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Three Essays on Tobacco Control Policy

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ABSTRACT

THREE ESSAYS ON TOBACCO CONTROL POLICY

By

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This dissertation consists of three chapters on the direct and indirect effects of tobacco control policy on risky health behaviors. Among the direct effects of tobacco taxes, I study cigarette and cigar use, while among the indirect effects, I study marijuana use and body weight.

In chapter one of my dissertation, I estimate the effect of tobacco taxes on youth cigarette, cigar, and marijuana use. Using data from the Youth Risk Behavioral Surveillance System (1999-2017) and a difference-in-differences approach, I exploit the variation in state cigarette and cigar tax over time to identify causal effects. I examine the cross-tax elasticities of cigarette and cigar use and find that, while an increase in cigar tax leads to a decrease in cigarette use, an increase in cigarette tax increases cigar use. I explain this asymmetry in cross-tax elasticity through a third product, marijuana, the most smoked product among youth. My findings imply that cigarettes and marijuana are substitutes, while cigars and marijuana are complements. Future anti-tobacco policies must take into account the high prevalence of marijuana and its role in tobacco use.

In the second chapter of my dissertation, I use quasi-experimental linear and non-

linear methods to reconcile the conflicting experimental and observational literature on smoking and body weight. I use the Behavior Risk Factor Surveillance System to study the relationship between cigarette taxes, smoking, and body weight. My findings suggest that specification choice and accounting for misreporting may be the missing links required to reconcile the two strands of research.

In the third chapter, using data from the Youth Risk Behavior Surveys, I document a diminishing marginal impact of cigarette tax rate on youth smoking using parametric and semi-parametric methods.

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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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1.1 Introduction

Tobacco use is the leading cause of preventable deaths in the United States (Centers for Disease Control (2014)). Most adult smokers start as teenagers and, once addicted, find it hard to quit smoking (Centers for Disease Control (2014, 2012); Glied (2002); Chaloupka and Warner (2000)). Thus, an effective intervention to reduce lifetime tobacco use is to deter youth smoking (Friedson and Rees (2020); Auld (2005); Glied (2002, 2003)). To reduce the high rates of youth smoking, government policy in the 1990s focused on reducing cigarette use rather than the full range of tobacco products such as large cigars, cigarillos, and little cigars (hereafter cigars). It led to cigarettes being more strictly regulated than cigars, even though they have similar adverse health effects (Reilly et al. (2018); Rosenberry et al. (2018); Pickworth et al. (2017)). Between 1999 and 2017, cigarette taxes increased by over 300 percent, while cigar taxes increased modestly by 60 percent. During the same period, cigarette use declined steeply, but cigar use remained prevalent and is currently more common than cigarette use (Wang et al. (2019); Gentzke et al. (2019); Warren et al. (2014)). Cigars are cost-effective (due to lower taxes), can be available in mild flavors, and can also come without health warnings (Cullen et al. (2019); Delnevo et al. (2017)). Although an extensive literature is available concerning the effects of cigarette tax on youth cigarette use, very little is known about the effects of cigar tax on cigarette and cigar use, and substitution from cigarettes to cigars. This study analyzes the effects of cigarette and cigar tax on youth cigarette and cigar use. I find a paradox in the cross-tax elasticities of cigarette and cigar use, not yet discussed in the literature. I investigate the paradox and find that marijuana use is one of the potential explanations.

In this paper, I estimate the effect of cigarette and cigar tax on cigarette and cigar use among high-school youth. I match ten waves of data from the Youth Risk Behavioral Surveillance System (YRBS) for the years 1999-2017 with state cigarette and cigar taxes. Using a difference-in-differences approach, I employ a logit model for estimation. The identification of causal effects comes from within-state variation in cigarette and cigar taxes. I begin by estimating the effect of cigarette and cigar tax on cigarette use. I find that an increase in cigarette tax has a small effect in reducing cigarette use, and an increase in cigar tax leads to a decrease in cigarette use. The cross-tax elasticity of cigarette use with cigar tax is negative. This implies that cigarettes and cigars are complements. I then estimate the effect of cigarette and cigar tax on cigar use. I find that cigar taxes do not affect cigar use, while an increase in cigarette tax increases cigar use. A one-percent increase in cigarette tax leads to a 0.10 percent increase in cigar use. The cross-tax elasticity of cigar use with cigarette tax is positive. This implies that cigarettes and cigars are substitutes. The opposite signs of the cross-tax elasticity of cigarettes and cigars present an interesting counter-intuitive finding.

I investigate several possible explanations for the opposite signs of cross-tax elasticity and find that one of the explanations is through a third product, marijuana. My findings suggest that an increase in cigarette tax increases marijuana use, similar to its effect on cigar use, while an increase in cigar tax leads to a decrease in marijuana use, similar to its effect on cigarettes. This implies that cigarettes and marijuana are substitutes, while cigars and marijuana are complements. My findings signal that the relationship between tobacco taxes and tobacco products is likely to be affected by marijuana use. Future policy for reducing youth smoking should take into account its unintended effects on marijuana use, and the interaction between marijuana and tobacco products.

This study makes a few additions to the literature on youth smoking. First, I estimate the effect of cigar tax on cigarette use and find a paradox not yet discussed in the literature. Second, I find that marijuana use can be a potential explanation for the paradox. Third, I provide new evidence on the effect of cigar tax on marijuana use. I find that cigars and marijuana are complements. Fourth, I provide evidence of substitution between cigarettes and marijuana. The rest of the paper is as follows. After a discussion of literature in section 2, section 3 and 4 discuss the data and empirical methods used. Results are presented in section 5, followed by the conclusions in section 6.

1.2 Literature Review

In this section, I first present the literature on tobacco taxes and cigarette use. Following that, I present the limited evidence on tobacco taxes, cigar use, and marijuana use.

Tobacco Taxes and Youth Cigarette Use

Cigarette use and cigarette taxes have received wide attention from researchers over the last three decades. Most studies from early 1990s use cross-sectional data to find negative own-price elasticity of cigarette use. The estimates from these studies are negative, large in magnitude, and range from 1.16 to 2.4. A concern with most of the earlier studies is that the estimates are correlational, and affected by unobserved factors that affect smoking or tax policy. Studies that use more variation in tax changes to identify causal effects find much smaller price elasticities of cigarette use. DeCicca et al. (2002) use the National Education Longitudinal Study (NELS 1988) to examine the impact of cigarette tax changes on the onset of smoking among 8th grade students. Using state fixed effects, they find estimates much smaller than reported in prior literature, suggesting that youth are insensitive to cigarette tax changes. DeCicca et al. (2008a) further decompose cigarette use into initiation and cessation, and find no evidence that cigarette taxes prevent inititation. Lillard et al. (2013) estimate the effect of cigarette tax on youth initiation using the NELS, PSID, and TUS-CPS data. The own-tax elasticities implied by their estimates range from -0.03 to -0.23, showing that youth are price inelastic in their smoking initiation decisions.

Carpenter and Cook (2008) (hereafter CC), Hansen et al. (2017) (hereafter HSR) and Anderson et al. (2020) (hereafter AMS) revisit this question using YRBS data. Using YRBS 1991-2005 and two-way fixed effects model CC find that cigarette tax reduces smoking participation and frequent smoking. Their elasticity estimates range from -0.23 and -0.56, which is smaller than earlier estimates. HSR update this analysis by adding four (2007, 2009, 2011, 2013) more waves of YRBS in 2017 and additionally two (2015, 2017) more waves of YRBS are added by HMS to the analysis by CC. Using a logit model with two-way fixed effects HSR (2017) find that cigarette taxes reduce cigarette use for the whole sample. Interestingly, when they restrict their sample to years 2007-2013, they do not find a statistically significant relationship between cigarette tax and smoking. Additionally, they report that including state-specific linear trends that control for time-varying factors across states diminishes the effect of cigarette taxes for the whole sample. Their explanation is that ”hardcore” young smokers drive the results for the 2007-2013 sample. AMS reiterate the findings of HSR (2017) and suggest that recent cigarette hikes have lost any observable bite. Their explanation is that tax hikes have driven price-sensitive youth out of the market, while price insensitive youth remain. While plausible, they are unable to test this potential explanation directly.

Evidence from international literature also suggests small participation elasticities of cigarette use. Sen and Wirjanto (2010) use panel data of 591 youths from Canada to estimate participation elasticities. The elasticities range from -0.10 to -0.14, which suggests that youth smoking is insensitive to price. Sen et al. (2010) exploit the time-series variation available within and across Canadian provinces. In particular, they study the dramatic (50%) reduction in cigarette excise taxes that occurred in February 1994 in most eastern provinces in Canada as well as significant increases within most provinces between 1994 and 2006. Their results suggest participation elasticities from -0.1 to -0.3 for teens aged 15 to 19 years, which is close to recent estimates from the US.

While there is extensive research on the response of cigarette use to cigarette taxes, little is known about the response of youth cigarette use to taxes on tobacco alternatives. To my knowledge, there are no estimates of the cross-tax elasticity of cigarette use among youth in the literature.

There are a few important points to learn from the literature about cigarette use. First, cigarette use among youth is decreasing. Second, the own-tax elasticity of cigarette use with cigarette tax is small and negative, suggesting that cigarette tax is not effective in reducing cigarette use. Third, there is no evidence on the cross-tax elasticity of cigarette use with cigar tax. This study makes the following contributions to the literature on cigarette use. First, I estimate the effect of cigarette tax on cigarette use, controlling for cigar tax, and the potential omitted variable bias. Second, I estimate the cross-tax elasticity of cigarette use with cigar tax. After analyzing cigarette use, I check for the possibility of substitution from cigarettes to cigars. In the next section, I summarize the literature on cigar use.

Tobacco Taxes and Youth Cigar Use

Cigars have received little attention in smoking literature. Most studies are descriptive and provide prevalence estimates. I present some descriptive statistics from research on cigar use among youth below. Following that, I provide evidence from studies that analyze how tobacco taxes affect cigar use.

Recent studies show an increasing prevalence of cigar use among youth over the last two decades. Delnevo et al. (2005) provide prevalence statistics for cigar use between 2001 and 2004 using the New Jersey Youth Tobacco Survey. They find that cigarette use declined by 29 percent between 2001 and 2004, while more students smoked cigars than cigarettes. Though they suggest a possibility of substitution across tobacco products due to the disproportional taxes on tobacco products, they are unable to test it. Elfassy et al. (2015) use the seven waves of the New York City YRBS from 2001-2013 to report trends in cigarettes, cigars, and ST use. They find that the prevalence of cigarettes has decreased while the prevalence of cigars and ST has increased among youth over their sample period.

Cigars can be more appealing than cigarettes for reasons such as price-differences, availability in different flavors, perception of safety, and for use with marijuana. King et al. (2014) assess the prevalence and correlates of flavored-little-cigar and flavored-cigarette use among U.S. middle and high school students in 2011. Using data from the 2011 National Youth Tobacco Survey, they find that more than two-fifths of middle and high school smokers report using flavored little cigars or flavored cigarettes. Delnevo et al. (2015), using the

National Survey on Drug Use and Health (NSDUH), finds that recent an increase in flavoured cigar consumption and preference among youth and young adults. Some studies also try to find an association between cigarette and cigar use. Leatherdale et al. (2011) examine the prevalence of cigar, cigarillo, and little cigar use among Canadian youth in grades 9-12. Their findings suggest that cigars are used by a substantial number of Canadian youth, many of whom do not smoke cigarettes. Schuster et al. (2013) use longitudinal data to study smoking patterns among adolescents. They find that cigar use is high among adolescent cigarette smokers and is strongly associated with being male. The above studies provide mixed evidence on whether cigarettes and cigars could be complements or substitutes.

Despite the increasing prevalence, only few studies examine how tobacco taxes/prices affect cigar use. Ringel et al. (2005) used the 1999 and 2000 waves of the National Youth Tobacco Survey to examine the effect of cigarette and cigar prices on cigars use. They use market-level scanner price information on prices and find that a rise in price of cigars is associated with a fall in cigars use. Additionally, they find that an increase in cigarette price is associated with a decline in cigar use. Their reported own-price elasticity of cigar use suggests that a one-percent increase in cigar price leads to a 0.34 percent decrease in cigar use. The cross-price elasticity of cigar use with cigarette price is negative and suggests that a one-percent increase in cigarette price leads 0.66 percent decrease in cigar use. Some studies use market-level scanner data to find the price elasticity of cigar use. Gammon et al. (2016) study the effect of cigarette and little cigar prices on little cigar sales using scanner data for 27 states and find that a one-percent increase in little cigar price 0.317 percent decrease in little cigar sales. The cross-price elasticity of little cigar use with cigarette tax is positive and suggests that a one-percent increase in cigarette tax leads to a 0.273 percent increase in little cigar sales. The elasticity estimates are large in magnitude and correlational, possibly due to the cross-sectional nature of the datasets. The above studies indicate that smokers might be avoiding the higher cost of cigarettes by switching to cigars. Jawad et al. (2018) conducts a meta-analysis and systematic review of price elasticities of non-cigarette tobacco products. For high-income countries (Finland, Italy, Spain, Taiwan, and USA), they find that the average own price elasticity of cigar use is between -0.9, which suggests that a one-percent increase in cigar tax is associated with a 0.9 percent decrease in cigar use. The estimates for cross-price elasticity are more sensitive and range from -1.2 to 1.1. There is no clear evidence of complementarity or substitution between cigarettes and cigars.

The only study that provides causal estimates of the impact of cigarette and cigar tax on cigars is by Hawkins et al. (2018). They use eight waves of state YRBS data from 1999-2013 to analyze the effect of cigarette and cigar tax on youth cigar use. They divide their sample by sex and find that higher cigarette taxes are associated with an increase in adolescent use of cigars, with a larger effect males. A 10 percent increase in cigarette taxes is associated with males being 1.5 pp increase in cigar use. Additionally, they find that cigar use is inelastic to cigar tax. Though they do not report the elasticity estimate, the cross-tax elasticity of cigar use with cigarette tax from their study is positive for both males and females. Their analysis of cigar use suggests that cigarettes and cigars are substitutes.

There are a few important points to learn from the literature on cigars. First, cigar use among youth is increasing and higher than cigarette use. Second, the own-tax elasticity of cigar use tax is small and negative. Third, while earlier studies suggest a negative cross-price elasticity of cigars with cigarette tax, more reliable recent evidence shows a positive cross-tax elasticity. This would suggest that cigars and cigarettes are substitutes. This study makes the following contributions to the literature on cigar use. First, I update the elasticity estimates for cigar use by adding four waves of YRBS data to Hawkins et al. (2018). I expand their sample by adding data from sixteen states. Additionally, they convert specific cigarette excise tax ($ amount) to ad-valorem (% of the price), which leads to the possibility of endogeneity as price changes might reflect demand (Gruber and Frakes (2006)). Second, I estimate the effects of tobacco tax on cigar use controlling for factors that vary across state and over time, and can lead to potential bias. Third, most studies do not address the relationship between tobacco products and marijuana. I provide some evidence to suggest that tobacco taxes and tobacco use might be related to marijuana use. In the next section, I discuss some evidence on tobacco taxes and marijuana use.

Tobacco Taxes and Youth Marijuana Use

Some studies provide descriptive evidence of the link between tobacco and marijuana use. In addition to being smoked on it's own, marijuana can also be combined with cigars for rolling blunts[1]. Studies show that marijuana can be used with both cigarettes and cigars, and can thus lead to initiation into tobacco use. A study by Delnevo et al. (2011) finds that adolescent and young adult smokers who use cigars may be engaging in blunting. Using data from the 2007 National Survey on Drug Use and Health, they find that young people recognize blunts as a form of marijuana use but do not recognize it as cigars use. This suggests that blunt use contributes to nicotine intake and we must recognize that blunt smoking might be a potential form of tobacco initiation. Weinberger et al. (2020) examined marijuana use and smoking initiation, persistence, and relapse over an year among

[1] Hollowing out some tobacco from a cigar and replacing it with marijuana.

a nationally representative sample of US adults. Using longitudinal data from the Population Assessment of Tobacco and Health Study, they find that marijuana use was associated with increased cigarette use initiation, decreased smoking cessation, and increased smoking relapse among adults in the United States.

To my knowledge, no papers have estimated the effect of cigar tax on marijuana use, and only a few papers have estimated the relationship between cigarette taxes and youth marijuana use. Farrelly et al. (1999) use data from the National Household Survey on Drug Abuse (NHSDA) for the period 1990-1996 and find that higher cigarette taxes are associated with decreases in the intensity of marijuana use among youth and may also lead to reductions in the probability of using marijuana. They suggest a negative cross-tax elasticity of marijuana use with cigarette tax. Anderson et al. (2020) use data from the 1991-2017 YRBS to estimate the effect of cigarette tax on marijuana use. They use a difference-in-differences approach to find that cigarette taxes have no effect on marijuana use.

There are a few points to learn from the literature on marijuana use. First, marijuana use is high among youth, and it can be used with cigars as well as cigarettes. Second, the evidence from Anderson et al. (2020) suggests that the elasticity of marijuana use with cigarette tax is small and insignificant. Third, there are no available estimates of the cross-tax elasticity of marijuana use with cigar tax. This study makes the following contributions to the literature on marijuana use. First, I estimate the cross-tax elasticity of marijuana use with cigar tax. Second, I estimate the cross-tax elasticity of marijuana use with cigarette tax, controlling for cigar tax. It accounts for potential omitted variable bias.

1.3 Data

This study uses data from the Youth Risk Behavioural Surveillance System (YRBS). The YRBS is a group of repeated cross-sectional surveys conducted every odd year since 1991. I use two YRBS datasets for this study, the National and State YRBS. Both datasets are administered by the Centers for Disease Control and Prevention (CDC) and include state and national school-based surveys of 9th through 12th-grade students. The questionnaires are related to student demographics and risky health behaviors, including tobacco and marijuana use. The questions asked in both the surveys are in the same language, and the national and state samples do not overlap[2]. All waves of the YRBS contain questions on cigarette use, but questions on cigar use were added since the 1999 wave[3]. Thus, I use data from 1999-2017, which covers cigarette, cigar, and marijuana use.

I pool the National and State YRBS for my analysis. There are a few differences between the National and State YRBS, which serve as a motivation to pool the two. First, while the National YRBS covers all 50 states and DC, the State YRBS is conducted by 46 states. The states of DC, Minnesota, Oregon, and Washington do not participate in the State YRBS, and Connecticut does not include questions on cigar use. Pooling the two datasets allows me to use all possible state tobacco tax changes for identification.

Second, the National YRBS has a smaller sample size per year as compared to the State YRBS. After eliminating missing observations on cigarette use, cigar use, age, gender,

[2]CDC suggests that their sampling is unlikely to create an overlap in the State and National YRBS primary sampling units. The National YRBS follows a three-stage sampling frame: primary sampling units (PSU), schools within PSU's, and classrooms within schools. The State YRBS follows a two-stage sampling process: schools are selected with probability proportional to enrollment size, classrooms with the school are selected. Schools, classes, and students who refuse to participate are not replaced.

[3]In 2013, e-cigarette use was added to the questionnaire.

grade, and race, I have 96,147 observations from the National YRBS as compared to 771,228 observations from the State YRBS. The combined YRBS has 867,375 observations, which is almost eight times larger than the National YRBS. Thus, pooling the two datasets leads to a larger sample size and more precise estimates.

I use questions regarding the use of cigarettes, cigars, marijuana, as well as demographics from the YRBS questionnaire. Regarding cigarette use, the respondents are asked:

"During the past 30 days, on how many days did you smoke cigarettes?"

Following the CDC, I define a respondent as a current cigarette user if they reported smoking cigarettes at least once in the past 30 days. Using the same information, I define them as frequent cigarette user if they reported smoking at least 20 days of the past 30 days.

Regarding cigar use, the respondents are asked:

"During the past 30 days, on how many days did you smoke cigars, cigarillos, or little cigars?"

I define a respondent as a current cigar user if they reported smoking cigars, cigarillos, or little cigars at least once in the past 30 days. Additionally, I define a student as a marijuana[4] user if they reported using marijuana at least once in the last 30 days.

I match the pooled YRBS data to cigarette and cigar taxes in January of each survey year. I obtain data on taxes from the CDC's State Tobacco Tracking and Activity Evaluation (STATE) system. My cigarette tax variable is the state excise tax rate on cigarettes. I convert the nominal tax rate to 2017 dollars using the Consumer Price Index for All Urban Consumers (CPI-U). There is wide within and across state variability in the cigarette tax. From 1999 to 2017, the average cigarette tax rose from $0.33 to $2.06 per pack of cigarettes.

[4] "During the past 30 days, how many times did you smoke marijuana?"

New York experienced the sharpest rise in state excise tax, rising from $0.56 per pack in 1999 to $4.35 per pack in 2017. Between 1999-2017, Montana, and North Dakota did not increase their cigarette tax[5]. Thus, my identification is based on tax changes in 47 states and DC. Of the states that changed tax in my sample, 12 states changed taxes once, while the rest changed tax at least twice. During the same period, the average cigar tax increased from 20% to 32% of the wholesale cost.

I use individual and state-level covariates to account for factors that might be correlated with youth smoking, cigarette, or cigar taxes. The individual-level characteristics include the respondents' age in years, gender (male as the base category), and race (Non-Hispanic white as the base category, non-Hispanic black, Hispanic, and other race). The first state-level control is an indicator variable for comprehensive smoke-free air laws, equal to one if smoking is banned at the workplace, restaurants, and bars, and zero otherwise. This information comes from the CDC STATE system. Other state-level controls I use are the presence of marijuana decriminalization laws, state per-capita personal income from the Bureau of Economic Analysis, and state unemployment rates from the Bureau of Labor Studies.

Table 1.1 presents summary statistics for the variables used in the analysis. On average, 15% of students are current cigarette smokers, 11% are cigar smokers, and 6% use ST. Cigarette use declined from 32% in 1999 to 8% in 2017. Frequent smoking declined substantially from 16% in 1999 to 2% in 2017. During the same period, cigar use declined from 17% to 8%. Marijuana use has seen a modest decline from 23% in 1999 to 19% in 2017.

[5]Nebraska is sampled in my survey from 2003, but it increased its tax before 2003. Thus, it does not provide any identifying variation.

Between 1999 and 2017, cigarette taxes rose from \$0.52 to \$1.94, by almost 300%. During the same period, cigar tax rose from 20% to 32% of the wholesale price, an increase of 60%. Other relevant policy changes during 1999-2017 include increasing smoking restrictions in public places and marijuana decriminalization. Currently, marijuana use is twice as prevalent as cigars or cigarettes, and youth often use multiple tobacco products.

Table 1.2 shows the interaction between marijuana and tobacco products. There is a high prevalence of multiple tobacco product use among marijuana users. On average, 47% of marijuana users smoke cigarettes, and 35% use cigars. Cigarette use among marijuana users declined from 72% in 1999 to 28% in 2017. Cigar use declined from 40% to 27%, and ST use increased from 10% to 13%. The use of marijuana and cigars among cigarette smokers is high. 61% of cigarette smokers use marijuana, and 46% use cigars. Compared to 1999, the use of marijuana, cigars, and ST among cigarette smokers has increased. This provides some descriptive evidence that there is widespread interaction between cigarettes, cigars, and marijuana.

Table 1.1 Summary Statistics: YRBS 1999-2017

	1999		2017		1999-2017	
	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
Current Cigarette User	0.32	0.47	0.08	0.27	0.15	0.36
Frequent Cigarette User	0.16	0.36	0.02	0.14	0.06	0.24
Current Cigar User	0.17	0.37	0.08	0.27	0.11	0.31
Current Marijuana Use	0.23	0.42	0.19	0.39	0.19	0.40
Current ST User	0.05	0.22	0.05	0.22	0.06	0.24
Age	16.08	1.21	15.80	1.22	15.88	1.24
Female	0.52	0.50	0.51	0.50	0.52	0.50
Grade 9	0.28	0.45	0.27	0.45	0.28	0.45
Grade 10	0.26	0.44	0.27	0.44	0.27	0.44
Grade 11	0.25	0.43	0.25	0.43	0.25	0.43
Grade 12	0.21	0.41	0.20	0.40	0.21	0.40
White	0.61	0.49	0.54	0.50	0.57	0.50
Black	0.20	0.40	0.15	0.35	0.15	0.35
Hispanic	0.09	0.29	0.18	0.38	0.15	0.36
Other Race	0.10	0.30	0.13	0.34	0.13	0.34
Cigarette Tax (2017 $)	0.52	0.37	1.94	0.95	1.58	1.00
Cigar Tax (Ad-valorem)	0.20	0.15	0.32	0.24	0.29	0.20
100% SFA	0.00	0.00	0.77	0.42	0.52	0.50
Marijuana Decriminalization law	0.18	0.38	0.64	0.48	0.37	0.48
State Unemployment Rate	4.81	0.93	4.58	0.80	6.24	1.85
Observations	37,218		138,499		867,375	

Notes: Data used are from ten waves of the Youth Risk Behavioural Surveillance System (YRBS) for years 1999-2017. Cigars stands for Alternative Tobacco Product (Cigar, cigarillo, little cigar). ST stands for Smokeless Tobacco (chewing tobacco, snuff, dissolvable). Current cigarette/cigars/ST users reported using the product for more than one day in the past 30 days. Frequent cigarette smokers reported smoking cigarettes more than 20 days in the past 30 days. Current marijuana users reported using marijuana more than once in the past 30 days. 100% SFA stands for a ban on smoking in bars, restaurants, and workplaces.

Table 1.2 Summary Statistics - Tobacco and Marijuana Use

	Marijuana User			Non-User		
	1999	2017	Total	1999	2017	Total
Current Cigarette Use	0.72	0.28	0.47	0.20	0.03	0.07
Frequent Cigarette Use	0.43	0.09	0.22	0.07	0.01	0.02
Current Cigar Use	0.40	0.27	0.35	0.09	0.03	0.05
Current ST Use	0.10	0.13	0.15	0.03	0.01	0.03
	Current Cigarette User			Non-User		
	1999	2017	Total	1999	2017	Total
Current Marijuana Use	0.51	0.69	0.61	0.09	0.15	0.12
Frequent Cigarette Use	0.47	0.27	0.39	-	-	-
Current Cigar Use	0.40	0.54	0.46	0.05	0.03	0.04
Current ST Use	0.11	0.34	0.25	0.02	0.02	0.03

Notes: Data used are from ten waves of the Youth Risk Behavioural Surveillance System (YRBS). Cigar stands for large cigar, cigarillo, little cigar. Current cigarette/cigars/ST users reported using the product more than one day in the past 30 days. Frequent cigarette smokers reported smoking cigarettes for more than 20 days in the past 30 days. Current marijuana users reported using marijuana more than once in the past 30 days.

1.4 Empirical Analysis

I use a generalized difference-in-differences approach to estimate the impact of cigarette and cigar taxes on cigarette, cigar, and marijuana use. Specifically, I estimate the following reduced form equation:

$$P(Y_{ist} = 1) = \beta_1 Cigarettetax_{st} + \beta_2 Cigartax_{st} + X'\gamma + \tau_t + \sigma_s \tag{1.1}$$

where subscripts denote individual i living in state s in year t. I use three main dependent variables for my analysis: current cigarette user, current cigar user, and current marijuana user. Each of these variables is an indicator variable equal to 1 if the respondent uses the product, 0 otherwise. I use a logit model to estimate the above equation. The main variables of interest are cigarette tax (2017 \$) and cigar tax (%) in state s in year t.

The vector X contains individual-level and state-level controls. The individual-level controls included are the respondents' age, gender, and race. The state-level controls include clean air policies (Restrictions on indoor smoking), e-cigarette policies (Minimum age purchase laws, presence of e-cigarette tax, indoor bans on e-cigarette use), marijuana decriminalization laws, and state economic conditions (state unemployment rate and per capita personal income). The vectors τ_t and σ_s represent year and state fixed effects, respectively. For all the models, standard errors are clustered at the state level in each regression.

Cigarette and cigar taxes vary across states and over time. I use state and year fixed effects in my model. Using state fixed effects accounts for time-invariant unobservables that vary across states. The use of year fixed effects accounts for unobservables that vary over time but are common to all the states. After accounting for both state-specific time-invariant and year-specific effects, the identification comes from within-state variation in cigarette and cigar tax. This type of DID is also referred to as a "two-way" fixed effects model (FE). For the current analysis, I observe at least one cigar tax change in 36 states and at least one cigarette tax change for 47 states and DC (Except Montana, Nebraska, and North Dakota).

1.5 Results

This section presents the results of my analysis. The main results are presented in tables 3, 4, and 5. Column 1 in both panels is the naive regression that does not include any covariates other than the cigarette and cigar tax. Column 2 adds additional covariates, column 3 adds state and year fixed effects, while column 4 adds both. The covariates not reported in the tables are race, gender, age, smoke-free air restrictions, marijuana decriminalization laws, state per capita personal income, and the state unemployment rate. I use logit regressions and report marginal effects as well as elasticities for cigarette and cigar taxes.

Tobacco Taxes and Youth Cigarette Use

Table 1.3 presents the effect of cigarette and cigar taxes on cigarette use. I show the responsiveness of cigarette use to cigarette taxes in panel 1 and both cigarette and cigar taxes in panel 2.

Panel 1 of Table 1.3 shows that cigarette tax has a small effect on cigarette use. The first column indicates that a one-dollar increase in cigarette tax decreases cigarette use by 4.5 percentage points (pp). The point estimate shows that the correlation between cigarette taxes and cigarette use is large and statistically significant. The elasticity is -

0.474, which implies that a one-percent increase in cigarette tax leads to a 0.474 percent decrease in cigarette use. Column 2 shows that, by contrast, cigarette taxes have a small and positive effect on cigarette use when we include covariates. The effect is not statistically significant. The addition of state and year fixed effects in column 3 reduces the magnitude of the elasticity estimate to 0.095. The addition of covariates, and state and year fixed effects in column 4 reduce the point estimate and elasticity further. It shows that a one-dollar increase in cigarette tax leads to a 0.8 pp decrease in cigarette use, implying an elasticity of 0.084. The elasticity in column 4 is five times lower than the naive estimate from column 1, and covariates/fixed effects absorb most of the correlation. The implication from panel 1 is that increasing cigarette tax has a small effect in reducing cigarette use. The addition of cigar tax, in panel 2, reduces the own-tax elasticity of cigarette use and makes it statistically insignificant.

Panel 2 of Table 1.3 adds cigar tax as an additional covariate to each of the specifications from panel 1. The first column indicates that a 100 pp increase in cigar tax leads to a 1.5 pp decrease in cigarette use. Although the point estimate shows a negative correlation, it is statistically insignificant. The cross-tax elasticity is -0.029, which implies that a one-percent increase in cigar tax leads to a 0.029 percent decrease in cigarette use. Column 2 adds covariates to the naive specification and indicates an increase in size as well as precision of the point estimate. Column 3, which adds state and year fixed effects to column 1, shows that a 100 pp increase in cigar tax leads to a 2.8 pp decrease in cigarette use. The point estimate is statistically significant and implies a negative cross-tax elasticity of 0.054. The point estimate decreases slightly (0.027 compared to 0.028) in column 4 when I include covariates and fixed effects. The cross-tax elasticity is negative and statistically significant. It shows that a one-percent increase in cigar tax leads to a 0.052 percent decrease in cigarette use. The negative cross-tax elasticity implies that cigarettes and cigars are complements.

The main results from panel 1 and panel 2 have two important implications. First, the own-tax elasticity of cigarette use is small and negative, implying that further cigarette tax increases might not be effective in reducing cigarette use. The findings that youth cigarette use is price-inelastic is consistent with the recent literature (Anderson et al. (2020); Hansen et al. (2017); Carpenter and Cook (2008); DeCicca et al. (2008b)). Second, I find that the cross-tax elasticity of cigarette use with cigar tax is negative, implying that cigarettes and cigars are complements.

Table 1.3 Tobacco Taxes and Youth Cigarette Use

	(1)	(2)	(3)	(4)
	Dependent Variable: Cigarette Use			
Panel 1				
Cigarette Tax	-0.045***	-0.012***	-0.009***	-0.008***
	(0.005)	(0.004)	(0.002)	(0.002)
Elasticity	[-0.474]	[-0.126]	[-0.095]	[-0.084]
Panel 2				
Cigarette Tax	-0.043***	0.005	-0.006**	-0.004
	(0.008)	(0.005)	(0.003)	(0.003)
Elasticity	[-0.453]	[0.053]	[-0.063]	[-0.042]
Cigar Tax	-0.015	-0.106***	-0.028*	-0.027**
	(0.043)	(0.027)	(0.015)	(0.014)
Elasticity	[-0.029]	[-0.205]	[-0.054]	[-0.052]
Observations	867,375	867,375	867,375	867,375
Additional Covariates		✓		✓
State and Year Fixed Effects			✓	✓

Notes: The above table presents marginal effects from the logistic regression of cigarette use on cigarette/cigar tax, using the YRBS 1999-2017. Panel (1) uses cigarette tax as the main variable of interest and panel (2) uses both cigarette and cigar tax. Column (1) does not include individual and state-level covariates. Columns (2), (3), (4) add state, year fixed effects, and additional covariates. Covariates not reported are race, gender, age, marijuana decriminalization laws, smoke-free air restrictions, e-cigarette minimum age laws, e-cigarette tax, state per capita personal income, and the state unemployment rate. The elasticities of cigarette use with respect to cigarette/cigar tax are reported in square brackets. Standard errors are clustered at the state level and are reported in parentheses.

Tobacco Taxes and Youth Cigar Use

Table 1.4 presents the effect of cigarette and cigar taxes on cigar use. I show the responsiveness of cigar use to cigar taxes in panel 1 and both cigar and cigarette taxes in panel 2.

In panel 1 of Table 1.4, the first column indicates that a 100 pp increase in cigar tax decreases cigar use by 5.9 pp. The point estimate that shows the correlation between cigar taxes and cigar use is large and statistically significant. The elasticity is -0.171, which implies that a one-percent increase in cigar tax leads to a 0.171 percent decrease in cigar use. Column 2 shows that the elasticity decreases to -0.128 when covariates are included. The addition of state and year fixed effects in column 3 reduces the magnitude of the elasticity estimate to 0.043 and reverses the sign. The addition of covariates and state and year fixed effects in column 4 increases the point estimate and elasticity further. It shows that a 100 pp increase in cigar tax leads to a 1.6 pp decrease in cigar use, implying an elasticity of 0.046. The addition of cigarette tax as a covariate in panel 2 reduces the own-tax elasticity of cigar tax to -0.0058. The own-tax elasticity is small, negative, and statistically insignificant, implying that increasing cigar tax does not have a significant effect on cigar use.

Panel 2 of Table 1.4 adds cigarette tax as an additional covariate to each of the specifications from panel 1. The first column indicates that a one-dollar increase in cigarette tax leads to a 1.7 pp decrease in cigar use. The point estimate is statistically significant. The cross-tax elasticity is -0.269, which implies that a one-percent increase in cigarette tax leads to a 0.269 percent decrease in cigarette use. The point estimate changes sign and reduces to 0.4 pp in column 2 when I add covariates. Column 3, which adds state and year fixed effects to column 1, shows that a one-dollar increase in cigarette tax leads to a 0.7 pp increase in cigar use. The point estimate is statistically significant and implies a cross-tax elasticity of 0.11. The point estimate decreases in column 4 when I include covariates as well as fixed effects. It implies that a one-percent increase in cigarette tax leads to a 0.095 percent increase in cigar use. The positive cross-tax elasticity of cigar use implies that cigarettes and cigars as substitutes. This is in contrast to the cross-tax elasticity in the analysis of cigarette use (Table 1.4), which showed that cigarettes and cigars are complements.

The main results have two important implications. First, the own-tax elasticity of cigar use is small and negative, implying that cigar taxes are not effective in reducing cigar use. My findings are consistent with the recent estimates on the own-tax elasticity of cigar use by Hawkins et al. (2018). Second, the cross-tax elasticity of cigar use with cigarette tax is positive, implying that cigarettes and cigars are substitutes. This is in contrast to the previous table analyzing cigarette use, which implied that cigarettes and cigars are complements. The asymmetric cross-tax elasticities of cigarettes and cigars present an interesting paradox. In the next section, I show that the cross-tax elasticity of marijuana use with cigarette and cigar tax drives the asymmetric cross-tax elasticities of cigarette and cigar use.

Tobacco Taxes and Youth Marijuana Use

Table 1.5 presents the effect of cigarette and cigar tax on marijuana use. I show the responsiveness of marijuana use to cigarette taxes in panel 1, cigar taxes in panel 2, and both cigarette and cigar taxes in panel 3.

Panel 1 of Table 1.5 shows that if we do not account for cigar tax, cigarette tax

Table 1.4 Tobacco Taxes and Youth Cigar Use

	(1)	(2)	(3)	(4)
	Dependent Variable: Cigar Use			
Panel 1				
Cigar Tax	-0.059**	-0.044***	0.015	0.016
	(0.026)	(0.016)	(0.014)	(0.013)
Elasticity	[-0.171]	[-0.128]	[0.043]	[0.046]
Panel 2				
Cigar Tax	-0.014	-0.054***	-0.005	-0.002
	(0.020)	(0.020)	(0.014)	(0.013)
Elasticity	[-0.041]	[-0.156]	[-0.014]	[-0.0058]
Cigarette Tax	-0.017***	0.004	0.007**	0.006**
	(0.003)	(0.003)	(0.003)	(0.003)
Elasticity	[-0.269]	[0.063]	[0.111]	[0.095]
Observations	867,375	867,375	867,375	867,375
Additional Covariates		✓		✓
State and Year Fixed Effects			✓	✓

Notes: The above table presents marginal effects from the logistic regression of cigar use on cigar/cigarette tax, using the YRBS 1999-2017. Panel (1) uses cigar tax as the main variable of interest and panel (2) uses both cigar and cigarette tax. Column (1) does not include individual and state-level covariates. Columns (2), (3), (4) add state, year fixed effects, and additional covariates. Covariates not reported are race, gender, age, marijuana decriminalization laws, smoke-free air restrictions, e-cigarette minimum age laws, e-cigarette tax, state per capita personal income, and the state unemployment rate. The elasticities of cigar use with respect to cigar/cigarette tax are reported in square brackets. Standard errors are clustered at the state level and are reported in parentheses.

appears to have no effect on marijuana use. The first column indicates that a one-dollar increase in cigarette tax has no effect on marijuana use. Column 2 shows that, by contrast, cigarette taxes have a small positive effect on marijuana use when I include covariates. The point estimate is not statistically significant and implies an elasticity of 0.058. The addition of state and year fixed effects in column 3 reduces the magnitude of the elasticity estimate to 0.033. The point estimate from column 4 shows that a one-dollar increase in cigarette tax leads to a 0.4 pp increase in marijuana use. The effect is not statistically significant. The implication from panel 1 is that marijuana use is not affected by cigarette taxes, which is similar to the findings of Anderson et al. (2020).

Panel 2 of Table 1.5 shows that if we do not account for cigarette tax, cigar tax appears to have no effect on marijuana use. The first column indicates that a 100 pp increase in cigar tax decreases marijuana use by 0.9 pp. The point estimate is small and statistically insignificant. It implies that a one-percent increase in cigar tax leads to a 0.014 percent decrease in marijuana use. The point estimate and elasticity remain small and statistically insignificant in column 2 (Addition of covariates) and column 3 (Addition of state and year fixed effects). The point estimate and elasticity reduce further with the addition of both covariates and state and year fixed effects in column 4. It shows that a 100 pp increase in cigar tax leads to a 0.4 pp decrease in marijuana use. The point estimate is statistically insignificant. The implication from panel 2 is that cigar taxes do not affect marijuana use. I test the validity of this result using both cigarette and cigar tax as covariates in panel 3.

In panel 3 of Table 1.5, the estimation equation consists of both cigarette and cigar tax as covariates. The first column indicates that a one-dollar increase in cigarette tax leads to a 0.2 pp increase in marijuana use. The point estimate is not statistically significant. The effect increases in magnitude and precision with the addition of covariates or fixed effects or both. Column 4 shows that a one-dollar increase in cigarette tax increases marijuana use by 0.8 pp. The point estimate is statistically significant and implies that a one-percent increase in cigarette tax leads to a 0.06 percent increase in marijuana use. The positive cross-tax elasticity of marijuana use with cigarette tax implies that marijuana and cigarettes are substitutes. Notably, the cross-tax elasticity of marijuana use with cigarette tax is positive, similar to the cross-tax elasticity of cigar use with cigarette tax.

Panel 3 also shows that cigar tax has a negative effect on marijuana use. The naive estimate in column 1 indicates that a 100 pp increase in cigar tax leads to a 1.3 pp decrease in marijuana use. The effect increases in magnitude and precision with the addition of covariates or fixed effects. Column 4 shows that a 100 pp increase in cigar tax decreases marijuana use by 2.8 pp. The point estimate is statistically significant and implies that a one-percent increase in cigar tax leads to a 0.047 percent decrease in marijuana use. The negative cross-tax elasticity of marijuana use with cigar tax implies that marijuana and cigars are complements.

The main results from the table show that cigarette and cigar taxes have opposite effects on marijuana use. There are three important implications. First, the cross-tax elasticity of marijuana use with cigarette tax is positive, which is the same sign as the elasticity of cigar use with cigarette tax. Second, the elasticity of marijuana use with cigar tax is negative, which is the same sign as the elasticity of cigarette use with cigar tax. The cross-tax elasticities imply that marijuana and cigarettes are substitutes, while marijuana and cigars are complements. Marijuana use drives the cross-tax elasticities of cigarette and cigar use, providing a potential explanation for the asymmetric cross-tax elasticities of cigarettes and cigars. If true, it would suggest that tobacco use has evolved into a form of marijuana use. Failure to account for marijuana consumption in the estimation of cigarette and cigar use will lead to omitted variable bias in the elasticity estimates of cigarette and cigar use.

Sensitivity Analysis

I test for alternative factors that could explain the asymmetric sign of the cross-tax elasticity of cigarette and cigar use.

Table 1.5 Tobacco Taxes and Youth Marijuana Use

	(1)	(2)	(3)	(4)
	Dependent Variable: Marijuana Use			
Panel 1				
Cigarette Tax	0.000	0.007	0.004	0.004
	(0.005)	(0.006)	(0.004)	(0.004)
Elasticity	[0.00]	[0.058]	[0.033]	[0.033]
Panel 2				
Cigar Tax	-0.009	-0.014	-0.008	-0.004
	(0.025)	(0.024)	(0.020)	(0.019)
Elasticity	[-0.014]	[-0.021]	[-0.012]	[-0.0061]
Panel 3				
Cigarette Tax	0.002	0.016***	0.009***	0.008*
	(0.006)	(0.006)	(0.003)	(0.004)
Elasticity	[0.017]	[0.133]	[0.075]	[0.066]
Cigar Tax	-0.013	-0.052*	-0.035***	-0.029***
	(0.034)	(0.029)	(0.012)	(0.010)
Elasticity	[-0.019]	[-0.079]	[-0.053]	[-0.043]
Observations	867,375	867,375	867,375	867,375
Additional Covariates		✓		✓
State and Year Fixed Effects			✓	✓

Notes: The above table presents marginal effects from the logistic regression of marijuana use on cigar/cigarette tax, using the YRBS 1999-2017. Panel (1) and (2) present results using the cigarette tax and cigarette tax as the main variables of interest, respectively. Panel (3) uses both cigarette and cigar tax as covariates. Column (1) does not include individual and state-level covariates. Columns (2), (3), (4) add state, year fixed effects, and additional covariates. Covariates not reported are race, gender, age, marijuana decriminalization laws, smoke-free air restrictions, e-cigarette minimum age laws, e-cigarette tax, state per capita personal income, and the state unemployment rate. The elasticities of marijuana use with respect to cigar/cigarette tax are reported in square brackets. Standard errors are clustered at the state level and are reported in parentheses.

1.5.0.1 Controlling for Marijuana Legalization

Table 1.6 shows the elasticities of cigarette, cigar, and marijuana use with cigarette and cigar tax, after controlling for recreational and medicinal marijuana legalization[6]. I use legalization laws as a proxy for marijuana prices, as they drive up the black market price of marijuana, making it more costly to supply illegally to minors (Anderson and Elsea (2015); Anderson et al. (2019, 2020)). The cross-tax elasticity of cigarette use with cigar tax is negative, though it is no longer statistically significant. The cross-tax elasticity of cigar

[6]Data on Marijuana Legalization provided by Smart and Pacula (2019)

use is positive and significant, similar to the main specification in Table 1.4. The cross-tax elasticity of marijuana use with cigarette and cigar tax is consistent with the main results. The elasticity of marijuana use with cigarette tax is positive and significant, suggesting that cigarettes and marijuana are substitutes. The elasticity of marijuana use with cigar tax is negative, suggesting that marijuana and cigars are complements. Controlling for marijuana legalization does not change the asymmetric signs of the cross-tax elasticities of cigarette, cigar, and marijuana use.

Table 1.6 Tobacco Taxes and Youth Smoking: Controlling for MLL

	(1) Cigarette	(2) Cigar	(3) Marijuana
Cigarette Tax	-0.002	0.008^{**}	0.009^{**}
	(0.003)	(0.003)	(0.004)
Elasticity	[-0.021]	[0.126]	[0.075]
Cigar Tax	-0.026	-0.002	-0.031^{***}
	(0.015)	(0.014)	(0.010)
Elasticity	[-0.050]	[-0.0058]	[-0.047]
Observations	867,375	867,375	867,375

Notes: The above table presents marginal effects from the logistic regression of cigarette/cigar/marijuana use on cigarette and cigar tax. All columns use state fixed effects, year fixed effects, and additional covariates. Covariates not reported are race, gender, age, marijuana decriminalization laws, smoke-free air restrictions, state per capita personal income, state unemployment rate, e-cigarette minimum age laws, dummy for e-cigarette tax, medicinal marijuana laws (MML), recreational marijuana laws (RML). The elasticity is reported in square brackets. The standard errors are clustered at the state level and are reported in parentheses.

1.5.0.2 Tobacco Taxes and Youth Smoking: YRBS 2007-2017

Table 1.7 shows results separately for the years 1999-2005 and 2007-2017. I find that cigarette taxes have no significant effect in reducing cigarette use for the years 2007-2017, which is consistent with recent studies (Anderson et al. (2020); Hansen et al. (2017)). For the years 2007-2017, the cross-tax elasticity of cigar use with cigarette tax for the later years is 18.

That is, a one-percent increase in cigarette tax leads to an 0.18 percent increase in cigar use. Consistent with previous results, the effect of cigarette tax on marijuana use is similar to its effects on cigar use. A one-percent increase in cigarette tax leads to a 0.104 percent increase in marijuana use. The results imply that switching to cigars and marijuana from cigarettes for the full sample is driven by cigarette tax hikes during 2007-2017. The cross-tax elasticity of cigarette and marijuana use is negative and statistically insignificant for both sets of sample years. In sum, the results are consistent with the hypothesis that cigarette taxes have lost their bite in reducing cigarette use (Anderson et al. (2020); Hansen et al. (2017)). Additionally, rising cigarette taxes contribute to an increase in cigar and marijuana use among youth.

Table 1.7 Tobacco Taxes and Youth Smoking: 1999-2005 vs. 2007-2017

	(1)	(2)	(3)	(4)	(5)	(6)
	1999-2005			2007-2017		
	Cigarette	Cigar	Marijuana	Cigarette	Cigar	Marijuana
Cigarette Tax	-0.005	-0.004	-0.007	0.003	0.009*	0.011*
	(0.009)	(0.006)	(0.007)	(0.006)	(0.005)	(0.006)
Elasticity	[-0.019]	[-0.027]	[-0.029]	[0.045]	[0.18]	[0.104]
Cigar Tax	-0.083	-0.036	-0.035	-0.013	-0.012	-0.031
	(0.060)	(0.039)	(0.046)	(0.020)	(0.021)	(0.020)
Elasticity	[-0.090]	[-0.069]	[-0.042]	[-0.034]	[-0.041]	[-0.051]
Observations	201,921	201,921	201,921	665,454	665,454	665,454

Notes: The above table presents marginal effects from the logistic regression of cigarette/cigar/marijuana use on cigarette and cigar tax. Columns (1)-(3) use the YRBS 1999-2005. Columns (4)-(6) use the YRBS 2007-2017. All columns use state fixed effects, year fixed effects, and additional covariates. Covariates not reported are race, gender, age, marijuana decriminalization laws, smoke-free air restrictions, state per capita personal income, state unemployment rate, e-cigarette minimum age laws, dummy for e-cigarette tax, medical marijuana laws (MML), recreational marijuana laws (RML). The elasticity is reported in square brackets. The standard errors are clustered at the state level and are reported in parentheses.

1.5.0.3 Tobacco Taxes and Youth Smoking: By Gender and Race

Table 1.8 shows the effect of cigarette and cigar tax on cigarette and cigar use by gender. For females, the elasticity of cigarette use with cigarette tax is negative and statistically significant. The elasticity of cigar and marijuana use with cigarette tax is positive and statistically significant. A one-percent increase in cigarette tax leads to an 0.18 percent increase in cigar use and a 0.06 percent increase in marijuana use. The cross-tax elasticity of cigarette use with cigar tax is negative, but it is statistically insignificant. Cigar taxes do not have a significant effect on cigarette, cigar, or marijuana use among females. Among males, an increase in cigarette taxes does not affect cigarette use but leads to an increase in cigar and marijuana use. An increase in cigar tax has a small and negative effect on cigarette, cigar, and marijuana use. The cross-tax elasticity of cigarettes and cigars is of opposite signs and is statistically significant. This suggests that higher cigarette taxes lead both males and females to switch from cigarettes to cigars and marijuana.

Table 1.9 shows the effect of cigarette and cigar tax on cigarette and cigar use by race. Among blacks, an increase in cigarette tax has no effect on cigarette use, but it leads to an increase in cigar and marijuana use. A one-percent increase in cigarette tax leads to a 0.28 percent increase in cigar use and a 0.15 percent increase in marijuana use. The cross-tax elasticities are large compared to the elasticity for whites or hispanics. The effect of cigar tax on cigarette and cigar use is small, negative, and statistically insignificant. The cross-tax elasticity of marijuana use with cigar tax is negative and statistically significant, suggesting that marijuana and cigars are complements. Among whites, cigarette taxes do not affect cigarette use but lead to an increase in cigar and marijuana use. The effect of cigar tax on cigarette, cigar, and marijuana use is small and negative. Among hispanics, higher cigarette taxes lead to an increase in cigar and marijuana use. In sum, higher cigarette tax leads to switching from cigarettes to cigar and marijuana. All groups consider cigar and marijuana as complements, and cigarettes and marijuana as substitutes.

Table 1.8 Tobacco Taxes and Youth Smoking: By Gender

	(1)	(2)	(3)	(4)	(5)	(6)
		Female			Male	
	Cigarette	Cigar	Marijuana	Cigarette	Cigar	Marijuana
Cigarette Tax	-0.008***	0.008***	0.007*	0.004	0.010***	0.010**
	(0.002)	(0.003)	(0.005)	(0.003)	(0.004)	(0.004)
Elasticity	[-0.090]	[0.181]	[0.061]	[0.039]	[0.113]	[0.075]
Cigar Tax	-0.014	0.002	-0.023	-0.032**	-0.007	-0.039**
	(0.018)	(0.017)	(0.016)	(0.014)	(0.018)	(0.016)
Elasticity	[-0.029]	[0.0083]	[-0.037]	[-0.058]	[-0.014]	[-0.054]
Observations	449,588	449,588	449,588	417,787	417,787	417,787
Mean	0.14	0.16	0.07	0.14	0.18	0.21

Notes: The above table presents marginal effects from the logistic regression of cigarette/cigar/marijuana use on cigarette and cigar tax, for males and females. Columns (1)-(3) show the estimates for the sample of females and columns (4)-(6) show the estimates for males. All columns use state fixed effects, year fixed effects, and additional covariates. Covariates not reported are race, gender, age, marijuana decriminalization laws, smoke-free air restrictions, state per capita personal income, state unemployment rate, e-cigarette minimum age laws, dummy for e-cigarette tax, medical marijuana laws (MML), recreational marijuana laws (RML). The elasticity is reported in square brackets. The standard errors are clustered at the state level and are reported in parentheses.

Table 1.9 Tobacco Taxes and Youth Smoking: By Race

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Black			White			Hispanic		
	Cigarette	Cigar	Marijuana	Cigarette	Cigar	Marijuana	Cigarette	Cigar	Marijuana
Cigarette Tax	-0.000	0.016***	0.019***	-0.003	0.008***	0.004*	0.002	0.011**	0.017**
	(0.004)	(0.005)	(0.006)	(0.003)	(0.003)	(0.002)	(0.005)	(0.005)	(0.007)
Elasticity	[0.00]	[0.279]	[0.149]	[-0.026]	[0.108]	[0.033]	[0.029]	[0.206]	[0.151]
Cigar Tax	-0.012	-0.005	-0.067**	-0.031*	-0.014	-0.021*	-0.008	0.000	-0.002
	(0.018)	(0.028)	(0.028)	(0.016)	(0.011)	(0.012)	(0.020)	(0.021)	(0.026)
Elasticity	[-0.036]	[-0.013]	[-0.080]	[-0.051]	[-0.036]	[-0.033]	[-0.021]	[0.00]	[-0.0032]
Observations	123,910	123,910	123,903	495,973	495,973	495,973	132,036	132,036	132,036

Notes: The above table presents marginal effects from the logistic regression of cigarette/cigar/marijuana use on cigarette and cigar tax, for blacks, whites, and hispanics. Columns (1)-(3) show the estimates for the sample of blacks, columns (4)-(6) show the estimates for whites, and columns (7)-(9) show the estimates for males. All columns use state fixed effects, year fixed effects, and additional covariates. Covariates not reported are race, gender, age, marijuana decriminalization laws, smoke-free air restrictions, state per capita personal income, state unemployment rate, e-cigarette minimum age laws, dummy for e-cigarette tax, medical marijuana laws (MML), recreational marijuana laws (RML). The elasticity is reported in square brackets. The standard errors are clustered at the state level and are reported in parentheses.

1.6 Conclusion

Since the 1990s, the focus of tobacco control policy is almost exclusively on reducing cigarette use. Cigarette taxes have increased substantially while cigar taxes have not caught up. It has created widening disparities in the price of cigarettes and cigars. Most prior research has analyzed changes in smoking exclusively through cigarette taxes and cigarette use, without much attention to cigar tax and cigar use. Using the National and State YRBS, I examine the effect of cigarette and cigar taxes on cigarette and cigar use among youth. I find that cigarette taxes marginally reduce cigarette use but increase cigar and marijuana use. Increases in cigar taxes have an even smaller effect on reducing cigarette and cigar use. Moreover, switching from cigarettes to cigars has been more prominent since the last decade, when larger cigarette tax increases took place. These results have important implications for policymakers, which I summarize below.

First, future increase in cigarette tax will not lead to a reduction in cigarette or cigar use. On the contrary, a further increase in cigarette tax is likely to lead to an increase in cigar use. Second, prior increase in cigar tax has not reduced cigarette or cigar use significantly among youth. As cigarettes become more expensive, cigars provide young smokers with an economically attractive alternative. Regulations on cigars are weaker than cigarettes, and cigar tax increases have been infrequent. Cigars offer a similar nicotine delivery system as cigarettes, are available in sweet flavors, and are significantly cheaper than cigarettes. Cigars allow young smokers to consume tobacco in a staggered manner as they come in smaller packs (two cigars compared to 20 cigarettes). Additionally, cigars are rolled in tobacco leaf (as opposed to cigarettes rolled in paper), making them an attractive complement to smoke on-the-go with marijuana. An effective policy to reduce cigarette and cigar use among youth would be to increase the cost of cigars (regulations on flavor, size, packs, and higher taxes). It is unlikely that stricter regulations on cigars will lead smokers to switch from cigars to cigarettes (cigarettes being five times as expensive as cigars per unit). Additionally, policymakers need to consider that the high prevalence of marijuana and its complementary relationship with cigars lowers the effectiveness of tobacco taxes in reducing cigar use.

This study contributes to the growing literature on tobacco control policy and its spillover effects on cigarette alternatives and marijuana. Unlike previous studies, I take into account different tobacco taxes to establish the relationship between cigarettes and cigars. However, this study suffers from some caveats that provide directions for future research. First, I assume that all individuals have similar tastes and can switch between cigarettes and cigars. It is possible that individuals have different tastes, and some non-price-sensitive cigarette users do not consider cigars as a viable alternative. This can be understood by estimating a full mixture model, with price-sensitive and non-price-sensitive cigarette users as two sub-groups. Second, although I use the information on cigarette tax, cigar tax, and marijuana legalization, I am unable to control for marijuana prices due to a lack of reliable data. An important extension would be to see how marijuana prices affect cigarette, cigar, and marijuana use. Third, the information in the YRBS data is self-reported, with potential misreporting in tobacco or marijuana use. Future research should account for misreporting and explore how tobacco and marijuana use is affected by tobacco taxes. Fourth, I focus on the short-term effects of tobacco taxes. There is a need for future research to understand the long-term relationship between tobacco taxes, tobacco, and marijuana use. Fifth, electronic cigarettes are a growing alternative to cigarettes among youth. It is essential to understand how the prevalence of e-cigarettes and related anti-smoking policies affect cigarette, cigar, and marijuana use among youth.

Chapter 2

Does Smoking Reduce Obesity? Reconciling the Conflicting Evidence

2.1 Introduction

Smoking and obesity are the two leading causes of preventable deaths in the United States.[1] Almost 40% of adults in the United States are obese (Hales et al. (2017)), while roughly 14% of adults in the U.S. smoke according to the CDC.[2] In Figure 2.1, we show that smoking decreased from roughly 22% to 15% between 2002 and 2017 according to the Behavioral Risk Factor Surveillance System (BRFSS), while obesity rose from roughly 26.75% to 28.25% during the same time period. While these trends do not present causal evidence, they raise the question of whether the decline in smoking has contributed to rising obesity. If so, the health benefits of anti-smoking policies may be somewhat overstated.

Using Grossman's human capital model of health, we can consider health a capital stock that depreciates over time but can be replenished by investment (Grossman, 1972). Assuming an individual starts at her optimal body weight, both smoking and weight gain represent disinvestments. Cigarettes contain nicotine, which suppresses appetite and increases one's metabolic rate. Thus, smoking cigarettes tends to reduce body weight, all else equal. However, smoking also reduces lung capacity, which may restrict physical activity and thereby increase body weight. Because the causal effect of smoking on obesity is theoretically ambiguous, determining the true relationship is an empirical question.

There exists a large literature examining the causal relationship between smoking and

[1]See `https://www.cdc.gov/tobacco/data_statistics/fact_sheets/health_effects/tobacco_related_mortality/index.htm` and `https://www.commonwealthfund.org/blog/2018/rising-obesity-united-states-public-health-crisis`

[2]See `https://www.cdc.gov/tobacco/data_statistics/fact_sheets/fast_facts/index.htm`, last accessed February 19, 2020

obesity, though the findings are quite mixed. Using BRFSS data from 1984 to 1999, Chou et al. (2004) suggest that reduced smoking increases body mass index (BMI) by finding increasing cigarette prices have a large positive effect on BMI. Using BRFSS data from 1984 to 2002/2005, Gruber and Frakes (2006) and Courtemanche (2009) examine this relationship more closely using cigarette taxes instead of cigarette prices.[3] The authors find that an increase in cigarette taxes (i.e. reduced smoking) *reduces* BMI. However, several other studies around the same time use different observational data or approaches to causal identification to suggest that reduced smoking increases BMI (Baum, 2009; Fang et al., 2009; Liu et al., 2010). Moreover, Courtemanche et al. (2018) also find that reduced smoking increases BMI using a randomized trial of smoking cessation treatments and clinically-measured carbon monoxide levels.

The mixed findings highlight the complexity of identifying the causal effect of smoking on BMI. In particular, two concerns stand out from the literature. First, estimates are sensitive to different approaches to identification and choice of specification. Second, estimates are sensitive to the source of data, which raises the concern that results obtained from self-reported data (as opposed to clinically-measured experimental data) are biased by systematic misreporting. For example, studies have found that accounting for misreporting can significantly impact estimates of marijuana usage, as well as the magnitude and direction of estimated effects of nutritional assistance programs on BMI and obesity (Greene et al., 2017; Almada et al., 2016; Nguimkeu et al., 2019). A large body of medical research has revealed widespread underreporting based on comparisons of self-reported smoking to biochemical

[3]Note that Courtemanche (2009) actually uses BRFSS data to examine the robustness of his central estimates obtained using the National Longitudinal Survey of Youth. Indeed, the author finds that both data sources yield similar estimates.

indicators of tobacco intake (e.g., Gorber et al., 2009; Nesson, 2017). To our knowledge, no one has examined how misreporting affects estimates of the impact of smoking on BMI.

In this paper, we aim to reconcile the mixed findings in the existing literature. To do this, we use BRFSS data from 2002 to 2017 to examine how specification choice and accounting for misreporting affect estimates of the effect of smoking on BMI. In particular, we employ a variety of linear and non-linear instrumental variables approaches, using cigarette taxes as plausibly exogeneous shifters of smoking behavior. To account for misreporting, we use the 2-step estimation procedure proposed by Nguimkeu et al. (2019). First, we replicate the findings of Gruber and Frakes (2006) and Courtemanche (2009) using our more recent data, obtaining estimates suggesting that reduced smoking reduces BMI. Second, we show that using a non-linear first stage yields estimates of roughly the same magnitude but the sign is flipped, suggesting that reduced smoking *increases* BMI. Finally, using a non-linear first stage and accounting for misreporting, we obtain estimates consistent with the experimental literature in both sign and magnitude. These findings suggest that both specification choice and accounting for misreporting may help reconcile the discrepancies among earlier studies. Our preferred estimate suggests that quitting smoking results in a 1.06 increase in BMI. From 2002-17, our findings suggest that the decline in smoking explains roughly 6% of the concurrent rise in obesity, reinforcing the notion that increased obesity may be an unintended consequence of anti-smoking policy efforts.

The rest of the paper is organized as follows. Section 2 provides a more detailed review of the literature. Section 3 describes our data. Section 4 develops our various approaches to estimation and presents our results. Section 5 concludes.

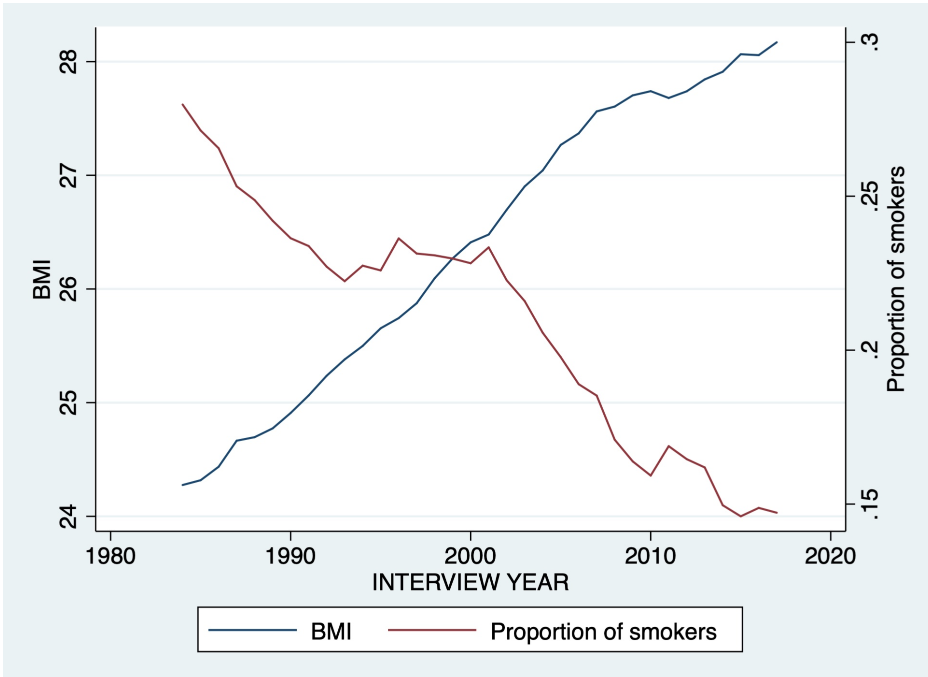


Figure 2.1 Trends in Smoking and Obesity (BRFSS 2002-2017)

2.2 Literature Review

Instrumental variables (IV) approaches are common in the literature on the relationship between smoking and body weight. This is because smoking behavior may be endogenous, as individuals generally self-select into smoking. There are also concerns about reverse causality. That is, body weight may affect an individual's decision of whether or not to smoke (Cawley et al., 2004; Rees and Sabia, 2010). The literature has primarily used cigarette prices and taxes as plausibly exogeneous instruments, as they shift the cost of smoking but do not seem to have a direct causal relationship with BMI.

In Table 2.3, we catalog the main estimates from earlier literature on the relationship between smoking and BMI. First, Chou et al. (2004) use BRFSS data from 1984 to 1999 to estimate the determinants of BMI and obesity, and find large positive effects of cigarette prices. Assuming cigarette prices affect individuals only through smoking, these results suggest that reductions in smoking contribute to increases in body weight. In contrast, using BRFSS data from 1984 to 2002 and cigarette taxes instead of prices, Gruber and Frakes (2006) examine this question more closely and find that reduced smoking reduces BMI and obesity rates. The authors propose two possible explanations for why their results differ from those of Chou et al. (2004). First, taxes may be better instruments than prices, as variation in prices may be driven by factors affecting both BMI and demand for cigarettes. Second, Gruber and Frakes (2006) use month-year fixed effects rather than quadratic time trends to more flexibly control for unobservable location-invariant shocks over time. However, the authors caution that their IV estimates may be implausibly large, as they suggest that individuals who quit smoking are 56% less likely to be obese. Courtemanche (2009) revisits Gruber and Frakes (2006) using both BRFSS and the National Longitudinal Survey of Youth (NLSY), and also finds that reduced smoking reduces BMI and obesity rates. Moreover, the author shows that this result is robust to a range of alternative linear specifications.

Other studies around the same time, however, challenge these findings. Baum (2009) use the NLSY for years 1979 through 2002 to estimate the body-weight effects of real cigarette prices and taxes. Using a standard differences-in-differences approach, they find that an increase in the cost of cigarettes (i.e. a reduction in smoking) increases BMI and obesity rates. Fang et al. (2009) use data from the China Health and Nutrition Survey (CHNS), leveraging number of cigarettes smoked and the local per-pack price of the most popular cigarette brand as IVs. Similarly, they find that reduced smoking increases BMI, though they find small and statistically insignificant effects on obesity rates. Liu et al. (2010) use worksite smoking bans along with BRFSS data from 1998 to 2006 to find further evidence suggesting reduced smoking increases obesity rates.

Table 2.1 Estimates from Previous Literature: BMI

Study	Indep. Var.	Data	Method	Estimate	Interpretation
Smoking Increases BMI					
Gruber and Frakes (2006)	Smoking Status	BRFSS 1984-2002	2SLS	5.705	Effect of smoking using cigarette taxes as an instrument
Courtemanche (2009)	Cigarette Prices	BRFSS 1984-2005	OLS	-0.234	Effect of a $1 increase in real price of cigarettes
Courtemanche (2009)	Cigarette Taxes	NLSY 1979	OLS	-0.343	Effect of a $1 increase in in real cigarette taxes
Smoking Decreases BMI					
Chou et al. (2004)	Cigarette Prices	BRFSS 1984-2002	OLS	0.486	Effect of a $1 increase in real price of cigarettes
Baum (2009)	Cigarette Taxes	NLSY 1979	OLS	0.328	Effect of a $1 increase in real cigarette taxes
Baum (2009)	Cigarette Prices	NLSY 1979	OLS	0.162	Effect of a $1 increase in real price of cigarettes
Fang et al. (2009)	Smoking Status	China Health and Nutrition Survey 2006	2SLS	-0.124	Effect of a one-cigarette increase in smoking (instruments: community smoking rate, cigarette prices)
Liu et al. (2010)	Smoking Status	BRFSS 1998-2006	2SLS	-2.054	Effect of workplace smoking bans
Courtemanche et al. (2018)	Smoking Status	Lung Health Study	RCT/2SLS	-2.202	Effect of smoking cessation treatment

Moreover, a more recent study by Courtemanche et al. (2018) uses clinical measurements from an experimental setting to provide additional consistent evidence. In particular, they use data from a randomized trial of smoking cessation treatments. Rather than relying on self-reported smoking, which seems to suffer from widespread underreporting, they observe clinically-measured carbon monoxide levels. They find that, in the short run, quitting smoking leads to a gain of 10-11 pounds at average height. In the long run, this effect increases to 11-12 pounds. However, this study focuses on individuals who are trying to quit smoking during the years 1990-1994, which some may argue casts doubt on the external validity of the findings.

If nothing else, the mixed evidence highlights the challenges inherent in estimating the causal relationship between smoking and body weight. In the following sections, we aim to reconcile these seemingly contradictory results. In particular, we argue that specification choice, functional form, and misreporting may explain the apparent discrepancies across previous studies.

2.3 Data

We start with repeated cross-sectional data from the 2002 through 2017 waves of the BRFSS, which is a health-focused telephone survey conducted by the CDC. The survey collects information on health-related risky behaviors among U.S. residents, as well as chronic health conditions and use of preventative services. The BRFSS was first established in 1984, with 12,258 participants across 14 states. In 2017, the data include approximately 430,000 participants from all 50 states and the District of Columbia. Starting in 2002, the BRFSS contains information on interviewer performance and whether a respondent fully or partially completed the interview. Because we use this information to account for possible misreporting, we restrict our main estimation sample to the years 2002-2017.

In particular, we use the core module that includes self-reported height, weight, demographics, tobacco use, health status, and chronic health conditions. Following Gruber and Frakes (2006), we restrict our sample to respondents under 65 years of age. After also dropping observations with missing data for our key variables of interest, our sample contains roughly 3.6 million observations. We calculate BMI using respondents' self-reported height and weight, removing extreme outliers by dropping the bottom and top 1% of BMI values, which leaves us with a range of 17.75 to 46.39. Using CDC guidelines, we define a current smoker as one who reported smoking at least 100 cigarettes during their lifetime, and, at the time of participation, reported that they currently smoke either some days or every day. We also use several additional individual-level covariates, including marital status, race, education, and income. As in Gruber and Frakes (2006), we include age-by-gender group indicators.

We supplement our BRFSS data with information on state excise cigarette taxes from the CDC's Tax Burden on Tobacco (TBOT) dataset. The TBOT reports tax rates at the end of October every year, along with the exact date(s) of any rate changes, which we use to determine monthly tax rates. We convert nominal tax rates to 2017 dollars using the Consumer Price Index for All Urban Consumers (CPI-U). Because the BRFSS asks individuals about their behavior over the past 30 days, we follow the previous literature in assigning tax rates to individuals based on the month preceding their survey interview. Finally, we include a time-varying measure of state-level unemployment from the Bureau of

Labor Studies to account for economic conditions that may be correlated with body weight, smoking, and/or cigarette taxes.

In Table 2.2, we report the summary statistics for the BRFSS 2002-2017, adjusted for the stratified sampling weights provided by the CDC. Using survey weights is important in this context, as BRFSS oversamples different population groups across different years depending on the guidance from the CDC. In our sample, 18% of respondents are identified as smokers. The average BMI among smokers is 27.11, which is lower than the average non-smoker BMI of 27.48. In addition, we see that 66% of individuals are white, 11% are black, and 6% are hispanic. In addition, 51% of individuals are female, 28-29% have a high school and/or technical degree, and 32% are college graduates.

In the sample, the mean (real) cigarette excise tax is $2.14. The highest observed nominal tax is $4.35 in New York and Connecticut in 2017, while the lowest in 2017 is $0.17 in Missouri (see Table 2.3). In addition to substantial across-state variation in tax magnitudes, taxes vary substantitally across time within states. Between 2002 and 2017, there were 132 nominal tax increases.

Finally, the variable we use to account for misreporting is called "Final Disposition" in the BRFSS data. This indicates whether the respondent fully completed their interview, and we use it to predict whether an individual is less likely to answer questions truthfully. We find that approximately 10% of respondents in our sample only partially complete their interview.

Table 2.2 Summary Statistics

	Non-Smoker		Smoker		Full Sample	
	Mean	SD	Mean	SD	Mean	SD
BMI	27.77	5.47	27.18	5.41	27.65	5.46
Current Cigarette Smoker	-	-	-	-	0.21	0.41
Cigarette Tax (2017 $)	2.25	1.06	2.10	1.03	2.22	1.06
Male	0.43	0.49	0.46	0.50	0.43	0.50
Black	0.08	0.28	0.09	0.29	0.08	0.28
White	0.78	0.42	0.77	0.42	0.78	0.42
Hispanic	0.08	0.27	0.06	0.24	0.07	0.26
Married	0.63	0.48	0.43	0.49	0.59	0.49
Divorced	0.13	0.34	0.23	0.42	0.15	0.36
Widowed	0.03	0.17	0.05	0.21	0.03	0.18
Grade 9-11	0.03	0.18	0.10	0.31	0.05	0.22
High School	0.24	0.43	0.38	0.49	0.27	0.44
Technical Degree	0.27	0.45	0.31	0.46	0.28	0.45
College Graduate	0.44	0.50	0.18	0.38	0.38	0.49
Disposition Rate	0.93	0.26	0.92	0.27	0.93	0.26
Income (2017 $)	69,497	38,164	48,445	35,449	65,088	38,575
State Unemployment Rate	5.96	2.09	5.97	2.04	5.96	2.08
Observations	2,843,312		753,331		3,596,643	

2.4 Estimation and Results

In this section, we start by obtaining naïve OLS estimates of the relationship between smoking and BMI via OLS, and then estimate a variety of IV specifications. Throughout, we follow the previous literature in specifying the following structural linear model of the relationship between BMI and smoking:

$$BMI_{isy} = \alpha S_{isy} + X_{isy}\beta + \tau_y + \mu_s + \epsilon_{isy} \tag{2.1}$$

where S indicates current smoking status for individual i in state s and month-year y. X is a vector of individual- and state-level covariates, τ_y are month-year fixed effects, μ_s are state fixed effects, and η_{isy} is an idiosyncratic error term. X includes individuals' income, race, marital status, education, age, gender, an age-by-gender interaction term, and state unemployment rate from the BLS.

In the first row, second panel of Table 2.4, we find that the naïve OLS estimate of the

Table 2.3 Variation in Nominal Cigarette Taxes (2002-2017)

State	Tax in January 2002 ($)	Tax in Dec 2017 ($)	No. of increases
Alabama	0.165	0.675	2
Alaska	1	2	3
Arizona	0.60	2	2
Arkansas	0.34	1.15	2
California	0.87	2.87	1
Colorado	0.20	0.84	1
Connecticut	0.50	4.35	8
Delaware	0.24	2.10	4
District of Columbia	0.65	2.50	2
Florida	0.339	1.339	1
Georgia	0.12	0.37	1
Hawaii	1	3.2	9
Idaho	0.28	0.57	1
Illinois	0.58	1.98	2
Indiana	0.155	0.995	2
Iowa	0.36	1.36	1
Kansas	0.24	1.29	3
Kentucky	0.03	0.60	3
Louisiana	0.24	1.08	3
Maine	1	2	1
Maryland	0.66	2	2
Massachusetts	0.76	3.51	3
Michigan	0.75	2	2
Minnesota	0.48	3.04	5
Mississippi	0.18	0.68	1
Missouri	0.17	0.17	0
Montana	0.18	1.70	2
Nebraska	0.34	0.64	1
Nevada	0.35	1.80	2
New Hampshire	0.52	1.78	5
New Jersey	0.80	2.70	5
New Mexico	0.21	1.66	2
New York	1.11	4.35	3
North Carolina	0.05	0.45	3
North Dakota	0.44	0.44	0
Ohio	0.24	1.60	3
Oklahama	0.23	1.03	1
Oregon	0.68	1.32	4
Pennsylvania	0.31	2.60	4
Rhode Island	1	4.25	7
South Carolina	0.07	0.57	1
South Dakota	0.33	1.53	2
Tennessee	0.13	0.62	2
Texas	0.41	1.41	1
Utah	0.515	1.70	2
Vermont	0.44	3.08	7
Virginia	0.025	0.30	2
Washington	1.425	3.025	3
West Virginia	0.17	1.20	2
Wisconsin	0.77	2.52	2
Wyoming	0.12	0.60	1

relationship between smoking and BMI is negative. For OLS, and all our other estimation approaches, we present three specifications. Column 1 presents the results when we only include state and month-year fixed effects. Column 2 presents the results when we only include the set of covariates (income, race, marital status, educational attainment, age-by-gender dummies, and state unemployment rate). Column 3 presents the results when including both the fixed effects and covariates. In our preferred specification, column 3, we estimate that quitting smoking is associated with a BMI increase of 0.7 (2.5% relative to average BMI of 27.65). For someone who is 5' 7" tall and 180 pounds (BMI = 28.2), this corresponds to an increase of roughly 4.5 pounds.

Our first IV approach is a standard two-stage least squares (2SLS) approach, which follows Gruber and Frakes (2006) closely. Next, we obtain estimates from two alternative IV approaches where the first stage is non-linear. The first is a corrected version of the "forbidden regression." The second is a control function approach. Finally, we implement a 2-step estimator proposed by Nguimkeu et al. (2019) to account for the possibility of misreporting. Following the large economic literature on smoking, we use cigarette taxes to instrument for smoking status throughout these analyses. In addition, we weight our regressions throughout by the BRFSS-provided stratified sampling weights.[4]

Two-Stage Least Squares

In this approach, following Gruber and Frakes (2006), the first stage is specified as a linear probability model:

$$S_{isy} = \psi Tax_{sy} + X_{isy}\phi + \tau_y + \mu_s + \eta_{isy} \tag{2.2}$$

Tax is the identifying instrument, which reflects the real excise cigarette tax (in 2017 $). The corresponding second stage is given by:

$$BMI_{isy} = \alpha \hat{S}_{isy} + X_{isy}\beta + \tau_y + \mu_s + \epsilon_{isy} \tag{2.3}$$

[4]See appendix for non-weighted estimates.

where $\hat{S}$ is predicted smoking status from the first stage, and α is the causal parameter of interest.

Table 2.4 presents the results from the analysis using 2SLS. In the first stage (row 1, top panel, column 3), we estimate that a \$1 increase in the real cigarette excise tax significantly reduces the probability of being a smoker by 0.6 percentage points. We also present the F-statistic, which is 16.4 for this specification, suggesting that cigarette taxes are indeed a strong predictor of smoking behavior. Note that we do not find a statistically significant direct relationship between BMI and cigarette taxes, where the reduced form estimate presented in column 3 of the middle panel suggests that a \$1 tax increase statistically insignificantly reduces BMI by 0.025. In row 2, column 3 of the bottom panel, our 2SLS estimate suggests that quitting smoking reduces BMI by roughly 4.2 (or a reduction of 27 pounds for someone 5' 7" tall and 180 pounds). While the standard error of the 2SLS estimate is roughly 50% larger than the coefficient, the magnitude is in line with the estimate of 5.7 from Gruber and Frakes (2006) using BRFSS data from 1984-2002 and our replication of that estimate of 5.34 (s.e. = 3.25). That said, as pointed out by the authors themselves, it still seems to be unreasonably large when compared to the rest of the literature.

In addition, it is also worth noting that the 2SLS estimates are relatively unstable across specifications. When including fixed effects but no covariates, the 2SLS estimate is similar but smaller (3.49). However, when including covariates and excluding fixed effects, the sign flips and the magnitude is much smaller (-0.79, more in line with the stable "naïve" OLS estimates of -0.4 to -0.7). In the following subsections, we show (1) the sign of the estimates is consistently negative when using a nonlinear first stage and (2) conditional on including covariates, the results are not nearly as sensitive to the inclusion of fixed effects. Moreover, we show that when using a nonlinear first stage that accounts for misreporting, the results are stable across all three specifications.

Nonlinear-Fits-as-Instruments

Next, we would like to estimate the relationship between smoking and BMI by using a probit model for the first stage to obtain predicted smoking status, and then substituting predicted smoking status for observed smoking status in a linear second stage. However, this was famously dubbed the "forbidden regression" by Hausman, since only a linear first stage estimated via OLS is "guaranteed to produce first-stage residuals that are uncorrelated with fitted values and covariates" (Angrist and Pischke, 2008).

To correct for this, we use the nonlinear-fits-as-instruments (hereafter, NFI) approach suggested by Newey (1990). The NFI approach has three stages, which we describe below as stages zero, one, and two. Stage zero is given by:

$$S_{isy} = \psi Tax_{sy} + X_{isy}\phi + \tau_y + \mu_s + \eta_{isy}, \tag{2.4}$$

which we estimate as a probit model to obtain predicted smoking status $\hat{S}$. Then, in stage one, we estimate S as a function of $\hat{S}$ along with our set of controls:

$$S_{isy} = \theta\hat{S}_{isy} + X_{isy}\gamma + \tau_y + \mu_s + u_{isy}. \tag{2.5}$$

Finally, we estimate stage two of the NFI model, where BMI is specified as a function of predicted smoking from stage one, $\tilde{S}$, and the set of controls:

$$BMI_{isy} = \alpha\tilde{S}_{isy} + X_{isy}\beta + \tau_y + \mu_s + \epsilon_{isy}. \tag{2.6}$$

Note that this approach identifies α using both taxes and nonlinearities in stage zero.

While using nonlinearities as identifying information may be somewhat undesirable, Newey (1990) shows that the resulting estimates are more efficient when the probit model is a better approximation of the first-stage conditional expectation function.

Turning to Table 2.4, row 2, column 3 of the top panel reveals a very similar first stage relationship as in 2SLS. Specifically, the estimate suggests that a $1 increase in the real cigarette excise tax reduces the probability of being a smoker by 0.006 percentage points. The first stage F-statistic is 11.38, again, suggesting that cigarette taxes are a strong predictor of smoking. However, turning to row 3, column 3 of the bottom panel, we find that the estimated effect is similar in magnitude to 2SLS but the direction of the relationship is flipped. That is, our NFI estimate suggests that quitting smoking increases BMI by 6.2 (roughly 40 pounds for someone 5' 7" tall and 180 pounds). In this specification, as opposed to 2SLS, the standard errors are much smaller and the coefficient is highly statistically significant. That said, the magnitude still appears to be unreasonably large relative to the rest of the literature. While the corresponding NFI estimate when we include fixed effects and exclude covariates is particularly unreasonably large (-20.9) and imprecise (s.e. = 16.2), when we include covariates the estimates are not as sensitive as 2SLS to the inclusion of fixed effects (-4.5 without fixed effects vs. -6.2 with fixed effects).

Control Function

Next, we estimate the causal relationship of interest using a nonlinear (probit) first stage via the control function (hereafter, CF) approach. Again, the first stage is given by:

$$S_{isy} = \psi Tax_{sy} + X_{isy}\phi + \tau_y + \mu_s + \eta_{isy}. \tag{2.7}$$

Table 2.4 Effect of Smoking on BMI

	(1)	(2)	(3)
First Stage: Regress Smoking Status on Cigarette Taxes			
2SLS	-0.005***	-0.015***	-0.006***
	(0.002)	(0.003)	(0.001)
	[9.18]	[20.77]	[16.38]
NFI & CF	-0.006***	-0.016***	-0.006***
	(0.002)	(0.004)	(0.002)
	[6.74]	[20.48]	[11.38]
2-Step	-0.005***	-0.018***	-0.008**
	(0.002)	(0.000)	(0.000)
	[128.16]	[66.90]	[36.95]
Linear Reduced Form: Regress BMI on Cigarette Taxes			
Cigarette Taxes	-0.019	0.012	-0.025
	(0.050)	(0.048)	(0.042)
Second Stage: Regress BMI on Predicted Smoking Status			
OLS	-0.404***	-0.707***	-0.707***
	(0.085)	(0.089)	(0.080)
2SLS	3.489	-0.792	4.200
	(8.260)	(3.081)	(6.302)
NFI	-20.890	-4.526***	-6.210***
	(16.168)	(1.248)	(0.631)
CF	-11.270	-5.482***	-7.366***
	(8.615)	(1.510)	(0.760)
2-Step	-0.678	-0.495	-1.059**
	(1.484)	(0.937)	(0.499)
Observations	3,596,643	3,596,643	3,596,643
State FE	✓		✓
Quarter-Year FE	✓		✓
All covariates		✓	✓

Notes: Standard errors are presented in parentheses and are robust to clustering at the state-level. F / Chi-Square statistics are presented in brackets. Data are from the BRFSS 2002-2017. We weight observations by BRFSS-provided survey weights. Estimators include ordinary least squares (OLS), instrumental variables (2SLS), Newey correction to the forbidden regression (NFI), the control function approach (CF), and 2-step to correct for misreporting (2S). For comparison purposes, the first stage estimates for NFI and CF that we present are obtained by calculating the marginal effects. The first stage estimates for 2S that we present are obtained by regressing predicted true smoking status on cigarette taxes and controls via OLS. Note: the coefficient on the disposition variable in the first stage of the 2-step estimator is -0.021 (s.e. is 0.010). * p<0.10, ** p<0.05, *** p<0.01.

After estimating this probit model, we obtain the residuals (Res) and use them as a covariate in the second stage. The second stage is given by:

$$BMI_{isy} = \alpha S_{isy} + \delta Res_{isy} + X_{isy}\beta + \tau_y + \mu_s + \epsilon_{isy} \tag{2.8}$$

where S is self-reported smoking status, Res are the residuals from the first stage, along with the usual set of additional controls. The estimated α from the second stage is our causal parameter of interest.

The results presented in row 2, column 3 of the top panel of Table 2.4 reflect the first stage estimates for both the NFI and CF approaches, as they both use the exact same first stage specification. Turning to row 4, column 3 of the bottom panel, we see that the estimate is quite similar to the NFI estimate (negative relationship) but slightly larger in magnitude (-7.4 vs. the NFI estimate of -6.2). As in the NFI specification, the standard error is much smaller than that of the 2SLS estimate and the coefficient of interest is highly statistically significant. Again, while the corresponding CF estimate when we include fixed effects and exclude covariates is unreasonably large (-11.3) and imprecise (s.e. = 8.6), when we include covariates the estimates are not as sensitive as 2SLS to the inclusion of fixed effects (-5.5 without fixed effects vs. -7.4 with fixed effects).

Even though the sign flips when using a nonlinear first stage, which is in line with the experimental literature, the estimates are still unreasonably large compared to the rest of the literature on the relationship between smoking and BMI. In the next subsection, we show that accounting for misreporting in smoking status has a substantial impact on the stability of the estimates while bringing them back within a reasonable range.

2-step estimator

Using the 2-step estimator allows us to account for misreporting of smoking in the BRFSS data, following Nguimkeu et al. (2019). Suppose the outcome variable of interest, BMI, is a function of correctly-measured exogeneous covariates X and the (true) smoking indicator, S^*. In particular, we specify:

$$BMI_{isy} = \alpha S^*_{isy} + X_{isy}\beta + \tau_y + \mu_s + \epsilon_{isy}. \tag{2.9}$$

where α is the key parameter of interest.

We model true smoking status (S^*) as a function of X and the exogenous instrument Tax:

$$S^*_{isy} = \mathbf{1}(\theta Tax_{sy} + X_{isy}\beta + \tau_y + \mu_s + v_{isy} \geq 0). \tag{2.10}$$

However, we only observe self-reported smoking status, S, which is a surrogate of one's true smoking status. Thus, we can consider $S_i = S^*_i d_i$, where d is an indicator of misreporting. If an individual truthfully reports their smoking status, $d = 1$ (0 otherwise). Note that $S = S^*$ for true non-smokers regardless of misreporting. That is, we assume a true non-smoker would not falsely report smoking. Next, we model d as a function of observable covariates X and disposition $Disp$:

$$d_{isy} = \mathbf{1}(\rho Disp_{isy} + X_{isy}\gamma + \tau_y + \mu_s + u_{isy} \geq 0) \tag{2.11}$$

such that:

$$\begin{aligned} S_{isy} &= S^*_{isy} d_{isy} \\ &= \mathbf{1}(\theta Tax_{sy} + X_{isy}\beta + \tau_y + \mu_s + v_{isy} \geq 0 \; ; \; \rho Disp_{isy} + X_{isy}\gamma + \tau_y + \mu_s + u_{isy} \geq 0). \end{aligned} \tag{2.12}$$

In the first step, we estimate equation Equation 2.12 using a partial observability probit model, which allows us to predict each individual's true smoking status, $\hat{S}^*$. In the second stage, we estimate equation Equation 2.9, substituting $\hat{S}^*$ in for S^*. The resulting α is the 2-step estimate of the true causal effect of smoking on obesity when accounting for endogenous misreporting.

The results of this approach, presented in row 3 of the top panel of Table 2.4, again yield very similar first-stage estimates of the effect of cigarette taxes on smoking. In our preferred specification (column 3), the first-stage F-statistic is 36.95, again suggesting that cigarette taxes are strong predictors of smoking. Turning to row 5 of the bottom panel, we find a negative relationship between smoking and obesity as in the NFI and CF approaches. However, here we find that quitting smoking leads to an increase in BMI of only 1.06 (about 7 pounds for someone 5' 7" tall and 180 pounds). Relative to our nonlinear specifications that do not consider misreporting, this estimate is much closer to the experimental 2SLS estimate obtained by Courtemanche et al. (2018), who find that quitting smoking increases BMI by 2.2 (about 14 pounds) when using clinically-measured carbon monoxide levels to determine smoking behavior among participants in a smoking cessation randomized trial. Moreover, we find that this 2-step approach yields a much more stable pattern of estimates across our three specifications, where the estimates only range between -0.50 and -1.06.

Heterogeneity

Finally, we use the 2-step estimator to examine heterogeneity in the effects of smoking on BMI across age, gender, and U.S. census region. While we will focus primarily on presenting the 2-step results, note that we also present the results of OLS, 2SLS, NFI, and CF estimation for comparison purposes.

Turning first to Table 2.5, our 2-step estimates suggest that cigarette taxes have a slightly larger impact on smoking among respondents aged 41-65: a reduction of 1.1 percentage points in the probability of being a smoker for a $1 increase in taxes compared to 0.8 points for those aged 18-40. In terms of the effects of smoking status on BMI, however, we find that quitting smoking has a larger positive impact on BMI among the 18-40 age group: an increase of 4.2 (27 pounds for someone 5' 7" and 180 pounds) compared to only 0.8 (5 pounds) among those aged 41-65.

Next, looking at Table 2.6, our 2-step estimates suggest that cigarette taxes have a substantially larger impact on smoking among female respondents: a 1.2 percentage point smoking reduction compared to 0.6 for men. Interestingly, we find that quitting smoking results in a significantly larger weight gain for men than women: 6.9 BMI points for men compared to a statistically insignificant decrease of 1.3 BMI points for women.

Finally, in Table 2.7, we examine regional differences in the relationship between smoking and BMI. Looking in row 5 of the bottom panel, we find that quitting smoking increases BMI by 3.2 to 4.5 in the northeast and midwest, while we find no statistically discernable relationship among respondents in the southern and western regions of the United States.

Table 2.5 Effect of Smoking on BMI by Age

	OLS	2SLS	NFI	CF	2-Step
Panel A: Age 18-40					
	First Stage (DV = Smoking Status)				
Cigarette Taxes	-	-0.006***	-0.006**	-0.006**	-0.008***
	-	(0.002)	(0.003)	(0.003)	(0.000)
	-	[8.62]	[5.17]	[5.17]	[75.27]
	Second Stage (DV = BMI)				
Smoking Status	-0.195**	4.039	-2.575***	-3.276***	-4.217***
	(0.082)	(9.465)	(0.539)	(0.676)	(0.542)
Observations	1,218,758	1,218,758	1,218,758	1,218,758	1,218,758
Panel B: Age 41-65					
	First Stage (DV = Smoking Status)				
Cigarette Taxes	-	-0.006***	-0.006***	-0.006***	-0.011***
	-	(0.001)	(0.001)	(0.001)	(0.000)
	-	[23.46]	[21.99]	[21.99]	[31.19]
	Second Stage (DV = BMI)				
Smoking Status	-1.422***	4.112	-6.058***	-7.184***	-0.784
	(0.079)	(4.690)	(0.485)	(0.595)	(0.791)
Observations	2,377,885	2,377,885	2,377,885	2,377,885	2,377,885

Notes: All specifications include covariates, year-quarter fixed effects, and state fixed effects. Standard errors are presented in parentheses and are robust to clustering at the state-level. F / Chi-Square statistics are presented in brackets. Data are from the BRFSS 2002-2017. We weight observations by BRFSS-provided survey weights. Estimators include ordinary least squares (OLS), instrumental variables (2SLS), Newey correction to the forbidden regression (NFI), the control function approach (CF), and 2-step to correct for misreporting (2S). For comparison purposes, the first stage estimates for NFI and CF that we present are obtained by calculating the marginal effects. The first stage estimates for 2S that we present are obtained by regressing predicted true smoking status on cigarette taxes and controls via OLS. * p<0.10, ** p<0.05, *** p<0.01.

Table 2.6 Effect of Smoking on BMI by Gender

	OLS	2SLS	NFI	CF	2-Step
Panel A: Male					
	First Stage (DV = Smoking Status)				
Cigarette Taxes	-	-0.005***	-0.005***	-0.005***	-0.006***
	-	(0.002)	(0.002)	(0.002)	(0.000)
	-	[7.57]	[6.77]	[6.77]	[76.07]
	Second Stage (DV = BMI)				
Smoking Status	-0.778***	3.871	-4.514***	-4.844***	-6.893***
	(0.091)	(5.632)	(0.532)	(0.579)	(0.499)
Observations	1,564,310	1,564,310	1,564,310	1,564,310	1,564,310
Panel B: Female					
	First Stage (DV = Smoking Status)				
Cigarette Taxes	-	-0.007***	-0.007***	-0.007***	-0.012***
	-	(0.001)	(0.002)	(0.002)	(0.000)
	-	[24.56]	[14.58]	[14.58]	[25.25]
	Second Stage (DV = BMI)				
Smoking Status	-0.521***	3.214	-0.876**	-1.002*	1.296
	(0.078)	(6.941)	(0.392)	(0.509)	(0.976)
Observations	2,032,333	2,032,333	2,032,333	2,032,333	2,032,333

All specifications include covariates, year-quarter fixed effects, and state fixed effects. Standard errors are presented in parentheses and are robust to clustering at the state-level. F / Chi-Square statistics are presented in brackets. Data are from the BRFSS 2002-2017. We weight observations by BRFSS-provided survey weights. Estimators include ordinary least squares (OLS), instrumental variables (2SLS), Newey correction to the forbidden regression (NFI), the control function approach (CF), and 2-step to correct for misreporting (2S). For comparison purposes, the first stage estimates for NFI and CF that we present are obtained by calculating the marginal effects. The first stage estimates for 2S that we present are obtained by regressing predicted true smoking status on cigarette taxes and controls via OLS. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 2.7 Effect of Smoking on BMI: By Census Region

	NE	MW	SO	WE
First Stage: Regress Smoking Status on Cigarette Taxes				
2SLS	-0.006***	-0.009**	-0.009	-0.000
	(0.002)	(0.003)	(0.006)	(0.002)
	[13.31]	[7.85]	[2.77]	[0.00]
NFI & CF	-0.007***	-0.010***	-0.011*	0.001
	(0.002)	(0.003)	(0.006)	(0.002)
	[10.63]	[9.17]	[3.50]	[0.39]
2-Step	-0.007***	-0.008***	-0.013*	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)
	[10.15]	[24.43]	[4.99]	[10.92]
Second Stage: Regress BMI on Predicted Smoking Status				
OLS	-0.507***	-0.799***	-0.932***	-0.340*
	(0.092)	(0.064)	(0.086)	(0.160)
2SLS	15.345***	1.233	3.901	-3,394
	(1.914)	(5.931)	(4.456)	(344,919)
NFI	-5.741***	-6.904***	-5.825***	-6.330***
	(0.830)	(0.804)	(0.406)	(0.800)
CF	-6.521***	-7.680***	-7.111***	-7.180***
	(0.969)	(0.926)	(0.544)	(0.892)
2-Step	-3.198***	-4.492***	0.673	-0.900
	(0.654)	(0.389)	(0.577)	(0.742)
Observations	696,936	879,807	1,122,084	897,816

Notes: All specifications include covariates, year-quarter fixed effects, and state fixed effects. Standard errors are presented in parentheses and are robust to clustering at the state-level. F / Chi-Square statistics are presented in brackets. Data are from the BRFSS 2002-2017. We weight observations by BRFSS-provided survey weights. Estimators include ordinary least squares (OLS), instrumental variables (2SLS), Newey correction to the forbidden regression (NFI), the control function approach (CF), and 2-step to correct for misreporting (2S). For comparison purposes, the first stage estimates for NFI and CF that we present are obtained by calculating the marginal effects. The first stage estimates for 2S that we present are obtained by regressing predicted true smoking status on cigarette taxes and controls via OLS. * p<0.10, ** p<0.05, *** p<0.01.

2.5 Conclusion

In this paper, we show that carefully considering functional form and accounting for misreporting may help explain the conflicting evidence across previous experimental and observational studies of the relationship between smoking and obesity. In particular, we show that using a nonlinear first stage to estimate the relationship between cigarette taxes and smoking status yields a negative relationship between smoking and BMI, which is consistent with the experimental literature and some of the earlier literature using observational data.

Moreover, we show that accounting for misreporting and using a nonlinear first stage with observational data yields estimates of similar sign and magnitude to this body of work. For example, our preferred estimate suggests that quitting smoking increases BMI by 1.06 (about 7 pounds for someone 5' 7" and 180 pounds), which is quite similar to the experimental estimate of 2.2 (14 pounds) from Courtemanche et al. (2018), but quite different from the estimated *decrease* in BMI of 4.2 to 5.7 (27 to 36 pounds) when following the approach of Gruber and Frakes (2006) using 2SLS and observational data.

Using our estimation sample and incorporating survey weights, the proportion of smokers declined from 25.4% in 2002 to 18.8% in 2017 while BMI rose from 26.7 to 27.9. Multiplying the 6.6 percentage point decline in smoking by our preferred estimate of 1.06 yields a BMI increase of 0.07, which explains roughly 6% of the overall BMI increase of 1.2 across the same time period.

More broadly, our work shows how certain functional form choices and failing to account for misreporting might result in biased estimates when using survey data and instrumental variables methods. These considerations have receive little attention in the literature on the effects of smoking. As such, a key contribution of this paper is to demonstrate how researchers might effectively address them in future work.

Chapter 3

Can Cigarette Taxes Still Reduce Teen Smoking?

3.1 Introduction

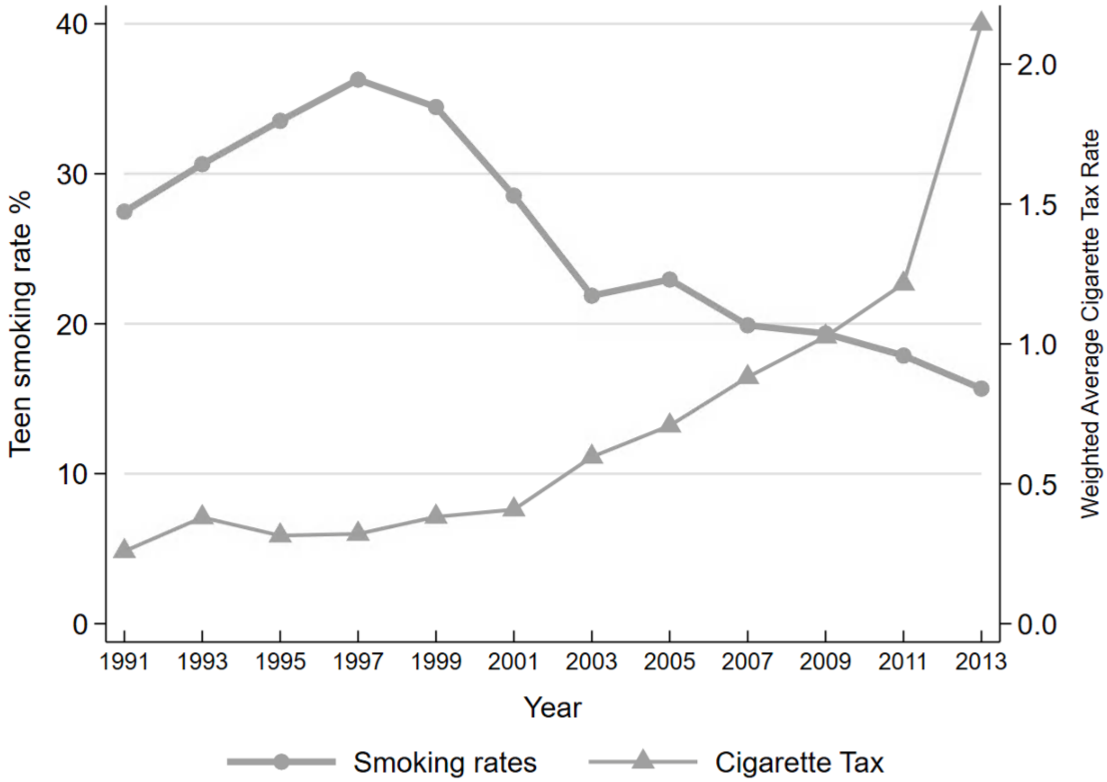
Cigarette smoking is the world's leading cause of preventable death, killing an estimated six million people per year (World Health Organization (2015)). In the U.S., despite large reductions in the smoking rate over the past several decades, smoking still leads to an estimated 480,000 deaths and $289 billion in costs from medical care and lost productivity. Public policies on tobacco control include taxation, bans on smoking in public places, regulated dispensation, criminalization, and informational campaigns, with taxation still being touted as the most effective intervention in the public health community. Many tobacco control efforts focus on teenagers, as 90% of adult smokers started before age 18 (US Department of Health Human Services 2014). These efforts have largely been successful, as the percentage of U.S. teens who smoke fell to 16% in 2013 from a high of 36% (see Figure 3.1).

An extensive literature has examined the relationship between cigarette excise taxes and teen smoking[1]. Early studies relied on cross-sectional approaches and generally found that higher taxes were associated with lower cigarette consumption (Baltagi and Goel (1987); Chapman and Richardson (1990); Seldon and Boyd (1991); Peterson et al. (1992); Chaloupka and Saffer (1992); Sung et al. (1994); Keeler et al. (1996)). Results from such studies may not be causally interpretable, however, as states with stronger anti-smoking sentiments tend to levy higher taxes and stronger sentiments discourage smoking directly[2]. Some researchers

[1]See Chaloupka and Warner (2000), Bader, Boisclair and Ferrence (2011), and Guindon (2013) for more detailed reviews of the literature than we provide here.

[2]For instance, the average state-level cigarette excise tax rates in New York are more than eightfold of that in North Carolina over the period 1991–2013 (Orzechowski Walker 2014). Surveys seeking to understand individual perception of smoking (DeCicca et al. 2008) also find that anti-smoking sentiments are much

Figure 3.1 Teen Smoking Rates and Weighted Average Cigarette Tax Rate



showed that the negative association persisted after controlling for proxies for state anti-smoking sentiment, such as smoke-free air laws and tobacco control expenditures, but it is difficult to capture all possible unobserved confounders in a cross-sectional design (Wasserman et al. (1991); Chaloupka and Saffer (1992); Chaloupka and Grossman (1996); Chaloupka et al. (1996); Lewit et al. (1997); Chaloupka and Pacula (1998); Tauras and Chaloupka (1999); Bardsley and Olekalns (1999)). More recent studies accounted for unobserved, time-invariant state characteristics by estimating state fixed effects models. Using data from the National Education Longitudinal Study (NELS), DeCicca et al. (2002) found little to no evidence of an effect of cigarette taxes on youth smoking initiation after including state fixed effects. Carpenter and Cook (2008) (hereafter CC) also included state fixed effects but used data

stronger in New York than in North Carolina.

from the Youth Risk Behavior Surveys (YRBS), which allows for a much larger sample size spanning a longer period (1991 to 2005) than the NELS does[3]. They found that higher taxes decreased youth smoking participation and frequent smoking, though the magnitude of the effects were smaller than those found in the associational literature. Most recently, Hansen et al. (2017) (hereinafter HSR) revisited CC's findings by extending the sample period to 2013[4]. Using state fixed effects models, they documented a negative relationship between cigarette taxes and youth smoking across the full sample period (1991-2013), but found no evidence of an effect in the most recent years (2007-2013). This is an important result, as it implies that further tax increases would not lead to further reductions in teen smoking. Additionally, they showed that the effect in the full sample period disappears if state-specific linear time trends are added. While including state trends helps account for time-varying unobservables that are correlated with tax changes and youth smoking, it does so at the cost of discarding potentially useful identifying variation in taxes. Accordingly, HSR are agnostic as to which model is preferable. The goal of our paper is to shed light on two important unresolved questions from HSR's work. First, why has the effect of cigarette taxes on youth smoking disappeared in recent years? We argue that the diminished tax effect can be attributed to an inherently nonlinear relationship[5]. Price-sensitive youth are the most likely to be responsive initially as the cigarette tax begins to rise. After decades of tax increases, price-sensitive youth may have already been driven from the market, leaving only price-insensitive youth who are not responsive to continued increases. The result is

[3] CC use micro-level data from the national YRBS as well as aggregate-level data from the state and local YRBS.

[4] Unlike CC, HSR use the micro-level data from both the national and state YRBS, which is the same data source we will employ.

[5] HSR mention this possibility in their conclusion.

a non-linear relationship between cigarette tax rate and youth smoking, with the marginal effect being strongest at low levels of taxation. The average baseline tax rate in 2007 may have been high enough to be in the flat portion of the curve, leading to HSR's finding of a null effect during the 2007-2013 period. We provide two pieces of evidence to support this proposition. First, we estimate the relationship between cigarette tax rate and youth smoking semi-parametrically using Yatchew's difference estimator. This allows the data to choose the appropriate functional form, conditional on state and year fixed effects as well as observable characteristics. The results indeed document a diminishing marginal effect of cigarette taxes on youth smoking, with the average 2007 tax rate lying in the relatively flat portion of the curve. This finding is robust to the use of parametric non-linear models such as a quadratic specification in taxes. Second, our hypothesis implies that tax increases should still effectively deter youth smoking in states with low baseline tax rates, since they are not yet on the flat of the curve. Consistent with this prediction, we show that higher taxes lead to statistically significant and economically meaningful reductions in teen smoking in 2007-2013 in states with low 2007 tax rates. In other words, raising cigarette taxes can still reduce teen smoking in some states, even if there is little to no effect across the country on average. The second unresolved question from HSR is whether, in light of the sensitivity of the full-sample-period results to the inclusion of state-specific time trends, there has ever actually been a true, causal effect of cigarette taxes on youth smoking in the first place. Interestingly, we find that the results using non-linear modeling approaches actually are robust to the inclusion of state trends. This means that conclusions on the relationship between cigarette taxes and youth smoking can be reached even without resolving the methodological debate about whether models with or without state trends are more appropriate.

3.2 Data

Following HSR, we use data from the national and state YRBS, spanning 1991 to 2013. The YRBS is one of the leading data sources on youth risky behaviors. Started in 1991, the YRBS has surveyed thousands of high school students around the country and has been implemented every other year. The national YRBS is conducted by Centers for Disease Control and Prevention (CDC) and the state YRBS, while coordinated by CDC, is usually administered by the participating state health departments or education agencies[6]. The advantage of pooling the national and state YRBS is sample size, as this leads to about six times more observations than the national YRBS alone. The YRBS' smoking question is, "During the past 30 days, on how many days did you smoke cigarettes?" Following CDC's benchmark, we define youth as current smokers if any day of smoking is reported and as frequent smokers if cigarettes are used more than 20 days over the past month. Our cigarette tax variable is the combined state and federal excise tax rate in effect at the end of March of each survey year. The prior literature generally includes only state tax rates since tax changes at the federal level are common to all states and thus absorbed by the time effects. We also include federal taxes because of our focus on nonlinearities: starting place along the distribution matters for the predicted marginal effect, and federal tax rate matters for the starting place. Our use of the tax rate from March, which follows CC and HSR, is done because

[6]Many states have authorized CDC to distribute their data for secondary analyses and for states that have not, we received their permissions to use the data. In addition, we have obtained permission from CDC to use state identifiers in the national YRBS dataset. The states included in both the national and state YRBS vary somewhat each year, though our inclusion of state fixed effects should mitigate any resulting bias in the econometric estimates. See HSR for state-by-year observation counts; our sample is nearly identical to theirs.

the YRBS does not provide survey dates and most states administer YRBS in the spring when school is in session. Finally, we convert the nominal tax rate to 2013 dollars using the Consumer Price Index for All Urban Consumers (CPI–U). We control for individual characteristics and state-level policies that could correlate with changes in both cigarette taxes and youth smoking. The individual-level controls are age in years as well as dummy variables for gender (female), race/ethnicity (non-Hispanic white (base category), non-Hispanic black, Hispanic, and other races), and grade level (9th grade (base category), 10th, 11th, and 12th). The first state-level control is an indicator variable for the comprehensiveness of smoke-free air laws, set equal to one if smoking is banned in government and private worksites, restaurants, and bars, and 0 otherwise. This information comes from the CDC STATE system. The other state-level variables are unemployment rates in March and the natural logarithm of per capita personal income, obtained from the Bureau of Labor Statistics and the Bureau of Economic Analysis. Table 3.1 presents summary statistics for the variables discussed above for CC's sample period of 1991-2005 and HSR's other sample periods of 1991-2013 and 2007-2013. From 1991-2005 to 2007-2013, youth smoking prevalence declined from 27% to 16% while cigarette taxes and the prevalence of comprehensive smoke-free air laws increased substantially. The demographic characteristics stayed relatively steady over time, aside from a modest shift in the racial/ethnic makeup of the population from non-Hispanic white to Hispanic and other groups. We also observe a higher unemployment rate in the later sample period, which corresponds with the Great Recession and gradual recovery.

Table 3.1 Summary Statistics

	1991-2005		2007-2013		1991-2013	
	Mean	SD	Mean	SD	Mean	SD
Current smoker	0.27	0.44	0.16	0.36	0.21	0.41
Frequent smoker	0.13	0.33	0.06	0.24	0.09	0.29
Female	0.51	0.50	0.52	0.50	0.52	0.50
Age	16.0	1.21	16.0	1.23	16.0	1.22
Non-Hispanic white	0.64	0.48	0.56	0.50	0.60	0.49
Non-Hispanic black	0.16	0.36	0.14	0.35	0.15	0.36
Hispanic	0.11	0.31	0.17	0.37	0.14	0.35
Others	0.09	0.29	0.13	0.34	0.11	0.32
9th grade	0.28	0.45	0.28	0.45	0.28	0.45
10th grade	0.27	0.44	0.27	0.44	0.27	0.44
11th grade	0.24	0.43	0.25	0.43	0.24	0.43
12th grade	0.21	0.41	0.21	0.41	0.21	0.41
Cigarette excise taxes (2013 $)	0.80	0.48	2.29	1.10	1.54	1.13
Comprehensive smoke-free air law	0.03	0.18	0.47	0.50	0.25	0.44
State unemployment rates	5.12	1.31	6.98	2.03	6.05	1.95
State per capita personal income	10.19	0.25	10.64	0.17	10.41	0.31
Observations	535,135		537,320		1,072,455	

Notes: Means and standard deviations (in brackets) are reported. We define youth as a current smoker if any day of smoking is reported. We define youth as a frequent smoker if she smoked cigarettes in 20 or more days over the past month. Cigarette excise taxes include federal and state tax rates, effective in March of a given survey year. Cigarette tax data come from The Tax Burden on Tobacco (Orzechowski and Walker 2014). We inflation adjust cigarette tax rates to 2013 dollars using CPI-U. Comprehensive smoke-free air law is an indicator variable set equal to one if smoking is restricted in government and private work places, restaurants, and bars, effective in February of a given survey year. Data come from CDC STATE System. State unemployment rates, in February of a given survey year, come from the Bureau of Labor Statistics. Per capita income comes from the Bureau of Economic Analysis (Reported in natural log).

3.3 Replication

We begin our empirical analysis by showing that we can replicate CC's and HSR's results with our data. Following their approaches, we estimate the average marginal effects of cigarette taxes on youth smoking by running logistic regressions of the form

$$Y^*_{ist} = \beta CigTax_{st} + \mathbf{X}'\gamma + \mathbf{w}_s + \mathbf{v}_t + \epsilon_{ist} \tag{3.1}$$

Y^*_{ist} is a latent variable reflecting the smoking frequency of youth i in state s in year t. We do not observe Y^*, but instead observe the dichotomous variables for current and frequent smoking. The vector X contains the aforementioned individual- and state-level controls. State fixed effects, which account for time-invariant state-specific unobserved heterogeneity, and year fixed effects, which capture any macro-level shocks common to all states, are denoted by w_s and v_t, respectively. By convention, we cluster standard errors at the state level (Bertrand, Duflo, and Mullainathan 2004, Cameron and Miller 2015). Table 3.2 reports the resulting average marginal effects of cigarette taxes on youth current and frequent smoking, alongside estimates from CC and HSR. The first three columns use CC's sample period of 1991-2005, the next two use HSR's full sample period of 1991-2013, and the final two use HSR's other sample period of interest: 2007-2013. As in HSR, we present results from the state, national, and combined state and national versions of the YRBS. The main takeaway from Table 3.2 is that we are able to replicate the results of CC and HSR fairly closely despite small differences in sample sizes. Using data from the national YRBS between 1991 and 2005, CC find that a dollar increase in the state cigarette excise tax is associated with a 5.9 percentage point (pp) decrease in youth smoking participation and a 4.1 pp decrease in frequent smoking. Using the same dataset, our calculation suggests that a one-dollar increase in the tax rate reduces youth smoking participation by 4.4 pp and frequent smoking by 2.6 pp. The magnitudes of the tax effects are slightly smaller in the dataset we use, but they are quite close to those of HSR (4.6 pp and 2.6 pp respectively)[7]. We also confirm HSR's result that the effects shrink roughly in half after adding data from the 2007-2013 waves and become statistically insignificant if the sample is restricted to just the 2007-2013 period. Also similarly to HSR, we find that the results remain qualitatively unchanged if we

[7] One reason that our estimates, as well as those of HSR, are smaller than those reported by CC is that CC calculated the "marginal" change in smoking rates resulting from a $1 change in cigarette tax using the linear projection function after running the logistic regression. We, and HSR, instead calculate the average marginal effects.

use the state or combined state and national versions of the YRBS as opposed to just the national version. Since the estimates are the most precise in the combined dataset, we will use that for the remainder of our analyses.

Table 3.3 presents the results for the combined dataset after adding state-specific linear time trends, alongside the corresponding estimates from HSR. Again, consistent with their results, cigarette tax is no longer statistically significantly associated with youth smoking once state trends are included, even during the period (1991-2005) over which taxes exhibit a strongly deterrent effect without the state trends. This could indicate that the observed relationship between cigarette taxes and youth smoking is attributable to time-varying state unobservables rather than a genuine causal effect. Alternatively, the state trends could be eliminating useful variation. After including them, only 5% of the variation in cigarette tax remains, and the effect of that small portion of the variation could conceivably be different from the genuine causal effect of the remaining, discarded variation. Researchers have debated the appropriateness of controlling for state trends in the cigarette taxation literature. See, for instance, the dialogue between Gruber and Frakes (2006) and Chou, Grossman, and Saffer (2006). We therefore take an agnostic view about which model is more appropriate and will show that, fortunately, the distinction will not ultimately matter after better accounting for nonlinearities.

3.4 Semi-parametric Analyses

In this section, we employ Yatchew's difference estimator to trace out the impacts of cigarette excise tax on youth smoking semi-parametrically, thereby allowing the data to choose the

Table 3.2 Cigarette Taxes and Youth Smoking

	CC	HSR	Current Paper	HSR	Current Paper	HSR	Current Paper
State YRBS							
Current Smoker	-0.027	-0.026***	-0.025***	-0.010**	-0.009**	0.007	0.006
	(N/A)	(0.009)	(0.007)	(0.005)	(0.004)	(0.006)	(0.005)
Frequent Smoker	-0.024	-0.019*	-0.023***	-0.007***	-0.008***	0.002	0.001
	(N/A)	(0.006)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)
N	181	409,385	431,298	883,691	913,239	474,306	481,941
National YRBS							
Current Smoker	-0.059	-0.046***	-0.044**	-0.028***	-0.020*	-0.011	-0.012
	(N/A)	(0.022)	(0.020)	(0.009)	(0.010)	(0.020)	(0.026)
Frequent Smoker	-0.041	-0.026*	-0.026*	-0.016***	-0.012*	-0.006	-0.009
	(N/A)	(0.014)	(0.015)	(0.006)	(0.007)	(0.015)	(0.020)
N	101,633	103,408	103,837	158,605	159,216	55,197 55,318	
Combined State and National YRBS							
Current Smoker	N/A	-0.030***	-0.030***	-0.011**	-0.011**	0.007	0.006
		(0.008)	(0.007)	(0.005)	(0.004)	(0.006)	(0.005)
Frequent Smoker	N/A	-0.019***	-0.020***	-0.007***	-0.007***	0.002	0.007
		(0.006)	(0.005)	(0.002)	(0.002)	(0.003)	(0.005)
N		512,793	535,135	1,042,296	1,072,455	529,503	537,320
Time Span	1991–2005	1991–2005	1991–2005	1991–2013	1991–2013	2007–2013	2007–2013

Notes: Standard errors, clustered at the state level, are shown in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. All regressions control for youth demographic characteristics (gender, race/ethnicity, grade levels, and age), state-level covariates (comprehensive smoke-free air laws, state unemployment rates, and natural logarithm of per capita personal income), and state and year fixed effects. CC=Carpenter and Cook (2008), HSR=Hansen, Sabia, and Rees (2017). Survey weights applied when the analysis sample uses data solely from the national YRBS.

Table 3.3 Cigarette Taxes and Youth Smoking: State-Specific Time Trends

	HSR	Current Paper	HSR	Current Paper	HSR	Current Paper
Current Smoker	-0.007	-0.004	0.003	0.002	-0.005	-0.006
	(0.008)	(0.008)	(0.009)	(0.008)	(0.006)	(0.006)
Frequent Smoker	-0.008	-0.008	0.000	0.000	0.001	0.000
	(0.006)	(0.006)	(0.005)	(0.005)	(0.003)	(0.004)
N	512,793	535,135	1,042,296	1,072,455	529,503	537,320
Time Span	1991–2005	1991–2005	1991–2013	1991–2013	2007–2013	2007–2013

Notes: Standard errors, clustered at the state level, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions control for youth demographic characteristics (gender, race/ethnicity, grade levels, and age), state-level covariates (comprehensive smoke-free air laws, state unemployment rates, and natural logarithm of per capita personal income), and state and year fixed effects. CC=Carpenter and Cook (2008), HSR=Hansen, Sabia, and Rees (2017). Survey weights applied when the analysis sample uses data solely from the national YRBS.

appropriate functional form (Yatchew 2003, 1997). The model becomes

$$Y_{ist} = \beta f(CigTax_{st}) + \mathbf{X}'\gamma + \mathbf{w}_s + \mathbf{v}_t + \epsilon_{ist} \tag{3.2}$$

where the key change is the flexible functional form for cigarette tax[8]. By construction, f(.) is a smooth, single-valued function with bounded first-order derivatives. Yatchew's difference estimator is partially linear; therefore, $'\gamma$, $f(CigTax_{s,t})$, and the error term

$$\epsilon_{ist}$$

are additively separable. A detailed explanation of the method is outside the scope of this paper but its logic can be summarized as follows. For ease of exposition, let $f(T_L)$ denote $f(CigTax_{s,t})$.

[8]Another change is that the discrete nature of the outcome variable is no longer formally modeled. To verify that this alone is inconsequential, Appendix Table A.1 reports the results using a linear probability model rather than logit. The estimates are very similar to the corresponding marginal effects from Table 3.1.

To begin with, the estimator arranges data in the order of $Tax_1 < Tax_2 << Tax_j$, where j indicates the total number of observations in the estimation sample. The specification in (2) is then estimated in the form of "first-order" differences:$y_I - y_{I-1} = \beta f(T_L - T_{L-1}) +' \gamma + \epsilon_{i,s,t} - \epsilon_{i,s,t-1}$, generalizing to

$$\sum_{n=1}^{m}(d_n y_{I-n}) = \gamma(\sum_{n=1}^{m}(d_n x_{I-n}) + \sum_{n=1}^{m} d_n f(T_{L-n}) + \sum_{n=1}^{m} d_n \epsilon_{I-n} \quad (3.3)$$

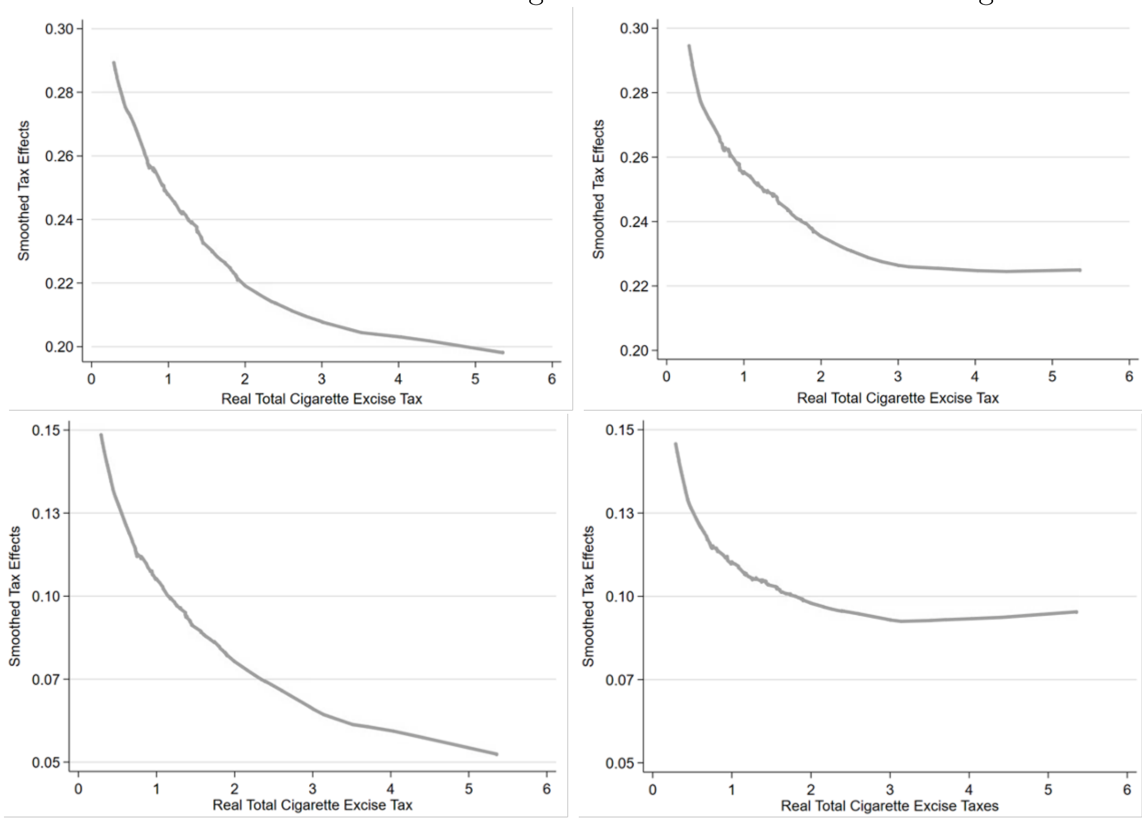
where m denotes the order differencing and $d_0, d_1, \ldots, d_m$ are the differencing coefficients that satisfy a pre-imposed condition:

$\sum_{n=1}^{m} d_n = 0$ and $\sum_{n=1}^{m} d_n^2 = 1$

Since T_L (cigarette excise tax) has a compact support, $\sum_{n=1}^{m} d_n f(T_{L-n})$ shrinks and is removed as the sample size increases. The parameter γ is estimated using OLS regression and the function f is derived by regressing $(y_I - \mathbf{X}'\hat{\gamma}_{dif})$ onT_L non-parametrically, analogous to a stylized locally weighted regression. The differencing order m affects the estimator's asymptotic efficiency and Monte Carlo simulations report noticeable efficiency gains from higher-order differencing (Lokshin 2006)[9]. In the following analyses, we set m equal to 7 because higher order differencing (8th-10th) is no longer associated with efficiency gains, only additional computational intensity. The upper panel of Figure 3.2 presents the smoothed tax effects $f(T_L)$ on youth current smoking while the bottom panel shows the effects on youth frequent smoking, both with data from the combined national and state YRBS, spanning 1991–2013. The figures on the left show the results without the state trends, while those on the right include them. It is apparent that the deterrent effect of cigarette tax on smoking

[9] 10th order differencing is the upper bound imposed by Yatchew's difference estimator.

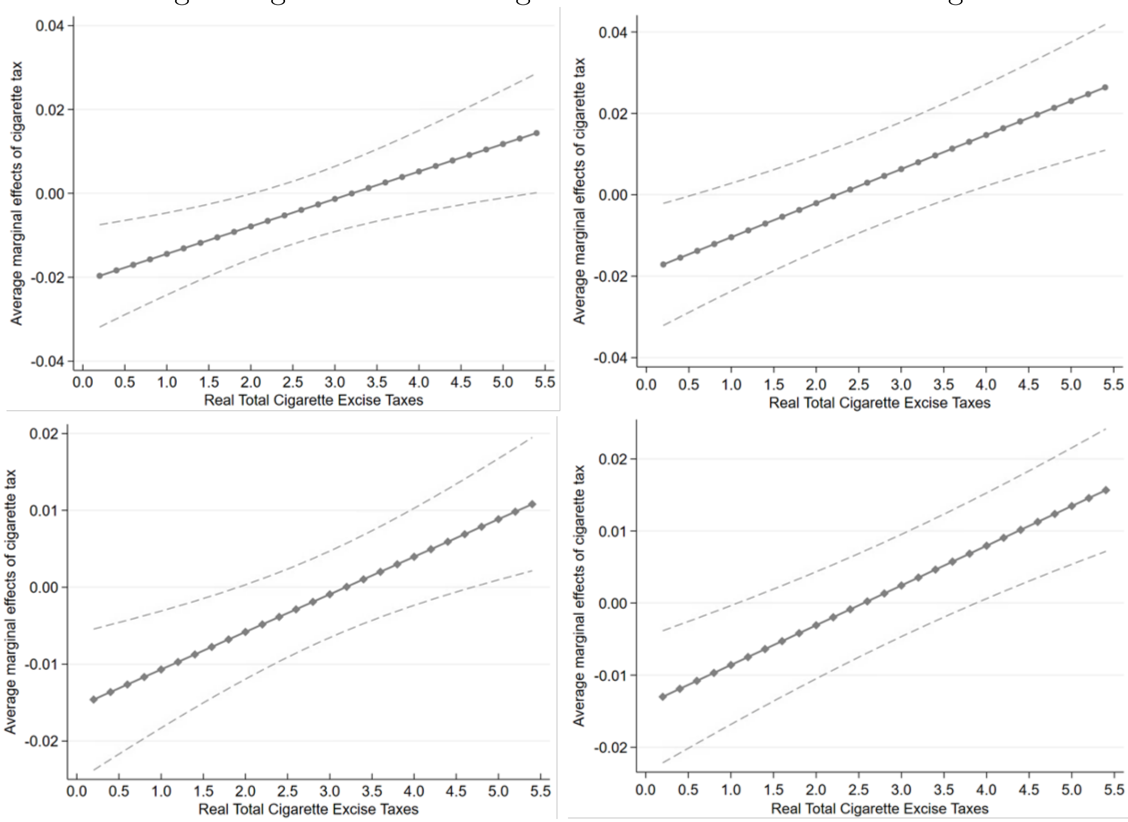
Figure 3.2 Semi-Parametric Estimates: Cigarette Taxes and Youth Smoking



is not constant but diminishing over the tax range. This pattern is particularly pronounced when we account for the state-specific time trends, as the predicted rates of both current and frequent smoking are essentially flat at tax rates of higher than $3.

Figure 3.3 depicts the same results in a different manner by plotting how the marginal effects, rather than the predicted smoking rates, change across the tax distribution. In all four graphs (both outcomes, with and without state trends), the marginal effect of taxes on youth smoking is negative and significant at low levels of taxation, but eventually becomes insignificant. The effects on current and frequent smoking turn insignificant at tax rates of approximately $2 per pack without state trends, compared to less than $1 once state trends are added. In three of the four graphs, the effects actually turn positive and significant at

Figure 3.3 Average Marginal Effects of Cigarette Taxes on Youth Smoking

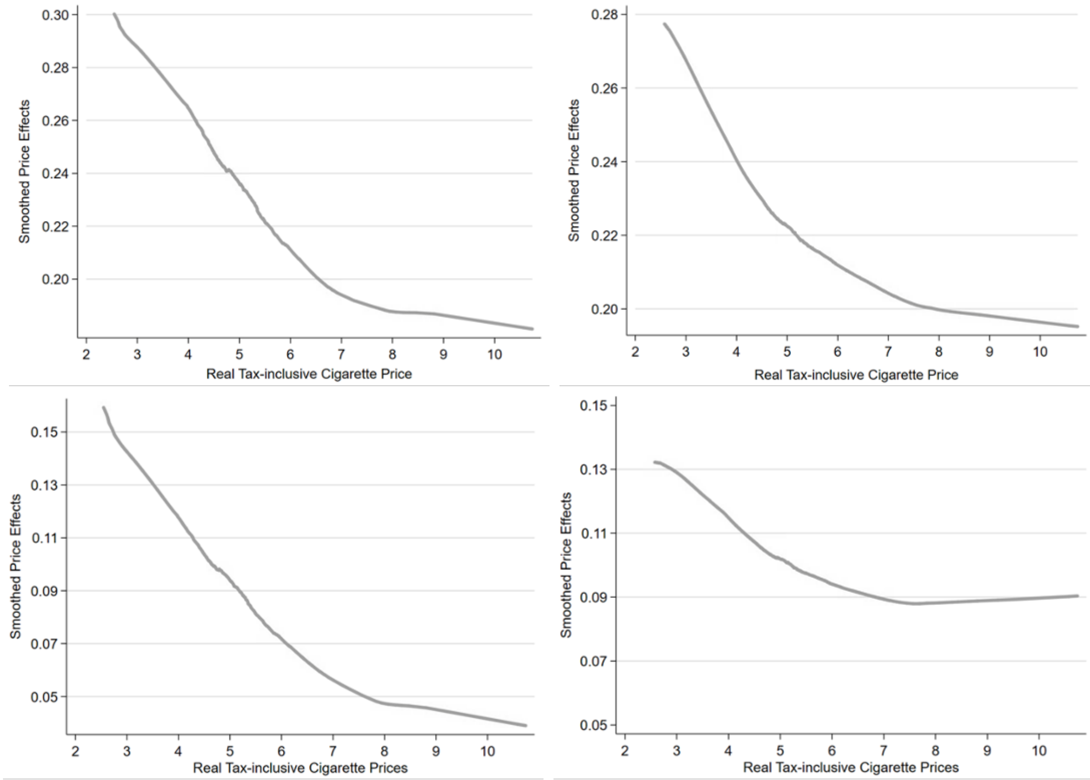


the highest tax rates, though we caution against a literal interpretation of that result since it is based on very few state-year combinations.

3.5 Ruling Out Alternative Explanations

The results from Figures 3.2 and 3.3 are consistent with the hypothesis that the disappearance of an effect of cigarette taxes on youth smoking is due to an inherently nonlinear relationship: steadily rising tax rates over several decades have driven price-sensitive consumers from the market, leaving only those who are price-insensitive continuing to smoke. This section conducts a variety of robustness checks in an effort to rule out possible alternative explanations.

Figure 3.4 Semi-Parametric Estimates: Cigarette Prices and Youth Smoking



Cigarette Prices Instead of Taxes

First, our hypothesis relates to consumer responses to cigarette prices, but our analyses thus far focus on taxes. A diminishing marginal effect of taxes does not necessarily imply a diminishing marginal effect of prices. For instance, if tax increases did not pass through as fully to prices in recent years, we might observe a diminishing effect of taxes but linear effect of prices. Figure 3.4 therefore presents results for a semi-parametric analysis identical to that in equation (3) but replacing tax rates with (tax-inclusive) per-pack prices. The shapes of all four graphs are similar to those from Figure 3.2, with a diminishing marginal effect being evident in all cases. Particular flattening appears to occur at a price of around $7 per pack.

Alternative Functional Forms

Another possible concern is that the observed pattern of nonlinearity might be an artifact of the particular functional form chosen by the Yatchew semi-parametric method. It is therefore useful to check if a similar pattern emerges using a simpler parametric nonlinear specification. Table 3.4 therefore reports results from a model including both cigarette tax and its square (i.e. quadratic in cigarette tax). Given the difficulty of interpreting nonlinear models with interaction terms (Ai and Norton 2003), we employ a linear probability model (LPM) rather than a logit. We therefore also present results for a LPM without the squared term in order to verify that they are similar to the average marginal effects from the logistic regressions reported previously[10]. The results from the quadratic specifications, for both outcomes and regardless of whether state trends are included, are consistent with those from the semi-parametric model. In all four cases, the coefficient estimate for cigarette tax is negative and significant while that for its square is positive and significant. This indicates a negative effect at low levels of taxation that gradually diminishes and eventually turns positive. Without state trends, the marginal effect turns positive at a tax rate of $3.50 for current smoker and $2.67 for frequent smoker, respectively. In the semiparametric results shown in Figure 3.3, the corresponding marginal effects both cross zero at around $3.20. With state trends, the marginal effects from the quadratic specification in Table 3.4 become positive at just $0.85 and $0.87 for current and frequent smoker, respectively. In the semiparametric models from

[10]Specifically, since the regressions in Table 3.4 all use the combined sample from 1991-2013, the -0.012 estimate for current smoker from the first column should be compared to the -0.011 estimate from the combined sample/1991-2013/current paper/current smoker cell in Table 3.2, while the -0.010 estimate for frequent smoker should be compared to the -0.007 estimate from the combined sample/1991-2013/current paper/frequent smoker cell in Table 3.2. Similarly, the estimates including state trends from the third column of Table 3.4 – 0.004 and 0.001, respectively – parallel the estimates of 0.002 and 0.000 from the fourth column of Table 3.3. Appendix Table A.1 reports results from linear probability models using other samples and time periods.

Figure 3.3, the marginal effects with state trends cross zero at somewhat higher rates of about \$2.20 and \$2.60. Nonetheless, the fact that the general shapes of the curves are the same is reassuring.

Table 3.4 Cigarette Taxes and Youth Smoking: Quadratic Specification

	Linear	Quadratic	Linear With State Trends	Quadratic With State Trends
Youth is a Current Smoker				
Cigarette Tax	-0.012*	-0.021***	0.004	-0.017**
	(0.006)	(0.006)	(0.008)	(0.008)
Cigarette Tax Squared		0.003***		0.004***
		(0.001)		(0.001)
Youth is a Frequent Smoker				
Cigarette Tax	-0.010**	-0.016***	0.001	-0.013***
	(0.004)	(0.005)	(0.005)	(0.005)
Cigarette Tax Squared		0.003***		0.003***
		(0.001)		(0.001)
Observations	1,072,455	1,072,455	1,072,455	1,072,455

Notes: Results are from linear probability models estimated using the combined state and national YBRS sample from 1991-2013. Standard errors, clustered at the state level, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions control for youth demographic characteristics (gender, race/ethnicity, grade levels, and age), state-level covariates (comprehensive smoke-free air laws, state unemployment rates, and natural logarithm of per capita personal income), and state and year fixed effects

Appendix Table A.2 further reports results from cubic, quartic, and quintic specifications, while Appendix Table A.3 does the same adding linear state-specific time trends. The higher-order polynomial terms are always insignificant, meaning that there is little additional information to be learned from parametric models beyond quadratic. Another issue related to functional form is whether the observed diminishing marginal effect of cigarette taxes on youth smoking could simply reflect a constant elasticity. If every 100% increase in the tax rate leads to the same percentage change in smoking, this would still show up as a diminishing marginal effect when the regression is run in levels[11]. Elasticities can be estimated using a log-log functional form, but that is not feasible with the individual-level YRBS data because of the dichotomous nature of the outcomes. We therefore aggregate the data to the state-by-year level and re-estimate regression equation (1) as a LPM with the natural logarithm of the percentage of youth who are current (or frequent) smokers as the dependent variable and the natural logarithm of the tax rate as the independent variable. The coefficient from this new specification can be interpreted as the (approximate) tax elasticity of cigarette consumption. We weight each observation by the number of individuals in the state-by-year cell to preserve comparability to individual-level estimates (Chetty, Looney, and Kroft 2007). The results in Table 3.5 show that the cigarette tax elasticity did change over time. Specifically, the tax elasticities from 1991-2005 are 17% for current smoking and 31% for frequent smoking. The elasticities shrink roughly in half after incorporating survey waves through 2013, and they turn positive but small and insignificant in 2007-2013 alone. This is essentially the same pattern observed in our main estimates from Table 3.2.

E-Cigarettes

E-cigarettes, whose popularity among teens rose sharply during the years added to the sample by HSR, are a possible confounding factor. Is it possible that the availability of this new nicotine delivery system accounts for the reduced tax elasticity observed above? The-

[11] To illustrate, suppose an increase in the tax rate from $0.50 to $1 reduces the smoking rate from 20% to 17%. This would mean a 100% increase in tax reduces smoking by 15%, for an elasticity of 0.15. At the same elasticity, an additional 100% increase in taxes to $2 would only decrease the smoking rate to 14.45%. Since we achieve a smaller decrease in smoking rates out of a larger increase in tax rates (in levels), it follows that for an equivalent increase in cigarette taxes ($0.5), we would have seen an even smaller decrease in smoking rates. Therefore, when estimated in levels, this constant elasticity would show up as a diminishing marginal effect, as the increase in taxes at higher values will always be a larger dollar amount than the increase in taxes at lower values, with the resulting percentage point change in the smoking rate being smaller.

Table 3.5 Cigarette Tax Elasticities using Aggregated State-Level Data

	1991-2005	2007-2013	1991-2013
Panel A: ln(% Current Smokers)			
ln(Cigarette Tax)	-0.172**	0.077	-0.088*
	(0.069)	(0.088)	(0.044)
Panel B: ln(% Frequent Smokers)			
ln(Cigarette Tax)	-0.313***	0.079	-0.153**
	(0.098)	(0.131)	(0.064)
N	299	188	487

Notes: Results are from linear probability models using the combined state and national YRBS sample. Standard errors, clustered at the state level, are shown in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions control for youth demographic characteristics (gender, race/ethnicity, grade levels, and age), state-level covariates (comprehensive smoke-free air laws, state unemployment rates, and natural logarithm of per capita personal income), and state and year fixed effects. Observations are weighted by the state/year cell size.

oretically, this seems unlikely, as the availability of a new substitute should, if anything, increase consumers' price responsiveness in the traditional cigarette market[12]. To further rule out this concern, we next conduct an analysis of the effect of cigarette taxes on youth e-cigarette use. Since the YRBS did not include e-cigarette questions during our study period, we instead use data from the National Youth Tobacco Survey (NYTS), which included such questions from 2011-2013. During this period, the percentage of youth who report ever using e-cigarettes rose from 3% to 8%, while the percentage reporting current use (past 30 days) rose from 1% to 3%. Therefore, while we are not able to study e-cigarettes until 2011, the very low rate of use as of 2011 suggests that it is unlikely that there was any meaningful effect of cigarette taxes on e-cigarette use prior to then. Table 3.6 reports the results from logistic regressions of the same form as equation (1) but with ever using and

[12]There is debate in the literature about whether cigarettes and e-cigarettes are actually substitutes. Conceivably, they could be complements if e-cigarettes serve as a gateway to subsequent traditional smoking. Available evidence from quasi-experimental studies of the impact of e-cigarette regulations on youth smoking seems to suggest that, if anything, the two products are substitutes (Friedman 2015, Pesko, Hughes, and Faisal 2016, Dave, Feng, and Pesko 2017). Moreover, even if they were complements, subsequent transition to traditional cigarettes might occur after the respondents leave high school, in which case they would be outside of our sample age range.

currently using e-cigarettes as the outcomes. The coefficient estimate for cigarette tax is small and statistically significant in both cases. These results suggest that our finding of a diminishing marginal effect of cigarette taxes on youth smoking is unlikely to be attributable to the introduction of e-cigarettes toward the end of our sample period.

Table 3.6 Cigarette Taxes and Youth E-Cigarette Use

Panel A: Youth Ever Used E-Cigarettes	
Cigarette Tax	-0.010
	(0.012)
N	57,178
Panel B: Youth is a Current E-Cigarette User	
Cigarette Tax	0.001
	(0.005)
N	56,916

Notes: Results are from logistic regressions using the National Youth Tobacco Survey: 2011-2013. Standard errors, clustered at the state level, are shown in parenthesis. Average marginal effects reported. Sampling weights are applied. All regressions control for youth demographic characteristics (gender, race/ethnicity, grade levels, and age), state-level covariates (comprehensive smoke-free air laws, state unemployment rates, and natural logarithm of per capita personal income), and state and year fixed effects.

3.6 Average Effects in States with Low Baseline Tax Rates

The results thus far provide, in our view, compelling evidence of fundamental nonlinearity in youths' responses to rising cigarette taxes, with effects being relatively strong at low baseline levels of taxation and small or zero at higher levels. This nonlinearity provides an explanation for the disappearance of an average effect of cigarette taxes on youth smoking in the 2007-2013 sample period. However, it also implies that youths in states with low tax rates at the start of this period may still have been responsive to tax increases, as their states were not yet on the "flat of the curve". In other words, price-sensitive consumers had not yet been driven out of the market in these states. To directly test this hypothesis, we re-estimate equation (1) for the 2007-2013 sample period, stratifying into three groups based on baseline (2007) state tax rates[13]. The first group consists of 13 states with baseline state taxes rates of no more than \$0.50 per pack. These states are AL, GA, FL, IA, KY, LA, MS, MO, NC, ND, SC, TN, and VA. Between 2007 and 2013, six of these states (FL, KY, MS, NC, SC, TN) raised cigarette taxes at least once, with Florida implementing the largest increase, from \$0.39 to \$1.39 per pack. The middle group consists of 25 states with baseline state tax rates between \$0.50 and \$1.50, while the high tax group contains 13 states with tax rates over \$1.50. Table 3.7 presents the results, both with and without state-specific linear time trends. Among these 13 states with low 2007 tax rates, taxes are indeed negatively and significantly associated with youth smoking during the 2007-2013 period. A one-dollar increase reduces current and frequent smoking by 0.9 and 1 pp, respectively, in the regressions without state trends and 2.3 and 2.5 pp in the regressions with them. The latter are similar to the magnitudes from the 1991-2005 period during which baseline (1991) tax rates in most states were low and, accordingly, CC, HSR, and the current study found statistically significant average effects across the whole country[14]. In contrast, the effects in the middle and high tax groups are all small and statistically insignificant. Since the vast majority of the population lives in the middle and high tax states as opposed to the low tax states, the null average effect during 2007-2013 pooling all states together is not surprising.

[13]Since the number of states in each stratified sample is relatively small, we use bootstrapped standard errors instead of clustering standard errors by state (Courtemanche and Zapata 2014).

[14]Appendix Table A.4 explores adding higher order polynomials and finds that quadratic, cubic, quartic, and quintic terms are all insignificant. This suggests that the cigarette tax effect is approximately linear for low-baseline-tax states, consistent with the tax increases occurring in the left portion of the curves from Figure 3.2, before the diminishing marginal effects become particularly evident.

Table 3.7 Cigarette Taxes and Youth Smoking: States Grouped by Tax

	Low Tax	Low Tax; State Trends	Med Tax	Med Tax; State Trends	High Tax	High Tax; State Trends
Current Smoker	-0.009**	-0.023**	0.004	-0.005	-0.005	-0.004
	[0.004]	[0.011]	[0.007]	[0.012]	[0.009]	[0.014]
Frequent Smoker	-0.010**	-0.025**	0.001	0.004	-0.005	0.002
	[0.004]	[0.009]	[0.003]	[0.008]	[0.006]	[0.006]
Observations	114,078	114,078	275,070	275,070	148,172	148,172

Notes: Results are estimated using the combined state and national YBRS sample. Bootstrapped standard errors are shown in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions control for youth demographic characteristics (gender, race/ethnicity, grade levels, and age), state-level covariates (comprehensive smoke-free air laws, state unemployment rates, and natural logarithm of per capita personal income), and state and year fixed effects. States included in the low tax group are AL, GA, FL, KY, IA, LA, MS, MO, NC, ND, SC, TN, and VA. States included in the medium tax group are AR, CA, CO, DC, DE, ID, IL, IN, KS, MD, MN, NE, NV, NH, NM, NY, OH, OK, OR, PA, TN, UT, WV, WI, and WY. States included in the high tax group are AK, AZ, CT, HI, ME, MA, MI, MT, NJ, RI, SD, VT, and WA.

3.7 Conclusion

A recent study by HSR finds that the deterrent effect of cigarette excise taxes on youth smoking documented by CC and others has disappeared in recent years, raising questions about their continued effectiveness in achieving public health objectives. In this paper, we provide evidence from semi-parametric and parametric models that this phenomenon is attributable to a fundamentally nonlinear relationship. Tax increases are effective in reducing youth smoking up to a point, but once tax rates are sufficiently high that price sensitive consumers have already left the market, further tax increases are ineffective (other than as a revenue source). During the 1991-2005 period examined by CC, initial tax rates were sufficiently low that subsequent increases did reduce youth smoking. In contrast, during the 2007-2013 period added by HSR, baseline tax rates in most states were sufficiently high that additional increases did not prove to be effective deterrents on average. However, in states that still had low taxes in 2007, tax increases in subsequent years did indeed reduce smoking. In other words, the disappearance of the average effect is simply due to the extensive nature of prior tax increases, as opposed to, for instance, an underlying change in consumer demand. Interestingly, our conclusions are not sensitive to the inclusion of state-specific linear time trends, which contrasts the sensitivity observed by HSR in regressions with a more restrictive functional form. Our findings have implications for cigarette tax policy moving forward. In principle, our results suggest that further tax increases would be effective in reducing youth smoking in states that still have low tax rates. For how many states might this be the case? The point estimates from our semi-parametric regressions imply that the "flat of the curve" begins at a tax rate of no less than \$2.20 or greater (Figure 3.3, upper right graph). As of this writing, sixteen states still had combined state and federal tax rates of under $2.20. The somewhat sizeable confidence intervals around our semi-parametric estimates mean that this is only a rough estimate, but there are likely at least some states that have not yet reached the flat of the curve. In other words, cigarette taxation can still be an effective tool to curb youth smoking in some cases.

APPENDIX

Table A1 Cigarette Taxes and Youth Smoking: Linear Probability Models

	1991-2005	1991-2013	2007-2013
State YRBS			
Current Smoker	-0.027***	-0.011**	0.006
	(0.009)	(0.005)	(0.006)
Frequent Smoker	-0.023***	-0.006	0.004
	(0.007)	(0.004)	(0.004)
N	431,298	913,239	481,941
National YRBS			
Current Smoker	-0.036**	-0.022**	-0.012
	(0.018)	(0.010)	(0.023)
Frequent Smoker	-0.025*	-0.010*	-0.008
	(0.014)	(0.006)	(0.018)
N	103,837	159,216	55,318
Combined State and National YRBS			
Current Smoker	-0.022***	-0.012*	0.008*
	(0.007)	(0.006)	(0.004)
Frequent Smoker	-0.017***	-0.010**	0.006**
	(0.005)	(0.004)	(0.003)
N	535,135	1,072,455	537,320

Notes: Standard errors, clustered at the state level, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions control for youth demographic characteristics (gender, race/ethnicity, grade levels, and age), state-level covariates (comprehensive smoke-free air laws, state unemployment rates, and natural logarithm of per capita personal income), and state and year fixed effects. CC=Carpenter and Cook (2008), HSR=Hansen, Sabia, and Rees (2017). Survey weights applied when the analysis sample uses data solely from the national YRBS.

Table A2 Cigarette Taxes and Youth Smoking: Different Functional Forms

	1	2	3	4	5
Panel A: Youth is a current smoker					
$CigaretteTaxes$	-0.012*	-0.021***	-0.007	-0.033	-0.029
	0.006	0.006	0.015	0.042	0.085
$CigaretteTaxes^2$		0.003***	-0.003	0.019	0.013
		0.001	0.007	0.032	0.090
$CigaretteTaxes^3$			0.001	-0.006	-0.003
			0.001	0.01	0.044
$CigaretteTaxes^4$				0.001	0.000
				0.001	0.010
$CigaretteTaxes^5$					0.000
					0.001
Panel B: Youth is a frequent smoker					
$CigaretteTaxes$	-0.010**	-0.016***	-0.011	-0.065^+	-0.093
	0.004	0.005	0.010	0.036	0.083
$CigaretteTaxes^2$		0.003***	-0.000	0.044	0.078
		0.001	0.005	0.028	0.088
$CigaretteTaxes^3$			0.001	-0.014	-0.032
			0.001	0.009	0.041
$CigaretteTaxes^4$				0.002	0.006
				0.001	0.009
$CigaretteTaxes^5$					-0.000
					0.001
Observations (Col 1-5)	1,072,455				

Notes: Results are estimated using the combined state and national YBRS sample and linear probability models. Standard errors, clustered at the state level, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ All regressions control for youth demographic characteristics (gender, race/ethnicity, grade levels, and age), state-level covariates (comprehensive smoke-free air laws, state unemployment rates, and natural logarithm of per capita personal income), and state and year fixed effects.

Table A3 Cigarette Taxes and Youth Smoking: Additional Functional Forms

	1	2	3	4	5
Panel A: Youth is a current smoker					
$CigaretteTaxes$	0.004	-0.017**	-0.023	-0.038	-0.209
	(0.008)	(0.008)	(0.019)	(0.063)	(0.144)
$CigaretteTaxes^2$		0.004***	0.006	0.018	0.213
		(0.001)	(0.010)	(0.047)	(0.147)
$CigaretteTaxes^3$			-0.000	-0.004	-0.103
			(0.001)	(0.014)	(0.067)
$CigaretteTaxes^4$				0.000	0.023
				(0.001)	(0.014)
$CigaretteTaxes^5$					-0.002
					(0.001)
Panel B: Youth is a frequent smoker					
$CigaretteTaxes$	0.001	-0.013***	-0.027*	-0.039	-0.111
	(0.005)	(0.005)	(0.011)	(0.037)	(0.091)
$CigaretteTaxes^2$		0.003***	0.009	0.019	0.101
		(0.001)	(0.006)	(0.027)	(0.093)
$CigaretteTaxes^3$			-0.001	-0.004	-0.045
			(0.001)	(0.008)	(0.043)
$CigaretteTaxes^4$				0.000	0.010
				(0.001)	(0.009)
$CigaretteTaxes^5$					-0.001
					(0.001)
Observations (Col 1-5)	1,072,455				

Notes: Results are estimated using the combined state and national YBRS sample and linear probability models. Standard errors, clustered at the state level, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ All regressions control for youth demographic characteristics (gender, race/ethnicity, grade levels, and age), state-level covariates (comprehensive smoke-free air laws, state unemployment rates, and natural logarithm of per capita personal income), and state and year fixed effects.

Table A4 Cigarette Taxes and Youth Smoking: Combined YRBS

	1	2	3	4	5
Panel A: Youth is a current smoker					
$CigaretteTaxes$	-0.015**	-0.012	-0.153	0.425	1.509
	(0.006)	(0.045)	(0.128)	(0.520)	(0.870)
$CigaretteTaxes^2$		-0.001	0.136	-0.692	-2.901
		(0.015)	(0.122)	(0.751)	(1.722)
$CigaretteTaxes^3$			-0.038	0.439	2.536
			(0.034)	(0.426)	(1.643)
$CigaretteTaxes^4$				-0.096	-1.026
				(0.083)	(0.747)
$CigaretteTaxes^5$					0.156
					(0.129)
Panel B: Youth is a frequent smoker					
$CigaretteTaxes$	-0.008*	0.010	-0.102	0.287	-0.645
	(0.004)	(0.022)	(0.078)	(0.297)	(0.633)
$CigaretteTaxes^2$		-0.006	0.103	-0.454	1.446
		(0.007)	(0.075)	(0.415)	(1.211)
$CigaretteTaxes^3$			-0.030	0.291	-1.512
			(0.021)	(0.234)	(1.099)
$CigaretteTaxes^4$				-0.064	0.735
				(0.046)	(0.475)
$CigaretteTaxes^5$					-0.134
					(0.078)
Observations	114,078	114,078	114,078	114,078	114,078

Notes: Results are estimated using the combined state and national YBRS sample and linear probability models. Bootstrapped standard errors, clustered at the state level, are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions control for youth demographic characteristics (gender, race/ethnicity, grade levels, and age), state-level covariates (comprehensive smoke-free air laws, state unemployment rates, and natural logarithm of per capita personal income), and state and year fixed effects.

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