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Tobacco sales prohibition and teen smoking☆

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ABSTRACT

We evaluate one of the most prevalent prohibitory policies: banning the sales of tobacco to teens. We exploit the staggered introduction of sales bans across Switzerland and analyze rich data from 2001 to 2016. The estimates do not indicate an immediate or long-run systematic reduction in the overall prevalence of smoking because of sales bans. We also examine a range of behavioral mechanisms that are key to understand the consequences of prohibitory policies such as habit formation, social appeal of smoking, circumvention behavior, or risk perceptions. Among others, we find that teens circumvent the bans by getting cigarettes from peers. Moreover, teens consider smokers less cool when a sales ban is in place, but they do not consider smoking more dangerous.

1. Introduction

Smoking is a major cause of cancer, heart disease, and early death (Jha and Peto, 2014; World Health Organization, 2016). In response, governments have introduced various tobacco control policies for adults and teens to improve public health. Targeting teens may be particularly effective since early smokers smoke more as adults, and many smokers start as teens (see, e.g., van Ours, 2006; DeCicca et al., 2008).[1]

A particularly widespread policy are bans of the sale of tobacco products to young people. Proponents of sales bans argue that they reduce teen smoking by making it more difficult for teens to get cigarettes and by signaling the danger of smoking.

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[1] For instance, more than 90% of smokers in the United States tried their first cigarette before they turned 18 (Health and Services, 2014). In Switzerland, more than half of smokers started smoking as teens (see Appendix, Figure A.1).

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Opponents counter that smoking may become more appealing — the forbidden fruit effect. They also highlight that teens can circumvent the restrictions by getting cigarettes from other sources, such as their peers, instead of from stores. The arguments about restricted access, circumvention, signaling the danger of drug consumption, and the forbidden fruit effect are fundamental to any assessment of the effects of prohibition (MacCoun, 1993; Miron and Zwiebel, 1995; Sunstein, 1996; Landman et al., 2002; Bénabou and Tirole, 2011; Jacobi and Sovinsky, 2016; García-Jimeno, 2016). The extent to which they matter is an open empirical question.

We examine these aspects in a comprehensive evaluation of tobacco sales bans across Switzerland exploiting a natural experiment to add causal evidence to the policy debate. So far, evidence on the manifold mechanisms that are crucial for the effectiveness of prohibitory policies is scarce. We analyze the variation in the introduction of sales bans across jurisdictions and time to assess the bans' effect on smoking prevalence, along with an assessment of a range of behavioral mechanisms. In particular, we examine habit formation, the forbidden fruit effect, the role of social norms, risk beliefs, procurement strategies, and the enforcement of the law. We use introduction dates across the 26 cantons of Switzerland and match this information with two data sets for Switzerland, yielding a sample of young people up to the age of 20 with more than 28,000 observations.

An advantage of the institutional setting in Switzerland is that cantons can introduce sales bans on tobacco, but the federal government sets taxes on tobacco and alcohol.[2] This combination of cantonal policy changes within federal institutions shared by all cantons reduces the risk of omitted variable bias and increases the likelihood of identifying the causal effect of sales bans. For example, changes in tobacco taxation and changes in cigarette prices could not distort our estimates. Importantly, we can also not reject common trends in smoking prevalence across cantons before the introduction of sales bans. The Swiss federal system thus offers a good setting in which to examine the impact of sales bans on smoking prevalence.

We find that the prohibition of tobacco sales to teens does overall not statistically significantly reduce the prevalence of smoking among teens. The point estimates suggest only a small aggregate reduction in smoking prevalence if any. When we examine different sub-samples, we observe point estimates ranging from an increase of 1.8 percentage points (not statistically significant) for teens with highly educated fathers to a reduction of 3.5 percentage points for teens with fathers with relatively less education from an initial smoking prevalence of around 19%. This is clearly less of an impact than suggested by country level case studies (see, e.g., Millett et al., 2011).

Regarding the behavioral mechanisms, we document several empirical patterns. First, we do not find any evidence for habit formation as we do not observe that sales bans systematically affect smoking intensity or smoking prevalence in the long-run. Second, we do not find evidence of a forbidden fruit effect triggered by the prohibition of tobacco sales to teens — one of the main arguments against prohibition. Sales bans do not make smoking more appealing. In contrast, we find that the appeal of smoking decreases with the introduction of sales bans. Third, when studying the effect on social norms, we do not find that sales bans change the perceived attitudes toward smoking of parents and friends. Fourth, we find no increase in the perceived danger of smoking in reaction to the bans. This result suggests that sales bans are not an informative signal that would change priors about the danger of smoking.

Fifth, we assess whether teens circumvent the bans by getting cigarettes from peers instead of stores. This could provide an explanation for the small effect of sales bans on the prevalence of smoking. Consistent with this argument, we find a reduction in smoking only among teens who do not have smoking peers. The estimates also suggest that teens substitute getting cigarettes from stores with getting cigarettes from peers after the introduction of a sales ban. The findings indicate that peer effects may work through access to cigarettes in addition to peer pressure. Lastly, we study the relevance of the enforcement of the law. We do not find differential effects of the sales ban depending on whether the canton conducts test purchases or on whether compliance with the law is high or low. However, we find that cantons that introduced a minimum sales age of 18 were more successful in reducing current smoking than cantons with a minimum sales age of 16. Cantons with a minimum age of 18 experience a reduction of between 1.0 and 2.2 percentage points (depending on the sample). This is consistent with additional results showing that the reduction in cigarettes bought in stores is driven by the sales bans with a minimum age of 18. The initial reduction in the prevalence of smoking does, however, not carry over to a long-term decrease in smoking.

Our evaluation of tobacco sales bans adds in several ways to previous literature on access restrictions for tobacco. First, we provide causal estimates of the effect on smoking prevalence, which has rarely been done. Previous studies on the prohibition of addictive goods to teens mainly examine the effects of getting access to cannabis and alcohol (Carpenter and Dobkin, 2009; Crost and Guerrero, 2012; Carpenter and Dobkin, 2015; Williams and Bretteville-Jensen, 2014; Anderson et al., 2015; Jacobi and Sovinsky, 2016; Carpenter et al., 2016; Marie and Zölitz, 2017). Moreover, studies investigating the impact of

[2] Regarding the regulation of the production, sale, and consumption of cannabis, the same federal law applies throughout Switzerland, i.e., *the Federal Act on Narcotics and Psychotropic Substances*. The law is also the same when it comes to the regulation of alcohol. For example, the minimum sales age is 16 for beer and wine and 18 for liquor throughout Switzerland.

tobacco control policies on teen smoking focus on taxes, clean indoor air laws, or access to e-cigarettes.[3] Policies restricting the access to tobacco, however, have received less attention (DeCicca et al., 2020).

Second, we study a series of outcomes that allow us to discuss behavioral mechanisms that are relevant to any prohibitory policy. Evidence for these mechanisms with respect to the introduction of sales bans is still missing (DiFranza, 2012; Institute of Medicine et al., 2015). For example, our findings regarding social norms and circumvention through peers complement the literature studying peer effects on risky behaviors (see, e.g., Cawley and Ruhm, 2011; Card and Giuliano, 2013; Elsner and Isphording, 2018; Lundborg, 2006; Clark and Lohéac, 2007) by showing that peers who smoke may act as gatekeepers.

Third, while we use spatial variation across time, existing studies on sales bans mainly use case studies for single countries. These studies compare the smoking prevalence before to the smoking prevalence after raising or introducing a minimum sales age (Rimpelä and Rainio, 2004; Hagquist et al., 2007; Millett et al., 2011). In all the analyses, the authors find a negative correlations between smoking prevalence and sales bans. Yet, the time series analyses do not allow the researcher to separate the impact of the sales ban from any general trend in smoking prevalence within countries. The analyses could thus lead to an overestimation of the effect of sales bans if there is a general decline in smoking.[4] A similar ambiguity about the identification of causal effects of sales bans applies to the study by Kuipers et al. (2017). The authors exploit five increases in minimum age and two introductions of sales bans in comparison to twelve control countries in the European Union. While two waves of cross-sectional data allow the authors to compare smoking prevalence before and after an extension of bans, they are not able to assess pre-trends. They find no statistically precisely estimated relationships between an increase in minimum sales ages and smoking prevalence.[5]

Fourth, rather than the introduction of sales bans, previous studies exploit changes in the enforcement of existing access restrictions for teenagers across the United States. For instance, a randomized control trial in six Massachusetts communities that strengthened enforcement of existing sales bans suggests a null effect on smoking (Rigotti et al., 1997). Abouk and Adams (2017b) find that more frequent compliance inspections lead to a reduction in smoking for girls but not for boys. Grucza et al. (2013) find, if anything, a small long-term effect of having been subject to stronger enforcement of access restrictions. Yörük and Yörük (2016) exploit individual-level panel data to estimate the impact of passing the minimum age threshold in a regression discontinuity design. They find a small temporary increase in tobacco consumption of individuals after passing the minimum age.

Lastly, we isolate the effect of sales bans from other tobacco control policies. Other studies use variation in indexes summarizing the strictness of diverse policies for teens in the United States. Findings are mixed, with studies reporting positive, negative, or statistically imprecisely estimated correlations between such indexes and smoking prevalence among teens (see, e.g., Gruber and Zinman, 2000; Cawley et al., 2006; Nesson, 2017).[6] As these indexes are a summary measure for diverse policy tools for preventing teen smoking (including packaging restrictions, regulations on possession, minimum smoking age, restrictions on free distribution of samples, advertising, licensing, and regulation of use) it is difficult to infer conclusions about specific policies. The policies could have diverging effects, making it difficult to interpret the effects of a 1-point change in an index.

The remainder of the paper is organized as follows: In Section 2, we describe the institutional setting and introduce the data. Section 3 provides some descriptive evidence and explains the empirical strategy. Section 4 shows the main results. In Section 5, we provide evidence for the behavioral mechanisms. Section 6 discusses the identifying assumptions and the robustness. Section 7 concludes.

[3] The literature is rather broad and includes for example: DeCicca et al. (2002); Adda and Cornaglia (2010); Odermatt and Stutzer (2018); Chaloupka and Wechsler (1997); DeCicca et al. (2008); Bharadwaj et al. (2014); Lillard et al. (2013); Hawkins et al. (2016); Hansen et al. (2017); Rees-Jones and Rozema (2019); Palali and van Ours (2019); Pfeifer et al. (2020); Abouk and Adams (2017a). For a recent review on these regulations, see DeCicca et al. (2020).

[4] Trends are also an issue in the study by Schneider et al. (2016), who examine the smoking prevalence among 9 to 12 graders in Needham in Massachusetts after an increase of the minimum sales age from 18 to 21. They compare the smoking prevalence in these communities to sixteen surrounding communities which did not increase the minimum sales age. Unfortunately, the authors can only observe smoking prevalence after the adoption of the higher sales age. The larger decline in the prevalence of smoking in the adopting community could therefore either be an effect of the increase in the minimum sales age or the spurious result of different trends.

[5] We are only aware of one case study by Macinko and Silver (2018) which uses both, several yearly waves of cross-sectional data prior to the adoption of a higher minimum legal purchase age and a comparison group. In their analysis, they assess the effect of an increase in the minimum legal sales age from 18 to the age of 21 in New York City in August 2014 on youth tobacco consumption in comparison to the rest of New York State as well as in comparison to four cities in Florida. While adolescent tobacco use slightly declined in New York City it declined even more in the control regions. Consequently, the authors estimate a relative increase in smoking prevalence after the increase in the minimum sales age. However, the study population was confined to students in seventh to twelve grade. Most of theses students were not directly affected but only indirectly through their older peers who newly experienced a limited access.

[6] Powell et al. (2005) use cross-sectional variation across states and find a negative correlation between the index and smoking prevalence. Nesson (2017) studies more recent data from the United States and finds a positive association of tobacco control policies and youth smoking prevalence as measured by a biomarker. In Appendix D, we provide a brief evaluation of the use of biomarker data in comparison to survey data to assess teen smoking prevalence.

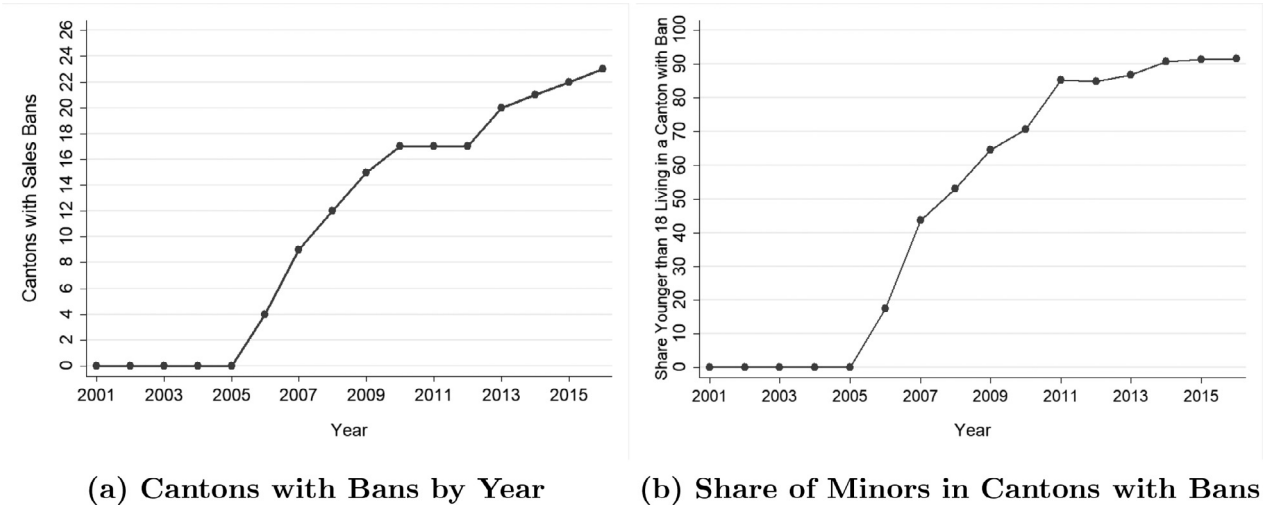

Fig. 1. Staggered introduction of sales bans across Switzerland.*Note:* The graphs show the gradual introduction of the sales bans across Swiss cantons from 2006 onwards. Panel (a) depicts the absolute number of cantons that have introduced a ban up to and including the given year on the x-axis. Panel (b) indicates the share of minors surveyed in the Tobacco and Addiction Monitoring that lives in a canton with a ban. *Data source:* Tobacco and Addiction Monitoring, 2001–2016.

2. Institutional setting and data description

2.1. Staggered introduction of sales bans in Switzerland

Before the introduction of the bans, individuals of any age could purchase tobacco products de jure. The introduction of the bans then prohibited the sale of tobacco to teens under a certain age. The laws cover processed and nonprocessed tobacco, chewing tobacco, snuff, and shisha tobacco.[7]

Swiss cantons started introducing sales bans in 2006.[8] Out of 26 cantons, 23 introduced bans on 17 different dates. Fig. 1 depicts the change in the number of cantons with a sales ban over time as well as the share of minors living in a canton with a ban by year. Figure A.2 provides an overview of the spatial distribution of the sales bans and Table A.1 shows the different introduction dates and the minimum sales ages implemented across cantons.[9] The minimum legal age in 12 of the 23 cantons that introduced a ban is 16, and in the other 11 cantons it is 18, the age at which individuals are legally considered adults. We refer to individuals below the age of 18 as minors and below the age of 21 as teens. The bans also apply to vending machines.

Most other institutional changes relevant to smoking behavior, such as taxes on tobacco and alcohol, are implemented for all cantons simultaneously on the federal level. There is no variation in tobacco taxes across cantons, which allows us to estimate the effect of sales bans separately from effects of tax changes.

Some cantons introduced advertisement bans for tobacco products and clean indoor air laws for public buildings, such as schools, in the same time period.[10] To account for these policies, we include indicators for other cantonal laws in robustness checks. In addition, some cantons rolled out information campaigns on tobacco. However, according to our investigations, all the cantons that started such prevention programs implemented them in recent years and at least 3 years after the introduction of their sales ban. E-cigarette regulation does not coincide with sales bans either. The policy did not change in the sample period, with the sale of e-cigarettes being illegal in Switzerland until 2018.[11]

[7] The law texts are similar across cantons. Here, as an example, is the law text from the canton of Bern: "HGG Art 16 Sales of Tobacco: 1. The distribution and sale of tobacco to children and adolescents under the age of 18 are prohibited. 2. The sales personnel have to check the age of the customer. To this end, it is allowed to demand an ID." On the website, it is further specified that "if necessary, the personnel must check the age with an ID. Non-adherence is a punishable offence." For details, see https://www.vol.be.ch/vol/de/index/direktion/organisation/beco/wdb_gewerberecht.thema.70.html. The fines can be up to $40,000 for an illegal sale.

[8] The introduction dates stem from the Swiss Federal Office of Public Health.

[9] Tables and figures with an alphabetic prefix can be found in the Appendix.

[10] Boes et al. (2015) show that clean indoor air laws in Switzerland reduced smoking among adults and affected going-out behavior, and Mazzonna and Salari (2018) show that indoor air laws reduced the incidence of heart attacks.

[11] During our sample period, less than 1% of the Swiss population smoked e-cigarettes and access to e-cigarettes was still difficult. The commercial sale of e-cigarettes containing nicotine has just been allowed in 2018; for details see: https://www.suchtpraevention-zh.ch/abhaengig-von/e-zigaretten-e-shishas/?L=0. Before that, one could import a maximum of 150 mg of "e-liquid" containing nicotine per year for self-use.

2.2. Data description

Tobacco and addiction monitoring We base the main analyses on quarterly cross-sectional data from the Tobacco and Addiction Monitoring survey for the years 2001 to 2016. This survey combines data from the Tobacco Monitoring survey, covering the years 2001 to 2010, with data from the follow-up Addiction Monitoring survey, covering the years 2011 to 2016 (Suchtmonitoring Schweiz (2017); Hornung et al., 2010). The data contain information on smoking behavior, attitudes toward smoking, and the perceived danger of smoking as well as on how teens get tobacco.

The variable capturing smoking prevalence stems from the question: "Do you smoke, even if only rarely?" It takes a value of 1 if the individual answers yes and 0 otherwise. From the entire population sample, we include 28,704 observations in the sample of people up to age 20 (or 48,481 in the sample up to age 27) for which we have non-missing values of the covariates, fixed effects, and the smoking status. Given our focus on teen smoking, one advantage of the data is that teens aged 14 to 17 are oversampled for all years; see Figure A.3.

The data further include geographic identifiers as well as information about the date of the interview. For the years 2005 to 2010, we know the exact date. For more recent years, we know the month and year of the interview. The information allows us to assess whether an individual lived in a canton with a ban at the time of the interview. We merge cantonal information on the density of physicians from the Swiss Statistical Office and on youth and adult unemployment from the State Secretariat for Economic Affairs (SECO) offered through Amstat with the survey data.[12] Table A.2 shows the descriptive statistics for the variables from the Tobacco and Addiction Monitoring survey, as well as the merged cantonal variables.

Health behaviour in school-aged children We use data from the Health Behaviour in School-aged Children (HBSC) survey from 2002 to 2014 to examine smoking behavior among teens age 11 to 15 years (Suchtmonitoring Schweiz and Swiss Cantons, 2014).[13] The data contain survey responses on health behaviors from high school students every 4 years. In particular, the data provide information on students' current smoking status, whether they have ever tried smoking, and their age at the first puff.[14] The possible answers to the question of whether the teens smoke are "every day," "at least once a week," "less than once a week," or "I don't smoke." We classify a teenager as a nonsmoker if he or she said "I don't smoke" and as a smoker otherwise.

We also know the canton of the student and the year of the interview. Data from the Health Behaviour in School-aged Children survey exist for only 15 cantons, of which 11 introduced a sales ban in the survey period. In 4 cantons, the sales ban was introduced either before or after the sample period. The reason why we have data for just a subset of cantons is that cantons had to pay extra to get a representative sample, and it is only those cantons for which we could get access to data with cantonal identifiers (for details about the data availability, see Table A.3). We again merge cantonal information as described above with the data set. Table A.2 gives the descriptive statistics.

3. Descriptive evidence and empirical strategy

3.1. Descriptive evidence

From 2001 to 2016, the prevalence of smoking decreased from roughly 20% to under 17% for ages 14 to 17; see Figure A.4.[15] For adults, smoking prevalence decreased from over 30% to under 25% over the same time period. From 2002 to 2014, based on the data from the HBSC survey, the share of smokers and the share of individuals who tried smoking among younger teens aged 11 to 15 also declined, as can be seen in Figure A.6. During the same time, the average age of the first puff increased. This draws attention to the question whether the implementation of the sales bans caused the decline in the prevalence of smoking among teens.

Fig. 2 shows the smoking trend among teens aged 14 to 17 years before and after the introduction of the laws. While there is a decrease from a smoking prevalence of 20% to around 17%, we do not find a change in the trend after the introduction of the laws. Yet, the simple before-and-after comparison does not exploit that cantons introduced laws at different points in time or did not introduce a law, aspects that we consider in our empirical strategy.

3.2. Empirical strategy

Estimation specification To identify the effects of the sales bans on smoking behavior and other outcomes, such as teens attitudes, we rely on the staggered introduction of the sales bans across jurisdictions and the aspect that they are binding only for people under a certain age. We exploit i) the variation between cantons with and without a sales ban in any given

[12] Youth unemployment is missing for the canton of Appenzell-Innerhoden for the years 2010 to 2013, which we interpolate linearly.

[13] We obtained written consent of the cantons that had a sample that allows for representativity within cantons to use their data. The data were then kindly provided by the organization Addiction Switzerland (Sucht Schweiz).

[14] The data also contain information on the number of cigarettes smoked from 2006 onward (we do not use the data on the number of cigarettes available for 2002 as students wrote down the exact number of cigarettes themselves, yielding highly unlikely numbers) and the frequency of smoking for the years 2002 through 2014 for smokers. The latter was only asked of the comparatively older students.

[15] This likely affects future smoking behavior as current smokers are often people who started early. Figure A.5 shows the relationship between initiation age and the likelihood of smoking currently.

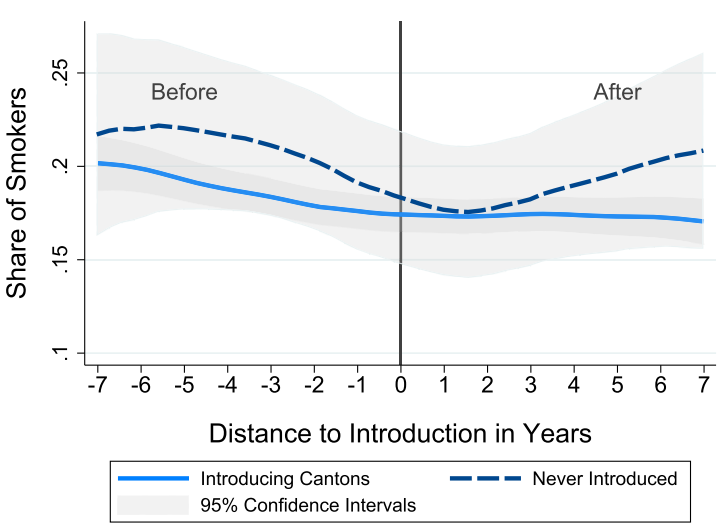

Fig. 2. Smoking trends for minors in Switzerland relative to the introduction of sales bans. *Note:* The figure indicates the trends in smoking prevalence among teens aged 14 to 17 years for cantons that introduced a sales ban (solid line) and cantons that did not (dashed line). The vertical line marks the introduction date. For cantons which never introduced a sales ban, we assume, for this figure only, 01.01.2009 as an introduction date which is slightly above the median introduction date. The blue trend line shows estimates from local linear regressions with bandwidth 2. The shading shows the corresponding 95% confidence intervals. The distance in years on the *x*-axis is continuous and based on the distance to either the precise date of the interview or the month of the interview. *Data source:* Tobacco and Addiction Monitoring, 2001–2016.

year, ii) the variation in a canton over time across years with and without a sales ban, and iii) the variation within cantons with a sales ban that comes from some individuals being subject to a binding sales ban due to their young age while others are not.

We estimate the effects of the sales bans with the following difference-in-difference specification:

$$\text{Smoker}_{ict} = \beta_1 \text{BindingSalesBan}_{ict} + \beta_2 \text{SalesBan}_{ct} + \gamma_a + \eta_c + \theta_y + X'_{ict}\delta + \varepsilon_{ict}$$

In the main analysis, the dependent variable is Smoker_{ict}, which is 1 if an individual smokes and 0 otherwise. The main coefficient of interest is β_1 for the effect of the variable $\text{BindingSalesBan}_{ict}$. $\text{BindingSalesBan}_{ict}$ is 1 if an individual i lives in a canton c with a sales ban in place and is younger than the minimum sales age at time t. The variable SalesBan_{ict} is 1 if a minimum sales age is in place in a canton.[16] It takes the value 1 irrespective of whether the individual is below or above the minimum sales age. We use several controls and fixed effects. To control for age-specific smoking in the most flexible way, we use age fixed effects, γ_a, for each discrete age. We also control for canton fixed effects, η_c, as well as year fixed effects, θ_y (denoted by Canton FE and Year FE in the tables). Accordingly, we absorb differential canton- and year-specific smoking behavior.

The term X'_{ict} captures additional control variables. For the Tobacco and Addiction Monitoring data we include a dummy for gender, foreign born, and dummies for household size. For the Health Behaviour in School-aged Children data we include gender as an individual-level covariate. The reason for the restrictive set of covariates is the limited availability of other socio-demographic information for minors. For both data sets we include physician density, youth unemployment, and general unemployment as cantonal controls. Standard errors are clustered on the level of cantons.[17]

Coefficients of interest and interpretation We focus on β_1, the coefficient capturing whether an individual is currently subject to a binding sales ban. It reflects the differential effect on individuals younger than the minimum age and is therefore a triple difference-in-differences estimate.

[16] In the specifications based on the Health Behaviour in School-aged Children, we cannot include the variable SalesBan_{ict}, as the sample only covers teens of age 11 to 15 years for which all sales bans are binding. We thus solely exploit the variation between cantons and over time for identification in this sample. The indicator variable of a ban being in place is 1 if the person was interviewed at least 7 days after the introduction of the ban in case we know the exact date of the interview. We include this one week window to allow for a depletion of stock in cigarettes. Since all bans were introduced on the first day of the respective month, it is also 1 if the individual was interviewed in the month of the introduction, in case we only know the month of the interview. For the Health Behaviour in School-aged Children data set we only have the interview year, so we define that the canton introduced the ban if the introduction year is greater than or equal to the year of the survey. The canton of St.Gallen introduced the law in October 2006, but since we do not know the month of the interview we count it as a year where it was not yet introduced. Dropping the observations from St.Gallen in 2006, however, does lead to very similar estimates from the Health Behaviour in School-aged Children data.

[17] The standard errors are of similar size in the main specifications when we use wild cluster bootstrapping to account for the small number of clusters (Cameron et al., 2008).

Table 1
Main effect of tobacco sales bans on teen smoking.

	Tobacco and addiction monitoring				Health behaviour in school-aged children		
	Ages 14–20			Ages 14–27	Ages 11–15		
Dependent variable	Smoker				Smoker	Smoked ever	Age first puff
Avg. for minors		0.178		0.295	0.104	0.287	12.503
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Binding Sales Ban	0.000	0.000	−0.000	0.003	0.008	0.012	0.001
	(0.009)	(0.009)	(0.008)	(0.010)	(0.007)	(0.014)	(0.051)
Sales Ban	−0.008	−0.010	−0.009	−0.010			
	(0.013)	(0.013)	(0.013)	(0.008)			
Experienced a Binding Sales Ban				0.004			
				(0.007)			
Age FE	X	X	X	X	X	X	X
Canton FE	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X
Ind. Controls		X	X	X	X	X	X
Cant. Controls			X	X	X	X	X
Observations	28,704	28,704	28,704	48,481	56,335	56,207	13,177
Canton Clusters	26	26	26	26	15	15	15
R-squared	0.06	0.07	0.07	0.05	0.07	0.14	0.25

Note: Estimated effects of sales bans on smoking in percentage points from linear least squares estimations. Binding Sales Ban indicates that an individual is currently subject to a sales ban. Sales Ban indicates that an individual lives in a canton with a sales ban. Experienced a Binding Sales Ban refers to individuals who, based on their canton of residence, were likely exposed to a binding ban in the past. Standard errors clustered on the level of cantons are in parentheses. Smoker refers to current regular smoking. Age First Puff indicates the reported age at which a high-school student tried smoking for the first time. It contains fewer observations since this question was posed to only a subsample of students who filled out the long questionnaire of the Health Behaviour in School-aged Children survey. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The coefficient β_2 is the second key coefficient for our identification strategy, as it captures i) potential other events correlated with the introduction of sales bans that affected smoking behavior, ii) lasting effects on individuals who were under a binding sales ban in the past, iii) potential immediate spillovers to older age groups which are not directly subject to a ban (for instance, it could change attitudes toward smoking of teens older than the minimum age).

As discussed in the section on the institutional environment, there is no reason to presume that other events took place when sales bans were introduced, which renders i) unlikely.[18] However, the sales ban coefficient could still capture the effects of having experienced a sales ban in the past or of immediate spillovers to older age groups. If so, the sales ban coefficient β_2 also contains meaningful information about the actual overall reduction in the prevalence of smoking. To gauge this overall reduction, we also discuss the estimates for linear combinations of β_1 and β_2 for some of the regressions.

For most results in the paper, however, we focus on the impact of currently being subject to a sales ban. The reasons are twofold: First, across the board we estimate no statistically significant or economically large effect β_2. Second, we do not find any evidence for a long-term effect of sales ban on peoples smoking behavior.

Sample selection We restrict the sample for our main analyses to people up to age 20.[19] There are several reasons for this age restriction: First, excluding people older than 20 years renders the control and treatment group more similar (i.e., composition effects in the population of older people and related changes in tobacco consumption play less of a role). Second, still including people older than 18 helps to increase the power for our estimates. Third, it makes the sample more consistent throughout our analyses, as the data on appeal of smoking and source of cigarettes are only available for individuals age 20 and younger and because the analysis based on the Health Behaviour in School-aged Children data only comprises children younger than 16 years.

We discuss the sample selection, identifying assumptions and corresponding tests, as well as robustness checks in more detail in Section 6.

4. The effect of tobacco sales bans on teen smoking

4.1. Overall effect on teen smoking

Table 1 presents the main regression results. In column (1) with the specification including age, canton, and year fixed effects, the coefficient for the binding sales ban is 0.0004 ($se = 0.009$). This is a less than 0.1 percentage point change in the propensity to smoke among teens exposed to a ban (the average smoking propensity being 18%). When we additionally include individual and cantonal controls in columns (2) and (3), the estimates remain very similar. All of them are not statistically different from 0. When considering the linear combination of the sales ban and the binding sales ban coefficients in

[18] We examine whether taking into account other policies affects the estimates in Section 6.

[19] However, the results remain very similar when using other age restrictions such as including people aged up to 27, using the full sample with no age restrictions, and when we restrict the sample to people being under the respective minimum age of a canton.

Table 2
Heterogeneous effects.

	Tobacco and addiction monitoring Ages 14–20						HBSC Ages 11–15	
Dependent variable	Smoker							
Sample	Male	Female	Foreign B.	Swiss B.	High Edu.	Low Edu.	Male	Female
Avg. for minors	0.184	0.171	0.168	0.182	0.175	0.192	0.109	0.098
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Binding Sales Ban	−0.004	0.004	−0.004	0.007	0.018	−0.035**	0.000	0.016
	(0.012)	(0.012)	(0.012)	(0.010)	(0.022)	(0.013)	(0.010)	(0.011)
Sales Ban	0.000	−0.017	0.014	−0.016	0.020	−0.009		
	(0.015)	(0.015)	(0.021)	(0.018)	(0.032)	(0.028)		
Age FE	X	X	X	X	X	X	X	X
Canton FE	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X
Ind. Controls	X	X	X	X	X	X	X	X
Cant. Controls	X	X	X	X	X	X	X	X
Observations	14,618	14,086	8,735	19,969	4,648	6,970	27,958	28,377
Canton Clusters	26	26	26	26	26	26	15	15
R-squared	0.07	0.07	0.07	0.07	0.09	0.09	0.07	0.08

Note: Estimated effects of sales bans on smoking in percentage points from linear least squares estimations. Binding Sales Ban indicates that an individual is currently subject to a sales ban. Sales Ban indicates that an individual lives in a canton with a sales ban. Standard errors clustered on the level of cantons in parentheses. HBSC is the abbreviation for Health Behaviour in School-aged Children. Foreign B. refers to individuals who were born outside of Switzerland and Swiss B. to individuals who were born in Switzerland. High Edu. and Low Edu. refer to the fathers' education which we only have for a subsample of the Tobacco Monitoring. We classify fathers as High Edu. if they have a high-school degree ("Matura"), higher degree after vocational training (e.g., "Meister"), a school of applied sciences degree, or a university degree. We classify fathers as Low Edu. if they have no degree, only mandatory schooling, or a vocational degree. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

column (3), we get an effect size of 0.009. While suggesting some reduction, the estimate remains statistically insignificant ($se = 0.014$).[20]

Based on data from the Health Behaviour in School-aged Children survey for 11- to 15-year-old teens, we also find no evidence for a reduction in current smoking or whether a teen ever tried smoking, nor for an increase in the age at first puff; see columns (5) to (7).

In column (4), we look explicitly at the effect of past experience to assess whether sales bans have an impact on long-term smoking behavior. It could be that the contemporaneous effect of a binding sales ban is close to 0 when studying the extensive margin, but that there is a reduction in the long-term, for example, because people change their risk perception. We examine whether past experience with a ban affects current smoking behavior by adding a dummy variable indicating that an individual older than the minimum age at the time of the survey was subject to a binding sales ban in the past. Thereby we have to assume that individuals were living in the same canton in the past. For this analysis, we expand the sample to people up to age 27, i.e., the highest age for which we have observations from individuals who experienced a sales ban at least for one year in their life.[21]

We do not find any evidence for a systematically different smoking prevalence among those who likely were restricted by a binding sales ban in the past. Alternatively dropping all those who have any previous experience with a ban leads to similar results, i.e., a point estimate for a binding sales ban of 0.006 ($se = 0.009$) and one of 0.013 ($se = 0.009$) for the sales ban in general (see column (7) of Table B.1).

4.2. Heterogeneity across subgroups

To assess the potential heterogeneity of the effect of sales bans on different subgroups, we split the sample according to gender, place of birth (born in or outside Switzerland), and education of the father. Table 2 presents the results. There is

[20] Table B.1 presents the results for further specifications excluding year fixed effects, excluding the general variable capturing sales ban, or including cantonal trends and canton x year fixed effects. Note that not controlling for year fixed effects leads to very significant coefficients for the sales ban dummy, likely capturing the general decline in smoking prevalence. Moreover, Table B.2 shows that all the effects remain small and statistically imprecisely estimated when restricting the sample to different ages younger than 20. The largest point estimate in terms of a reduction in smoking is −1.1 percentage points (not stat. sign.) when we restrict the sample to people younger than the (future) minimum sales age (or younger than 18 in the cantons that did not introduce a ban during the observation period).

[21] Note that the variation of the variable capturing the introduction of a sales ban and the one capturing past experience with it is very similar in the sample restricted to teens because only in the years briefly after the introduction of a sales ban, there are individuals who had not experienced a sales ban: In the first year after an introduction (of a sales ban for those younger than 18), 18, 19, and 20 years old individuals have not experienced a binding sales ban themselves, in the second year, only the 19 and 20 years old, and in the third year only the 20 years old. In all subsequent years of data, everybody is either currently bound by a ban or experienced it in previous years. This strong overlap in the variation of the variable indicating the presence of sales bans and the variable indicating experience with a sales ban leads to a high variance inflation factor in the sample only including individuals up to age 20. Therefore, we extend the sample for this analysis up to age 27.

Table 3
Intensity of tobacco consumption.

	Tobacco and addiction monitoring Ages 14–20			Health behaviour in school-aged children Ages 11–15	
Dependent variable	Packs smokers	Daily smoker	>=100 Cigar.	Smoke freq.	Cigarettes per week
Avg.	0.4380	0.130	0.187	1.939	0.318
	(1)	(2)	(3)	(4)	(5)
Binding Sales Ban	−0.001	−0.001	0.011	−0.057	0.008
	(0.019)	(0.011)	(0.017)	(0.060)	(0.023)
Sales Ban	−0.010	0.001	0.006		
	(0.014)	(0.017)	(0.015)		
Age FE	X	X	X	X	X
Canton FE	X	X	X	X	X
Year FE	X	X	X	X	X
Ind. Controls	X	X	X	X	X
Cant. Controls	X	X	X	X	X
Observations	4,685	18,763	9,147	5,832	35,242
Canton Clusters	26	26	26	15	15
R-squared	0.06	0.18	0.08	0.09	0.03

Note: Estimated effects of sales bans from linear least squares estimations. Binding Sales Ban indicates that an individual is currently subject to a sales ban. Sales Ban indicates that an individual lives in a canton with a sales ban. Standard errors clustered on the level of cantons in parentheses. Packs Smokers refers to the number of packs smoked by regular smokers. Daily Smoker refers to whether individuals are daily smokers as opposed to less frequent smokers (more than once a week, once a week, less than once a week, or nonsmokers). >=100 Cigar. refers to an individual having smoked more than 100 cigarettes in their life. Smoke Freq. is available only for smokers in all waves of the Health Behaviour in School-aged Children data and takes the values 1 "less than once a week", 2 "at least once a week", and 3 "daily". Cigarettes per Week is available for the years 2006, 2010, and 2014. It takes the values 0, 1 (which means less or equal to 1), 5 (<=5), 11 (<=11), 19 (<= 19), or 20 (>=20). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

no statistically discernible difference in the effect of sales bans on the prevalence of smoking across gender (in both data sets) or birthplace, see specifications (1) to (4) and (7) to (8). Only teens with a father who has a relatively low educational attainment (no degree, only mandatory schooling, or a vocational degree) seem to smoke less if they are exposed to a sales ban. The effect size amounts to a reduction in the smoking prevalence of 3.5 percentage points. This is considerable given the average smoking propensity of 19% in this group. Taken together, we observe no overall reduction in smoking prevalence. And even when exploring heterogeneity, the estimates suggest a limited reduction of smoking prevalence.

5. Behavioral mechanisms: habits, beliefs, and peers

The institutional setting and the rich data offer a unique opportunity to test potential behavioral mechanisms. While different mechanisms are often used as arguments in the policy discussion surrounding prohibiting the sale of addictive goods, empirical evidence is scarce. First, we discuss the potential relevance of habit formation by looking at different measures capturing smoking intensity and by exploring lag-specifications. Second, we discuss the forbidden fruit effect, i.e., the idea that sales bans might increase the appeal of smoking among teens. Third and closely related, we assess the relevance of norms and the peer effects by studying the consequences of the laws on attitudes of parents and friends toward smokers. Fourth, we investigate the potential information signal of the law by looking at whether it shifts beliefs about the risk of smoking. Fifth, we analyze the impact on procurement strategies by studying whether teens circumvent the restrictions by getting cigarettes from other sources. Lastly, we assess the relevance of law enforcement by incorporating additional data on test purchases and by looking at the relevance of the minimum sales age either being 16 or 18.

5.1. Habit formation: smoking intensity and lagged consumption

Habit formation is often discussed in the context of addictive substances, where a consumption stock builds up based on the level of consumption over time. In turn, consumption reacts only slowly to a policy change that increases the cost of consumption. The notion of a slow adaption of consumption of addictive substances is in line with the finding that for cigarette smoking, the price elasticity is bigger in the long- compared to the short-term (e.g., DeCicca et al., 2020). We explore the relevance of habit formation in the context of sales bans first by assessing their effect on various indicators of smoking intensity, and second by considering lags of sales bans.

Table 3 presents the results for smoking intensity and thus complements the findings for the prevalence of smoking, i.e., the extensive margin, with results for the intensive margin. However, we do not see sizable and statistically significant effects on the five alternative outcomes, such as the amount of cigarettes smoked or the frequency to smoke.

We test whether a binding sales ban affects the formation of the consumption stock stock with specifications including up to three lags. These empirical models with lags also address the issue that sales bans might have needed time to become fully effective. The results in Table B.3, however, do not offer any evidence that a reduction in consumption occurred later on. This is also in line with the finding discussed above in Section 4 that there is no significant effect in early adulthood when having experienced a binding sales ban as a teenager.

Table 4
Effects on appeal of smoking.

	Tobacco and addiction monitoring Ages 14–20							
Sample	All						Smokers	
	Smokers are more:							
Dependent variable	Cool	Attractive	Appreciative	Happy	Successful	Avg.	Succesful	Avg.
Avg.	0.307	0.230	0.320	0.263	0.312	0.288	0.368	0.367
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Binding Sales Ban	−0.026*	0.008	−0.041**	−0.020	−0.017	−0.021	−0.077*	−0.031*
	(0.013)	(0.018)	(0.018)	(0.021)	(0.021)	(0.013)	(0.037)	(0.017)
Sales Ban	−0.012	−0.005	−0.018	0.011	−0.005	−0.003	0.050	0.020
	(0.018)	(0.014)	(0.023)	(0.017)	(0.015)	(0.008)	(0.045)	(0.023)
Age FE	X	X	X	X	X	X	X	X
Canton FE	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X
Ind. Controls	X	X	X	X	X	X	X	X
Cant. Controls	X	X	X	X	X	X	X	X
Observations	6,010	5,983	5,988	5,950	5,939	5,662	1,532	1,446
Canton Clusters	26	26	26	26	26	26	26	26
R-squared	0.03	0.07	0.08	0.02	0.07	0.06	0.06	0.05

Note: Estimated effects of sales bans on attitudes toward smoking from linear least squares estimations. Binding Sales Ban indicates that an individual is currently subject to a sales ban. Sales Ban indicates that an individual lives in a canton with a sales ban. Standard errors clustered on the level of cantons in parentheses. Cool, attractive, appreciative, happy, successful are variables on a scale from 0 to 1, where 0 is nonsmokers are, e.g., cooler than smokers, 0.5 means equally cool, 1 refers to smokers are cooler. Avg. refers to the average from the dependent variables in columns (1) through (5). Data on these attitudes is available for the years from 2001 to 2010 only from a subsample of adolescents surveyed in the Tobacco Monitoring. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.2. Forbidden fruit: appeal of smoking

The forbidden fruit effect suggests that sales bans increase the appeal of smoking among teens, potentially leading to an increase in smoking prevalence (MacCoun, 1993; Landman et al., 2002; García-Jimeno, 2016). If smoking gets more appealing because of the introductions of sales bans, it would provide an explanation for why we do not find large negative effects of sales bans on smoking behavior.

We examine whether smoking gets more appealing with data on attitudes toward smokers from teens aged 14 to 20. The youth questionnaire of the Tobacco Monitoring survey for the years 2001 to 2010 contains questions about whether the respondents think smokers are relatively cooler, more attractive, more appreciative, happier, or more successful than nonsmokers. For instance, we code the relative coolness of smokers as 1 if the respondents think smokers are cooler, 0.5 if they think smokers are as cool as nonsmokers, and 0 if they think nonsmokers are cooler than smokers.

The attitudes toward smokers are strongly correlated with smoking behavior; see Table B.4, columns (1) to (6). Finding smokers appealing relates to a higher propensity for being a smoker. But do these attitudes change as a consequence of a sales ban? The results shown in Table 4 indicate, if anything, a reduction in the appeal of smoking when respondents are subject to a sales ban. After the introduction of a law, teens think smokers are less cool and appreciative; see columns (1) and (3). The point estimates in column (8) show a statistically significant reduction of the relative appeal of smokers, even among smokers themselves. Smokers think that they are less successful when a ban is in place, which is what drives the decline in relative appeal of smokers among smokers; see column (7).

In addition, we explore whether there are heterogeneous effects on attitudes based on socio-demographics in Table B.5. The strongest reduction in the appeal of smoking occurs among respondents who are foreign born and those with a father with relatively less education. However, note that we only have a rather limited sample to run heterogeneity analyses for mechanisms and therefore abstain from drawing firm conclusions from these explorations.

In sum, our analysis shows, if anything, a reduction in the appeal of smoking because of sales bans.[22] This is the opposite of what the forbidden fruit effect suggests.

5.3. Social norms: perceived attitudes of parents and friends

In addition, we study smokers' and nonsmokers' perceived attitudes of peers and parents toward smoking; see Table 5. Data on these attitudes is available for the years from 2001 to 2010 from a subsample of adolescents surveyed in the Tobacco Monitoring. In column (1) the dependent variable is 1 if respondents think friends "would find it rather bad if I stopped

[22] Consistent with these findings, descriptive evidence from the Health Behaviour in School-aged Children survey suggests that most high-school students who try smoking do it out of curiosity (79%) or because it relaxes them (51%) rather than because they feel cooler doing so (12%). This might also explain why the reduction in the appeal of smoking does not translate into a decline in smoking.

Table 5
Effects on perceived danger and perceived attitudes of parents and friends.

	Tobacco and addiction monitoring Ages 14–20				
Sample	Smokers		Nonsmokers		All
Dependent variable	Peers for stopping	Parents ok w. smoking	Peers for starting	Parents ok w. smoking	Smoking is dangerous
Avg.	2.595	0.502	1.397	0.244	3.719
	(1)	(2)	(3)	(4)	(5)
Binding Sales Ban	0.121*	0.051	−0.006	−0.004	−0.005
	(0.063)	(0.033)	(0.028)	(0.023)	(0.054)
Sales Ban	−0.032	−0.002	−0.042	−0.013	0.042
	(0.047)	(0.039)	(0.030)	(0.027)	(0.056)
Age FE	X	X	X	X	X
Canton FE	X	X	X	X	X
Year FE	X	X	X	X	X
Ind. Controls	X	X	X	X	X
Cant. Controls	X	X	X	X	X
Observations	1,572	1,552	4,466	4,378	7,036
Canton Clusters	26	26	26	26	26
R-squared	0.09	0.12	0.04	0.09	0.06

Note: Estimated effects of sales bans on attitudes toward smoking from linear least squares estimations. Binding Sales Ban indicates that an individual is currently subject to a sales ban. Sales Ban indicates that an individual lives in a canton with a sales ban. Peers for Stopping and Peers for Starting reflect answers to the questions whether respondents think friends "would find it rather bad if I stopped/started smoking," 2 if their friends "wouldn't care," and 3 if they "would like if I stopped/started smoking." Parents Ok w. Smoking reflects answers to the questions whether parents are (or would be) okay if their children were smoking at home, where the answer "yes" is coded as 1, "no" as 0, and "it depends" as 0.5. "Dangerous to Smoke" refers to respondents perceived danger of smoking on a scale from 1 to 6, where 6 is very dangerous and 1 is not at all dangerous. Standard errors clustered on the level of cantons in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

smoking," 2 if their friends "wouldn't care," and 3 if they "would like if I stopped smoking." In line with the findings on the other attitude measures, a sales ban leads smokers to think more often that their peers would like them to stop smoking. In column (3) we exploit for the sample of nonsmokers the variable Peers for Starting, ranging from 1 to 3, where 3 indicates that friends would find it rather good if a nonsmoker began smoking. We do not find a systematic impact of the perception of nonsmokers about whether their peers want them to start smoking.

We also explore smokers' and nonsmokers' perceptions of their parents' views, shown in columns (2) and (4). The dependent variable captures whether respondents agree with the statement that parents are (or would be) okay if their children were smoking at home, where the answer "yes" is coded as 1, "no" as 0, and "it depends" as 0.5. We do not find a statistically significant impact of the sales bans on these attitudes.

5.4. Risk beliefs: perceived danger

We also assess the potential information signal of the law by examining whether smoking is perceived more dangerous by teens after the introduction of the sales ban. We use a question on perceived danger of smoking, available from 2001 to 2010 and in 2012 from the Tobacco and Addiction Monitoring survey. The variable "Dangerous to Smoke" in Table 5 and B.4 refers to a scale from 1 to 6, where 6 is very dangerous and 1 is not at all dangerous.

The perceived danger strongly correlates with smoking behavior; see Table B.4. Higher perceived danger relates to a lower propensity for being a smoker (which is consistent with Lundborg and Lindgren, 2004 and Lundborg, 2007). However, the results shown in column (5) of Table 5 do not indicate a statistically significant effect of sales bans on the perceived danger of smoking. When exploring heterogeneous effects on perceived danger in Table B.5, we document a reduction of perceived danger among respondents with relatively highly educated fathers.

5.5. Procurement strategies: circumvention

Circumvention through peers In this subsection, we present our findings on whether teens circumvent the restrictions by getting cigarettes from sources other than stores when a sales ban is in place. For instance, minors may get cigarettes from their friends, siblings, or even from their parents (Hansen et al., 2013). Figures B.1 and B.2 show the different sources from which teens get cigarettes. Every fourth minor who smokes does not buy the cigarettes herself but gets them from friends, siblings, or parents.[23]

Table 6, column (1) shows the impact of a sales ban on the likelihood of teens purchasing cigarettes for themselves. The dependent variable takes a value of 1 if the smokers purchase the cigarettes themselves, 0.5 if they sometimes do, and

[23] None of the teens answering this question stated that they got the cigarettes on the black market. We have additional data on the sources of the last pack from 2007 to 2011 according to which only 3 of 527 minors said that they bought the cigarettes abroad or on the black market, i.e., in an unofficial store.

Table 6
Circumvention through peers and smoking behavior.

	Tobacco and addiction monitoring, Ages 14–20				
Dependent variable	Own purchase (smokers)	Smoker Sample			
	Yes = 1	All	All with info. on peers	Peers are: smokers	Peers are: nonsmokers
Avg. for minors:	0.756	0.257	0.190	0.371	0.071
	(1)	(2)	(3)	(4)	(5)
Binding Sales Ban	−0.088**	−0.003	−0.014	0.010	−0.069**
	(0.043)	(0.008)	(0.025)	(0.031)	(0.026)
Sales Ban	0.007	−0.009	−0.006	0.014	0.007
	(0.027)	(0.019)	(0.027)	(0.036)	(0.023)
Age FE	X	X	X	X	X
Canton FE	X	X	X	X	X
Year FE	X	X	X	X	X
Ind. Controls	X	X	X	X	X
Cant. Controls	X	X	X	X	X
Observations	2,245	15,735	5,481	2,507	2,974
Canton Clusters	26	26	26	26	26
R-squared	0.08	0.09	0.10	0.08	0.07

Note: Estimated effects of sales bans on purchasing behavior and smoking from linear least squares estimations. Binding Sales Ban indicates that an individual is currently subject to a sales ban. Sales Ban indicates that an individual lives in a canton with a sales ban. Standard errors clustered on the level of cantons are in parentheses. Own Purchase refers to a variable that captures whether smokers bought cigarettes themselves, e.g., in a store, or whether they got them from somewhere else, e.g., through their parents. Own Purchase takes a value of 1 if the smokers purchase the cigarettes themselves, 0.5 if they sometimes do, and 0 if they never do. These data are available only for smokers and the years 2001, 2002, 2004, 2005, 2007–2012, 2014, and 2016 from a subsample of adolescents surveyed in the Tobacco and Addiction Monitoring survey. Columns (2) to (5) only contain data from the Tobacco Monitoring survey for the years 2001–2010. All denotes using all data from these years. All with Info. on Peers refers to the sample where we know whether the individuals have peers who smoke. Peers Are: Smokers is 1 if at least one sibling or at least half of the respondent's friends smoke. Peers Are: Nonsmokers refers to all other observations. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

0 if they never do. We find a statistically significant reduction in the likelihood of a teen stating she purchases cigarettes exclusively for herself of 9 percentage points ($se = 4.3$). Teens switch from purchasing cigarettes themselves to getting them through parents or friends.[24]

It seems reasonable that circumventing sales bans by getting cigarettes from sources other than stores is easier for teens with peers who smoke. Accordingly, we also expect a differential effectiveness of sales bans for teens with peers (siblings or friends) who smoke. First, we show the aggregate effect of a sales ban for the sample in this age range in column (2) and for those for whom we have information on peers' smoking behavior in column (3). We then estimate the effect of sales bans separately for teens whose peers do or do not smoke, shown in columns (4) and (5). In line with the argument that teens with peers who smoke can circumvent the bans more easily, we find no statistically significant reduction in smoking prevalence for teens with peers who smoke. However, we do see statistically significant point estimates for teens with peers who do not smoke. Among teens with peers who do not smoke, the introduction of a binding sales ban reduces the likelihood of smoking by more than 6 percentage points. Note, however, that this specification relies on a relatively small subsample.[25]

Differential changes in attitudes toward smoking do not drive the difference in effects across the two groups. If anything, we see a larger reduction in positive attitudes toward smoking among teens whose peers smoke (regressions not shown). The index of positive attitudes with an average of 0.326 among individuals with peers who smoke decreases by 0.039 ($se = 0.018$), whereas it does not statistically significantly change for teens with peers who do not smoke (0.007, $se = 0.015$, $avg. = 0.254$). When looking at the heterogeneity of the effect on purchasing behavior in Table B.5, we find that foreign born teens show the strongest reduction in their demand of buying cigarettes in person.

Circumvention through traveling Another possibility to circumvent the sales bans is to travel to cantons without a sales ban. This is possible because of the staggered introduction of the sales bans — some cantons have neighboring cantons that do not have a sales ban in place. Furthermore, teens could also engage in cross-country shopping.

To examine the possibility of cross-canton circumvention, we generate a variable capturing the distance between the municipality the teen lives in and the closest municipality that is in a canton without a binding sales ban in place.[26] We

[24] Unfortunately, we do not have information on peers' age to assess if they are above the minimum sales age. It seems plausible though that there is variation in the age of peers with some peers being above the minimum sales age.

[25] Splitting the sample according to whether peers smoke or not does potentially introduce a selection bias, as the policy may alter peer smoking, leading to an increase in the number of smokers who have non-smoking peers. Accordingly, the finding that the effect on smoking prevalence is higher among smokers with non-smoking peers could not only be because of the missing access to cigarettes from friends, but also due to the well-established direct effect via reduced smoking behavior among peers. However, as we do not find economically and statistically significant effect of the policy on smoking behavior, we expect the selection bias to be marginal.

[26] To calculate the distance we use municipality centroids based on the inhabited area from the Swiss Federal Statistical Office. We then identify the 10 closest municipalities in another canton. Of those, we calculate the distance to the closest community with a nonbinding minimum sales age for teens

then include an interaction term between the distance to a municipality and sales ban for minors subject to a ban. The corresponding regressions can be found in Table B.6, columns (1) and (2). We find that minors living far from the next municipality where they could buy cigarettes are not differentially affected by the bans.

We then check potential cross-country shopping, shown in column (3). We drop all cantons that had a neighboring country with a lower minimum age at any point, which does not yield larger negative effects of sales bans.

Circumvention through vending machines Another option to circumvent the sales ban is by getting cigarettes from public vending machines, as 16 cantons granted a grace period to upgrade vending machines with ID readers. The grace period varies from 9 months to 3 years, the median grace period is 1 year. In a further regression, shown in column (4) of Table B.6, we include an interaction with whether the grace period for vending machines had ended. That is, the indicator variable takes a value of 1 starting on the date when the vending machines needed to be equipped with an ID reader. The results show that the lagged updating of vending machines did not attenuate the effect of sales bans.

5.6. Enforcement of the law: test purchases and minimum age

Test purchases In order to study the prevalence of violations of the law, we collected data about the results of test purchases. A test purchase wave is when the canton sends teens below the minimum age to buy cigarettes at different shops.[27] We complement the information from publicly available data on test purchases with a survey of health officials in the cantons on test purchases. Our final data contain the year of the test purchases, the number of stores tested, and the share of sales made to teens below the minimum age. In total, we have the information from 53 waves of test purchases from 14 cantons with close to 7,000 tested stores over all years.[28]

Based on the data on test purchases, we assess the extent to which cigarettes sales violate the sales bans across Switzerland.[29] The median share of sales to minors across cantons and years was 33%, which is a substantial cut compared to full access. In Figure B.3 we show the share of forbidden sales over time for cantons for which we have data on test purchases. While access was cut drastically after the introduction of the bans, the graph shows only a slightly decreasing trend of sales personnel not adhering to the law.

The heterogeneity underlying this trend is substantial, with some cantons having as little as 25% of sales in stores violating the law, while in other cantons the share is 60%. In Table B.7 columns (1) to (3), we show the results when we take the information on test purchases into account. We do not find differential effects of the sales ban depending on whether the canton conducts test purchases or on whether compliance with the law is high or low.

Heterogeneity in the minimum sales age We explore potential differences in the impact of a sales ban depending on whether teens up to the age of 16 or 18 are subject to them. Note that only five cantons introduced a law with minimum age of 18. Accordingly, it is statistically difficult to tease apart potentially heterogeneous policy effects and thus any findings have to be interpreted with caution.[30] Table B.8 shows the results. They suggest that cantons that introduced a minimum sales age of 18 were more successful in reducing smoking than cantons with a minimum sales age of 16. Depending on the specification, the former cantons experience a reduction of between 1.0 ($se = 0.8$) and 2.2 percentage points ($se = 0.9$). This is consistent with additional results showing that the reduction in cigarettes bought in stores is driven by the sales bans with a minimum age of 18. When we examine longer term effects of the different minimum ages, however, we do not see an indication of reductions for either regime.

6. Discussion of identifying assumptions and robustness

There are different challenges to our identification when the described variation (see Section 3 on the empirical strategy for details) is used in a difference-in-differences model to estimate the effects of the sales bans. In our context, these challenges refer to i) potentially differential trends in the prevalence of smoking between treated and untreated cantons because of differences in the composition of people across cantons, ii) potential policy changes or interventions that occurred concurrently with the introduction of sales bans and affected peoples smoking behavior, and iii) potential long-term effects from having experienced a sales ban in the past or immediate spillover effects to groups that are not restricted by the sales bans.

In this paragraph, we briefly mention some of the different strategies we pursued throughout the previous analyses to address some of the challenges, before presenting additional analyses that further establish the robustness of our results

subject to a ban in their canton. As we have zip-code-level data available only for 2001 to 2014, we can use data only from this period. We also lose some observations because of an imperfect match between community numbers used by the Swiss Federal Statistical Office and zip codes.

[27] There is not a common practice for punishing law violations. Generally, the shop owner sells cigarettes illegally for the first time, the common practice is for the canton to remind the shop owner of the law and to threaten legal punishment (fines) in the case of a repeated offense. They usually also invite the shop owners to send their personnel to courses where they are taught how to comply with the law.

[28] We have the information on the number of tested stores for 50 of the 53 test purchase waves. The total number of stores tested in these 50 waves is 6605 with an average of 132 shops tested in each wave. Smaller cantons tend to test fewer shops.

[29] Abouk and Adams (2017b) find that an increase in random test purchases reduces smoking incidence if a minimum sales ban is in place. In our case, cantons choose the timing and intensity of test purchases.

[30] Diagnostic tests of the variation inflation factor indeed indicate that there is very little variation conditional on fixed effects (as the rule of thumb value of 10 is surpassed).

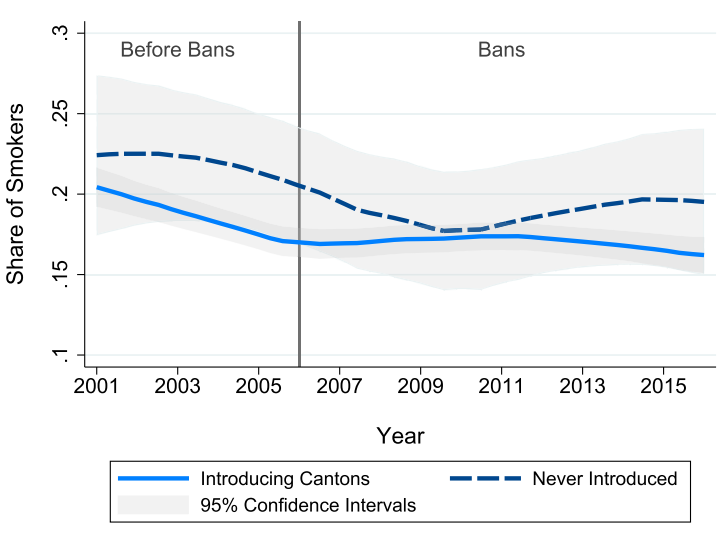

Fig. 3. Difference in trends of smoking prevalence among minors in introducing and control cantons. *Note:* The figure indicates the trends in smoking prevalence among teens aged 14 to 17 years for cantons that introduced a sales ban (solid line) and cantons that did not (dashed line). The vertical line marks 2006, the first year cantons started to introduce sales bans. The estimates stem from local polynomial regressions with bandwidth 2. The shading shows the corresponding 95% confidence intervals. *Data source:* Tobacco and Addiction Monitoring, 2001–2016.

in the following two sub-sections. First, we restrict the sample for our main analyses to people up to age 20, therefore keeping a homogeneous sample which makes people in the treatment and control group more comparable compared to when including all age groups. However, in the reduced sample, those teens between the age of 16 (or 18) and 20 in the comparison group are then likely to have been exposed to a sales ban in the past. We therefore, second, consider individuals past experience with a binding sales ban either with a separate control variable or in the interpretation of the variable indicating the presence of a sales ban in a canton (see particularly Table 1 and Section 3).

In the following, we further discuss the challenge of potentially diverging pre-trends, conduct balance checks regarding the composition of observations in the treatment and control group, consider other potentially confounding policies, and assess the sensitivity of our main specification to the exclusion of single cantons.

6.1. Common trends

When assessing the validity of the common-trends assumption, we concentrate on smoking prevalence, since it is the main variable of interest. Fig. 3 shows the trends for cantons that did and did not introduce sales bans. Before the first bans were introduced in 2006, the trends look similar across the two groups. Consistent with our main findings, we do not see a reduction in the prevalence of smoking in cantons that introduced the laws when compared to cantons that did not.

We next examine whether the pre-trends differ statistically significantly and provide an array of tests. In Table C.1, columns (1) to (4), we document that when restricting the sample to the years before the first introductions in 2006, cantons with bans, or with bans introduced early or late, did not have statistically significantly different trends in smoking prevalence when compared to cantons that did not introduce the laws. This holds true for smoking prevalence among minors (aged 14 to 17) as well as for non-minors younger than 21. The cantons with a minimum age of 18 also had similar trends before the first introductions in 2006 compared to cantons with a lower or no minimum age, as shown in columns (5) and (6).

Further assessing whether differential trends drive our results, we show that the estimated impact of sales bans does not change much when we add separate canton-specific linear time trends in column (1) of Table C.2.

Since we exploit variation in the timing of the ban introduction, we check whether there are lead effects of the bans; see Table C.2. If there are lead effects, this might indicate that cantons introduced the laws as a reaction to changes in smoking prevalence. Column (2) shows that we do not see a statistically distinguishable smoking prevalence a year before the introduction of bans. To avoid the possibility that extrapolation from pre-trends drives the results, we use only the years from 2003 onward, shown in column (3), which does not affect the coefficient estimates. In column (4), we apply a detrending approach recently suggested by Goodman-Bacon (2018) to account for possibly differing magnitudes in trends. Thereby, untreated observations are used to estimate linear pre-intervention trends for all states involving all control variables that are then deducted from the dependent variable. Considering the magnitude of pre-trends across cantons, the estimated coefficient for the variable binding sales ban of 0.003 ($se = 0.013$) does not change the above result of no sizable overall effect of the sales bans. In sum, we cannot reject the assumption of common trends.

6.2. Balance and robustness checks

Balance checks The estimation results for the policy variables would be biased if the introduction of the sales bans were to coincide with systematic changes in the composition in our sample. This might be the case if the introduction of the sales bans is endogenous with respect to certain covariates or if people move because of sales bans. In order to assess this possibility, we do different balance checks. The results in Table C.3 indicate that across the four specifications there is no statistically significant relationships of the sales bans and covariates. For instance, we do not see that the share of minors observed in a canton and year differs conditional on a sales ban. Moreover, we assess whether based on the same covariates it is possible to predict the timing of the introduction, which would suggest endogeneity of the policy introduction. In Table C.4 we estimate the relationship between the canton/year-level of covariates and the year of the introduction, only using cantons that introduced a sales ban.[31] We do not find that the variation in the covariates predicts the timing of the introduction.

Homogenous observation windows To render the treatment group more homogenous and to give more equal weight to early and late introducers, we set the observation periods for all cantons symmetric around the introduction date. The specifications in Table C.5 applying 2 to 7 year symmetric windows lead to similar inferences.

Advertisement bans and indoor air laws We further check the sensitivity of the estimation results to the consideration of other policy changes, i.e., indoor air laws and advertisement bans. Thereby, we also include interaction terms in the empirical model between an indicator for the treatment group and these other policies. Thus, we allow the treatment group to be differently affected by the advertisement bans and the indoor air laws. Table C.6 shows the results indicating that the main finding is robust to this extension. In other words, the estimates of the sales bans remain small, when we condition on other policies.

Sample of cantons In Figure C.1, we show the results of a sensitivity test when dropping separately one canton at a time from the sample. The 95% confidence intervals of the coefficient for the variable binding sales ban include both 0 and the point estimate from the main specification (3) in Table 1 in all the cases. The result is the same when we drop all observations of a specific canton×year pair.[32] The latter test is akin to the test devised by Goodman-Bacon (2018) adapted to our specification. The result of this robustness test implies that none of the canton×year observations have an overly large (negative) weight so that they would drive the estimates. In sum, the two tests indicate a limited influence of specific years or cantons on the estimates.

7. Conclusion

Prohibiting the sale of tobacco to teens below a certain age is one of the most widely implemented policies to reduce smoking. However, the evidence on whether the policy discourages teen smoking is scarce. In addition, little is known about factors which may enhance or reduce the impact of tobacco sales bans, such as the expressive function of the law, circumvention, enforcement, and the potential increase in the appeal of smoking. These aspects lie at the core of the general discussion about arguments for and against prohibition.

We contribute to the literature on prohibition by providing evidence on the impact of sales bans on smoking behavior, attitudes toward smoking, perceived danger, and purchasing behavior. We analyze two data sets for Switzerland and exploit unique variation in the introduction of sales bans.

The prohibition of tobacco sales to people younger than 16 or 18 years does not statistically significantly reduce the prevalence of smoking among teens in Switzerland. The estimated overall effects of sales bans on teen smoking scatter around 0 when we use alternative specifications, alternative surveys, alternative dependent variables for the extensive and the intensive margin, and across various robustness checks. The set of results sustain the argument of a statistically and economically small aggregate effect. The failure to reject the null hypothesis of no aggregate effect of the sales bans in this setting on smoking is not because of imprecise estimates, but because of the small magnitude of the coefficient estimates.

We study different factors that could drive the small aggregate effect, such as a forbidden fruit effect or circumvention of the bans. Our estimates suggest that teens substitute getting cigarettes from stores with getting cigarettes from peers and parents. Consistent with this, we document a reduction in smoking prevalence among those teens who have peers who mostly do not smoke. At the same time, the forbidden fruit effect does not drive the small aggregate effect of sales bans on smoking prevalence. The appeal of smoking, if anything, decreases because of the sales bans while the perceived danger of smoking does not change.

Our results have several implications. First, in comparison to alcohol sales restrictions that seem to reduce teen drinking (see, e.g., Carpenter et al., 2007; Carpenter et al., 2016), tobacco sales bans turn out to have little effect, if any. Future research should thus investigate the possible reasons for the discrepancy. A crucial aspect might be differences in the minimum legal purchase age. Research on alcohol sales restrictions is often about the US with a minimum sales age of 21. With this age restriction, many teens might not have direct access to a peer who can legally buy alcohol. Second, the limited effectiveness is not because of failed enforcement. The observed less than perfect enforcement is part of any policy in a

[31] Assuming an introduction date in the last year of our sample period for cantons which did not introduce a sales ban during the sample period yields equivalent results.

[32] The results remain quantitatively very similar when we jointly drop the three cantons that have never introduced a sales ban.

non-police state. In fact, we have to expect that the very limited effect in Switzerland would also be observed in other regions as smoking rates across Switzerland are high and law enforcement is comparatively strong. Third, as a complement to previous literature arguing peers affect smoking through social pressure (Powell et al., 2005; Clark and Etilé, 2006; Lucks et al., 2017), our results indicate that peers play a crucial role in teens' access to addictive goods (see also Hansen et al., 2013). Fourth, given the limited effectiveness of bans and taxes, considering and testing behavioral interventions such as pictorial warnings on cigarette packs (Noar et al., 2016) may be fruitful.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2021.06.002

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