states with fewer exceptions. It is less inconvenient to retire to a separate smoking room in a restaurant than to have to leave the restaurant. Therefore, we expect the (intensity) ban-effect to be greater in states with a stricter smoking ban (H 2). More specifically, we expect the strongest effect to be in Bavaria, which allowed no separate smoking rooms.

Second, a public smoking ban does not affect all smokers. Only those who visit public places are exposed to the ban and to additional inconveniences.

Therefore, we expect the (intensity) ban-effect to gain strength the more often a smoker frequents public places (H 3).

3. Previous Literature

Until recently, researchers seemed to agree that clean indoor laws negatively affect active smoking (see Levy / Friend, 2003 for a comprehensive review of the US evidence). In the last few years, however, as research has begun to evaluate specific policies making use of natural experiments, several studies have raised serious doubts about the effect of public smoking bans (for short reviews see Anger / Kvasnicka / Siedler, 2010; Bitler / Carpenter / Zavodny, 2009). Overall, findings are still quite inconclusive. Our interpretation is that many previous studies used bad data and/or faulty methods. Some studies used cross-sectional data, others relied on aggregate state-level time-series, but relatively few studies used individual data in a quasi-experimental setting (see the studies cited by Levy / Friend, 2003). Moreover, the methods employed most often are cross-sectional in nature. As a consequence, many existing studies did not properly control for pre-treatment differences and may therefore be biased due to unobserved heterogeneity.

For Germany, Anger / Kvasnicka / Siedler (2010, AKS in the following), using SOEP data, present analyses on ban-effects. As predicted by H 1, they found negative effects of a smoking ban on both smoking intensity and prevalence. However, these effects were small and not significant. Thus, their results do not support H 1. On the other hand, they found supportive evidence for interaction effects (which they term heterogeneous effects). Respondents living in the strictest states (Bavaria, Saxony, and Lower-Saxony) experienced a significant negative ban-effect, a result supporting H 2. AKS (2010) used an indirect strategy to test H 3. Hypothesizing that certain groups, i.e. young adults, unmarried persons, city-dwellers, etc., visit public places more often, they expected stronger ban-effects for these groups. Indeed they found significant negative ban-effects for them. This is indirect evidence in support of H 3. AKS (2010) use the same data (SOEP) as we do. In this paper we intend to push their analyses further by using different methodology.

4. Data

We use data from the SOEP version 1984–2008 (see Wagner / Frick / Schupp, 2007). In the years 2002, 2004, 2006, and 2008, respondents were asked whether they currently smoked cigarettes, pipe, or cigars. If their answer was yes, they were asked for the average number of cigarettes, pipes, or cigars smoked per day. Using all information on smoking behaviour given by SOEP respondents in these four waves, we extracted 87,953 person-years.[1]

From this we constructed two dependent variables. First, we built a dichotomous smoking-prevalence variable as to whether somebody is currently a smoker ("smoker"). Note that this variable includes cigarette, pipe, and cigar smokers. Second, a count variable (smoking intensity) records the average number smoked per day ("number smoked"). For this variable we simply added up the number of cigarettes, pipes, or cigars smoked per day.

Our independent variable of main interest is a treatment indicator that indicates whether or not the respondent lived under a public smoking ban at the time of the interview. Prior to 2008 none of the respondents was subject to a smoking ban. However, due to the different enforcement dates of the state smoking bans, in 2008 some respondents lived under a ban, while others did not. To construct this variable we used the information on state-specific enforcement dates provided by AKS (2010, Table 1). Of the SOEP respondents in 2008, 62% lived under a smoking ban, while 38% did not.

To investigate the hypotheses on interaction effects (H 2 and H 3), we needed variables on strictness and exposure. The strictness variable is a four-level classification of the German states. We used the information provided by AKS (2010, Table 1). The strictest state was Bavaria. Moderately strict bans were enforced in Saxony and Lower-Saxony. Less strict laws had been enforced by April 2008 (at this time most SOEP interviews were finished) in Baden-Württemberg, Hamburg, Hesse, Rhineland-Palatinate, and Schleswig-Holstein. The fourth category comprises all other states, where the smoking ban had not yet been enforced by April 2008.[2] We estimated separate ban-effects for the first three strictness categories (the respondents not living under a smoking ban are the reference group).

The exposure variable is an individual-level indicator variable indicating whether the respondent goes out to restaurants, bars, or discos at least once a week. This variable was measured not every year, but only in 2008. However, for our purpose this does not constitute a problem. It would be a problem if we wanted to investigate the effect of going out on smoking behaviour. However, we wanted to know whether smoking behaviour changed in those respondents, who frequently visited public places in 2008 and thereby were exposed to the smoking ban. Therefore, the ban-effect was estimated separately for those going out at least once a week and those going out less frequently.

1 A Stata do-file for extracting and analyzing the data is available from the first author (bruederl@uni-mannheim.de).

2 Saarland enforced its ban in June 2008 and should therefore be in the fourth category. However, due to small numbers, the Saarland is not coded separately in the SOEP, but grouped with Rhineland-Palatinate. Thus, respondents from the Saarland are misclassified in category three.

5. Identifying Policy Effects with Data from a Panel Survey

Identifying policy effects from individual-level panel survey data is difficult if the policy reform affects all panel participants at the same point in time. In that case there is no control group available and it is difficult to separate age, period, and treatment effects. One needs very plausible arguments that there are no period effects (an example for this strategy is Winkelmann, 2004).

In our case, however, we have a natural experiment. Those SOEP respondents who were not living under a smoking ban in 2008 are the control group. A natural experiment is obviously very helpful for estimating policy effects. However, different models are used in the literature when estimating policy effects from natural experiments. Some authors use pooled OLS (POLS) (e.g. AKS, 2010). The model can be formulated as:

$$s_{it} = \alpha + y_{it}^{2008}\beta + \left(y_{it}^{2008} * t_i\right)\delta + X_{it}\eta + \varepsilon_{it}\,,$$

where s_{it} denotes the dependent variable ("smoke" or "number smoked") for person i at time t, y^{2008} is the dummy for year 2008 (there could be more period dummies), t is a dummy for the treatment group, and X are control variables. The interaction term of year 2008 and treatment group is the treatment indicator. The general idea is to estimate a period trend (via period dummies) and to measure the deviation that the treatment produced from this general trend. Here β denotes the period effect and δ provides the POLS estimate of the ban-effect. This works well if treatment assignment is random. But if assignment is systematic, estimates of treatment effects will be biased. In our application, for instance, there are systematic pre-treatment differences in smoking behaviour between the control and treatment groups because in the states introducing a smoking ban early smoking prevalence was already low prior to enforcement (see Figure 1 below). Therefore, AKS (2010) include a full set of state dummies, as well as a full set of dummies for the different SOEP subsamples, and a full set of dummies for the survey month (to capture the period effects more precisely). However, there is always the danger that one might not have controlled for all relevant variables. Then POLS estimates of policy effects will be biased.

Therefore, a better approach is to use the difference-in-differences (DID) model (as, for instance, Ziebarth / Karlsson, 2009 do):

$$s_{it} = \alpha + y_{it}^{2008}\beta + t_i\gamma + \left(y_{it}^{2008} * t_i\right)\delta + X_{it}\eta + \varepsilon_{it}\,.$$

Here, inclusion of the time-constant treatment group dummy t_i controls for pre-treatment differences γ between the treatment and control groups. The model thus implements a straightforward before-after comparison with control group. POLS lacks the treatment group dummy t_i, and therefore does not control for possible pre-treatment differences.

With individual-level panel data, however, an even better approach includes individual fixed-effects (FE) (this approach is also recommended by Wooldridge, 2010, 315). The FE model can be written as:

$$s_{it} = \alpha_i + y_{it}^{2008}\beta + \left(y_{it}^{2008} * t_i\right)\delta + X_{it}\eta + \varepsilon_{it}\,,$$

where the fixed-effects α_i summarize all time-constant unobserved individual differences. As is well known, estimation can be done after within-transformation, whereby all time-constant variables are eliminated from the equation. Therefore, it is neither necessary nor possible to control explicitly for the treatment group dummy t_i. Nevertheless, the FE model not only controls for group-specific pre-treatment differences, but by means of individual fixed-effects all time-constant heterogeneity is eliminated. The FE estimator is less efficient than the DID estimator, because FE estimation requires at least two person-years for each person, leading to a smaller sample size. However, the benefits of eliminating individual heterogeneity should by far outweigh the loss in efficiency.

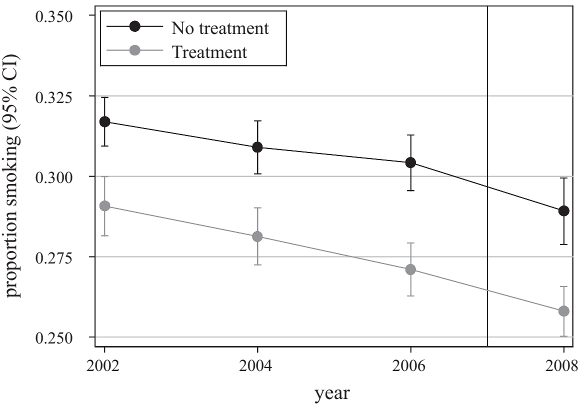
In the following we use such FE models to test our hypotheses. As control variable, we only include age (linear, quadratic, and cubic term) to model age effects. This is important, because a panel grows older and therefore possible age effects might distort the treatment effect. Other controls (labour force status, education, and marital status) did not change the results.

Since we had data from four waves, we included not only the period dummy for 2008. However, due to the APC-problem (age-period-cohort) we had to impose restrictions on the period effects (age is included explicitly and cohort implicitly in the FE models). We decided to restrict the period effects 2002 and 2004 to be equal and included period dummies only for 2006 and 2008. Thus, 2002 and 2004 are the reference category. The general idea is that FE methodology helps to get "clean" estimates of the period and age effects. The effect of the treatment indicator then captures the "true" treatment effect and is not biased by either period or age effects. As always, the central assumption underlying FE methodology is that control and treatment groups show common time trends (the same assumption, to be sure, has to be valid with POLS and DID estimation).

Finally, some remarks on the models used: "Smoker" is a dummy variable and one should use a logit (or probit) model. "Number smoked" is a count variable and one should use a count data model (see for instance Winkelmann, 2004). However, we (as AKS, 2010) decided to use linear regression models. The reason is that the less restrictive assumptions underlying logit or count data models come at a price: Interpretation is much more awkward (especially if one is using interaction effects, see Mood, 2010). Therefore, we will present only results from linear models. However, results from logit and count data models are qualitatively similar.

6. Results

First, we tested whether the introduction of public smoking bans reduced the intensity and the prevalence of smoking (H 1). Descriptive evidence for H 1 can be gained by plotting the proportion of respondents who have smoked over the years (including confidence intervals (CI), see Figure 1). We plotted two curves: one for the treatment group (2008 living under a smoking ban), the other one for the control group. The vertical line indicates (the rough) time of treatment for those treated. In both groups we see a declining trend. If H 1 were true, we would expect to see a "kink" in the trend line for the treatment group in 2008. This is not the case. If at all, we see such a kink in the control group (the decline is not significant since the confidence intervals overlap). Thus, this descriptive analysis does not support H 1. However, looking at aggregate trends certainly can not give a definitive answer. For instance, the aggregated, cross-sectional proportions might be affected by panel attrition and the addition of



Source: SOEP 2002, 2004, 2006, 2008, own calculations.

Figure 1: Smoking prevalence over time by treatment (person-years = 87,953)

new subsamples.[3] Therefore, we now will present the results of individual-level regressions.

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3 23,779 respondents participated in the SOEP 2002. Of these, 8,707 (37%) no longer participated in 2008. Attrition may bias our results if it is related to smoking behaviour.

Results of our FE regressions are presented in Table 1. The enforcement of a smoking ban is estimated to reduce the proportion of smokers by 0.1 percentage points. This effect is not significant. The estimated ban-effect on smoking intensity is even positive, though also very small and not significant. These are obviously negligible quantities and one has to conclude that a smoking ban does not affect smoking behaviour. Thus our multivariate analyses also fail to support H 1.

Table 1

FE Models of Smoking Behaviour

	Model (1) Smoker (1 = yes)	Model (2) Number smoked
Treatment (ban-effect)	-0.001	0.024
	(0.20)	(0.35)
Year 2006	0.005	0.311**
	(1.53)	(5.07)
Year 2008	0.003	0.509**
	(0.57)	(4.75)
Age	0.026**	0.563**
	(6.72)	(9.09)
Age^2 / 10	-0.007**	-0.175**
	(9.52)	(14.54)
Age^3 / 1000	0.004**	0.112**
	(9.85)	(15.06)
Constant	0.183*	5.640**
	(2.45)	(4.55)
within R^2	0.008	0.015
Person-years	81,914	81,706

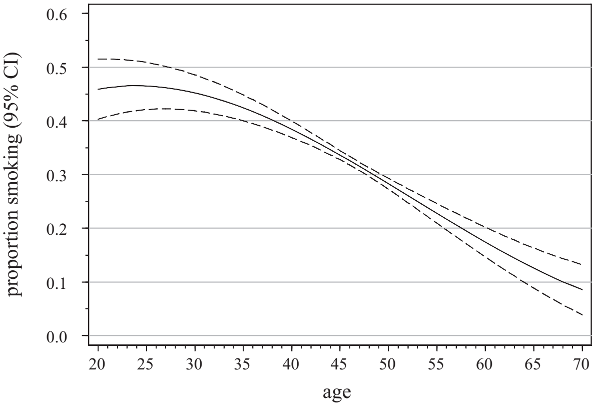
Notes: Panel robust t-statistics in parentheses. * significant at 5 % level; ** significant at 1 % level.

Source: SOEP 2002, 2004, 2006, 2008, own calculations.

It is instructive to have a look at the effects of the control variables. Looking at the period effects for "smoker", we see that there is either no time trend or else a slightly positive one. The drop in the proportion of smokers, as seen in Figure 1, is apparently not due to a general tendency to smoke less, but to the ageing of the sample. Moreover, we see a non monotonic age effect. For Model

We used the 2002 information and estimated a logistic regression model of an attrition indicator on smoking prevalence and intensity, state dummies, dummies for marital status, age and gender. Although the coefficients of the state dummies were jointly significant, neither smoking nor the number of cigarettes was significantly related to participation in 2008. Thus, we see no potential for attrition to significantly affect our results.

(1) the estimated growth curve is plotted in Figure 2 (year dummies and treatment indicator set to zero). Smoking prevalence increases up to age 24 and then declines over the life course.



Source: SOEP 2002, 2004, 2006, 2008, own calculations.

Figure 2: Smoking prevalence over age (based on estimates from Model (1), Table 1)

Now we test our interaction hypotheses. The models are specified as in Table1. However, now the ban-effect is split up by strictness (H 2), resp. exposure (H 3). Results are presented in Table 2 (only the estimated ban-effects are presented). Panel A gives the ban-effects according to strictness. As can be seen,

Table 2

FE Models of Smoking Behaviour; Interaction Effects

	Model (1) Smoker (1 = yes)	Model (2) Number smoked
Panel A: ban-effect by strictness		
Very strict smoking ban	0.002 (0.35)	0.029 (0.28)
Moderately strict smoking ban	0.006 (1.12)	0.126 (1.35)
Less strict smoking ban	-0.006 (1.19)	-0.031 (0.37)
Person-years	81,914	81,706
Panel B: ban-effect by exposure		
High exposure	0.002 (0.33)	0.105 (0.98)
Low exposure	-0.002 (0.45)	-0.000 (0.01)
Person-years	67,755	67,595

Notes: Entries are estimated ban-effects from FE models. Reference group in both cases are respondents not living under a smoking ban. Panel robust t-statistics in parentheses.

Source: SOEP 2002, 2004, 2006, 2008, own calculations.

strictness does not strengthen the ban-effect (or make it more negative). On the contrary, the effect is positive in the state with the strictest regulation (Bavaria). Moreover, none of the ban-effects is significant. This contradicts H 2. Concerning H 3, evidence is again contradictory (Panel B). Under high exposure, the ban-effect is positive, which is again contrary to expectations. Both under high and low exposure effects of the ban are small and insignificant. Therefore, these results contradict H 3 as well.

7. Conclusion

All our findings are negative. There is no discernible reduction in active smoking following introduction of smoking bans in Germany's federal states. Nor do stricter smoking bans produce an effect on smoking behaviour. In Bavaria, the state with the strictest smoking ban, smoking even increased after introduction of the ban (effect not significant). Finally, we also found no significant ban-effect on those going out at least once a week (high exposure). Thus, overall there is not one shred of evidence that smoking bans do affect smoking behaviour. Our results support the findings of the more recent empirical literature on the effects of smoking bans in the U.S. (e.g. Adda / Cornaglia, 2010).

The theoretical implication is that both the inconvenience and the ostracism hypotheses are wrong. Having to leave the restaurant to smoke a cigarette does not reduce one's cigarette consumption. Nor do the additional social costs introduced by smoking bans reduce the number of smokers. For policy makers this means that they can not expect positive side-effects of a smoking ban on active smoking.

A limitation of our study is that we could investigate only a short-term ban-effect, because we observed smoking behaviour only for 2008, a few months after enforcement of the bans started. Especially the ostracism hypothesis could be interpreted as a prediction about long-term effects. To overcome this limitation one has to await the release of the 2010 SOEP data.

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