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Gregor Pfeifer
Mirjam Reutter
Kristina Strohmaier

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Universität Duisburg-Essen, Department of Economics
Universitätsstr. 12, 45117 Essen, Germany

RWI Leibniz-Institut für Wirtschaftsforschung
Hohenzollernstr. 1-3, 45128 Essen, Germany

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Gregor Pfeifer, Mirjam Reutter, and Kristina Strohmaier[1]

Goodbye Smokers' Corner: Health Effects of School Smoking Bans

Abstract

We estimate the causal impact of school smoking bans in Germany on the propensity and intensity of smoking. Using representative longitudinal data, we use variation in state, year, age cohort, school track, and survey time for implementation of such smoking bans to identify the effects of interest. The estimates from our multiple-differences approach show that six to ten years after intervention, propensity towards smoking is reduced by 7-16 percent, while the number of smoked cigarettes per day decreases by 8-13 percent. Our results still hold if we account for the clustered data structure by evaluating the effects with randomization inference.

JEL Classification: I12, I18, C12, C21

Keywords: School smoking ban; cigarette consumption; treatment effects; difference-in-differences; randomization inference

February 2017

1 Gregor Pfeifer, University of Hohenheim; Mirjam Reutter, University of Hohenheim; Kristina Strohmaier, RUB and University of Hohenheim. *– The authors are grateful for valuable remarks from Silke Anger, Frauke Peter and Alfonso Sousa-Poza. The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.* *– All correspondence to: Kristina Strohmaier, Ruhr University Bochum, Department of Economics, Universitätsstr. 150, 44801 Bochum, Germany; kristina.strohmaier@ruhr-uni-bochum.de*

1 Introduction

Smoking is one of the leading causes of preventable death (World Health Organization, 2009). In recent years, many countries have introduced public smoking bans to reduce the exposure of non-smokers to second-hand smoke and encourage people to reduce the amount they smoke or stop entirely. However, the most important goal of anti-smoking interventions is to stop people in younger age cohorts (i.e., school children) from starting to smoke at all. This demographic subgroup is not only the most vulnerable, but also the most valuable in terms of its future economic prospects. But this value is highly dependent on health and well-being.

This paper evaluates the short- and medium-term effects of school smoking bans on individual smoking behavior in Germany, a country with relatively high smoking rates among industrialized countries (Shafey et al., 2009).[1] In 2003, for example, 114,647 deaths and 1.6 million years of potential life lost were attributable to smoking, for a total cost of 21 billion euros (Neubauer et al., 2006). More than ten years later, Germany still ranks high for age-standardized smoking prevalence among European countries, at 30.3 percent (World Health Organization, 2017). Germany introduced general smoking bans across federal states between 2007 and 2008, but school smoking bans had already been adopted between 2004 and 2008. Hence, we are able to use variation across states and years to identify the effect of school smoking bans on individuals' smoking propensity and intensity. Using individual-level data from the German Socio-Economic Panel (SOEP), a representative annual survey of 11,000 households covering 20,000 individuals, we also exploit variation in age cohorts, the secondary school track, and the timing of the survey interview.

While Chaloupka and Warner (2000) provide a comprehensive overview of the economics of smoking, more specific studies related to smoking behavior have investigated, i.a., the effects of price changes induced by excise taxes on cigarette consumption (e.g., Wasserman et al., 1991; Becker et al., 1994; Yurekli and Zhang, 2000; Tauras, 2006), the impact of legal restrictions on youth access to tobacco products (Chaloupka and Grossman, 1996; Gruber and Zinman, 2001; Kvasnicka, 2010), the effects of anti-smoking interventions on hospitalization (e.g., Sargent et al., 2004; Shetty et al., 2010; Marlow, 2012; Sargent et al., 2012; Adams et al., 2013; Kvasnicka et al., 2016), or the effects of public smoking bans on the exposure of non-smokers to second-hand smoke (Jiménez-Ruiz et al., 2008; Carpenter, 2009; Adda and Cornaglia, 2010). More closely related to our study are papers exploring the effects of workplace smoking bans. One notable such paper is the study by Evans et al. (1999), who find that workplace bans in the U.S. significantly reduced smoking prevalence and daily tobacco consumption among employed smokers. A review by Fichtenberg and Glantz (2002) concludes that workplace smoking restrictions were effective in reducing cigarette consumption and smoking prevalence. Even more closely related to our study are those papers investigating the effects of public smoking bans on individual smoking behavior. However, work in this area remains in-

[1] However, most research focuses on the U.S., where smoking rates are considerably lower than in European countries, particularly among young people (Shafey et al., 2009).

conclusive and has focused mainly on the U.S. Early research into the impact of indoor air legislation on smoking behavior produced ambiguous results (e.g., Wasserman et al., 1991; Chaloupka, 1992; Chaloupka and Saffer, 1992; Keeler et al., 1993; Sung et al., 1994; Chaloupka and Grossman, 1996). The same holds true for more recent studies (Yurekli and Zhang, 2000; Tauras, 2006; Adda and Cornaglia, 2010). Interestingly, Adda and Cornaglia (2010) do not find any evidence that smoking bans in the U.S. had a direct causal impact on either smoking prevalence or smoking cessation. However, they were able to show that smoking bans had adverse effects on non-smokers, especially on young children, by displacing smokers from public to private places.

The closest study to ours is the paper by Anger et al. (2011), who also use SOEP data to evaluate the 2007/2008 *general* public smoking ban in Germany with respect to short-term smoking propensity and intensity. While the authors find that the introduction of the general ban did not change the population's average smoking behavior in the short-run,[2] our study focusing on *school* children and young adults yields notably different results. Our findings strongly suggest that smoking bans targeted at schools are effective. We find a significant decrease in probability of taking up smoking, of 7-16 percent. For smoking intensity, results are somewhat less pronounced, which seems plausible since it might be easier to not start smoking in the first place than to quit or even reduce tobacco consumption. Nevertheless, our estimates indicate a reduction of up to 1 cigarette per day, which amounts to a decrease in smoked cigarettes of 8-13 percent. These results still holds if we consider the particular data structure with its small number of clusters evaluating the effects by the use of randomization inference.

This paper contributes to the literature in several regards. While most of the related literature focuses on the U.S., we provide evidence on the effect of smoking bans for Germany, a country which still has comparably high smoking rates. Even though there are some studies examining general smoking bans in Germany (particularly Anger et al., 2011), to the best of our knowledge, no other paper explicitly focuses on school smoking bans.[3] Using SOEP data for 2002 to 2014, we are able to differentiate between the two types of interventions, and can explicitly evaluate the smoking ban which targets probably the most important subgroup of the population: school children and young adults. To achieve this, our identification strategy not only exploits state-year variation in the introduction of these school smoking bans, but also variation stemming from different secondary school tracks and age cohorts, which implicitly feature different exposure time to the intervention. We also make use of additional variation introduced by the timing

[2] Using a different method, Brüderl and Ludwig (2011) broadly confirm the main results from Anger et al. (2011). However, Anger et al. (2011) show that individuals who frequent bars and restaurants exhibited a 2 percentage point lower propensity towards smoking following the introduction of a smoking ban. Their likelihood of smoking regularly also fell, as did their average daily cigarette consumption. Studies specifically focusing on smoking bans and the hospitality sector are, e.g., Ahlfeldt and Maennig (2010) or Kvasnicka and Tauchmann (2012).

[3] The only published study we know of specifically targeting school children (in the U.S.) is a medical short-term experiment from 2000, where the authors conclude that bans on smoking at school "may" reduce teenage smoking (Wakefield et al., 2000). Chaloupka and Grossman (1996) comprise a side-story on schools. In the context of students and irrespective of smoking bans, Powell et al. (2005) find that peer effects play a significant role in youth smoking behavior.

of the survey interview within a given year. Hence, we not only control for the general smoking ban when evaluating the school smoking ban, we also use a multiple-difference design to identify the effects of interest. Finally, we treat the 16 federal states of Germany as clusters and, due to how small this number is, apply randomization inference (RI) to be as conservative as possible when assessing the significance of the results. This simple and intuitive inferential technique gives valid p-values even for error terms which are clustered, have unknown structures or are otherwise complex. To date, randomization inference has not been applied in the smoking literature, even though it is a common method in other fields of treatment effect evaluation (Barrios et al., 2012; Cattaneo et al., 2015; Erikson et al., 2010; Ho and Imai, 2006; Small et al., 2008). Ultimately, we introduce an augmented RI permutation procedure, where we consider all the factors determining our treatment indicator.

The remainder proceeds as follows: Section 2 explains the institutional background, introduces the data, and provides initial descriptive evidence. Section 3 lays out the econometric model and Section 4 discusses the findings. Section 5 concludes.

2 Background, Data, and Descriptive Evidence

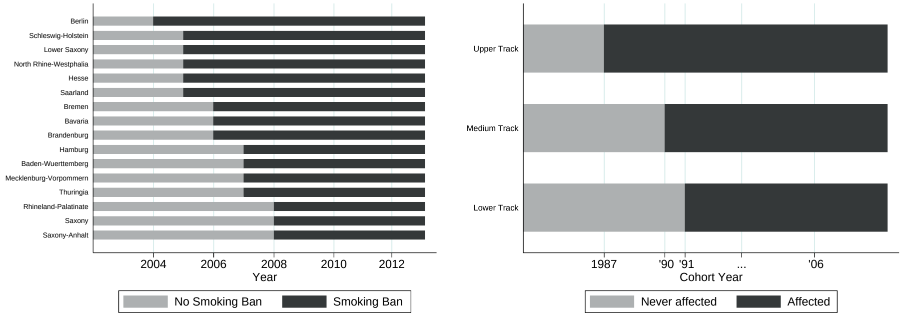
The first German federal state to introduce a school smoking ban was Berlin in 2004 (see Figure 1 (a)). Five other states followed in August 2005: Hesse, Lower-Saxony, North Rhine-Westphalia, Saarland, and Schleswig-Holstein. Bavaria, Brandenburg, and Bremen introduced their ban one year later. In 2007, schools in Baden-Wuerttemberg, Hamburg, Mecklenburg-West Pomerania, and Thuringia followed. The last federal states to implement a school smoking ban were Rhineland-Palatinate, Saxony, and Saxony-Anhalt in 2008.[4]

In addition to these differences between states and time, our data allows us to introduce even more layers of variation. In Germany, one can broadly differentiate between a lower (Hauptschule), a medium (Realschule), and an upper (Gymnasium) secondary school track, which not only differ with respect to academic subjects, but also how long they last (5, 6, and 9 years, respectively). This introduces additional treatment variation as becomes clear from Figure 1 (b), which shows example data for the federal state of Bavaria, which introduced the reform in 2005. As apparent from the figure, no one born before 1987 was affected by the ban. Thus, even individuals with the highest amount of school exposure (upper track) were not affected by the reform, as they had already finished school before 2005. However, starting from the 1987 age cohorts, the treatment status then depends on the attended school track. Analogously, for all other states, the interaction between cohort and school track helps identify the effects of school smoking bans, since some cohorts might be fully affected by the ban, affected only for particular years, or not affected at all.[5] Finally, the time of the SOEP survey interview varies within a given year,

[4] Note that between 2007 and 2008, Germany also introduced general public smoking bans. Hence, there are some states that were subject to both bans at the same time. In this study, we solely focus on the German school smoking ban and precisely control for the general ban in all our regressions.

[5] Thus, our treatment assignment and identification strategy is similar to the setting of Pischke (2007),

Figure 1: Variation of Treatment across and within Federal States in Germany



(a) Variation across States

(b) Variation across School Tracks

Note: Subfigure (a) displays the start of school smoking bans across the federal states. Subfigure (b) plots variation across school tracks within the state of Bavaria.

which provides variation in treatment status among individuals in the same state, cohort, and school track during the year the ban was introduced.[6] In general, we consider an individual as treated if she was ever exposed to a school smoking ban during her time at secondary school.

We employ data from the German Socio-Economic Panel (SOEP), a representative annual household panel of about $20,000$ individuals in around $11,000$ households (Wagner et al., 2007). Adult household members are regularly interviewed on socio-economic and demographic topics including education, income, employment, and health. Biannually from 2002 onwards, respondents are additionally asked whether they currently smoke, and if so, how many cigarettes on average per day.In our empirical analysis, we consider two outcome measures: (i) the propensity towards smoking, and (ii) the intensity of smoking. The SOEP further provides information on the month in which survey respondents were interviewed, the year and month of birth, as well as information on whether an individual has repeated a grade once or twice. Thus, we can precisely determine the treatment status of each individual. For our analysis, we restrict the sample to birth cohorts from 1985 to 1996 (last survey cohort), while excluding observations with extreme values for cigarette consumption with more than 50 cigarettes a day. Table 2 in the appendix provides descriptive statistics for all outcome and explanatory variables. In our full sample, 29.9 percent of individuals smoke. Smokers consume on average 11.3 cigarettes a day, whereas the unconditional cigarette consumption in the full sample is only 3.4.[7] Our sample consists of 48.2 percent of men, and the average age is 20.6 years.

who analyzes short- and long-term effects (scholastic and labor market performances, respectively) of short school years in Germany.

[6] The majority of survey respondents are interviewed between February and May. However, interview months do actually vary over the entire year.

[7] Throughout the paper, we will follow Anger et al. (2011) and also report the unconditional estimates, where we use the full sample as compared to the conditional case, where we exclude non-smokers.

3 Estimation

The variation in the exposure to school smoking bans across states, years, cohorts, school tracks, and interview months can be exploited with a multiple Difference-in-Differences (DiD) approach to identify causal effects on smoking behavior. We estimate the following model:

$$\mathrm{y}_{isyct} = \alpha + \beta \mathrm{D}_{syct} + \gamma_s + \delta_y + \eta_c + \theta_t + \mathrm{X}'\lambda + \gamma_s * \delta_y + \epsilon_{isyct}, \tag{1}$$

where y_{iscyt} is either a binary variable indicating an individual's smoking status or the number of cigarettes smoked per day. D_{syct} indicates whether an individual is or was ever affected by the smoking ban at school and depends on the combination of state, year, cohort, school track, and interview month. Under the common trend assumption, β identifies the effect of school smoking bans on the respective outcome of interest. $\gamma_s, \delta_y, \eta_c$, and θ_t are state, year, cohort, and school track fixed effects, respectively. The vector X' controls for individual and family background characteristics such as gender, age, employment status, migrant background, and household income. It also includes a dummy indicating the introduction of the general public smoking ban.[8] For all specifications, we relax the common trend assumption by including interactions of state and year fixed effects $\gamma_s * \delta_y$ (Angrist and Pischke, 2009). Analogously, we include interactions between school track and year fixed effects, $\theta_t * \delta_y$, in the richest specification of our regressions, which is also our preferred one.

To validly estimate the effect of school smoking bans within a DiD approach, two important factors must be considered. First, the causal effect of the intervention can only be identified under the common trend assumption, meaning that in absence of the intervention, treatment and control group should have evolved in the same way. This assumption can never be tested directly, but there are several methods to examine its plausibility. Since we have multiple pre-treatment periods in our data, we can implement a causality check in the spirit of Granger (1969). The intuition behind such a test is that if effects appear prior to the school smoking ban rather than vice versa, this would raise doubts about the identification strategy. To investigate this, we include three successive leads (anticipatory effects) to our preferred specification (i.e., it then contains placebo indicators ranging from one to three years prior to the actual intervention). Table 3 in the appendix shows no placebo effect is statistically significant for any of the outcome variables, indicating that the common trend assumption is not violated.

The second issue involves the correct calculation of standard errors. Many DiD applications face the problem of clustered or serial correlated data, where conventional inference methods lead to standard errors which are, in most cases, too small (Bertrand et al., 2004). A common remedy for such situations is the use of cluster-robust standard errors. However, this only leads to asymptotically valid inference, so the problem still persists in finite samples (Cameron and Miller, 2015). As the number of clusters in our study is quite

[8] Note that both the country-wide introduction of electronic verification of customers' age in cigarette vending machines as well as the increase in the minimum legal smoking age from 16 to 18 in 2007 are captured by θ_t.

small (16 states), we apply randomization inference, which originated in Fisher (1935), to obtain valid p-values. Early discussions of RI can be found in, e.g., Edgington (1995), Kennedy and Cade (1996), and Manly (2006). More recently, Barrios et al. (2012) provide a detailed theoretical and empirical explanation of why researchers should care about the data structure beyond state-level clustering. The application of RI is not restricted to DiD approaches (Erikson et al., 2010); it can also be applied within the regression discontinuity design (Cattaneo et al., 2015) or the potential outcome framework (Ho and Imai, 2006). The basic idea of such randomization tests is intuitive and very applicable due to its weak assumptions. This inferential technique changes the conventional thought experiment of repeated sampling from an underlying population to fixed data and repeated permutations of, e.g., the treatment variable. The general procedure can be summarized in four steps. First, the test statistic is computed for the original data set. Second, the data is permuted and the new test statistic is computed. Third, the second step is repeated, e.g., $10,000$ times, as in our study. Finally, the original test statistic is compared with the distribution generated in the third step and a decision is made. Thus, we create the reference distribution for testing with the data at hand and do not have to rely on the conventional t-distribution. Since, for clustered data, the randomization distribution often has fatter tails than the t-distribution, we evaluate the effects of interest more conservatively.

In our application, individuals' treatment statuses do not depend on just one variable (e.g., the state of residence), which is often the case for policy interventions (e.g., Erikson et al., 2010), but on several factors: state, year, age cohort, school track, and interview month. Thus, we perform an augmented randomization procedure by permuting *all* treatment-determining variables randomly and assigning the treatment status thereafter, according to such pseudo characteristics.

4 Results

The medium-term results based on the sample comprising survey years 2002 to 2014 are presented in Table 1. As outlined in Equation (1), all regressions include a full set of state, year, school track, and age cohort fixed effects. They also contain a dummy variable for the introduction of the general smoking ban in Germany. We further control for gender, age, age squared, migration background, employment status, and (log) household income. To relax the common trend assumption, we additionally include state-specific time trends in all specifications. Lastly, track-specific time trends are included in Column (3), our preferred specification. Standard errors are shown in brackets below the coefficient estimates: conventional in Column (1), heteroskedasticity-robust and clustered at the state level in (2) as well as in (3). We further evaluate the results of Specification (3) by using randomization inference to obtain valid p-values despite the small number of clusters; this is shown in Figure 2. Table 4 in the appendix reports analogous results for the short-term effect of the reform based on the subsample comprising survey years 2002 to 2010.

Panel A in Table 1 reports the results for the effect of the school smoking ban on propensity towards smoking using a linear probability model.[9] The coefficient for the treatment indicator is negative and statistically significant in all specifications, suggesting that the introduction of the school smoking ban exerts a negative impact on propensity towards smoking.[10] Quantitatively, the ban decreases the propensity towards smoking by around 2.7 percentage points. Relative to the mean smoking propensity of (currently) unaffected individuals of 36 percent, this amounts to a decrease of 7 percent.[11] The analogous short-term effect of the reform (Panel A of Table 4 in the appendix) is even more pronounced with a negative estimate ranging between 4.2 and 5.4 percentage points. Compared with the baseline average of 34 percent, this translates into a decrease in the propensity to smoke of up to 16 percent.

Moreover, our results stay robust if we take the small number of clusters into account and evaluate the effects more conservatively by applying randomization inference. This can also be seen from Figure 2 (a), which we will discuss in more detail below. The corresponding, one-sided p-value for the effect of the smoking ban on propensity towards smoking is 0.087. We test one-sided alternatives with the permutation procedure for two reasons. First, we expect the impact of the reform to be negative for both outcomes, which is confirmed over all specifications and in line with the related health literature (e.g., Evans et al., 1999; Fichtenberg and Glantz, 2002; Yurekli and Zhang, 2000; Tauras, 2006). Second, this is a common approach in the RI literature in general (e.g., Barrios et al., 2012, Cattaneo et al., 2015, Ho and Imai, 2006).

Panel B of Table 1 reports the results regarding smoking intensity for the full sample as in Anger et al. (2011).[12] The coefficient estimate of interest is statistically significant and negative in all specifications. The reform has decreased the number of smoked cigarettes per day by about 0.56, which translates into a decline of about 13 percent when compared with the pre-treatment average of 4.4 cigarettes per day over all survey participants, i.e., smokers and non-smokers. This effect stays statistically significant when evaluated using RI, see Figure 2 (b) (p-value: 0.0781). If we restrict the sample to individuals who reported being a smoker (Panel C), the coefficient for smoking intensity almost doubles to -0.9. Due to the smaller sample size, the significance of the effect decreases to a certain extent, although, still remaining statistically significant at at least the 10 percent level. This is also true if the randomization procedure is employed, see Figure 2 (c) (p-value: 0.0517). Quantitatively, this means that smokers reduced their cigarette consumption by almost 1 cigarette per day due to the ban. Translated into relative figures based on a baseline comparison among smokers of 12 cigarettes per day, the intervention had

[9] We estimate and report results of linear probability models for the ease of interpretation. Our findings are robust to the use of alternative estimation methods, e.g., probit regressions.

[10] The sign of the control variables are as expected and as found in the previous literature. For instance, men have a higher probability of being a smoker. As our sample comprises individuals aged 16 to 29, the coefficient of age is positive. The coefficient of age squared (not reported) is negative, though, suggesting a concave relationship.

[11] Note that the relative effect determined at the overall mean is 9 percent, so our evaluation based on the baseline/pre-treatment mean is more conservative.

[12] There, the authors decide to examine unconditional rather than conditional demand, since the latter would not yield a causal interpretation.

Table 1: Regression Results 2014

	(1)	(2)	(3)
Panel A: Propensity (N=10,790)			
Treated	-0.0275*	-0.0275*	-0.0272*
	(0.0151)	(0.0143)	(0.0145)
Male	0.0152*	0.0152	0.0153
	(0.0085)	(0.0148)	(0.0148)
Age	0.1234***	0.1234***	0.1217***
	(0.0228)	(0.0273)	(0.0278)
Panel B: Intensity (All, N=10,812)			
Treated	-0.5694***	-0.5694***	-0.5567***
	(0.2089)	(0.1515)	(0.1566)
Male	0.3925***	0.3925*	0.3945*
	(0.1169)	(0.2102)	(0.2104)
Age	1.8141***	1.8141***	1.7538***
	(0.3145)	(0.3499)	(0.3633)
Panel C: Intensity (Cond., N=3,223)			
Treated	-0.9723**	-0.9723*	-0.9414*
	(0.4387)	(0.5185)	(0.5057)
Male	0.7452***	0.7452*	0.7370*
	(0.2393)	(0.3527)	(0.3548)
Age	2.0064***	2.0064***	1.9457***
	(0.6434)	(0.5833)	(0.5604)
State-Specific Time Trend	x	x	x
Track-Specific Time Trend	-	-	x
Inference	Conventional	Clustered SEs	Clustered SEs

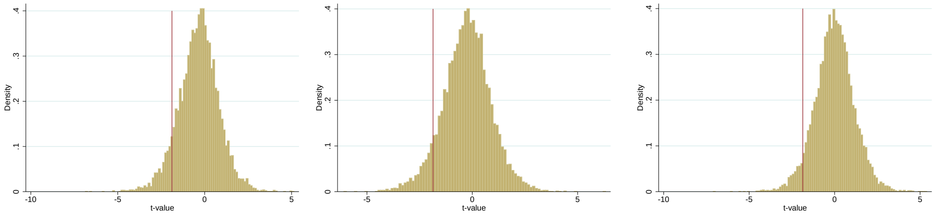
Note: The table shows regression results for the sample 2002 to 2014. All columns include year, state, school track, and age cohort fixed effects. Moreover, age, age squared, migration status, employment status, (log) household income, and a state-year specific time trend are controlled for. In Specification (3), we additionally include an education-year specific trend. Inference denotes either conventional standard errors or standard errors clustered at the state level. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

a substantial impact on the number of smoked cigarettes with a decrease of about 8 percent.[13] Analogous short-term effects of the reform (Panels B and C of Table 4 in the appendix) are in line with the medium-term results, showing very similar point estimates.

Figure 2 depicts the RI evaluation of the school smoking ban effect on the respective outcome variables. Subfigures (a), (b), and (c) correspond to Panels (A), (B), and (C) of Specification (3) in Table 1, respectively. By comparing the original t-values (red, vertical lines) with the randomization distributions (brown bars), the significance of the effects can be determined. The graphical inspection clearly shows that it is feasible to test a one-sided alternative and compare the original t-value to more extreme values to the left. For the probability towards smoking, depicted in Subfigure (a), the original t-value is located to the extreme left side, indicating a significantly different effect from zero, with a one-sided p-value of 0.087. A similar picture emerges for smoking intensity (Subfigures (b) and (c)), where the original t-values are also located to the extreme left sides of the respective distributions. As a consequence, the effect remains statistically significant for a one-sided

[13] Note that our results for smoking intensity do not change significantly if we control for self-selection via a standard Heckman procedure.

Figure 2: Randomization Distribution and t-Values for 2014 Estimates



(a) Propensity (b) Intensity (Full Sample) (c) Intensity (Conditional)

Note: The figure shows the randomization distributions (brown bars) with 10,000 permutations for the effects on smoking propensity (Subfigure (a)) and intensity (Subfigures (b) and (c) for the conditional effect) for the sample from 2002 to 2014. The red, vertical lines indicate the original t-values. The figure corresponds to Specification (3) of Table 1. The corresponding one-sided RI p-values for the treatment effects are 0.087 (Panel A), 0.0781 (Panel B), 0.0517 (Panel C).

test of the full sample (0.0781), becoming more significant for the smoker restricted sample (0.0517).[14]

5 Conclusion

We analyzed the impact of school smoking bans on individual smoking behavior in Germany. Exploiting the timing of the introduction of the ban across different federal states, the variation stemming from different secondary school tracks and age cohorts, and the timing of the survey interview within a given year, we were able to identify effects up to ten years after the intervention (medium-term analysis). We also restrict the sample to estimate respective short-term impacts. Over both analyses, we find that smoking bans targeted at schools cause probability of smoking to decline by 7-16 percent. Concerning smoking intensity, our estimates indicate a reduction of about 8-13 percent. These findings strongly deviate from previous studies focusing on general smoking interventions in Germany, e.g., Anger et al. (2011) and Brüderl and Ludwig (2011), who find no effects for the overall population. However, our results are broadly in line with the literature on workplace smoking bans in the U.S. For instance, Evans et al. (1999) and Fichtenberg and Glantz (2002) report a decrease in conditional smoking intensity of about one to two cigarettes per day. With respect to smoking prevalence, these authors report a reduction of about four percent, whereas our propensity estimates are much more pronounced—up to a factor of four. Overall, we therefore conclude that future anti-smoking initiatives should target school children, i.e., (very) young age cohorts. From a tobacco industry point of view, this group is very important since it might comprise the next wave of consumers, which could generate income flows for years to come. From a health policy point of view, this group is even more important, since it is not yet affected by tobacco and its addictive influences and, most notably, reacts strongly to anti-smoking interventions.

[14] Table 4 in the appendix reports analogous RI figures for our short-term analysis: corresponding p-values are 0.0729 (Panel A), 0.1430 (Panel B), and 0.0845 (Panel C). Hence—when examined most conservatively—for two of the three outcomes of interest, our regressions yield statistically significant treatment effects.

This should raise hope among health-conscious governments, because such reactions are still observable many years after the initial treatment.

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A Appendix

Table 2: Summary Statistics

	Mean	SD	Min	Max	N
Outcomes					
Smoker	0.299	0.458	0.0	1.0	10,815
#Cig per Day (full sample)	3.367	6.404	0.0	50.0	10,837
#Cig per Day (conditional)	11.300	6.926	0.0	50.0	3,229
Controls					
Male	0.482	0.500	0.0	1.0	12,212
Age	20.593	3.242	15.0	29.0	12,212
Employed	0.507	0.500	0.0	1.0	12,212
Migration Background	0.255	0.436	0.0	1.0	12,193
Household Income (in 1,000 €)	10.430	0.714	2.4	13.5	12,206
Education					
Lowest Track	0.181	0.385	0.0	1.0	12,212
Middle Track	0.285	0.451	0.0	1.0	12,212
Highest Track	0.277	0.447	0.0	1.0	12,212
Still at School	0.257	0.437	0.0	1.0	12,212

Note: The table reports means, standard deviations (SD), minimum and maximum values, as well as the number of observations (N) for the variables of our analysis. #Cig. per Day is the number of cigarettes, cigars, and pipes smoked per day for the full sample and for smokers only (conditional).

Table 3: Common Trend Plausibility Check

	(1) Propensity	(2) Intensity (All)	(3) Intensity (Cond.)
Placebo Reform -1	-0.0008	0.2772	0.3814
	(0.0376)	(0.4967)	(0.4619)
Placebo Reform -2	0.0705	0.7098	-1.5334
	(0.0753)	(1.0248)	(1.2542)
Placebo Reform -3	0.1493	1.4511	-3.2609
	(0.1199)	(1.6025)	(2.9996)
N	10,790	10,812	3,223

Note: The table shows results of the common trend plausibility check. For this, placebo reform indicators for 1, 2, and 3 years prior to the intervention are included to Specification (3) of Table 1. Standard errors in parentheses are clustered on state-level. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Regression Results 2010

	(1)	(2)	(3)
Panel A: Propensity (N=5,336)			
Treated	-0.0539**	-0.0539**	-0.0424*
	(0.0220)	(0.0220)	(0.0205)
Male	-0.0119	-0.0119	-0.0114
	(0.0122)	(0.0212)	(0.0210)
Age	0.1256**	0.1256**	0.1272**
	(0.0547)	(0.0440)	(0.0463)
Panel B: Intensity (All, N=5,343)			
Treated	-0.7490**	-0.7490***	-0.5973**
	(0.2914)	(0.2461)	(0.2468)
Male	0.0721	0.0721	0.0784
	(0.1619)	(0.3020)	(0.3002)
Age	2.2618***	2.2618***	2.2176***
	(0.7262)	(0.6615)	(0.7357)
Panel C: Intensity (Cond., N=1,578)			
Treated	-1.0813*	-1.0813*	-0.9051
	(0.6526)	(0.5912)	(0.6227)
Male	0.5781*	0.5781	0.5828
	(0.3325)	(0.3930)	(0.3920)
Age	3.5127**	3.5127***	3.4334***
	(1.5365)	(1.0162)	(1.1215)
State-Specific Time Trend	x	x	x
Track-Specific Time Trend	-	-	x
Inference	Conventional	Clustered SEs	Clustered SEs

Note: The table shows regression results for the sample 2002 to 2010. All columns include year, state, school track, and age cohort fixed effects. Moreover, age, age squared, migration status, employment status, (log) household income, and a state-year specific time trend are controlled for. In Specification (3), we additionally include an education-year specific trend. Inference denotes either conventional standard errors, standard errors clustered at the state level. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The corresponding one-sided RI p-values for the treatment effects of Specification (3) are 0.0729 (Panel A), 0.1430 (Panel B), 0.0845 (Panel C).