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CIGARETTE TAXES AND YOUTH SMOKING: NEW EVIDENCE FROM NATIONAL, STATE, & LOCAL YOUTH RISK BEHAVIOR SURVEYS

Christopher Carpenter
Philip J. Cook

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1050 Massachusetts Avenue
Cambridge, MA 02138
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Carpenter is Assistant Professor of Economics/Public Policy, The Paul Merage School of Business at UC Irvine, kittc@uci.edu, 443 SB UC Irvine, Irvine, CA 92697-3125. Cook is ITT/Terry Sanford Professor of Public Policy Studies, pcook@duke.edu, 215 Terry Sanford Institute, Durham, NC 27708.
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ABSTRACT

Several studies have examined the effects of state cigarette tax increases on youth substance use over the 1990s, with most -- but not all -- finding that higher taxes reduce youth consumption of tobacco. We advance the literature by using data from the 1991-2005 waves of the national Youth Risk Behavior Surveys (YRBS), providing information on over 100,000 high school age youths. We also are the first to make use of hundreds of independently fielded state and local versions of the YRBS, reflecting data from over 750,000 youths. Importantly, these data are to our knowledge the only sources of relevant information on youth smoking that were explicitly designed to be representative of the sampled state or locality. We estimate two-way fixed effects models of the effect of state cigarette taxes on youth smoking, controlling for survey demographics and area and year fixed effects. Our most consistent finding is that -- contrary to some recent research -- the large state tobacco tax increases of the past 15 years were associated with significant reductions in smoking participation and frequent smoking by youths. Our price elasticity estimates for smoking participation by high school youths are generally smaller than previous cross-sectional approaches but are similar to recent quasi-experimental estimates.

Christopher Carpenter
University of California, Irvine
The Paul Merage School of Business
443 SB
Irvine, CA 92697-3125
and NBER
kittc@uci.edu

Philip J. Cook
Terry Sanford Institute of Public Policy
Box 90245
Duke University
Durham, NC 27708
and NBER
pcook@duke.edu

1. Introduction

Voters in the state of California recently rejected the largest potential tobacco tax increase in history: a 13 cent tax increase per cigarette, or a $2.60 increase in the tax on a pack of 20 cigarettes. Television ads in support of "Prop 86" indicated that the new tax would "reduce teen smoking by 43 percent." The magnitude of this figure reflects the conventional wisdom in research and policy circles that the smoking behaviors of youths and young adults are highly sensitive to price, more so than for adults who as a group may have better established habits.

Despite this conventional wisdom, however, a series of recent studies in the economics literature has called into question whether higher cigarette taxes will "put out the fires". Using panel data on youths from the National Educational Longitudinal Study (NELS), DeCicca et al. (2002) find that once time-invariant state fixed effects are accounted for, youth smoking initiation is statistically unrelated to cigarette taxes. More recently, DeCicca et al. (2004, 2006) argue that the strong negative cross sectional association between cigarette taxes and youth smoking may be more properly attributable to cross-state differences in previously unobserved state anti-smoking sentiment. Using novel data on adult attitudes about smoking, DeCicca et al. (2006) create state-specific measures of anti-smoking attitudes; once they account for this anti-smoking variable in the cross section, the negative price coefficient becomes small and statistically insignificant.

These recent findings stand in contrast to a body of research that has found teens to be responsive to the price of cigarettes, usually proxied by state excise taxes. Much of the earlier work on this topic used cross-section data and found a negative relationship

between youth smoking and state-specific prices and taxes (Lewit, Coate and Grossman 1981, Chaloupka and Grossman 1996, Harris and Chan 1999, and others). Importantly, however, this relationship has also been found in studies that use quasi-experimental methods. These types of approaches include state fixed effects as an alternative way to account for factors that are typically unobserved (such as anti-smoking sentiment). In this approach, within state changes in prices or taxes rather than interstate differences identify the teen smoking effect. Significant tax responsiveness using this approach has been estimated for: 1) teen mothers (Gruber 2000, Ringel and Evans 2001, and others); 2) high school seniors (Dee 1999, Gruber and Zinman 2001, and others); and 3) young adults age 18-20 (Sloan and Trogdon 2004). These quasi-experimental studies, however, return estimates of the tax responsiveness of teen smoking that differ with time period and sample and are not always significant.[1]

In this paper we provide new evidence on the effects of state cigarette taxes on the consumption of cigarettes by high school teens. Specifically, we use repeated cross section data from the national, state, and local Youth Risk Behavior Surveys over the period 1991-2005. Our econometric methods are straightforward and involve regression-adjusted difference-in-differences estimation for all data sources, controlling for cigarette taxes, demographics, clean indoor air laws, and area and year fixed effects. This approach effectively compares the change in outcomes for youths in states that increased their cigarette tax to the associated change in outcomes for youths in states that did not

[1] Gruber (2000), for example, does not find younger teens to be responsive to prices, while Dee (1999) finds tax responsiveness only in the latter half of his sample period. Ringel and Evans (2001) find teen mothers to be least responsive to cigarette tax hikes (relative to older mothers), while Sloan and Trogdon (2004) find significant responsiveness for 18-20 year olds but not 21-24 year olds.

experience a tax increase in that year. Across all of our data sources, we find statistically significant evidence that higher cigarette taxes reduce youth smoking participation and frequent smoking. Our separate analyses of the national, state, and local YRBS data suggest that a one-dollar increase in the tax per pack would reduce smoking participation by 3-6 percentage points, or about 10-20 percent. These estimates translate into price elasticities of smoking participation for high school youths in the range -.23 to -.56, which are slightly lower than most previous cross-sectional estimates but very similar to other recent quasi-experimental approaches.

Our research makes several contributions to the literature. First, we extend previous national YRBS analyses (which have only used data through 1997) by making use of data through 2005, the most recent year of the biennial survey. Using these more recent data allows us to consider over twice as many cigarette tax increases as in previous research using the national YRBS. Moreover, many of these tax hikes in the period following the 1998 Master Settlement Agreement (MSA) between US states and the major cigarette companies were quite large. Few quasi-experimental studies have examined tax responsiveness in the post-MSA era (Sloan and Trogdon (2004) and Tauras et al. (2005) are important exceptions).

Second, we provide new estimates from hundreds of state and local versions of the YRBS coordinated by state and local public health departments. Together these surveys include over three quarters of a million high school students over the period 1993-2005. Specifically, we use published aggregate statistics from these data to estimate weighted least squares models of youth substance use in a common quasi-experimental framework. No previous research has used these data to estimate the tax

responsiveness of youth smoking. A major advantage of these data is that – unlike nearly all of the youth survey data used previously to estimate the tax/smoking relationship (including Monitoring the future (MTF), national Youth Risk Behavior Surveys (YRBS), NELS, the National Household Surveys on Drug Abuse (NHSDA) and the National Longitudinal Surveys of Youth 1997 (NLSY97)) – these state and local YRBS surveys were explicitly designed to be representative of the sampled state or locality. As such, we are able to provide suggestive evidence on the degree of bias from using non-representative surveys – an oft-cited limitation of this literature.

Finally, we provide new results on the importance of controlling for state anti-smoking sentiment, as recently highlighted by DeCicca et al. (2006). Specifically, we use YRBS data to replicate their main qualitative finding that the cross-sectional association between cigarette taxes and youth smoking is reduced when we directly control for state adult anti-smoking sentiment (which itself is strongly and inversely related to youth smoking). Despite this, the tax estimate on youth smoking remains negative and statistically significant even after controlling for anti-smoking sentiment in the cross section. We also show inclusion of anti-smoking sentiment does not alter the conclusions drawn from our preferred difference-in-differences models.

The paper proceeds as follows. In Section 2 we outline the empirical challenges facing researchers in this literature and describe a handful of studies that use quasi-experimental methods to evaluate the effects of state cigarette taxes on youth smoking. Section 3 presents the data and empirical approach, and Section 4 presents the results. Section 5 offers a discussion and concludes.

2. Motivation, Empirical Challenges, and Literature Review

A review of the microeconometric literature relating state cigarette taxes to youth smoking is beyond the scope of this paper; we direct readers to Chaloupka and Warner (2000) for an excellent review. Briefly, however, we note that a critical issue in this research area has been the difficulty in accounting for typically unobserved state anti-smoking sentiment. The concern is that state cigarette taxes and other tobacco control policies are likely to be correlated with preferences of individuals in those states. In a typical cross section analysis, this problem will tend to attribute too much explanatory power to taxes and tobacco control policies; even in the absence of a true "causal" effect of taxes on youth smoking, for example, this unobserved state anti-smoking sentiment might produce a spurious negative association between state taxes and youth smoking.

To be certain, researchers have long recognized this fundamental omitted variables bias problem, and a variety of approaches have been used to address these concerns. Several studies in this literature, for example, attempt to proxy for unobserved state anti-smoking sentiment by including controls for whether respondents live in tobacco producing states – where anti-smoking sentiment is likely to be low. Another common approach is to include a control for aggregate, overall cigarette consumption in the state (either current or lagged), while other studies include controls for percent of state residents that belong to various religions. To the extent that these variables capture the previously unmeasured state anti-smoking sentiment, their inclusion should reduce the severity of the bias on the tax and other policy coefficients in a model of youth smoking.

An alternative approach for dealing with state anti-smoking sentiment has been to estimate models that include year and state fixed effects. The advantage of this approach is that it removes any time-invariant characteristics about states that are likely to bias tax coefficients in models of youth smoking.[2] Indeed, to the extent that state anti-smoking sentiment does not change over time, this approach unambiguously purges tax and other policy estimates from the associated bias. Of course, there are a handful of issues related to the feasibility of state fixed effects models. First, they require multiple observations on states; one-time cross-sectional surveys cannot support inclusion of state dummies. A second complication of the quasi-experimental approach is that – even with repeated observations on states – the empirical set up requires variation in the variable of interest within states over time (else the tax variable will be perfectly collinear with state dummies). That is, the data must span a period witnessing state changes in excise taxes on cigarettes or other changes in public policy toward youth smoking.[3]

Several studies using this quasi-experimental method have found strong evidence that youth smoking responds to tax changes. Ringel and Evans (2001) used smoking information from birth certificate records for teen mothers over the period 1989-1995. They found significant tax responsiveness of smoking participation among these young mothers in state fixed effects models, though the estimated sensitivity for teens was not greater than for older women. Dee (1999) used data from the Monitoring the Future

[2] This approach to estimating price effects on health-behavior choices was introduced by Cook and Tauchen (1982) and has become standard in the econometric literature (Cook 2007).

[3] A common diagnostic used to evaluate the severity of this problem in the context of cigarette taxes is to regress the tax on state and year dummies. In this auxiliary regression, a high R-squared (upwards of .90) is usually viewed as problematic. Our data spanning many tax changes largely avoids this problem: the R-squared from a regression of taxes on state and year dummies in the 1991-2005 YRBS is just .70.

study over the 1977-1992 period to estimate effects of state cigarette taxes on youth smoking and drinking participation. He found a robust own-price effect on smoking for youths, but only in the latter part of the sample period. Gruber (2001) and Gruber and Zinman (2000) pooled data from the first four waves of the national YRBS (1991-1997) and estimated reduced form models of youth cigarette consumption as a function of individual demographic characteristics, state excise taxes on cigarettes, and clean indoor air laws. They found a modest tax effect on consumption that was confined to older youths (high school seniors).[4] Sloan and Trogdon (2004) used data from the Behavioral Risk Factor Surveillance System (BRFSS) on adults over age 18 and found a significant price response of smoking among young adults age 18-20 in models with state and year fixed effects. Across a variety of samples, then, cigarette taxes have been shown to significantly reduce teen smoking even in models with state fixed effects, though not for every sample.

Despite this body of evidence, the real effects of cigarette taxes on youth smoking from quasi-experimental models have themselves been called into question by a series of recent papers that have used longitudinal data on youths from the National Educational Longitudinal Study (NELS:88). This skeptical view was initiated by DeCicca et al. (2002), who estimated discrete time hazard models of youth smoking initiation, controlling for state fixed effects, and found no association between state cigarette taxes and youth smoking. DeCicca et al. (2004) again reported a null finding in their analysis of NELS data; they used variation in effective cigarette prices faced by youths who

[4] Gruber (2001) and Gruber and Zinman (2000) did not find consistent effects of clean indoor air policies on smoking participation, though there was some evidence that government worksite restrictions reduced smoking intensity.

"moved" across state lines versus youths who "stayed" in a state as an alternative identification strategy.

Most recently, DeCicca et al. (2006) analyze two waves of the NELS (1992 and 2000), and report results that support the absence of a substantive association between state cigarette taxes and youth smoking initiation. This paper takes a different and novel approach to dealing with concerns about state anti-smoking sentiment. Using data on adult attitudes about smoking from the Tobacco Use Supplements of the Current Population Survey (CPS) over the 1990's and a standard factor analysis technique, the authors create a state-specific anti-smoking measure. This allows them to directly control for a newly observed measure of state anti-smoking sentiment instead of including alternative proxies such as aggregate cigarette production or "tobacco producing state" indicators. They found that the strong cross-sectional negative association between cigarette prices and youth smoking initiation was not robust to inclusion of this anti-smoking sentiment measure. Equally important, all of the variation in youth smoking "loaded onto" the anti-smoking sentiment measure, which was a statistically significant predictor of youth smoking in the NELS cross section. DeCicca et al. (2006) interpret this result and the continued failure of state fixed effects models to return significant youth smoking effects in the NELS as evidence that cigarette taxes are not an effective means to reduce youth smoking initiation.

Thus it is fair to say that there is still no consensus on whether taxes have a true causal effect on youth participation in smoking.

3. Research Design and Data Description

To estimate the effects of state cigarette tax increases on youth smoking and drinking, we employ restricted use area-identified versions of the 1991-2005 national Youth Risk Behavior Surveys (YRBS), in conjunction with the independent state and local versions of the YRBS. The national surveys – which are distinct from the state and local surveys and were not designed to be representative below the national level – are coordinated every other year by the Centers for Disease Control and are administered to high school students at school in the spring.[5] The purpose of the YRBS is to monitor the prevalence of youth behaviors that most influence health, including use of alcohol, tobacco, and other drugs.

These data provide standard demographic characteristics, information on a variety of behaviors, and the state of survey (requested in a restricted use version of the data directly through CDC). We restrict attention to youths with no missing data on the demographic variables and key outcomes of interest (smoking in the past month), yielding over 100,000 youths. These data have been used by economists in policy evaluations similar to ours (e.g. Gruber and Zinman 2001). The YRBS data produce estimates of past month smoking that closely track the trends from other commonly used data on youths, such as the Monitoring the Future Study and the National Household Surveys on Drug Abuse (Gruber 2000).

Specifically, youths are asked: "During the past 30 days, on how many days did you smoke cigarettes?" We create a variable called Smoker that equals 1 if the person

[5] In the 2005 national YRBS, for example, a probability sample of 203 schools was selected from the universe of public and private schools with at least one of the grades 9-12. One or two classrooms from each grade of each of these sample schools was administered a questionnaire (MMWR 2006, p. 2).

reported any days of smoking and zero otherwise. We also consider an indicator variable called Frequent Smoker that equals 1 if the person reported smoking on at least 20 days in the past 30 and zero otherwise. We chose these outcome variables because they are consistently reported for every state and local YRBS in the *MMWR* publications (described below).[6]

We use a straightforward two-way fixed-effects framework to estimate the effect of state cigarette taxes on youth smoking in the national YRBS data. Specifically, we use standard logit-regression model of the form:

(1) $\ln (p_{ist}/1- p_{ist}) = \beta_0 + \beta_1 X_{ist} + \beta_2(\text{Cigarette Tax})_{st} + \beta_3 Z_{st} + \beta_4 \text{State} + \beta_5 \text{Year} + \varepsilon_{ist}$

where p_{ist} is the probability that the individual has smoked in the last month (or smoked frequently in the last month). X_{ist} is a vector of individual demographic characteristics, including dummies for: female, black, other race, Hispanic, grade, and age. Z is a vector that includes the state unemployment rate and indicators for clean indoor air laws in venues likely to affect high school students.[7] State is a vector of state dummies, and Year is a vector of year dummies. Cigarette Tax is the state tax on a pack of cigarettes in 2005

[6] The *MMWR* publications do not report measures of smoking intensity other than the frequent smoking outcome. Cigarettes smoked per day, for example, is not reported. Similarly, smokeless tobacco use (snuff or chew) is not consistently reported for the state or local data. Given that we are interested in comparing tax estimates on smoking across the national, state, and local YRBS data, we focus attention on the outcomes consistently reported across all data sources.

[7] We use venue coding from the Robert Wood Johnson's ImpacTeen program. Specifically, we control for restrictions in government worksites, schools, private worksites, shopping malls, and restaurants. Examples of venues we exclude include child care centers. Results are not sensitive to including other venues, and no venue-specific restriction was consistently significant in the predicted direction. These null findings are similar to those from Gruber and Zinman (2000).

dollars.[8] The coefficient of interest, β_2, captures the relative effect of state cigarette taxes on youth smoking by comparing within area increases in state cigarette taxes to the associated outcomes for youths in states that did not experience a cigarette tax increase in that year. Standard errors are clustered at the state level throughout (Bertrand, Duflo, and Mullainathan 2004).

In addition to the national YRBS, we also make use of aggregate statistics from the state and local YRBS. These surveys are coordinated by public health officials in the respective states and include standard questions that reproduce those in the national survey. As described above, an important feature of these data is that the majority of the state and local efforts were explicitly designed to be representative of the state or locality in question. To our knowledge, these weighted state and local surveys are the only consistent state/year panel of representative data on smoking among high school students. The coverage of state surveys is extensive (see Appendix Table 5), while the local YRBS modules are generally concentrated in large urban centers such as Los Angeles, New York City, Boston, and Chicago (see Appendix Table 6 for a complete list). The state and local YRBS are fielded every other year, and the aggregate estimates of the health outcomes based on the underlying microdata are published in *Morbidity and Mortality Weekly Report*. Included in each report are various characteristics for each site. Specifically, we observe: the relevant sample size on which the estimates are based;

[8] These data come from *The Tax Burden on Tobacco* and the Campaign for Tobacco Free Kids. We use the tax in effect as of March of the survey year (i.e. the current tax). Note that we ignore the fact that a handful of localities also impose cigarette taxes. We are not aware of a consistent state/year panel of local cigarette taxes, and previous research similarly does not control for local taxes. We also estimated models that included controls for the square of the cigarette tax in addition to the tax itself. The cigarette tax estimate was large, negative, and statistically significant while the coefficient on the squared tax was smaller and statistically insignificant.

whether the survey was unweighted or weighted to be representative; the overall, student, and school response rates; the fraction of the school population that is white, black, other race, and Hispanic; the fraction of the population that is in each grade (9-12); and various aggregate outcomes with respect to substance use.

For our state and local YRBS analyses, we estimate separate weighted least squares models where the weights are the relevant sample sizes of the surveys on which the substance use rates are based. In particular, we estimate the following OLS regressions:

(2) $\ln(Y_{at} / 1 - Y_{at}) = \beta_0 + \beta_1 X_{at} + \beta_2 (\text{Cigarette Tax})_{at} + \beta_3 Z_{at} + \beta_4 \text{Area} + \beta_5 \text{Year} + \varepsilon_{at}$

where a denotes area (city or state) and t denotes survey year. Since we only observe the aggregate outcomes reported in the MMWR publications, Y_{at} is the fraction of the sample reporting the behavior in question (smoking or frequent smoking). X_{at} is a vector of sample characteristics that includes: overall response rate, school response rate, student response rate, percent grade 10, percent grade 11, percent grade 12, percent black, percent other race, and percent Hispanic. The variables in Z are as described above. Area is a vector of either state dummies or city dummies, depending on the dataset. Year is a vector of survey year dummies. β_2 is again the coefficient of interest, and standard errors are clustered at the state level.

Appendix Tables 4 – 6 present the state/year and city/year combinations for which we have national, state, and local YRBS data, respectively. We also shade the area/year combinations in which a state increased its excise tax on cigarettes to illustrate the extent

of within-place variation over our sample period.[9] Several patterns merit discussion. First, the national and state YRBS surveys have a wide but far from complete coverage of states. Second, there are numerous state policy experiments with respect to excise tax increases that we can examine in these data. Third, the more recent data – since 1999, for example – contains more data points and more policy experiments than the earlier 1991-1997 data analyzed by previous research.

4. Results

We present descriptive statistics for the national, state, and local YRBS in Tables 1a and 1b. The national YRBS data suggest that over the entire period almost 30 percent of high school youths smoked cigarettes in the past month, and 13 percent smoked on at least 20 of the previous 30 days. The state YRBS surveys produce similar rates of past month smoking (Table 1b) but the local YRBS produce lower estimates. This difference likely reflects the fact that the local surveys were concentrated in urban areas; only 19 percent of the local samples are white students, for example. As such, the lower rates in the local YRBS data are explained by the well-documented lower rates of teen smoking by non-white youths. Table 1b also illustrates the key advantage of these local and state surveys: the vast majority were explicitly designed to be representative (88 and 79 percent, respectively).

[9] Note that because the surveys are administered at schools in the Spring, we are not able to use *all* of the policy changes shaded in the Tables. If a state changed its excise tax in the fall, for example, we highlight it in the Tables but any data observed in that year is coded as being from the "pre" tax period. In all cases, we assume the surveys were administered in March of the survey year. Individual discussions with survey administrators and the CDC highlighted that while there is no way to verify when surveys were administered, February and March are common months for administration of the YRBS.

Table 2 presents the baseline cross-section and quasi-experimental estimates for the outcome indicating any smoking in the past month from the national, state, and local YRBS data. Across all three data sources we find consistent evidence that state excise taxes on cigarettes are negatively related to youth smoking participation. Importantly, we find that the difference-in-differences estimates (based on regressions with fixed effects included) are somewhat smaller than the "cross section" estimates without fixed effects, consistent with the idea that the cross section estimates are biased because unobserved state characteristics are correlated with cigarette taxes and smoking outcomes. Even after accounting for time-invariant area characteristics, however, we find across all three data sources that increases in state cigarette taxes significantly reduce youth smoking participation. Appendix Table 1 presents detailed coefficients on the control variables in the smoking participation model using the national YRBS data.[10]

In Table 3 we present the estimates from the frequent smoking outcome (defined as smoking on at least 20 of the past 30 days). The format of Table 3 mirrors that of Table 2, with similar coefficient estimates (although in this case the inclusion of fixed

[10] We also estimated models separately by race in the national YRBS data (the only source for which we have the microdata on race). Tax estimates for white youths were slightly larger than the baseline estimates and statistically distinguishable from zero. Tax estimates for nonwhite youths were negative, smaller than the baseline, and not statistically significant (we could not rule out either large tax responsiveness or no tax responsiveness). We also considered models separately before and after the Master Settlement Agreement in 1998. The pre-MSA analysis on the national YRBS data essentially mirrored Gruber's (2001) finding: taxes were negatively related to youth smoking, but the estimates were not significant in the full sample. The post-MSA observations returned imprecisely estimated null tax estimates in the national data. We also performed this same exercise on the state YRBS data and obtained the opposite pattern: the pre-MSA tax coefficient (using only 1993, 1995, and 1997 observations) was an imprecisely estimated zero, while the post-MSA tax coefficient was large and negative. We therefore view the investigation into pre/post MSA differences in tax responsiveness as inconclusive.

effects results in larger coefficients in two of three cases). To translate the point estimates from the logit regressions into something more intuitive, we compute the point estimate for change in probability of smoking (or frequent smoking) using as the baseline the mean smoking rates over the sample period from the national YRBS (29.5% for smoking participation and 13.5% for frequent smoking). We provide point estimates based on the high and low estimates of responsiveness from the regression results. In sum, a $1.00 increase in tax would reduce smoking prevalence from 29.5% to 26.8% (state surveys data) or 23.6% (national survey). In other words, the reduction would be 2.7 – 5.9 percentage points. From the baseline of 29.5%, this is a reduction of about 9 – 20%.

Projections from regression estimates in Tables 2 and 3

	Sample mean, national YRBS	Estimated rates following $1.00 increase in tax: national data	Estimated rates following $1.00 increase in tax: State surveys
Prevalence of smoking	29.5%	23.6%	26.8%
Change		-5.9%	-2.7%
Prevalence of frequent smoking	13.5%	9.4%	11.1%
Change		-4.1%	-2.4%

Similarly, the reduction in the prevalence of frequent smoking ranges from 2.4 to 4.1 percentage points, which is about 18 – 30% of the sample mean (13.5%).

Note that "frequent smoking" is the only measure of smoking intensity that is available for all three sets of YRBS surveys, and that is why we focus on this measure. The national survey, but not the state and city surveys, also includes an item on number of cigarettes smoked in the last month. Appendix Table 1 reports the results of regressing that measure (in log form) on the same specification as reported in Tables 2 and 3. The

coefficient estimate for the tax rate is essentially zero. Since the sample for that regression is restricted to smokers, this result implies that those youths who continue to smoke when taxes are raised smoke the same amount on average as the larger group who smoked at the previous (lower) tax rate. One interpretation is that tax has no effect on the intensive margin for smokers. Another interpretation is that the "zero" effect is the result of two processes that tend to cancel out – those who quit in response to higher taxes tend to be the lighter smokers (thus raising the average of those who continue), and those who continue smoking in fact smoke less than they otherwise would have.

In Table 4 we provide some evidence on the sensitivity of our main smoking estimates to the quantity and quality of the data. Recall that our analyses of state and local YRBS data have thus far restricted attention to observations that are constructed to be unbiased, in the sense that sample weights were used in computing the population estimates. Table 4 reprints the state and local YRBS estimates from this restricted data set, and also displays the results of including other state YRBS observations for which population estimates are computed from survey results without weighting (creating a real possibility of bias). In the state YRBS analysis, this is a sizable number of observations – about 50, or over one quarter of the sample size when using weighted surveys only. For the local YRBS analysis, the absolute discrepancy is smaller (14), but as a percentage of the weighted surveys (14%), it is still substantial. The estimates in Table 4 suggest that restricting attention to the weighted estimates for the state YRBS analysis has very little effect on coefficient and standard error estimates.

In Table 5 we explore the robustness of our main smoking results to inclusion of direct controls for state anti-smoking sentiment. Recall that this approach was recently

used by DeCicca et al. (2006) as an alternative way to test for the strength of the cross-sectional relationship between taxes and youth smoking, as well as to provide a commentary on the ability of previous approaches to appropriately capture state anti-smoking sentiment. We use the direct anti-smoking sentiment measures created by DeCicca et al. to examine whether their main empirical result holds up in the YRBS.[11]

The results of these exercises are presented in Table 5 for smoking participation.[12] First, in Row 1 we report the cross-sectional results (without fixed effects) for the state cigarette tax and anti-smoking sentiment. When entered by itself the tax coefficient is significantly negative (as was shown previously in Table 2). The same is true when we enter sentiment directly without controlling for the state cigarette tax. When we include both measures in Column 3, we reproduce the main DeCicca et al. (2006) finding: the tax coefficient becomes smaller, with much of the estimated variation "loading onto" the sentiment measure. Notably, however, our tax estimate remains negative, sizable, and statistically significant at the ten percent level. That is, using this alternative approach for dealing with state anti-smoking sentiment, we continue to find evidence for a significant role for state cigarette taxes.[13] This finding is consistent both with a significant tax

[11] The anti-smoking sentiment measure is created from the 1992/93, 1995/96, 1998/99, and 2000/2001 Tobacco Use Supplements to the Current Population Survey. This yields direct measures of sentiment for 1993, 1995, 1999, and 2001 in our data. In private correspondence with Don Kenkel (12/08/2006) we have obtained a complete state specific anti-sentiment measure for all years of our sample (1991-2005) based on linear interpolations from the years for which direct sentiment values are observed. Models that restricted attention only to those years for which the sentiment variable is directly observed produced similar results.

[12] Models for frequent smoking produced similar results and are not presented here to conserve space.

[13] The coefficient estimate suggests that a one dollar increase in cigarette taxes would reduce youth smoking participation by 2.8 percentage points.

responsiveness of youth smoking and with the claim that failing to account for unobserved anti-smoking sentiment overstates the magnitude of this relationship.

In Row 2 of Table 5 we further explore this issue by presenting the associated difference-in-differences estimates of youth smoking in models that do and do not control for the state sentiment measure. Note here that there is very little within state variation in state anti-smoking sentiment once state dummies are included in the model; as such, this exercise is not intended to be a "test" of sentiment but rather a comment on the sensitivity of the tax estimate to inclusion of sentiment in a model that includes state dummies. We again find that in models that include state and year dummies, taxes and sentiment entered separately are both negatively related to youth smoking (recall that higher values of the sentiment measure reflect more anti-smoking attitudes). We also find that the tax estimate remains largely unchanged (i.e. it is still large, negative, and statistically significant) once we account for state anti-smoking sentiment in the difference-in-differences model. Finally, we note that although the standard errors on the sentiment variable become much larger once we include state dummies (given their strong collinearity), the point estimates are largely unchanged. That is, the limited within state variation in sentiment that does exist continues to have a large estimated negative relationship with youth smoking that is very similar to the cross-sectional estimate. Again, we interpret the patterns in Table 5 as consistent both with a strong effect of cigarette taxes on youth smoking and with the possibility that state anti-smoking sentiment exerts independently meaningful effects on smoking outcomes.

The fact that we find significant effects of the cigarette tax raises a natural question: do these estimates reflect "real" price effects or are they confounded by other

changes that tended to occur at the same time and place as the tax increases?[14] To partially address the possibility that the tax change was linked in that sense to other changes influencing health behaviors, we experimented with other items found in the national YRBS. Reassuringly, we found that none of the following were statistically related to cigarette tax increases: the likelihood of using cocaine in the past month and the likelihood of carrying a weapon in the past month.[15] (The results are not reported here but are available upon request). Thus, the significant cigarette tax effects we estimate for tobacco consumption are unlikely to be attributable to, say, coincident public health campaigns.

5. Discussion and Conclusion

The results in Tables 2-5 indicate that state cigarette tax increases of the past 15 years were effective at reducing smoking participation and frequent smoking by high school students. Across our analyses of three distinct data sets, we find qualitatively similar estimates: an increase in the state cigarette tax reduces the probability a youth reports past 30 day smoking and frequent smoking.

A remaining issue is whether the magnitude of our estimated cigarette tax effects seems reasonable. Recall that television advertisements for the proposed $2.60 cigarette tax increase in California claimed that it would decrease teen smoking by 43 percent. How does that compare with our estimates? If we use the mean of smoking participation

[14] It could be, for example, that states use the funds raised from cigarette taxes to promote general public health initiatives. In that case, one may worry that a negative association between state cigarette taxes and "bad" youth outcomes, even in the presence of state and year dummies, is not reflecting the effects of changing tobacco consumption per se.
[15] We also found that the likelihood of using snuff or chew in the past month was unrelated to cigarette taxes.

in the national YRBS data as a baseline (29%), then from the results in Table 2, the range of point estimates following a $2.60 increase is from 13.6% (national survey) to 22% (state surveys). The proportional reduction in the first case is 53%, even larger than the ad claim, but the second estimate is just a 24% reduction. It seems fair to say that if the advertising is to be faulted, it is for conveying a false sense of precision rather than for exaggeration.

An alternative and useful way to think about magnitudes in this context is to compare the implied price elasticities from our national, state, and local YRBS analyses to those from the previous literature. DeCicca et al. (2002) report from several sources that the "consensus" estimate regarding the price elasticity of youth smoking was about -0.7, which suggests that a 10 percent increase in the price of cigarettes would reduce youth smoking by about 7 percent. Given a mean tax in our national YRBS sample of 52 cents, we estimate a tax elasticity of teen smoking to be -0.106. Calculating the price elasticity implied by this estimate requires information on the pass through of state cigarette taxes to prices as well as the fraction of the total price attributable to the tax. The latter figure is directly calculable from our data using information on real prices from the Tax Burden on Tobacco; over our sample taxes constitute about 17 percent of the retail price. For the pass-through of taxes to prices we use an estimate from Keeler et al. (1996) of 1.11; that is, a one dollar increase in taxes increases the average retail price by one dollar and eleven cents. Using these estimates in conjunction with our estimated tax elasticity from the national YRBS data suggests a price elasticity of -0.56, which is fairly close to the "consensus" estimate of -0.7. Our state YRBS estimates, however, produce a notably smaller tax elasticity estimate of -0.047; this translates to a teen price elasticity of

smoking participation of about -0.25.[16] This estimate is very close to the estimate of -0.27 reported in Sloan and Trogdon (2004) for 18-20 year olds, which also was derived from a model with state and year fixed effects and included the post-MSA period.

Several caveats and limitations are in order. Some of these are typical to studies that use youth survey data to evaluate public policies regarding youth consumption of harmful substances. For example, the YRBS surveys are administered to youths at school. This procedure necessarily excludes youths absent from school on the survey day, as well as high school dropouts. These missing youths may be more or less responsive to cigarette prices than those who answered the survey. Whether our estimates can be reliably extended to the entire population of high-school-aged youths is unknown. Second, the YRBS substance use data are all self reported. This is an obvious limitation to studies such as ours, and there is little we can do to correct for systematic errors in reporting. Reporting errors will lead to bias in coefficient estimates if the under reporting rate is systematically related to changes in state excise taxes.[17]

It should also be noted that we have not specified the mechanisms by which an increase in cigarette taxes affect youth smoking decisions. Most high-school students are

[16] The price elasticity estimate from the city/local YRBS data falls in between these estimates at -0.49.

[17] Only about three percent of youths do not report information on smoking outcomes. In an evaluation context we were most concerned that failure to report smoking behavior may be correlated with cigarette tax changes. To evaluate this problem, we first estimated a model where we related the likelihood of missing information on smoking outcomes to the cigarette taxes; although the coefficient on the cigarette tax was positive, it was very small and statistically insignificant. Specifically, we estimated that a one dollar increase in the state cigarette tax would increase the probability of missing data on smoking outcomes by .06 percentage points, or only about 2 percent. Moreover, recoding all observations with missing information on smoking as past 30 day smokers (i.e. making the extreme assumption that all non-responders are actually smokers) did not change our main results: the tax coefficient in this model was very similar to the baseline presented below and remained statistically significant at the five percent level.

too young to legally buy cigarettes, and most of them obtain their cigarettes from social sources (friends and family). An increase in cigarette taxes may make potential sources more reluctant to provide youths with cigarettes, or lead them to charge more. The prevalence of smoking among friends and family may also be influential through social contagion processes (Krauth 2005, Powell and Chaloupka 2005, and others), so that the increase in taxes influences youth smoking indirectly by influencing the smoking rates in their social environment. Perhaps the best interpretation of our results is that they reflect a reduced form of direct and indirect influences on youth decisions. Regardless, these results offer new support for the belief that raising cigarette taxes will help discourage youths from smoking.

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Table 1a: Means of key variables, National YRBS 1991-2005

	National YRBS Surveys
Past 30 day smoker	.29
Past 30 day frequent smoker (smoked on at least 20 of past 30 days)	.13
State Cigarette tax (in 2005 dollars)	.52
Female	.49
Black non Hispanic	.13
Other race	.07
Hispanic	.13
Grade 9	.26
Grade 10	.25
Grade 11	.24
Grade 12	.24
Age	16.1
N	101,633

Weighted means.

Table 1b: Means of key variables, State and Local YRBS 1993-2005

	YRBS Local Surveys	YRBS State Surveys
Percent from representative surveys	.88	.79
Overall response rate	.71	.65
School response rate	.98	.81
Student response rate	.73	.81
Female	.51	.50
Grade 9	.33	.29
Grade 10	.26	.26
Grade 11	.22	.23
Grade 12	.19	.21
White	.19	.68
Cigarette tax (in 2005 dollars)	.62	.51
Past 30 day smoker	.19	.29
Frequent smoker	.06	.14

State and Local YRBS means are unweighted.

Table 2: The effect of state cigarette taxes on youth smoking participation, Alternative data sets and specifications

	(1)	(2)	
	"Cross Section" with no fixed effects	"Diff in diff" With fixed effects	N
(1) National YRBS data, 1991-2005: Logit regressions	-.361*** (.092)	-.286*** (.101)	101,633
(2) State YRBS data, 1993-2005: OLS regressions on ln(p/1-p)	-.145 (.101)	-.131*** (.046)	181
(3) City YRBS data, 1993-2005 OLS regressions on ln(p/1-p)	-.347** (.131)	-.243* (.137)	97
Year indicators?	No	Yes	
State or city indicators?	No	Yes	

Note: Each cell in columns 1 and 2 represent the results of a different regression. Demographic controls for row 1 include gender, race, grade, and age dummies. Robust standard errors in parentheses adjusted for clustering at the state level.
Controls for rows 2 and 3 include overall response rate, school response rate, student response rate, percent grade 10, percent grade 11, percent grade 12, percent black, percent other race, and percent Hispanic
Additional controls in all models include the state unemployment rate and ImpacTeen Clean Indoor Air ratings for restaurants, public schools, shopping malls, government worksites, and private worksites. The cigarette taxes are entered in 2005 US dollars.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: The effect of state cigarette taxes on youth frequent-smoking participation, Alternative data sets and specifications

	(1)	(2)	
	"Cross Section" with no fixed effects	"Diff in diff" With fixed effects	N
(1) National YRBS data, 1991-2005: Logit regressions	-.409*** (.103)	-.352*** (.147)	101,633
(2) State YRBS data, 1993-2005: OLS regressions on ln(p/1-p)	-.166 (.114)	-.202*** (.057)	181
(3) City YRBS data, 1993-2005 OLS regressions on ln(p/1-p)	-.254 (.156)	-.331* (.178)	97
Year indicators?	No	Yes	
State or city indicators?	No	Yes	

Note: Each cell in columns 1 and 2 represent the results of a different regression. Demographic controls for row 1 include gender, race, grade, and age dummies. Robust standard errors in parentheses adjusted for clustering at the state level.
Controls for rows 2 and 3 include overall response rate, school response rate, student response rate, percent grade 10, percent grade 11, percent grade 12, percent black, percent other race, and percent Hispanic
Additional controls in all models include the state unemployment rate and ImpacTeen Clean Indoor Air ratings for restaurants, public schools, shopping malls, government worksites, and private worksites. The cigarette taxes are entered in 2005 US dollars.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: The effect of state cigarette taxes on youth smoking participation, Sensitivity to inclusion of "unrepresentative" observations

	(1)		(2)	
	All available observations	N	Unbiased observations only	N
Dependent variables: Ln(p/1-p)				
(1) State YRBS data, 1993-2005:				
Current smoker	-.115** (.044)	230	-.131*** (.046)	181
Frequent smoker	-.165*** (.053)		-.202*** (.057)	
(2) City YRBS data, 1993-2005				
current smoker	-.198 (.126)	111	-.243* (.137)	97
frequent smoker	-.392 (.226)		-.331* (.178)	

Note: All models include area and year fixed effect dummies.
Each coefficient estimate is from a different regression.
An "unbiased" observation is a weighted average of survey responses for a particular year and state or city; the weights are computed to produce an unbiased estimate of the population rate for that area and year.
All models are estimated using least squares, weighted by the sample size on which each observation is based.
Controls in all models include the overall response rate, school response rate, student response rate, percent grade 10, percent grade 11, percent grade 12, percent black, percent other race, and percent Hispanic state unemployment rate and ImpacTeen Clean Indoor Air ratings for restaurants, public schools, shopping malls, government worksites, and private worksites.
The cigarette taxes are entered in 2005 US dollars.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: The effect of state cigarette taxes and state anti-smoking sentiment on youth smoking participation, National YRBS data 1991-2005

	(1)	(2)	(3)	
	Tax only	Sentiment only	Tax & Sentiment	N
(1) "Cross section" No fixed effects				
Cigarette tax	-.361*** (.092)		-.136* (.080)	101,633
Anti-smoking sentiment		-.875*** (.146)	-.778*** (.143)	
(2) "Diff in Diff" State & Year Fixed Effects				
Cigarette tax	-.286*** (.101)		-.273** (.109)	101,633
Anti-smoking sentiment		-.864 (.671)	-.694 (.614)	

Note:
Each cell contains results from a different regression.

All models are estimated using least squares, weighted by the sample size on which each observation is based.
Controls in all models include the state unemployment rate and ImpacTeen Clean Indoor Air ratings for restaurants, public schools, shopping malls, government worksites, and private worksites.
The cigarette taxes are entered in 2005 US dollars.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table 1: Expanded set of coefficient estimates
Logit coefficients reported for Smoking Participation Outcome
Outcome for Smoking Intensity is log of the # cigarettes smoked last month (among smokers) using OLS
1991-2005, National YRBS

Variable	Smoking Participation	Smoking Intensity
Real cigarette tax, $2005 dollars	-.286***	-.012
	(.101)	(.166)
Indoor air – restaurants	-.046	-.041
	(.088)	(.159)
Indoor air – public schools	.009	.066
	(.027)	(.050)
Indoor air – shopping malls	.081	-.140
	(.150)	(.236)
Indoor air – government sites	.025	-.070
	(.087)	(.102)
Indoor air – private worksites	-.061	.256
	(.167)	(.267)
Unemployment rate	-.015	.089
	(.035)	(.058)
Black	-1.066***	-1.124***
	(.078)	(.121)
Other race	-.228***	-.043
	(.053)	(.112)
Hispanic	-.151***	-.602***
	(.036)	(.078)
Female	.027	-.105***
	(.029)	(.049)
R squared	.044	.072
N	101633	27028

See notes to Table 2. A full set of state, year, age, and grade dummies are also included but not reported. They are available upon request.

Appendix Table 2:
Distribution of states and cities by # years observed

# years observed in sample	# states in national YRBS	# states in state YRBS	# cities in local YRBS
1	7	3	5
2	7	3	5
3	4	5	3
4	5	6	4
5	4	2	1
6	10	14	4
7	3	14	5
8	7	N/A	N/A

Appendix Table 3:
Number of site observations in each year and data source

# years observed in sample	# states in that year in national YRBS	# states in that year in state YRBS	# cities in that year in local YRBS
1991	22	N/A	N/A
1993	28	31	11
1995	24	32	14
1997	28	34	15
1999	22	34	13
2001	29	35	17
2003	28	33	16
2005	31	41	18

Appendix Table 4:
N indicates national YRBS data for that state in that year
Shaded Cell Indicates State Increased Cigarette Tax That Year or Previous Year

Location	1991	1993	1995	1997	1999	2001	2003	2005
Alabama		N	N	N	N	N	N	
Alaska								
Arizona		N		N	N	N	N	N
Arkansas		N	N	N			N	
California	N	N	N	N	N	N	N	N
Colorado	N	N	N	N		N		
Connecticut				N				N
Delaware			N				N	
DC			N					
Florida	N	N	N	N	N	N	N	N
Georgia	N	N	N	N	N	N	N	N
Hawaii					N			
Idaho						N		N
Illinois	N	N	N		N	N	N	N
Indiana	N					N	N	N
Iowa			N	N				N
Kansas		N		N			N	N
Kentucky								N
Louisiana			N	N	N		N	N
Maine		N	N	N	N	N	N	
Maryland	N	N		N			N	
Massachusetts		N	N	N		N	N	N
Michigan	N	N	N	N	N	N	N	N
Minnesota		N						N
Mississippi	N	N	N	N	N	N		
Missouri	N	N	N		N	N	N	N
Montana						N		
Nebraska		N						
Nevada						N		
NewHampshire	N							
New Jersey	N			N	N	N	N	N
New Mexico	N	N		N		N	N	
New York	N	N	N	N	N	N	N	N
North Carolina		N	N	N	N	N		N
North Dakota								
Ohio	N	N	N	N	N	N	N	N
Oklahoma				N		N		N
Oregon		N				N		N
Pennsylvania	N	N	N	N	N		N	N
Rhode Island					N			
South Carolina	N	N		N		N	N	N

South Dakota	N						N	
Tennessee		N	N	N	N	N		N
Texas	N	N	N	N	N	N	N	N
Utah							N	N
Vermont	N						N	
Virginia	N		N		N		N	N
Washington	N	N	N	N		N		N
West Virginia		N				N		N
Wisconsin				N	N	N	N	N
Wyoming								

Appendix Table 5:
S indicates state YRBS data for that state in that year
Shaded Cell Indicates State Increased Cigarette Tax That Year or Previous Year

Location	1993	1995	1997	1999	2001	2003	2005
Alabama	S	S	S	S	S	S	S
Alaska		S		S		S	
Arizona						S	S
Arkansas	S	S	S	S	S		S
California		S	S				
Colorado		S	S		S		S
Connecticut			S	S			S
Delaware	S	S	S	S	S	S	S
DC	S	S	S	S	S	S	S
Florida			S	S	S	S	S
Georgia	S	S				S	S
Hawaii	S	S	S	S	S		S
Idaho	S	S			S	S	
Illinois	S	S		S	S		
Indiana					S	S	S
Iowa			S	S	S		S
Kansas							S
Kentucky	S		S	S	S	S	S
Louisiana	S		S	S	S		
Maine	S	S	S	S	S	S	S
Maryland							S
Massachusetts	S	S	S	S	S	S	S
Michigan		S	S	S	S	S	S
Minnesota							
Mississippi	S	S	S	S	S	S	S
Missouri		S	S	S	S	S	S
Montana	S	S	S	S	S	S	S
Nebraska	S	S		S	S	S	S
Nevada	S	S	S	S	S	S	S
New Hampshire	S	S	S	S	S	S	S
New Jersey	S	S	S	S	S		S
New Mexico	S			S			S
New York	S		S	S	S	S	S
North Carolina	S	S	S		S	S	S
North Dakota		S	S	S	S	S	S
Ohio	S	S	S	S		S	S
Oklahoma						S	S
Oregon	S						
Pennsylvania							

Rhode Island		S	S		S	S	S
South Carolina	S	S	S	S	S		S
South Dakota	S	S	S	S	S	S	S
Tennessee	S	S	S	S	S	S	S
Texas					S	S	S
Utah	S	S	S	S	S	S	S
Vermont	S	S	S	S	S	S	S
Virginia							
Washington							
West Virginia	S	S	S	S		S	S
Wisconsin	S		S	S	S	S	S
Wyoming	S	S	S	S	S	S	S

Appendix Table 6:
Cities participating in the Local YRBS (X indicates Data for That Year)
Shaded Cell Indicates State Increased Cigarette Tax That Year or Previous Year

Location	1993	1995	1997	1999	2001	2003	2005
Los Angeles, CA		X	X		X	X	
San Bernardino, CA				X	X	X	X
San Diego, CA	X	X	X	X	X	X	X
San Francisco, CA	X	X	X	X	X		X
Denver, CO		X					
Broward County, FL						X	X
Ft. Lauderdale, FL	X	X	X	X	X		
Hillsborough Cty, FL							X
Miami, FL	X	X	X	X	X	X	X
Palm Beach, FL				X	X	X	X
Orange County, FL						X	X
Orlando, FL					X		
DeKalb County, GA						X	X
Chicago, IL	X	X	X	X	X	X	X
New Orleans, LA	X	X	X	X	X	X	X
Baltimore, MD			X				X
Boston, MA	X	X	X	X	X	X	X
Detroit, MI		X	X	X	X	X	X
Jersey City, NJ	X	X	X				
Newark, NJ			X				
New York, NY	X		X	X	X	X	X
Charlotte, NC							X
Philadelphia, PA	X	X	X	X	X	X	
Memphis, TN						X	X
Dallas, TX	X	X	X	X	X	X	X
Houston, TX		X	X	X	X		
Seattle, WA	X	X			X		
Milwaukee, WI					X	X	X