Journal of Health Economics 44 (2015) 300–308

Contents lists available at ScienceDirect

Journal of Health Economics

journal homepage: www.elsevier.com/locate/econbase

How does electronic cigarette access affect adolescent smoking?

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ARTICLE INFO

Article history:
Received 10 September 2014
Received in revised form 8 October 2015
Accepted 11 October 2015
Available online 19 October 2015

JEL codes:
I12
I18

Keywords:
Smoking
Electronic cigarettes
Cigarettes
Adolescent behavior

ABSTRACT

Understanding electronic cigarettes' effect on tobacco smoking is a central economic and policy issue. This paper examines the causal impact of e-cigarette access on conventional cigarette use by adolescents. Regression analyses consider how state bans on e-cigarette sales to minors influence smoking rates among 12 to 17 year olds. Such bans yield a statistically significant 0.9 percentage point increase in recent smoking in this age group, relative to states without such bans. Results are robust to multiple specifications as well as several falsification and placebo checks. This effect is both consistent with e-cigarette access reducing smoking among minors, and large: banning electronic cigarette sales to minors counteracts 70 percent of the downward pre-trend in teen cigarette smoking for a given two-year period.

1. Introduction

Appropriate electronic cigarette regulation has become one of the central debates in public health policy, with particular interest in how this product affects conventional cigarette use (i.e., smoking)[1]. Since e-cigarettes deliver nicotine, the same addictive substance as cigarettes, but can be less expensive and are thought to be less risky, some claim that they reduce smoking by leading smokers and would-be smokers to substitute away from cigarettes (harm reduction) (e.g., Cahn and Siegel, 2011; Polosa et al., 2013)[2]. Others maintain that e-cigarettes increase smoking by inducing initiation among users who would not otherwise smoke (gateway effects), reducing stigma around smoking (renormalization), and/or lowering the full costs of addiction (e.g., by facilitating nicotine use where smoking is prohibited) (e.g., Fairchild et al., 2014; Gostin and Glasner, 2014; Time for e-cigarette and regulation, 2013). As teenagers are responsible for the majority of U.S. smoking initiation, such effects may be particularly evident in this age group. Thus, this paper tests for a causal impact of e-cigarette access on adolescent smoking.

Several studies have examined the teen vaping–smoking relationship, yet potential confounders limit causal interpretation. For example, Dutra and Glantz (2014) find that e-cigarette and cigarette use are positively correlated, which some interpret as evidence of gateway effects (e.g., Chen, 2014; Fernandez, 2014). Yet this could be explained by individuals who are more attracted to experimentation *ex ante* being more likely to try both products, regardless of any causal effect of one product on demand for the other.

Moreover, the vaping–smoking relationship may vary between population groups. For example, e-cigarette use is associated with a greater intention to quit smoking among smokers in high school (Lee et al., 2013; Dutra and Glantz, 2014) but not college (Sutfin et al., 2013). Thus, average population estimates may mask group-specific effects[3].

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[1] Inhaling on an e-cigarette releases vapor and is thus called "vaping," not "smoking." Throughout this paper, the term "cigarettes" used on its own refers to conventional cigarettes, while "e-cigarettes" signifies electronic cigarettes.

[2] An August 2009 post on blu e-cigarettes describes the starter kit's 25 cartridges as equivalent to 350 cigarettes, and prices the entire kit (including these cartridges along with chargers, batteries, and an atomizer) at $59.99 (Blu Electronic Cigarette Products, 2009). At the average 2009 price of $5.68 per pack, 350 cigarettes would cost $99.40 (Orzechowski and Walker, 2012). In a few low tax states, however, the price differential does not necessarily favor e-cigarettes.

[3] Despite evidence suggesting that e-cigarettes may serve as an effective cessation tool among adult smokers who use them specifically for that purpose (e.g., Brown et al., 2014), adult smokers' e-cigarette use does not appear to be associated with smoking cessation at a population level (Grana et al., 2014b; Adkison et al., 2013). Yet results for adults may not generalize to teenagers, particularly since shifts in teen use may operate primarily through initiation, while those for adults relate more to cessation. Thus, further discussion of adult e-cigarette use is omitted.

http://dx.doi.org/10.1016/j.jhealeco.2015.10.003
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Focusing on minors, this analysis exploits state policy changes to test the causal impact of reduced e-cigarette access on teen smoking rates. Specifically, prior to January 1, 2014, 24 states banned e-cigarette sales to minors. Regressions use state-level data, specifically two-year average smoking rates from the National Survey on Drug Use and Health, to consider the impact of these bans on the recent smoking rate among 12 to 17 year olds, controlling for state and period fixed effects as well as state cigarette taxes, the presence of smoke-free air laws, medical marijuana legalization, a variety of demographic characteristics, and smoking rates among 18 to 25 year olds. Bans on e-cigarette sales to minors yield a statistically significant 0.9 percentage point increase in the recent smoking rate among 12 to 17 year olds, relative to states without such bans. This effect is both consistent with e-cigarettes reducing smoking among minors, and large: on average, state smoking rates for this age group fell 1.3 percentage points per two-year interval from 2002 to 2009, the year before the first bans went into effect. A 0.9 percentage point increase in smoking counters 70 percent of that downward trend for a given two-year period.

As regular smoking first spikes at age 16 (Lillard et al., 2013), these findings suggest that banning e-cigarette sales to those under age 16 may be preferable to an under-18 ban, in terms of the effect on teen smoking[4]. This policy implication does not account for the bans' affect on e-cigarette use per se and associated costs, as state-level data on e-cigarette use are not available for the period of analysis.

This paper offers several contributions to the e-cigarette literature. First, the empirical findings provide the first causal evidence that e-cigarette access reduces teen smoking. In existing research, which tends to identify participation in one behavior directly off of engagement in the other, unobserved factors shaping both smoking and e-cigarette use have hampered causal inference. This paper sidesteps that problem by identifying changes in smoking and e-cigarette use off of exogenous changes in state policy. Results are robust to multiple specifications as well as falsification and placebo tests. Furthermore, the increase in teen smoking in response to such bans is likely unexpected: e-cigarette policy debates to date have not discussed such consequences.

The paper proceeds as follows: Section 2 presents a conceptual framework for the relationship between e-cigarette and cigarette use, while Section 3 tests how state bans on e-cigarette sales to minors impact smoking among 12 to 17 year olds. Section 4 discusses the empirical findings and concludes.

2. Conceptual framework

Let consumers choose consumption of cigarettes (C), e-cigarettes (E), and a composite good (X) to maximize the following:

$$W_t = U(X_t, E_t, C_t; S_t) + \sum_s \delta^{s\bullet} \mu_{t+s}(E_{t+s-1}, C_{t+s-1}, \mu_{t+s-1})^{\bullet} U(X_{t+s}, E_{t+s}, C_{t+s}) \quad (1)$$

This utility function applies the economic definition of addiction – a greater addictive stock of nicotine (S_t) raises one's current period marginal utility for nicotine consumption ($\partial^2 U_t/\partial S_t \partial N_t > 0$) – but, because it focuses on youths, assumes that consumers do not anticipate the impact of current consumption of addictive goods on their future marginal utility from consumption (i.e., no adjacent complementarity). δ is a typical discount factor, while μ_{t+s} captures one's likelihood of being alive at period $t+s$ as a function of past e-cigarette and cigarette use. Utility is maximized subject to a standard budget constraint with exogenous income, the price of X normalized to 1, and prices for cigarettes and e-cigarettes denoted P_C and P_E: $Y = X + P_E{}^{\bullet}E + P_C{}^{\bullet}C$[5].

First order conditions yield the following equation:

$$\frac{\left[\partial U_t/\partial C_t + \sum_s \delta^s U_{t+s}{}^{\bullet}\,\partial\mu_{t+s}/\partial C_t\right]}{P_C} = \frac{\left[\partial U_t/\partial E_t + \sum_s \delta^s U_{t+s}{}^{\bullet}\,\partial\mu_{t+s}/\partial E_t\right]}{P_E} = \frac{\partial U_t}{\partial X} \quad (2)$$

Thus, consumption of conventional and electronic cigarettes is guided by individual discount rates, perceived health effects, and prices, alongside the current period marginal utility of consumption. Current evidence indicates that e-cigarettes have some health costs but are less dangerous than conventional cigarettes, so the future-utility terms above will be negative for a fully informed consumer (Pisinger and Døssing, 2014). Thus, those with higher discount factors will be less likely to purchase either good and, all else equal, more unlikely to use cigarettes than e-cigarettes.

Neither representative data on e-cigarette prices nor a conversion factor allowing the prices of cigarettes and e-cigarettes to be compared in terms of a common unit (e.g., cost per inhalation) are available for the period in question[6]. Comparing the 2009 price of a blu e-cigarette starter kit (advertised as equivalent to 350 cigarettes) with the average 2009 price for the equivalent number of conventional cigarettes yields costs of $59.99 and $99.40, respectively (Blu Electronic Cigarette Products, 2009; Orzechowski and Walker, 2012). Thus, e-cigarettes cost less than cigarettes per use in all but the lowest cigarette tax states. If making e-cigarettes more accessible is analogous to decreasing the price of e-cigarettes from infinity (at their introduction) to the observed prices, the substitution and income effects should drive cigarette consumption in opposite directions as this price falls, leaving the net effect on cigarette consumption ambiguous.

Even with consumers who do not anticipate adjacent complementarity, a full understanding of the relationship between past and current consumption of these products requires consideration of possible cross-product reinforcement effects (i.e., via the addictive stock of nicotine). Specifically, if a higher addictive stock has a greater impact on the marginal utility from cigarettes than e-cigarettes (e.g., if the former delivers a higher dose of nicotine per use), past e-cigarette use could raise the current period marginal utility of cigarette use more than that of e-cigarette use, through a reinforcement effect. This could incentivize take-up of conventional cigarettes (i.e., a gateway effect).

Whether such cross-product reinforcement effects exist and dominate the substitution effect arising from e-cigarettes' introduction is an empirical question. Absent price data, this can be examined by testing how an intervention that restricts access to e-cigarettes affects smoking. To that end, the analysis below examines how state bans on e-cigarette sales to minors affect adolescent smoking.

[4] This implication is based on the impact on smoking alone, and assumes (consistent with the current literature) that the health costs of conventional cigarettes exceed those of e-cigarettes (Pisinger and Døssing, 2014).

[5] Prices represent full costs per use (e.g., including the cost if caught smoking as a minor), not just the purchase price.

[6] This author is aware of only one paper that analyzes consumption responses to e-cigarette prices, but these prices exclude those for online purchases (Huang et al., 2014). The authors find that higher cigarette prices yield consistently positive by statistically insignificant effects on e-cigarette purchases. Their analysis neither requires nor attempts a conversion factor to make the cigarette and e-cigarette prices refer to a common unit of consumption (e.g., inhalations). Because e-cigarettes are designed to provide many more uses than a single cigarette, adjusting list prices to reflect this is important when considering the products' relative prices.

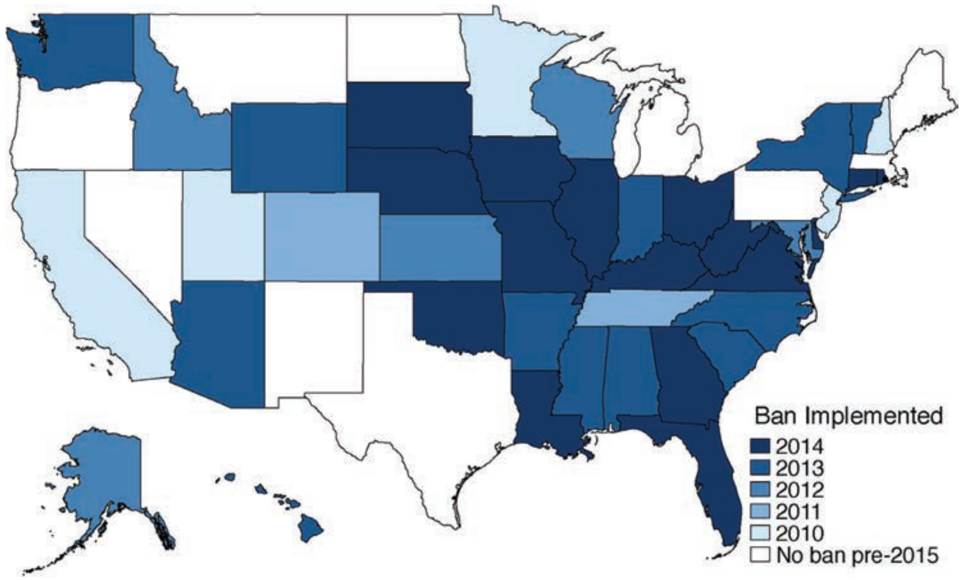

Fig. 1. State implementation of bans on electronic cigarette sales to minors. *Notes:* Data are from Marynak et al. (2014).

3. State bans on electronic cigarette sales to minors

Electronic cigarettes entered the U.S. market in 2007, the same year that Ruyan, the Chinese company that invented e-cigarettes, received an international patent. Though the Food and Drug Administration (FDA) banned e-cigarette imports in 2008, a legal case challenging this ban dragged from the spring of 2009 into December of 2010 (Riker et al., 2012). Absent clear FDA regulation, and with a variety of marketing tactics available to e-cigarettes that had been restricted for cigarettes, states began enacting restrictions to limit youths' e-cigarette access (see Fig. 1)[7]. The first such ban went into effect in New Jersey on March 13th, 2010. By January 1st of 2013, 13 states had bans on e-cigarette sale to minors in effect, with 11 more following before January 1, 2014 (Marynak et al., 2014). This section's analyses use these bans as proxies for youth e-cigarette access, identifying minors' smoking-responses to e-cigarettes off of state-by-year variation in ban presence[8].

3.1. Data and methods

From the 2002–2003 to 2012–2013 periods, recent cigarette smoking rates among 12 to 17 year olds fell from 13.5 percent to 6.7 percent, while those for 18 to 25 year olds dropped from 42.1 to 32.8 percent (see Table 1). Though e-cigarettes entered the U.S. market in the middle of this period, their advertising and sales did not take off until after 2010. Both more than quadrupled from 2010 to 2012, such that youth access rose greatly in states without bans on e-cigarettes sales to minors, but not necessarily in states with such bans (Elliott, 2013; Statistic Brain Research Institute, 2013).

Using state-specific two year averages of 12 to 17 year olds' recent smoking rates – having smoked a cigarette in the past 30 days – from the National Survey on Drug Use and Health (NSDUH), Fig. 2 examines trends in minors' smoking in states that did versus did not ban e-cigarette sales to minors by January 1, 2013 (the midpoint of the last two-year period for which NSDUH data are available). In all years, these rates are within 1.5 percentage points of each other, with standard deviations ranging from 1.5 to 2.8 in the years before the first ban went into effect. Plotting the gap in these rates over time, along with a range of one standard deviation above and below each point, shows that these gaps are neither statistically different from zero nor statistically different from analogous gaps calculated for the 18 to 25 year old cohort (see Appendix Fig. A1). This observation, and the fact that teen smoking trends appear parallel in the pre-period, suggests that recent smoking trends were similar in states that would and would not go on to ban e-cigarette sales to minors by the start of 2013. To test the parallel trends hypothesis, I limit consideration to the pre-2010 period (i.e., before the first ban) and regress the smoking rate among 12 to 17 year olds on an indicator for whether the state banned sales to minors by January 1, 2013 interacted with period fixed effects, with additional variables controlling for state fixed effects along with state demographics, cigarette tax rates, and indicators for smoke-free air laws and medical marijuana legalization. Consistent with the parallel trends assumption, none of these interaction terms are statistically significant, and all are close to zero ($|\beta| < 0.005$).

OLS analyses of the NSDUH data consider the following regression:

$$Smoke12to17_{SY} = \beta_0 + \beta_1 Ban_{SY} + \beta_2 CigTax_{SY} + \beta_3 SmokeFree_{SY} + \beta_4 MML_{SY} + \beta_5 Smoke18to25_{SY} + \theta X_{SY} + \lambda State_S + \gamma Year_Y + \varepsilon_{SY}, \quad (3)$$

where *Smoking12to17*$_{SY}$ is the recent smoking rate – having smoked a cigarette in the past 30 days – for 12 to 17 year olds in state S during two-year period Y. Smoke*18to25*$_{SY}$ is the analogous rate for 18 to 25 year olds. To control for regulations expected to shape teen smoking, Eq. (3) includes state and two-year period fixed

[7] While recent research indicates that 2012 e-cigarette marketing emphasized harm reduction and use for cessation (Richardson et al., 2014a,b), a 2014 *Sports Illustrated* swimsuit edition ad suggests that more traditional messaging (i.e., sex sells) is also in play (Elliott, 2014).

[8] Some have questioned whether such bans prevent teens from accessing e-cigarettes online. However, even if the bans are only effective for retail locations, they could still reduce access by preventing teens from purchasing and using e-cigarettes at a moment's notice (and perhaps requiring a credit card to do so). In this case, a statistically significant impact of such bans might reflect a tendency towards impulsivity or present-bias in teen substance use, wherein having to purchase e-cigarettes well in advance reduces adolescents' propensity to buy them.

Table 1
Summary statistics.

	2002–2003	2004–2005	2006–2007	2008–2009	2010–2011	2012–2013
Recent smoking rate, ages 12–17	13.5%	12.1%	10.8%	9.6%	8.7%	6.7%
Recent smoking rate, ages 18–25	42.1%	40.7%	39.2%	37.2%	35.6%	32.8%
Recent smoking rate, ages 26-plus	25.6%	24.7%	24.8%	24.1%	23.4%	23.2%
Policy variables						
Ban on e-cigarette sales to minors	0	0	0	0	9.8%	25.5%
Proportion of period ban was in effect	0	0	0	0	8.6%	27.0%
State cigarette tax ($)	0.73	0.96	1.11	1.29	1.48	1.49
	(0.51)	(0.62)	(0.69)	(0.78)	(0.93)	(0.98)
Smoke free air law	2.0%	5.9%	19.6%	35.3%	51.0%	52.9%
Medical marijuana legal	15.7%	19.6%	21.6%	25.5%	29.4%	35.3%
State demographics						
Median household income	54,932	55,008	56,244	54,541	52,968	52,787
	(8210)	(8102)	(8460)	(8204)	(7785)	(8240)
State unemployment rate	5.47%	5.05%	4.38%	6.88%	8.45%	7.04%
Population size	5673,825	5784,959	5892,349	5990,846	6081,740	6176,891
	(6386,612)	(6530,369)	(6655,620)	(6752,585)	(6865,520)	(7004,902)
Percent under age 18	24.9%	24.3%	24.2%	23.9%	23.6%	23.1%
Percent Black	11.3%	11.3%	11.4%	11.4%	11.5%	11.6%
Percent other non-white race	7.2%	7.6%	8.0%	8.4%	8.7%	9.0%
Percent Hispanic	8.6%	9.1%	9.7%	10.3%	10.7%	11.1%
N	51	51	51	51	51	51

Notes: Observations are means for all 50 states and the District of Columbia, with standard deviations in parentheses for variables not given as percentages. Smoking data come from the National Survey on Drug Use and Health. Information on electronic cigarette bans and smoke free air laws are from Marynak et al. (2014), while that on medical marijuana legalization comes from Choi et al. (2014). Median household income data and demographic data are from U.S. Census Bureau tables, while unemployment rates are from the Bureau of Labor Statistics. Cigarette tax rates come from the CDC state trends application.

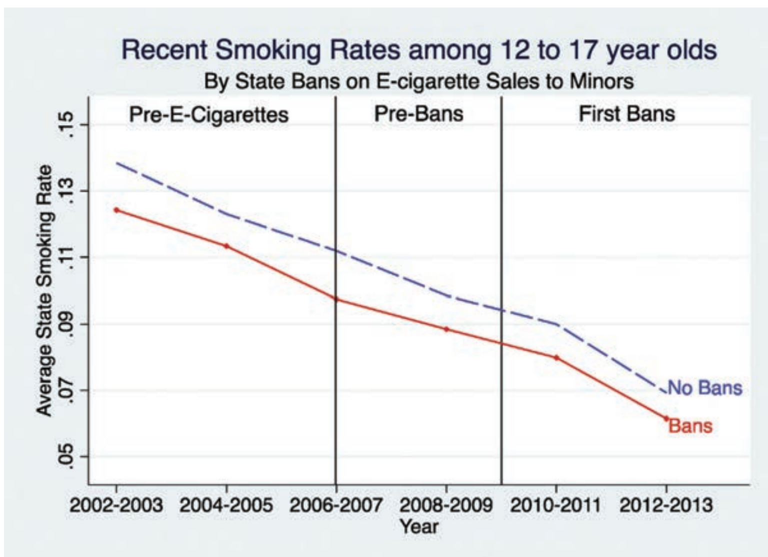

Fig. 2. State recent smoking rates for ages 12 to 17, by bans on e-cigarette sales to minors. *Notes:* Cross-state averages of age 12 to 17 recent smoking rates – having smoked a cigarette in the past 30 days – from the National Survey on Drug Use and Health are plotted by two-year periods, grouping states by whether a ban on e-cigarette sales to minors was in effect by January 1, 2013 ("Ban") or not ("No Ban").

effects ($State_S$, $Year_Y$) as well as policy variables: cigarette tax rates ($CigTax_{SY}$), binary indicators for smoke-free air laws ($SmokeFree_{SY}$), and binary indicators for whether medical marijuana is legal (MML_{SY}). Including the smoking rate among 18 to 25 year olds helps address concerns about confounding due to further policies that impact both teens and young adults, for which state level data are unavailable (e.g., advertising and anti-smoking campaigns). Given differential trends in youth smoking by race and ethnicity, a vector of demographic variables (X_{SY}) adjusts for the percent of state *S*'s population identifying as Black, as a different racial minority, and as Hispanic in period *Y*. Additional demographic controls include the state's total population and percent under age 18, as well as median household income and the unemployment rate, to account for the impact of economic conditions on smoking[9]. Ban_{SY} captures state bans on e-cigarette sales to minors, defined in one of two ways, depending on the specification: either as a binary indicator for whether state *S* had a ban on e-cigarette sales to minors in effect by period *Y*'s halfway point (e.g., as of January 1, 2013 for

[9] Tax, unemployment, and income data are from the CDC (2014), BLS (2014), and Census Bureau (2014), respectively. Tax and income variables are CPI adjusted to 2013 dollars. Other demographic data come from the U.S. census's state intercensal estimates available on the census website.

Table 2
Impact of bans on electronic cigarette sales to minors on recent smoking rates among 12 to 17 year olds.

	Recent smoking rate, 12 to 17 year olds					
Ban variable	Binary indicator			Proportion of survey period with ban in effect		
Limited sample	No (1)	No (2)	Yes (3)	No (4)	No (5)	Yes (6)
Ban on e-cigarette sales to minors	0.0065*	0.0069***	0.0067**	0.0093**	0.0095***	0.0094***
	(0.0034)	(0.0025)	(0.0028)	(0.0040)	(0.0027)	(0.0031)
Recent smoking rate, ages 18–25		0.2480***	0.2887***		0.2473***	0.2872***
		(0.0322)	(0.0351)		(0.0315)	(0.0342)
Policy controls						
State cigarette tax	0.0005	0.0020	0.0042*	0.0006	0.0021	0.0043**
	(0.0023)	(0.0019)	(0.0022)	(0.0023)	(0.0019)	(0.0021)
Smoke free air law	0.0031	0.0031	0.0027	0.0031	0.0032	0.0026
	(0.0025)	(0.0020)	(0.0025)	(0.0024)	(0.0020)	(0.0025)
Medical marijuana legal	−0.0038	−0.0030	−0.0004	−0.0038	−0.0030	−0.0009
	(0.0029)	(0.0025)	(0.0030)	(0.0028)	(0.0024)	(0.0030)
Period fixed effects						
2004–2005	−0.0172***	−0.0142***	−0.0127***	−0.0172***	−0.0143***	−0.0128***
	(0.0030)	(0.0027)	(0.0029)	(0.0030)	(0.0026)	(0.0029)
2006–2007	−0.0313***	−0.0250***	−0.0238***	−0.0315***	−0.0253***	−0.0241***
	(0.0042)	(0.0033)	(0.0040)	(0.0041)	(0.0033)	(0.0039)
2008–2009	−0.0432***	−0.0320***	−0.0297***	−0.0435***	−0.0324***	−0.0302***
	(0.0048)	(0.0042)	(0.0050)	(0.0047)	(0.0040)	(0.0048)
2010–2011	−0.0531***	−0.0383***	−0.0342***	−0.0537***	−0.0390***	−0.0352***
	(0.0059)	(0.0051)	(0.0060)	(0.0058)	(0.0049)	(0.0058)
2012–2013	−0.0768***	−0.0553***	−0.0494***	−0.0781***	−0.0567***	−0.0513***
	(0.0068)	(0.0061)	(0.0071)	(0.0066)	(0.0059)	(0.0068)
Constant	0.2832***	0.1753***	0.1649**	0.2815***	0.1737***	0.1637**
	(0.0690)	(0.0628)	(0.0790)	(0.0667)	(0.0604)	(0.0767)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	306	306	240	306	306	240
Adjusted *R*-square	0.896	0.923	0.924	0.897	0.923	0.925
Mean (recent smoking rate, ages 12 to 17)	0.102	0.102	0.103	0.102	0.102	0.103

Notes: Linear probability model coefficients are presented with standard errors in parentheses. Analyses use state-level data on recent smoking rates for 2002-2003, 2004-2005, 2006-2007, 2008-2009, 2010-2011, and 2012-2013, from the National Survey on Drug Use and Health. In columns 1 through 3, bans on electronic cigarette sales to minors are captured by binary indicators of whether the ban went into effect before the period's halfway point (e.g., by January 1, 2011 for the 2010-2011 period). In columns 4 through 6, the ban variable is the proportion of the survey period when the ban was in effect. Limited sample regressions only include those states that enacted a ban on sales to minors before January 1, 2015. All monetary units are in real 2013 dollars. All controls are indicated. Demographic controls with coefficients not listed above are the number of state residents, percent Black, percent other racial minority, percent Hispanic, percent under age 18, the median household income, and the state unemployment rate. SEs are clustered by state.
*** (**) [*]Denotes statistical significance at the 1% (5%) [10%] level, respectively.

the 2012–2013 period), or as the proportion of the survey period during which such a ban was in effect in state *S*. Thus, β_1 captures the effect of such bans on smoking among minors[10].

While the main regression includes all states and years, a specification check will drop those states that did not have a ban in effect by January 1, 2015 to further address concerns that the control states may not be valid counterfactuals for the treatment states, due to unobserved factors related to policy endogeneity. Notably, only 10 states and the District of Columbia lack such a ban by that date; 24 states had bans in effect prior to January 1, 2014, with a further 16 enacting them over the course of 2014[11].

Two falsification tests and a placebo test are considered. The first adds a next-period-ban indicator to the baseline regression to verify that β_1 is not driven by a time-varying characteristic common to states that are about to enact such bans. The second considers whether bans on e-cigarette sales to minors impact smoking among non-minors, which would implicate a driver other than the ban itself (e.g., greater information about smoking's risks). Specifically, it runs the Eq. (3) regression with smoking rates among 18 to 25 year olds' as the dependent variable, and the 26-and-older smoking rate as the control. The final test assigns placebo-bans at random such that the proportion of state-period observations assigned a placebo bans equals the proportion observed to have a ban in place during the period of analysis (5.9%). It then runs the baseline specification on these false-bans instead of the observed bans, repeating the randomized assignment and regression 25 times to test how often the placebos yield statistically significant effects.

3.2. Results

Table 2 presents analyses of Eq. (3), with columns 1 through 3 considering a binary indicator for bans on e-cigarette sales to minors, while columns 4 through 6 use the proportion of the survey period when such a ban was in effect. In both cases, the first specification omits the control for smoking rates among 18 to 25 year olds. Estimating Eq. (3) with no controls besides state and period fixed effects indicates that smoking rates fell more quickly over time, a result borne out by every specification in Table 2 as well[12].

[10] If the variation in state bans is largely explained by state and period fixed effects, these collinearities could result in biased coefficients. To test this, I regress the ban variable on state and period fixed effects alone, and verify that the *R*-squared falls below 0.9. Reassuringly, the *R*-squared equals 0.37, while the adjusted *R*-squared is 0.24.

[11] While a synthetic control approach was considered in an earlier version of this paper, the rather short time period – NSDUH data prior to 2002 is not comparable to the later data due to methodological changes, limiting the data series – and the presence of 24 states with bans in effect during the survey period suggests that this method is not appropriate here.

[12] Indeed, even without additional controls, this specification's ban coefficients—0.006 with a binary ban and 0.009 with a proportion (full regressions not presented here)—are similar to those estimated in Table 2.

Table 3
Falsification tests for impact of bans on electronic cigarette sales to minors on recent smoking.

Dependent variable	Smoking rate, ages 12–17	Smoking rate, ages 18–25	
Ban variable	Binary (1)	Binary (2)	Proportion (3)
Ban on e-cigarette sales to minors	0.0069***	0.0029	0.0043
	(0.0025)	(0.0070)	(0.0087)
Next period ban on e-cigarette sales to minors	−0.0002		
	(0.0020)		
Recent smoking rate, ages 18–25	0.2479***		
	(0.0325)		
Recent smoking rate, ages 26+		0.6382***	0.6386***
		(0.1159)	(0.1150)
Policy controls			
State cigarette tax	0.0021	−0.0027	−0.0027
	(0.0019)	(0.0036)	(0.0037)
Smoke free air law	0.0031	0.0025	0.0025
	(0.0020)	(0.0041)	(0.0041)
Medical marijuana legal	−0.0030	−0.0032	−0.0032
	(0.0025)	(0.0056)	(0.0057)
Constant	0.1755***	0.2182**	0.2172**
	(0.0634)	(0.1035)	(0.1032)
State and year fixed effects	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes
N	306	306	306
Adjusted *R*-square	0.922	0.890	0.890

Notes: Linear probability model coefficients are presented with standard errors in parentheses. Analyses use state-level data on recent smoking rates by age group for 2002–2003, 2004–2005, 2006–2007, 2008–2009, 2010–2011, and 2012–2013, from the National Survey on Drug Use and Health. With the exception of tax rates and the ban indicator in column 3 (which gives the proportion of the survey period when the ban was in effect), all policy variables are binary indicators for whether the policy was in effect by the period's halfway point (e.g., by January 1, 2011 for the 2010-2011 period). As the leads falsification test (column 1) uses a binary indicator for leads on the bans, it is only carried out with the binary ban indicator, not the proportion version. All specifications include state and survey period fixed effects as well as demographic controls, specifically, the number of state residents, percent Black, percent other racial minority, percent Hispanic, percent under age 18, median household income, and the state unemployment rate. Median household income and tax rates are in real 2013 dollar units. SEs are clustered by state.
*** (**) [*] Denotes statistical significance at the 1% (5%) [10%] level, respectively.

Incorporating demographic and policy controls, the coefficients on state tax rates as well as indicators for smoke free air laws and medical marijuana legalization are small and, in all but one specification, statistically insignificant at conventional levels[13].

Regressions using the binary indicator for state bans on e-cigarette sales to minors find that such bans yield a positive and statistically significant 0.7 percentage point increase in recent smoking rates among 12 to 17 year olds, relative to the rate in states that had not implemented such bans. Limiting the sample to states that implemented bans before 2015 does not change this result.

Using the proportion of the survey period in which these bans were in place instead of a binary ban indicator results in even larger effects: over a 2 year period, such bans yield a 0.9 percentage point increase in the recent smoking rate, statistically significant at the 1 percent level. Again, restricting the sample to those states with bans implemented prior to 2015 does not change this result[14].

The larger effects on the continuous ban measures make sense: eleven states' bans went into effect in 2013, but after January 1st of that year, and thus are coded as a 0 in the binary ban indicator for 2012–2013. If these bans influenced teen smoking in 2013, the binary ban indicator's coefficient would be biased toward zero, but not the coefficient using the proportion of the year that the ban was in effect.

Yet even beyond that, there are several reasons to suspect that both sets of ban coefficients estimated in Table 2 may represent lower bounds on the true effect's magnitude. Several localities restricted e-cigarette sales to minors, even in states that did not do so. Thus, the impact of local bans on teen access to e-cigarettes in no-ban states could bias β_1 toward zero. Additionally, some states and localities banned e-cigarette sales to 18 year olds (e.g., Utah), potentially affecting the control for 18 to 25 year olds' recent smoking rates. Taken together, these observations suggest that all ban coefficients estimated here should be viewed as lower bounds.

Table 3 presents falsification tests, with column 1 considering whether next period bans impact current period smoking. As the leads variable is binary, this check is only run with the binary ban variable. The same-period ban effect remains statistically significant and similarly sized, while leads on these bans show a statistically insignificant and small coefficient ($\beta = -0.0002$). This result suggests that the ban coefficient is not driven by information about future bans or a time-varying state characteristic that manifested just before the bans went into effect.

Repeating the Eq. (3) analysis with smoking rates among 18 to 25 year olds as the outcome, first with the binary ban indicator and then with the continuous ban variable, columns 2 and 3 do not find evidence that the bans on e-cigarette sales to minors influence smoking among non-minors ($|\beta| < 0.005$, p-value > 0.6)[15]. Alongside Table 2, these tests' results provide evidence that state bans on

[13] The tax coefficients may reflect relatively small changes in state tax rates. Controlling for smoking rates among 18 to 25 year olds yields more positive tax effects, consistent with the observed tendency of younger teens to respond less to cigarette taxes than older adolescents (e.g., Gruber and Zinman, 2001).

[14] Repeating these analyses with the NSDUH rates for any recent tobacco product use (i.e., past month use of cigarettes, smokeless tobacco, cigars, or pipe tobacco) instead of cigarette smoking alone yields positive but statistically insignificant coefficients ranging from 0.4 to 0.6 percentage points (Appendix Table A1). This is consistent with bans shifting teen cigarette smoking but not the other tobacco products considered here, though the exact effects cannot be separated out with the aggregated data.

[15] Repeating this regression without controlling for the smoking rate among those ages 26 and older also yields a small and statistically insignificant ban coefficient (results not shown here).

e-cigarette sales to minors influenced smoking rates only once in place, and only among the target age group.

As a robustness check, state-period observations are randomly assigned to a binary placebo-ban, such that the proportion of observations with a placebo ban equals the proportion with the (true) binary ban indicator. The baseline regression is then run with the placebo ban variable in place of the true bans. Repeating this exercise 25 times, only one iteration yields a statistically significant coefficient on the false-ban.

Thus, the analysis of state bans on e-cigarette sales to minors indicates that these restrictions on e-cigarette access increase adolescent smoking by 0.9 percentage points, with the impact only evident once the ban goes into effect, and only among those subject to the ban (i.e., under age 18).

4. Conclusion

Across the board, this paper's analyses find that reducing e-cigarette access increases smoking among 12 to 17 year olds. The effect is large: over the 8 years preceding the first bans on e-cigarette sales to minors, states recent smoking rates for this age group fell an average of 1.3 percentage points every two years. The estimated 0.9 percentage point rise in smoking due to bans on e-cigarette sales to minors counters 70 percent of this downward trend for a given two-year period, in states that implemented such bans.

This paper offers several key contributions. Analyzing how state bans on e-cigarette sales to minors impact teen smoking rates yields the first causal evidence of e-cigarettes' impact on adolescent smoking. These results are robust to multiple specifications, and supported by a series of falsification and placebo tests. They find that, prior to 2014, banning e-cigarette access increased teen smoking rates.

The paper has several limitations. First and foremost, the NSDUH data only provide state smoking rates for two-year periods and do not observe e-cigarette use, preventing regressions from accounting for more granular trends and limiting identifying variation. Future work will address this as more data become available, particularly on e-cigarette use. Second, the outcome variable is recent cigarette use, yet the ideal smoking variable would capture habitual cigarette use, which is not provided in the state-aggregated NDSUH data. However, as the focus is on youths, even intermittent use may be a key concern if it signals a higher likelihood of regular smoking in the future. A third limitation has to do with the e-cigarette market itself: as it is quite young and evolving quickly, this paper's analyses may not reflect relationships at market equilibrium. For example, if the observed response among teens is partially a reaction to the controversy around e-cigarettes, their behavior may change as that controversy abates, the product becomes less novel, or, with a greater role of large cigarette companies in the e-cigarette market, marketing of both cigarettes and e-cigarettes shifts.

Finally, this analysis does not measure electronic cigarette use, and thus cannot speak to shifts in that behavior or its long run effects. Consideration is limited to the potential costs and benefits of e-cigarette access in terms of its impact on cigarette smoking. The potential long run health effects from e-cigarettes themselves, as well as complementarities with other risky behaviors (e.g., alcohol consumption), are not addressed. As data on such consequences become available, they will clarify the product's full costs and benefits. In particular, evidence of substantial variation in the particulate matter and toxins produced by e-cigarettes of different types with different flavorings suggests that future analyses should attend to the demand for and health effects of different kinds of e-cigarettes (e.g., flavored e-liquid, higher voltage devices) (Grana et al., 2014a, b; Kosmider et al., 2014).

This paper's findings will prove surprising for many: policy discussions to date have not considered that banning e-cigarette sales to minors might *increase* teen smoking. Assuming that e-cigarettes are indeed less risky to one's health than traditional cigarettes, as suggested by existing evidence on the subject, this result calls such bans into question. Yet it is not a straightforward guide to regulation: beyond the fact that the market had not reached equilibrium by 2013, an FDA decision not to ban e-cigarette sales to minors after having announced this intention could be seen as sanctioning teen vaping, introducing distinct costs not addressed here. A middle ground that recognizes the potential for yet unknown long run costs of e-cigarette use might involve banning sales to those younger than 16 instead of 18, as initiation of regular smoking first spikes at the former age (Lillard et al., 2013.

Acknowledgements

I am grateful to David Cutler, Richard Frank, Claudia Goldin, Frank Sloan, Jody Sindelar, Martin Anderson, Sebastian Bauhoff, Shivaani Prakash, Mark Schlesinger, and Sam Richardson for helpful comments and discussion, and to the Radcliffe Institute for Advanced Study, for fellowship funding that helped support this research.

Appendix A.

Fig. A1; Table A1.

Table A1
Impact of bans on electronic cigarette sales to minors on recent tobacco product use by 12 to 17 year olds.

	Recent tobacco product use rate, 12 to 17 year olds					
Ban variable	Binary indicator			Proportion of survey period with ban in effect		
Limited sample	No (1)	No (2)	Yes (3)	No (4)	No (5)	Yes (6)
Ban on electronic cigarette sales to minors	0.4147 (0.3966)	0.4368 (0.3187)	0.4415 (0.3326)	0.5815 (0.4764)	0.5324 (0.3901)	0.5580 (0.4142)
Tobacco product use rate, ages 18–25		0.2918*** (0.0449)	0.3197*** (0.0510)		0.2906*** (0.0444)	0.3182*** (0.0505)
Policy controls						
State cigarette tax	0.0733 (0.2856)	0.2475 (0.2440)	0.4886* (0.2840)	0.0788 (0.2856)	0.2536 (0.2437)	0.4950* (0.2827)
Smoke free air law	0.2994 (0.2797)	0.1611 (0.2353)	0.0006 (0.2810)	0.3021 (0.2788)	0.1658 (0.2336)	−0.0019 (0.2753)
Medical marijuana legal	−0.5514 (0.3542)	−0.4722 (0.3250)	−0.1460 (0.2815)	−0.5548 (0.3506)	−0.4773 (0.3245)	−0.1754 (0.2872)

Table A1 (*Continued*)

	Recent tobacco product use rate, 12 to 17 year olds					
Ban variable	Binary indicator			Proportion of survey period with ban in effect		
Limited sample	No (1)	No (2)	Yes (3)	No (4)	No (5)	Yes (6)
Period fixed effects						
2004–2005	−1.6998***	−1.5734***	−1.5269***	−1.7060***	−1.5821***	−1.5379***
	(0.3483)	(0.3129)	(0.3266)	(0.3460)	(0.3104)	(0.3236)
2006–2007	−3.1055***	−2.6726***	−2.6994***	−3.1180***	−2.6906***	−2.7193***
	(0.5110)	(0.4295)	(0.5022)	(0.5084)	(0.4247)	(0.4964)
2008–2009	−4.3196***	−3.4315***	−3.3786***	−4.3409***	−3.4613***	−3.4220***
	(0.5640)	(0.5016)	(0.5838)	(0.5548)	(0.4910)	(0.5706)
2010–2011	−5.3943***	−4.2243***	−3.9779***	−5.4319***	−4.2666***	−4.0439***
	(0.6785)	(0.5768)	(0.6675)	(0.6679)	(0.5685)	(0.6590)
2012–2013	−8.1970***	−6.4287***	−6.1595***	−8.2786***	−6.5077***	−6.2737***
	(0.8384)	(0.7426)	(0.8535)	(0.8332)	(0.7394)	(0.8584)
Constant	33.5277***	19.3949***	18.3410*	33.4134***	19.3205***	18.2995*
	(7.3091)	(6.8729)	(9.3559)	(7.1880)	(6.7661)	(9.2482)
Demographic	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	306	306	240	306	306	240
Adjusted *R*-square	0.885	0.910	0.912	0.885	0.910	0.912
Mean(recent tobacco product use rate)	12.946	12.946	13.001	12.946	12.946	13.001

Notes: Linear probability model coefficients are presented with standard errors in parentheses. Regressions use state-level data on rates of tobacco product use – cigarettes, smokeless tobacco, cigars, or pipe tobacco – in the past 30 days by age group for 2002–2003, 2004–2005, 2006–2007, 2008–2009, 2010–2011, and 2012–2013, from the National Survey on Drug Use and Health. In columns 1 through 3, bans on electronic cigarette sales to minors are captured by binary indicators of whether they went into effect before the period's halfway point (e.g., by January 1, 2011 for the 2010–2011 period). In columns 4 through 6, the ban variable is the proportion of the survey period when the ban was in effect. Limited sample regressions include only those states that enacted a ban on sales to minors before January 1, 2015. All monetary units are in real 2013 dollars. All controls are indicated. Demographic controls with coefficients not listed are the number of state residents, percent Black, percent other racial minority, percent Hispanic, percent under age 18, the median household income, and the state unemployment rate. SEs are clustered by state.
*** (**) [*] Denotes statistical significance at the 1% (5%) [10%] level, respectively.

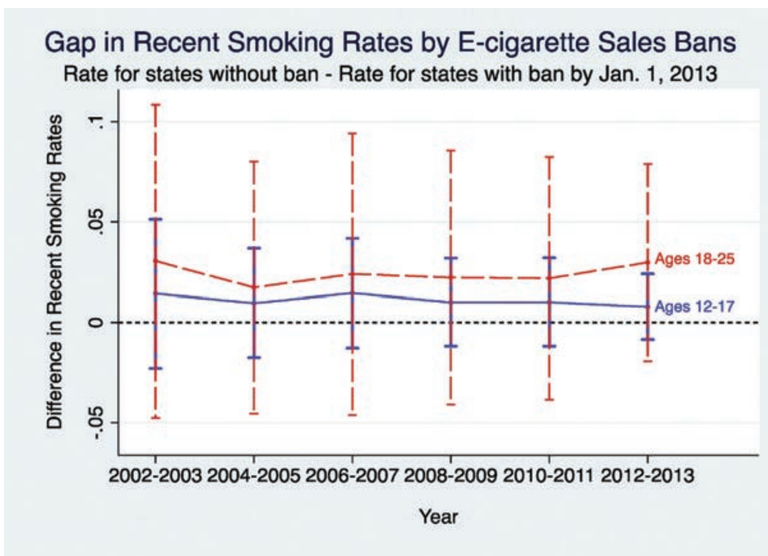

Fig. A1. Gaps in recent smoking rates by electronic cigarette sales bans. *Notes:* This figure uses state-specific two-year averages of recent smoking rates – having smoked a cigarette in the past 30 days – for ages 12 to 17 and ages 18 to 25 from the National Survey on Drug Use and Health. Grouping states by whether a ban on e-cigarette sales to minors was in effect by January 1, 2013, this figure plots, for each two year period, the gap in the average recent smoking rate between states that did and did not implement such bans ($Rate_{No\ Ban\ by\ 2013} - Rate_{Ban\ by\ 2013}$), for each age group. A range of ±1 standard deviation around each gap is delineated.

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