Economic Inquiry

DO HIGHER TOBACCO TAXES REDUCE ADULT SMOKING? NEW EVIDENCE OF THE EFFECT OF RECENT CIGARETTE TAX INCREASES ON ADULT SMOKING

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There is a general consensus among policymakers that raising tobacco taxes reduces cigarette consumption. However, evidence that tobacco taxes reduce adult smoking is relatively sparse. In this paper, we extend the literature in two ways: using data from the Current Population Survey Tobacco Use Supplements we focus on recent, large tax changes, which provide the best opportunity to empirically observe a response in cigarette consumption, and employ a novel paired difference-in-differences technique to estimate the association between tax increases and cigarette consumption. Estimates indicate that, for adults, the association between cigarette taxes and either smoking participation or smoking intensity is negative, small, and not usually statistically significant. Our evidence suggests that increases in cigarette taxes are associated with small decreases in cigarette consumption and that it will take sizable tax increases, on the order of 100%, to decrease smoking by as much as 5%. (*JEL* I18, I12)

I. INTRODUCTION

There is a general consensus among policymakers that raising tobacco taxes reduces cigarette consumption.[1] However, evidence that tobacco taxes reduce adult smoking is relatively sparse (Gallet and List 2003). For example, casual inspection of trends in tobacco taxes and tobacco use does not suggest a strong inverse relationship between taxes and consumption. As shown in Figure 1, tobacco taxes remained relatively constant in real terms between 1983 and 1990, but tobacco use declined continuously during this period. From 1990 to 1998, tobacco taxes increased by approximately 20%, but tobacco use continued to decline at about the same rate as between 1983 and 1990. Finally, from 1998 to 2008 tobacco taxes nearly doubled and there were over 100 increases in state tobacco taxes, yet tobacco use remained on its long run trend toward less use with no noticeable break as taxes began to increase significantly. More sophisticated empirical evidence, which we review below, also does not provide strong support for the hypothesis that increases in tobacco taxes reduce adult cigarette consumption.

The paucity of evidence regarding the association between tobacco taxes and adult cigarette consumption is inconsistent with the widespread support for taxes as a way to reduce smoking. While support for tobacco taxes also stems from government preferences for raising revenue outside the traditional methods of sales, income, and property taxes, the primary political justification for higher tobacco taxes is the public health argument that assumes that higher taxes will reduce smoking (DeCicca, Kenkel, and Liu 2010a; Hines 2007). Given this justification, we believe it is important to revisit the issue of whether higher tobacco taxes reduce smoking,

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1. For reviews see Chaloupka and Warner (2000) and Gallet and List (2003).

ABBREVIATIONS

AIC: Akaike Information Criterion
BRFSS: Behavioral Risk Factor Surveillance System
CPS-TUS: Current Population Survey Tobacco Use Supplements
DiD: Difference-in-Differences
GLM: Generalized Linear Model
MSA: Metropolitan Statistical Area
NHIS: National Health Interview Survey

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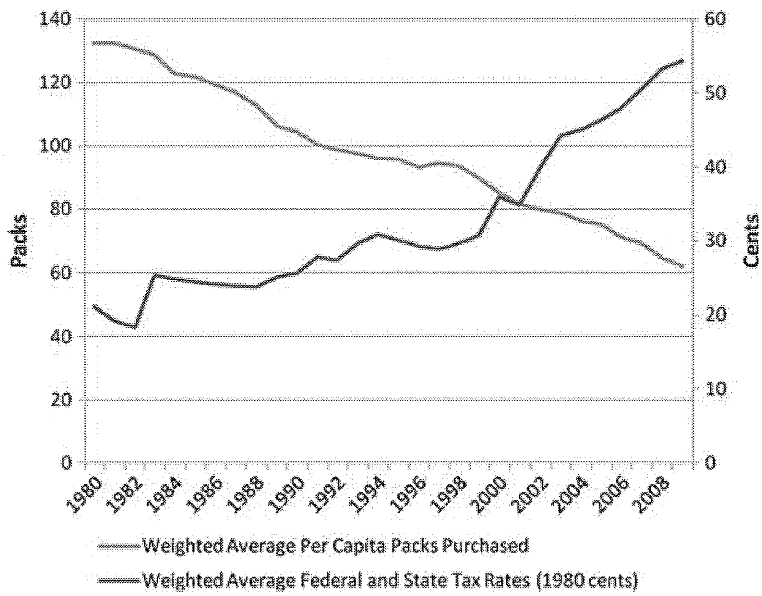
FIGURE 1
Cigarette Consumption and Cigarette Excise Taxes



Source: Orzechowski and Walker, *Tax Burden on Tobacco* (2009).

particularly for adults because there is limited evidence for this group. In addition, revisiting the issue is warranted because the effect of tax increases is expected to differ today from past experience as tobacco taxes are quite high and the current pool of smokers is likely to be dominated by those who have strong preferences for smoking. Thus, previous estimates of the effect of tax increases may not be relevant to current tax changes. Finally, while there are strong theoretical reasons to expect that tobacco taxes reduce smoking, there are also mechanisms by which smoking may be relatively unaffected by taxes. For example, brand loyalty provides some amount of market power to firms, and changes in taxes may not have large effects on retail cigarette prices (DeCicca, Kenkel, and Liu 2010b). Consumers can also shift brands (price points) and seek new, cheaper purchasing outlets including purchases on the black market (e.g., interstate smuggling) to offset some of the effects of taxes (DeCicca, Kenkel, and Liu 2010a; Hyland et al. 2005; Merriman 2010; Stehr 2005).

In this paper, we use data from the Current Population Survey Tobacco Use Supplements (CPS-TUS) to study the effect of recent, large changes in tobacco taxes on both the propensity to smoke and the number of cigarettes smoked (i.e., smoking intensity). We follow previous researchers and obtain estimates of the association between state tobacco taxes and smoking using a standard approach that controls for state and year fixed effects. Using this method, we found that for adult smokers aged 18–74, a 10% tax increase is associated with 0.3–0.6% decrease in smoking participation and 0.3–0.4% decrease in smoking intensity. More surprisingly, given past research suggesting that youth smoking is more sensitive to taxes and prices, we find very little difference by age in the association between cigarette taxes and cigarette consumption. A 10% increase in state cigarette tax is associated with: 0.3–0.7% decrease in smoking participation for those aged 18–34; 0.2–0.4% decrease in smoking participation for those aged 35–54; and 0.3–0.6% decrease in smoking participation for those aged 55–74. Similarly, a 10% increase in state cigarette tax is associated with: 0.3–0.5% decrease in smoking intensity for those aged 18–34; 0.3% decrease in smoking intensity for those aged 35–54; and 0.3–0.4% decrease in smoking intensity for those aged 55–74. Finally, standard errors of estimates are of a magnitude that rule out cigarette tax elasticities with respect to smoking participation (intensity) among adults greater (more negative) than −0.12 (−0.13). Notably, the small elasticities are not explained by tax avoidance through cross-border cigarette purchases. We tested this hypothesis by allowing the effect of a tax increase to differ for those living 0–60 miles, 61–120 miles, and 120 or more miles from a lower-tax-state border. Results from these analyses suggest that cross-border cigarette purchases have little effect on tax elasticity estimates.

We also obtained estimates of associations between tobacco taxes and cigarette consumption using a paired difference-in-differences (DiD) approach that relies on comparing matched treatment and control states (i.e., those states that increased their cigarette excise tax and those that did not) before and after instances of tax increases in the treatment states. The use of a DiD approach allows us to explicitly test the identifying assumption of our research design, which is not possible with the standard, state and year fixed effects method. Surprisingly, a paired DiD approach has rarely been used to study the effect of tobacco taxes even though it is well suited for this purpose.[2] Estimates obtained using this approach are similar to estimates obtained using the standard approach; for those aged 18–74, a 10% increase in cigarette taxes is associated with a −0.2% change in both smoking participation and smoking intensity. Tests of the validity of the identifying assumption underlying the DiD research design suggest that it is a valid approach, although small spurious tax elasticities of smoking intensity of approximately −0.1 could not be ruled out. Considering all the evidence, we conclude that there is insufficient justification for the widespread belief that raising cigarette taxes will significantly reduce cigarette consumption among adults, even young adults. Our evidence suggests that, at best, increases in cigarette taxes will be associated with a small decrease in cigarette consumption and that it will take very sizable tax increases, on the order of 100%, to decrease smoking by as much as 5%.

2. The study by Lien and Evans (2005) is the only other study we are aware of, and this study examined tobacco use among pregnant women in four states. The study of Abadie, Diamond, and Hainmueller (2010) is also an example of the type of DiD approach we employ. In this paper, we use both the traditional DiD and Abadie, Diamond, and Hainmueller (2010) approaches.

II. PREVIOUS RESEARCH ON THE ASSOCIATION BETWEEN ADULT SMOKING AND TOBACCO TAXES AND PRICES

Traditionally, researchers have focused attention on teens when examining the association between cigarette taxes and smoking, and most studies report evidence that teens reduce consumption when prices (taxes) are increased and that the price responsiveness of teens is greater than that for adults (Gallet and List 2003). However, estimates reported in the literature pertaining to adults are quite varied and there is a relative lack of study of the association between taxes (prices) and adult smoking. Here we review several, often cited studies of the association between cigarette taxes (prices) and adult smoking.

Using data from the 1976 National Health Interview Survey (NHIS), Lewit and Coate (1982) found that younger smokers were more price responsive than older smokers and that the bulk of the effect of price increases worked through the decision to smoke. The authors reported a smoking participation price elasticity estimate for adults aged 35 and over of −0.15 and a smoking intensity elasticity of −0.07. With data from the same survey, Evans and Farrelly (1998) and Farrelly et al. (2001) estimated smoking participation and smoking intensity tax elasticities for a variety of years from 1976 to 1993. Like Lewit and Coate, they found that younger smokers were more responsive to changes in cigarette prices (through taxes), but reported no association between taxes and participation for adults over the age of 40. Wasserman et al. (1991) used data from seven waves of the NHIS from 1970 to 1985 and found that both the smoking participation and smoking intensity price elasticities for adults varied substantially over time. Participation price elasticities for adults ranged from 0.06 in 1970 to −0.17 in 1985. Similarly, smoking intensity price elasticities for adults ranged from 0.01 in 1970 to −0.09 in 1985.

Ohsfeldt, Boyle, and Capilouto (1998) used data from the CPS-TUS for September 1992, January 1993, and May 1993 to estimate the association between cigarette taxes and cigarette consumption for males. Participation tax elasticity estimates for teens and young adults ranged from −0.15 to −0.22. However, for adults aged 45 and over the participation elasticity was estimated to be −0.07. Using nine waves of the CPS-TUS from 1992 to 1999, Tauras (2006) reported participation price elasticity estimates for adults aged 18 and older of −0.12 and intensity elasticity estimates of −0.07. Finally, DeCicca and McLeod (2008) used data from the Behavioral Risk Factor Surveillance System (BRFSS) to estimate the association between cigarette tax increases in the post-2001 period, which were generally larger than previous tax increases, and smoking participation among adults aged 45–59 and 45–64. The authors reported participation elasticities centered on −0.3 for 45–59-year olds and −0.2 for 45–64-year olds. These estimates are substantially larger than those reported by Ohsfeldt, Boyle, and Capilouto (1998) and an order of magnitude larger than those reported by Farrelly et al. (2001).

In sum, a relatively small number of studies have examined the association between tobacco taxes (prices) and adult smoking and these studies have not produced a consensus finding. In their review of studies estimating adult responsiveness to changes in cigarette prices, Gallet and List (2003) reported a median price elasticity for adults aged 24 and older of −0.32 with individual estimates varying widely. In the studies just reviewed, adult smoking participation price elasticities ranged from −0.74 to 0.06 while price elasticities of adult smoking intensity ranged from −0.28 to 0.01. In addition, with the exception of DeCicca and McLeod (2008), no study has utilized recent increases in state cigarette excise taxes to examine the association between taxes and adult smokers. In this paper, we extend the literature by examining the association between tobacco taxes and adult smoking, which is under studied, using recent, large tax changes and a novel, paired DiD research design.

III. DATA

We use data from 15 waves of the CPS-TUS, which is a survey of tobacco use sponsored by the National Cancer Institute spanning the years 1995–2007.[3] The CPS-TUS asks several questions regarding tobacco usage including whether the respondent was an everyday or someday smoker. In addition, if the respondent is classified as an everyday smoker, the survey asks for the average number of cigarettes smoked each day.[4] We define smokers to be everyday smokers and consider someday smokers to be nonsmokers in order to maintain consistency in our estimates of smoking intensity.[5] We construct two dependent variables. The first is a measure of smoking propensity and is a binary variable equal to one if the respondent is an everyday smoker and zero otherwise. The second dependent variable is a measure of smoking intensity and is equal to the average number of cigarettes smoked daily (this variable equals zero if the respondent is a nonsmoker).

The CPS-TUS also contains demographic information including age, sex, race, education, marital status, employment status, and family income, which are used in the analyses. We limit the sample to adults aged 18–74. Descriptive statistics for the sample can be found in Table 1.

A. *State Cigarette Excise Taxes*

Increasing state cigarette excise tax rates has been viewed as an effective and politically popular method for states to raise revenues in order to meet their fiscal responsibilities (Campaign for Tobacco Free Kids State Tax Report 2010). Notably, recent increases in state cigarette taxes have been large in both absolute and relative terms, and provide greater variation, relative to earlier tax increases, to study the association between taxes and smoking (DeCicca and McLeod 2008). For example, the weighted average, combined federal and state tax rate more than doubled from 1980 to 2009 (see Figure 1), and most of this was because of changes in state taxes. Similarly, average tax increases over all states from 2005 to 2009 have been greater than $0.30 per pack (in 1990 dollars); nearly double the average increases in the previous 5-year period (see Figure 2). Our empirical strategy, detailed in the following section, utilizes these large increases in order to identify the association between cigarette taxes and smoking

3. TUS data used in the analyses are from the following waves of the CPS: September 1995, January and May 1996, September 1998, January and May 1999, June and November 2001, February 2002, February, June, and November 2003, May and August 2006, and January 2007. January and May 2000 are omitted because they lack data on smoking intensity.

4. There are a small number of respondents who report everyday smoking status, but are missing smoking frequency. These respondents are included in our analyses of smoking participation, but excluded from intensity analyses.

5. Alternatively, we conducted the analyses defining both everyday and someday smokers as smokers. Results using this definition of smoking are reported in Tables A2 and A3. While the magnitudes of the results are somewhat altered, the conclusions are similar.

TABLE 1
Descriptive Statistics for CPS-TUS Variables

Variable	Mean	Standard Deviation	Minimum	Maximum
Probability of daily smoker	0.177	0.382	0	1
Average number of daily cigarettes				
Full sample	2.68	7.58	0	99
Everyday smokers	18.62	10.12	1	99
Age	43.22	14.91	18	74
Male	0.475		0	1
Race				
White	0.764		0	1
Black	0.093		0	1
Hispanic	0.090		0	1
Asian	0.036		0	1
Other race	0.017		0	1
Education				
Less than high school	0.141		0	1
High school	0.325			
Some college	0.279		0	1
College plus	0.256		0	1
Employment				
Unemployed	0.035		0	1
Not in labor force	0.267		0	1
Family income				
$1–9,999	0.068		0	1
$10,000–24,999	0.165		0	1
$25,000–49,999	0.273		0	1
$50,000–74,999	0.181		0	1
$75,000 plus	0.313		0	1
Marital status				
Married	0.601		0	1
Never married	0.232		0	1
Other marital status	0.168		0	1
Observations	1,058,480			

Notes: A small number of respondents report everyday smoking status, but are missing smoking frequency. These respondents are included in our analyses of smoking participation, but excluded from intensity analyses. The number of respondents with nonmissing smoking intensity information totals 1,017,331. All descriptive statistics with the exception of "Average number of daily cigarettes" are calculated using the full sample of 1,058,480 respondents.

participation and smoking intensity. We obtain data on state cigarette tax changes from *The Tax Burden on Tobacco*, an annual compilation detailing tobacco taxes and tobacco revenue published by the consulting firm Orzechowski and Walker.

IV. EMPIRICAL STRATEGY

A. State and Year Fixed Effects

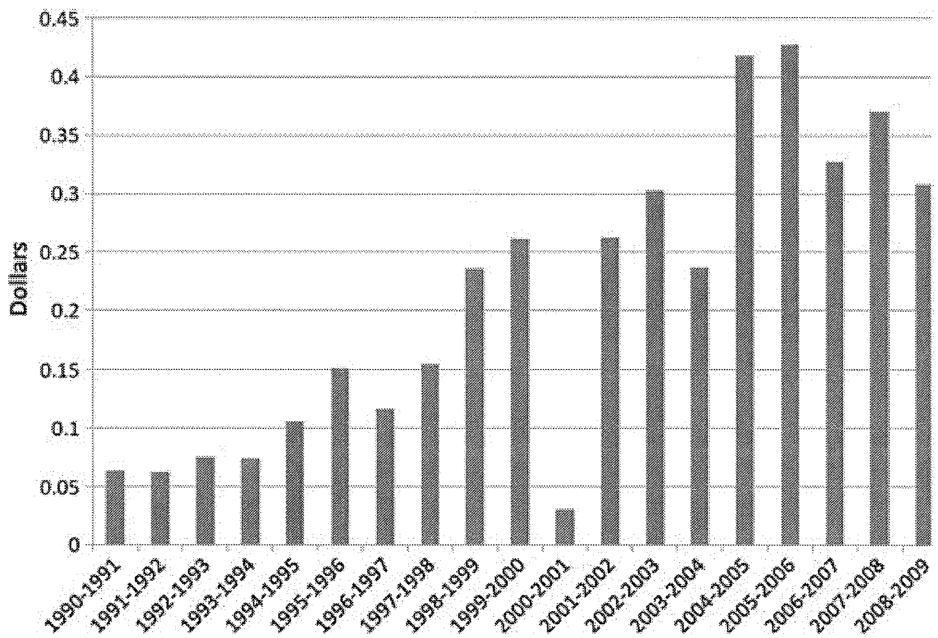
Our initial empirical approach follows previous studies by estimating a logistic regression model of the association between state cigarette excise taxes and smoking propensity that includes state and year fixed effects along with indicators of state anti-smoking policies. Specifically, we estimate the following model:

(1)
$$P(Smoke = 1)_{ijt} = G(\beta_o + \beta_1 Tax_{jt} + \beta_2 X_{jit} + \beta_3 rest_{jt} + \beta_4 wksite_{jt} + \delta_j + \gamma_t)$$

where $j = 1, \ldots, J$ (states) and $t = 1, \ldots, T$ (CPS-TUS waves).

In Equation (1), the dependent variable is equal to 1 if the respondent is an everyday smoker and 0 otherwise; *Tax* is the real, state cigarette excise tax in 1995 dollars; *X* is a vector of demographic variables that includes age, age squared, sex, race, education, employment status, marital status, and family income; *rest* is a variable equal to 1 if state *j* has a restaurant

FIGURE 2
Average Cigarette Tax Increase Across All States (in 1990 Dollars)



Source: Orzechowski and Walker, *Tax Burden on Tobacco* (2009).

smoking ban in effect and 0 otherwise; *wksite* is a variable equal to 1 if state j has a work site smoking ban in effect and 0 otherwise[6]; δ and γ are state and TUS-wave fixed effects, respectively; and $G[\cdot]$ is the logistic function. The coefficient of interest, β_1, is an estimate of the association between real state cigarette excise taxes and smoking propensity.[7]

Next, we obtain estimates of the association between state cigarette excise taxes and average daily cigarette consumption (i.e., smoking intensity) using a generalized linear model (GLM) with log-link and negative binomial distribution. A GLM model of this form is appropriate for the count nature of the data.[8] The smoking intensity model is as follows:

$$(2)\quad g(Cigs)_{ijt} = \beta_0 + \beta_1 Tax_{jt} + \beta_2 X_{ijt} + \beta_3 rest_{jt} + \beta_4 wksite_{jt} + \delta_j + \gamma_t, Cigs \sim NB$$

where $j = 1, \ldots, J$ (states) and $t = 1, \ldots, T$ (CPS-TUS waves).

The dependent variable in Equation (2) is the average number of daily cigarettes consumed; $g[\cdot]$ is a log-link function; and *NB* signifies that smoking intensity is modeled using a negative binomial distribution. The independent variables in Equation (2) are defined similarly to those in Equation (1) while the coefficient of interest is again β_1.[9]

B. Paired DiD

The standard state and time fixed effects approach uses all states that do not change taxes as controls for states that do change taxes, as well as the same states that change taxes, but in different periods. As Abadie, Diamond, and Hainmueller (2010) show in a

6. We thank Phil DeCicca for providing us the data on state smoking policies.

7. We estimated models using CPS-TUS sample weights with results nearly identical to the un-weighted estimates reported below.

8. We conducted a modified Park test as suggested by Manning and Mullahy (2001) which recommended a gamma class model. Additionally, when choosing a distribution, the Akaike Information Criterion (AIC) was lowest for the negative binomial model though simple OLS models resulted in largely similar findings.

9. We use the same symbols in Equations (1) and (2) to conserve on notation.

similar context (tobacco control policy), not all states are likely to be good controls for states that change taxes. One approach to address this issue is to limit comparison states to those that are arguably more appropriate (Lien and Evans 2005). Accordingly, we focus on a "large change sample" composed of 19 states enacting 22 of the largest tax increases during the period covered by our data (see Table A1 for the large change sample composition).[10] The decision to focus on large tax increases is motivated by the argument that changes in consumption should be largest for the largest tax increases (DeCicca and McLeod 2008).

To select control states for our sample of large tax increase states (i.e., treatment states), we used two sampled t-tests to compare differences in mean smoking rates in the CPS-TUS wave prior to the tax increase. States with statistically similar rates of smoking in the pre-period that had no corresponding change in cigarette tax were paired with the treatment state enacting the tax increase. As an example, Oklahoma's state cigarette excise tax was raised from \$0.23 to \$1.03 on January 1, 2005. The CPS-TUS waves immediately preceding (November 2003) and following (May 2006) January 1, 2005 were chosen as the pre- and post-increase periods, respectively. Six states (Kentucky, Missouri, Indiana, Kansas, Tennessee, and West Virginia) had rates of smoking participation and smoking intensity statistically similar to Oklahoma's in the pre-period; however, from November 2003 to May 2006, Kentucky also enacted a cigarette tax increase and, therefore, was excluded from the group of control states. This process was repeated for all 22 instances of tax increases in the "large change sample" resulting in an average of 11.7 states qualifying as control states for each large tax increase treatment state.

We then used data from the pre- and post-tax increase waves of the CPS-TUS to estimate the following models:

$$(3)\quad P(smoke=1)_{ijt} = G\Big(\alpha_0 + \alpha_1 Tax_{jt} + \alpha_2 X_{ijt} + \alpha_3 rest_{jt} + \alpha_4 wksite_{jt} + \sum_{jk} \pi_{jk} State_{jk} + \sum_{tk} \rho_{tk} Post_{tk} + \sum_{tk} \sigma_{tk} Group_{tk}\Big)$$

$$(4)\quad g(cigs)_{ijt} = \alpha_0 + \alpha_1 Tax_{jt} + a_2 X_{ijt} + \alpha_3 rest_{jt} + \alpha_4 wksite_{jt} + \sum\nolimits_{jk} \pi_{jk} State_{jk} + \sum\nolimits_{tk} \rho_{tk} Post_{tk} + \sum_{tk} \sigma_{tk} Group_{tk},\ Cigs \sim NB$$

where $j = 1, \ldots, J$ (states); $t = 1, \ldots, T$ (CPS-TUS waves); and $k = 1, \ldots, 22$ (treatment/control groupings).

The dependent variables in Equations (3) and (4) are the same as those used in Equations (1) and (2); *smoke* is an indicator for everyday smoking status and *cigs* is the average number of daily cigarettes consumed. *Tax* is the real state cigarette tax in 1995 dollars in both the pre- and post-tax increase periods (note that this is unchanged for the control state); X is the same vector of demographic variables used in Equations (1) and (2); *State* is an indicator for state j in treatment/control group k; *Post* is an indicator for the post-tax increase period for treatment/control group k; and *Group* is an indicator for the state (treatment and controls) and time (pre- and post-tax increase) grouping. Note that there is only one pre- and post-tax change period for each of the 22 large tax changes analyzed.

To further illustrate the paired DiD strategy, consider the aforementioned tax increase in Oklahoma on January 1, 2005. In this case, we would use CPS-TUS data from the pre- (November 2003) and post-tax (May 2006) periods and estimate models analogous to Equations (3) and (4). Here, *State* is an indicator for each state in the grouping, *Post* is equal to one if the TUS-wave is May 2006, and *Group* is equal to 1 if the state is Oklahoma or one of the control states matched with Oklahoma and the TUS-wave is either November 2003 or May 2006.[11]

10. We use the 22 state tax increases greater than \$0.55 for which we are able to identify appropriate treatment and control states. Specifically, we have no post-period data for the \$1.00 increases implemented by South Dakota and Texas on January 1, 2007. Similarly, we were unable to identify appropriate control states for Alaska. Note that DeCicca and McLeod (2008) used all tax changes between 2000 and 2005 and did not limit the analysis to large increases, although during this period many increases were relatively large.

11. In this case, in which there is only one tax change, the variable *Group* would drop out.

The coefficient estimate of the *Tax* variable is identified through state variation resulting from Oklahoma's increasing cigarette tax rate and the control states' tax rates remaining constant. Essentially, this paired DiD approach pools 22 separate DiD analyses using the 22 tax increases in the large change sample and a set of treatment and control states for each tax change.

C. Placebo Analysis

To test the validity of the paired, DiD research design, we created a placebo experiment in which we chose the same treatment/control groupings, but in periods when there were no tax changes for either the treatment state or control states. We then randomly assigned a \$0.50 tax increase to one of the states in the group and estimated Equations (3) and (4). Essentially, we created a series of "pseudo" tax increases using states and time periods where no actual tax changes occurred. If the paired DiD analysis is valid, then we expect estimates from the placebo experiment to be zero because no actual tax increase took place. Were we to estimate a nonzero effect of tax in the placebo experiment, this would suggest a spurious relationship between taxes and smoking behavior. In fact, the validity of the research design is supported by this placebo analysis; coefficients on (pseudo) tax are statistically insignificant and all placebo estimates are small for both dependent variables. We report these results below. Notably, no such placebo test is available in the context of the traditional empirical approach of using all states and controlling for state and year effects.

V. RESULTS

A. Age-group 18–74

Table 2 presents results for the full sample—persons aged between 18 and 74. Logit estimates related to the probability of smoking are listed in columns 1 through 4 and GLM estimates related to the average number of cigarettes smoked are listed in columns 5 through 8.

The estimate in column 1 was obtained using the common, two-way fixed effects model that controls for state and TUS-wave effects. The estimate indicates that a \$1 increase in cigarette taxes is associated with a small decrease in the probability of being a daily (everyday) smoker. The marginal effect is −0.007—a 4% decrease in smoking propensity. The implied elasticity is −0.026, which is quite small. The fixed effects estimate in column 2 was obtained using a sample that was limited to persons living in 1 of the 19 states that experienced a large tax change and it is similar to the estimate in column 1—a small, negative association between cigarette taxes and smoking participation with an implied elasticity of −0.058. Column 3 lists the estimate of the association between cigarette taxes and smoking participation from the paired, DiD regression model that used a sample of persons in the 19 states that experienced a large tax change and their corresponding control states, but only in the periods just prior to and just after the tax change occurred. The estimate in column 3 is negative, close to zero, and not statistically significant—a \$1 increase in cigarette taxes is associated with a 2% decrease in smoking participation. The implied elasticity is −0.015. Finally, in column 4, we present the estimate from the placebo experiment. The estimate is virtually zero. The near-zero estimate from the placebo analysis suggests that the paired, DiD research design is valid. Standard errors associated with the paired, DiD estimate in column 3 rule out an elasticity greater (more negative) than −0.1. Overall, estimates in columns 1 through 4 of Table 2 indicate that increases in cigarette taxes have very little effect on smoking participation for persons aged between 18 and 74.

Columns 5 through 8 of Table 2 present estimates of associations between cigarette taxes and the average number of cigarettes smoked daily. Estimates in columns 5 through 8 are quite similar and suggest that the association between cigarette taxes and the quantity of cigarettes smoked is negative, though small in magnitude. The implied tax elasticities of cigarette consumption range from −0.019 to −0.035. Standard errors of the estimates rule out tax elasticities greater (more negative) than −0.13. Point estimates suggest that a 100% increase in cigarette taxes would reduce the average number of cigarettes smoked by between 2 and 4%. The DiD estimate in column 8 from the placebo analysis is negative, small, and not statistically significant; the implied elasticity is −0.037. Here too the placebo estimate supports the validity of the paired DiD approach.

B. Estimates by Age Group

Previous studies suggest that the responsiveness of smoking to changes in taxes and prices

TABLE 2
Estimates of the Effect of Cigarette Tax on Smoking: Adults Aged 18–74

	Probability of Daily Smoking				Average Number of Daily Cigarettes			
	Full Sample	Large Tax Change Sample			Full Sample	Large Tax Change Sample		
Age-group 18–74	Two-Way Fixed Effects (1)	Two-Way Fixed Effects (2)	Paired DiD (3)	Placebo DiD (4)	Two-Way Fixed Effects (5)	Two-Way Fixed Effects (6)	Paired DiD (7)	Placebo DiD (8)
Tax (in 1995 dollars)	−0.060**	−0.087	−0.036	0.003	−0.064**	−0.046	−0.031	−0.065
	(0.028)	(0.054)	(0.059)	(0.030)	(0.031)	(0.064)	(0.068)	(0.051)
Marginal effect	[−0.007]	[−0.010]	[−0.003]	[0.000]	—	—	—	—
Elasticity	−0.026**	−0.058	−0.015	0.001	−0.032**	−0.035	−0.019	−0.037
	(0.012)	(0.036)	(0.039)	(0.015)	(0.016)	(0.050)	(0.040)	(0.029)
Mean tax change (1995 $s)	0.408	0.518	0.696	0.00	0.408	0.518	0.696	0.00
Mean tax (1995 $s)	0.502	0.774	0.595	0.457	0.502	0.774	0.594	0.458
Mean of dep. variable	0.177	0.169	0.165	0.179	2.69	2.42	2.32	2.72
Observations	1,058,480	300,309	343,210	400,317	1,017,331	288,601	330,492	385,087

Notes: Smokers are defined as everyday smokers. Someday smokers are defined as nonsmokers. The full sample includes waves of the CPS-TUS from September 1995 to January 2007, excluding January and May 2000. The composition of the "Large Tax Change Sample" is detailed in Table A1. Column (2) utilizes all states in the "Large Tax Change Sample" from 2000 to 2007. Column (3) utilizes the state tax increases detailed in Table A1 and all corresponding control states (selection process described in Section 4.2). Column (4) utilizes a variant of the "Large Tax Change Sample" described in Section 4.3. Estimates in columns 1 through 4 are from a logistic regression model of smoking participation. Estimates in columns 5 through 8 are from a GLM with a log link and negative binomial distribution of smoking intensity. Marginal effects on probability from logistic regressions are in brackets. Standard errors (in parentheses) are calculated using the delta method and are constructed allowing for nonindependence (clustering) at the state level. Standard errors of elasticity estimates calculated using the delta method.
*0.05 $< p$ value $\leq$ 0.10; **0.01 $< p$ value $\leq$ 0.05; ***p value $\leq$ 0.01.

is greater for younger persons than older persons. To assess this hypothesis using our data, we divided the sample into three age-groups: 18–34, 35–54, and 55–74.[12] We re-estimated all models presented in Table 2 for these age-groups. Table 3 presents these estimates.

We begin with the top panel of Table 3, which lists estimates for persons aged between 18 and 34. Fixed-effects estimates in columns 1 and 2 and columns 5 and 6 are negative and small with implied tax elasticities between −0.033 and −0.069. Standard errors associated with these estimates rule out tax elasticities greater (more negative) than −0.2. Estimates in columns 3 and 7 from the paired DiD are small, positive and not statistically significant with elasticities of 0.04 and 0.009, respectively. Again, placebo estimates in columns 4 and 8 suggest that the paired DiD research design is valid, as estimates are close to zero and not statistically significant indicating tax elasticities of 0.004 and −0.051.

12. Additionally, DeCicca and McLeod (2008) find larger elasticity estimates for low-educated and low-income individuals. We find similar patterns when we stratify by education and income (not reported here), however all elasticity estimates for adults remain quite small.

The middle panel of Table 3 presents estimates for the sample of persons aged 35–54. Fixed effects estimates in columns 1 and 2 and columns 5 and 6 are negative, small, and not statistically significant. Tax elasticities with respect to cigarette participation are between −0.015 and −0.043, and tax elasticities with respect to the average number of daily cigarettes are between −0.027 and −0.031. Estimates in columns 3 and 7 from the paired DiD are negative, small, and not statistically significant; elasticities are −0.013 and −0.051. Estimates from the placebo analyses again support the validity of the paired DiD research design with small, statistically insignificant elasticities of −0.004 for smoking participation and −0.067 for smoking intensity.

The bottom panel of Table 3 presents results for the sample age-group 55–74. Fixed effects estimates (columns 1, 3, 5, and 6) are small, negative, and not always statistically significant with implied tax elasticities of between −0.030 and −0.064. The paired DiD and placebo estimates are small and not statistically significant with elasticities of 0.008 for smoking participation and −0.023 for smoking intensity. It is

TABLE 3
Estimates of the Effect of Cigarette Tax on Smoking by Age

	Probability of Daily Smoking				Average Number of Daily Cigarettes			
	Full Sample	Large Tax Change Sample			Full Sample	Large Tax Change Sample		
	Two-Way Fixed Effects (1)	Two-Way Fixed Effects (2)	Paired DiD (3)	Placebo DiD (4)	Two-Way Fixed Effects (5)	Two-Way Fixed Effects (6)	Paired DiD (7)	Placebo DiD (8)
Age-group 18–34								
Tax (in 1995 dollars)	−0.077*	−0.107	0.080	0.008	−0.068	−0.070	0.016	−0.095
	(0.045)	(0.109)	(0.106)	(0.070)	(0.043)	(0.112)	(0.064)	(0.075)
Marginal effect	[−0.009]	[−0.013]	[0.009]	[0.001]	—	—	—	—
Elasticity	−0.033*	−0.069	0.040	0.004	−0.034	−0.053	0.009	−0.051
	(0.019)	(0.071)	(0.054)	(0.034)	(0.021)	(0.085)	(0.038)	(0.042)
Mean of dep. variable	0.179	0.176	0.167	0.181	2.29	2.09	1.95	2.34
Observations	338,499	91,065	106,217	127,055	323,302	86,693	101,617	122,651
Age-group 35–54								
Tax (in 1995 dollars)	−0.035	−0.065	−0.026	−0.007	−0.053	−0.039	−0.085	−0.116
	(0.039)	(0.073)	(0.098)	(0.049)	(0.042)	(0.071)	(0.077)	(0.074)
Marginal effect	[−0.005]	[−0.009]	[−0.003]	[−0.001]	—	—	—	—
Elasticity	−0.015	−0.043	−0.013	−0.004	−0.027	−0.031	−0.051	−0.067
	(0.016)	(0.048)	(0.050)	(0.024)	(0.021)	(0.055)	(0.046)	(0.042)
Mean of dep. variable	0.199	0.189	0.185	0.202	3.23	2.89	2.78	3.30
Observations	458,772	132,205	148,807	173,988	439,493	126,712	142,850	166,029
Age-group 55–74								
Tax (in 1995 dollars)	−0.067*	−0.092	−0.055	0.016	−0.081*	−0.039	0.044	−0.039
	(0.037)	(0.063)	(0.076)	(0.093)	(0.045)	(0.092)	(0.105)	(0.073)
Marginal effect	[−0.007]	[−0.009]	[−0.005]	[0.002]	—	—	—	—
Elasticity	−0.030*	−0.064	−0.022	0.008	−0.041*	−0.031	0.026	−0.023
	(0.017)	(0.044)	(0.045)	(0.047)	(0.023)	(0.073)	(0.063)	(0.043)
Mean of dep. variable	0.136	0.126	0.128	0.137	2.23	2.01	1.99	2.24
Observations	261,209	77,039	88,186	99,274	254,536	75,196	86,025	96,407

Notes: Smokers are defined as everyday smokers. Someday smokers are defined as nonsmokers. The full sample includes waves of the CPS-TUS from September 1995 to January 2007, excluding January and May 2000. The composition of the "Large Tax Change Sample" is detailed in Table A1. Column (2) utilizes all states in the "Large Tax Change Sample" from 2000 to 2007. Column (3) utilizes the state tax increases detailed in Table A1 and all corresponding control states (selection process described in Section 4.2). Column (4) utilizes a variant of the "Large Tax Change Sample" described in Section 4.3. Estimates in columns 1 through 4 are from a logistic regression model of smoking participation. Estimates in columns 5 through 8 are from a GLM with a log link and negative binomial distribution of smoking intensity. Marginal effects on probability from logistic regressions are in brackets. Standard errors (in parentheses) are calculated using the delta method and are constructed allowing for nonindependence (clustering) at the state level. Standard errors of elasticity estimates calculated using the delta method.

*0.05 $< p$ value $\leq$ 0.10; **0.01 $< p$ value $\leq$ 0.05; ***p value $\leq$ 0.01.

notable that estimates in Table 3 provide no evidence to support the hypothesis that smoking behavior is more responsive to taxes (prices) among younger persons than older persons.

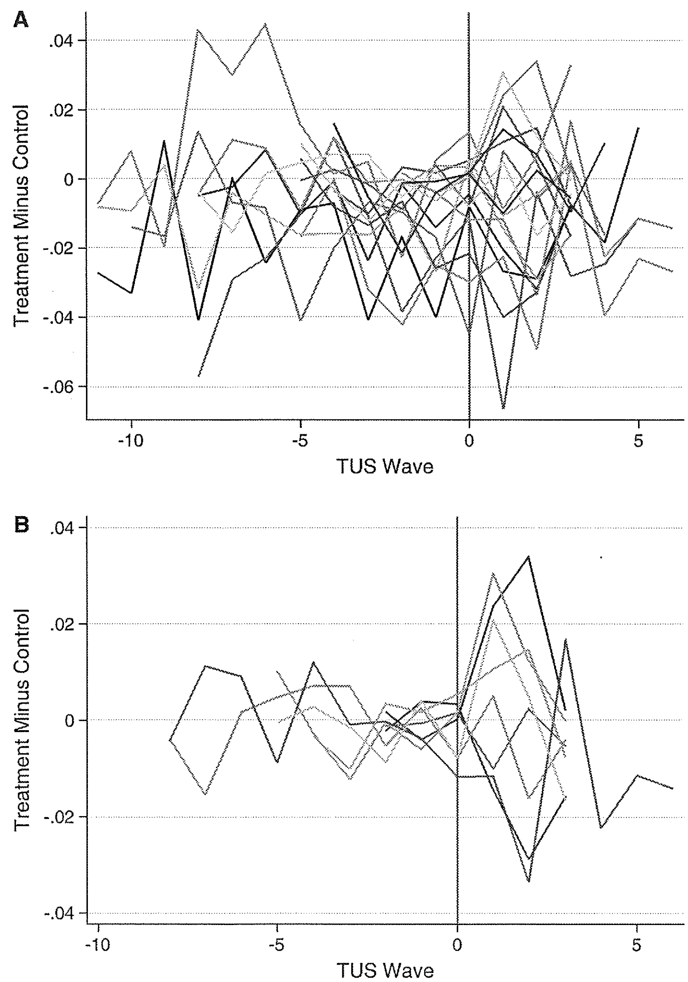
C. Synthetic Control Approach

As an alternative to the paired DiD strategy we also conducted a synthetic control approach similar to that used by Abadie, Diamond, and Hainmueller (2010). The main difference between the DiD approach and the synthetic control method is how the control group is selected. In the DiD approach, we paired treatment and potential control states on pre-period means and selected states that were well matched. The Abadie, Diamond, and Hainmueller (2010) approach selects potential control states by matching pre-tax increase trends in a variety of predictor variables to generate a single control state that is a weighted average of all potential control states.[13] Post-tax increase predicted outcomes in the synthetic control state are

13. In our analysis, pre-tax increase predictor variables included state mean values of smoking propensity, sex, age, family income, marital status, education, and employment.

FIGURE 3

Differences in Smoking Participation Rates (Treatment States minus Synthetic Controls)



Notes: (A) Treatment states include 18 of the 22 tax changes in the large change sample. Period zero indicates the final wave of the pre-tax increase TUS. (B) Treatment states include all those for which the root mean squared prediction error was less than 0.01 (KS, ME, MI, NY, OH, OR, PA, and WA).

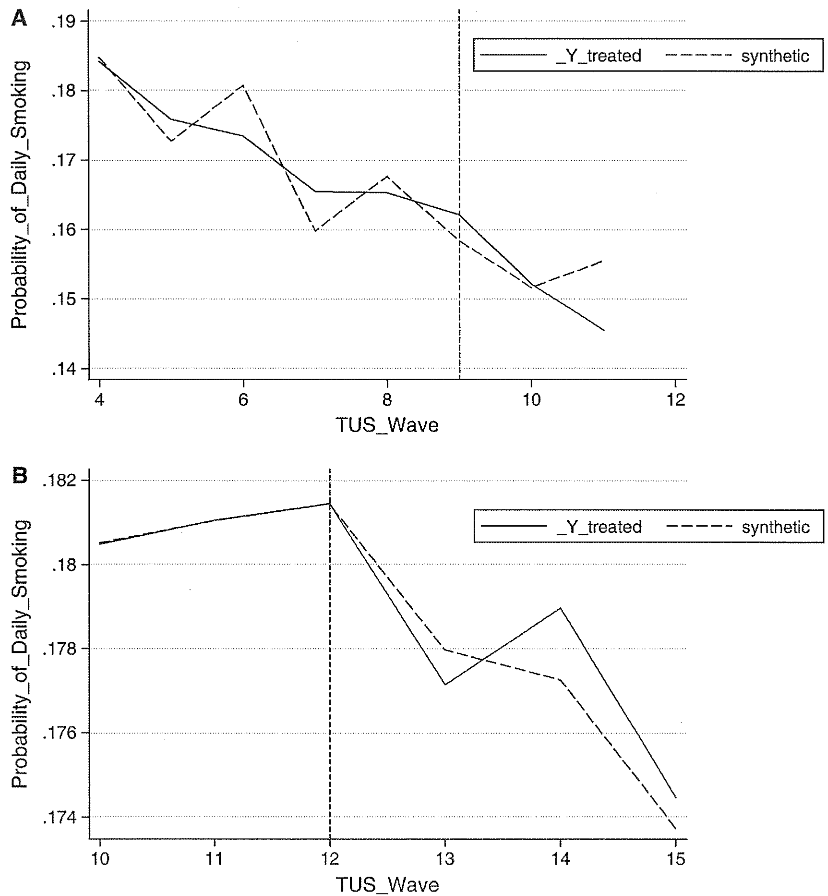
then compared to the actual outcomes observed in the treatment state.

Figure 3A presents differences in smoking rates between treatment and control states for 18 of the 22 tax changes in the large change sample.[14] The individual state experiments in Figure 3A have been normalized so that period (CPS-TUS Wave) zero corresponds to the last pre-tax increase observation. As indicated in Figure 3A, there is no noticeable break in the trend after the tax change between treatment and control states with respect to the propensity to smoke among adults. Thus, according to Figure 3A, tax increases had virtually no effect on smoking rates. However, it is also the case that for many of the experiments, the treatment and control states are not well matched in the pre-tax increase period (i.e., a majority of the experiment plots in Figure 3A are less than zero even in the pre-tax increase period).

Figure 3B repeats the analysis presented in Figure 3A, but is limited to treatment states

14. Tax increases occurring in Arizona (December 8, 2006), Rhode Island (July 1, 2004), Vermont (July 1, 2006), and New Jersey (July 1, 2003) lack sufficient pre- or post-tax increase periods to be included in this analysis.

FIGURE 4
Smoking Rates for Treatment Groups and Synthetic Controls



Notes: (A) Treatment states are all those in the large change sample with a tax change occurring between February 2002 and February 2003 (AZ, KS, MA, NJ, OR, and PA). (B) Treatment states are all those in the large change sample with a tax change occurring between November 2003 and May 2006 (CO, ME, MI, MN, OH, and OK).

and synthetic controls that are matched closely on pre-tax increase trends.[15] Figure 3B also suggests no consistent pattern in the effect of tax increases on smoking propensity among adults. Average post-tax increase smoking propensity in states that experienced a tax increase was only slightly smaller than in synthetic control states (0.1655 vs. 0.1662).

Finally, exploiting the fact that a number of states experienced tax increases between two specific waves of the TUS, we grouped several treatment states together into one treatment group and repeated the analyses in Figure 3A and B. These results are displayed in Figure 4A and B. Figure 4A includes all tax increases in the large change sample occurring between February 2002 and February 2003 and Figure 4B includes all tax increases occurring between November 2003 and May 2006. Figure 4A shows a consistently declining trend in smoking propensity in treated and control states and the groups appear to be relatively well matched. Post-tax, smoking rates are slightly higher in the treated states than in the synthetic control group with the exception of the final period. In Figure 4B, treatment and control states are especially well matched. Smoking rates in the treated states were initially lower than in the synthetic

15. Experiments included in this well-matched group include all those for which the root mean squared prediction error is less than 0.01.

control in the periods immediately following the tax increase, but smoking rates in the treatment states eventually surpassed and remained slightly above the predicted post-tax increase synthetic control rates.[16] Overall, the results from the synthetic control analyses support the paired DiD results reported earlier; tobacco tax increases have a small, negative effect that is difficult to distinguish from zero.

D. Are Tax Elasticities Larger When Border State Taxes Are Considered?

One of the potential mechanisms for avoiding state cigarette taxes is to cross state borders to purchase cigarettes in states with lower tax rates (Lovenheim 2008). Not accounting for this possibility may be a partial explanation for the small tax elasticities reported in Tables 2 and 3. To assess this hypothesis, we re-estimated the two-way, fixed-effects models allowing the effect of tax to differ by the distance to the nearest state with a lower cigarette tax. For this analysis, the sample is limited to persons living in metropolitan statistical areas (MSAs) because this is the only group for which we could measure the distance to the nearest low-tax state using CPS-TUS data. We follow the method used by DeCicca, Kenkel, and Liu (2010a) and measure distance from the center of the MSA to the nearest low-tax border using Google Maps.[17] An advantage of this method is that distance is measured by roadway rather than "as the crow flies." This allows for a more accurate measure of the actual travel distance required to purchase cigarettes in a lower tax state. We allowed the tax to have a different effect for persons living 0–60 miles, 61–120 miles, and 120 or more miles from a low-tax state.[18] Estimates from this analysis are reported in Table 4.

In column 1 of Table 4, we report the estimate from the same specification used in column 1 of Table 2 to assess whether changes in sample composition affect our estimates, as the sample used to obtain estimates in Table 4 includes only those living in MSAs. The estimate (marginal effect) is −0.003 with an implied elasticity of −0.013. These are very similar to estimates in Table 2. Column 2 reports estimates from the expanded specification that allows the effect of tax to differ by distance to the nearest low-tax state. While there is evidence that tax increases have a larger (more negative) effect on smoking participation and smoking intensity in MSAs farther from low-tax state borders, the effect does not appear to be large in magnitude.[19]

Estimates in other columns of Table 4 support a similar conclusion. Estimates in columns 3, 5, and 7 are from models that are identical to those in Table 2, but include only the sample of persons living in MSAs. These estimates are all similar to the analogous estimates in Table 2 indicating that changes in sample composition are unimportant. For MSAs in states included in the large change sample, estimates of the effect of tax by distance reveal that the effect of tax is slightly larger (more negative) for distances between 61 and 120 miles than for distances between 0 and 60 miles. However, in the large change sample MSAs farthest from low-tax states show smaller, not larger, tax effects. In short, there is little evidence that the small elasticities reported in Table 2 are because of bias arising from cross-border cigarette purchases.

VI. DISCUSSION

The magnitude of the association between state cigarette taxes and smoking is unresolved, especially for older smokers. A review of the literature by Chaloupka and Warner (2000) suggests price elasticities for smoking participation centered on −0.5 to −0.3, though more recent estimates using state and time fixed effects methods find smaller estimates on the order of −0.3 to −0.1. An analysis of 17 price elasticity estimates by Gallet and List (2003) found a median elasticity value of −0.32 for adults aged 24 and older. Estimates of price elasticities for smoking intensity, though fewer in number, tend

16. What appears to be a marked change in trend in smoking propensity at the time of the tax increase in Figure 4B is because of a 3-year gap between the final pre-tax period and the initial post-tax period. States included in this treatment group experienced a steady decline in smoking participation during this period similar to those states in Figure 4A.

17. A number of MSAs in our data cross state lines. To account for this, we use state/MSA pairs rather than MSA alone.

18. Alternatively, we allowed the effect of border distance to vary by 0–20 miles, 21–40 miles, and 41 or more miles from the nearest low-tax border and arrived at very similar results.

19. This corresponds to direct evidence from six waves of the CPS-TUS that include a question asking the respondent if their last pack of cigarettes was purchased in a state other than their state of residence. Less than 15% of state/MSA pairs have a proportion of respondents greater than 0.10 reporting an out-of-state purchase of their last pack of cigarettes.

TABLE 4

Estimates of the Effect of Cigarette Tax on Smoking: Adults Aged 18–74 by Distance to Nearest Low-Tax State MSA Sample

	Probability of Daily Smoking				Average Number of Daily Cigarettes			
	Full Sample	Large Tax Change Sample			Full Sample	Large Tax Change Sample		
Age-group 18–74	Two-Way Fixed Effects (1)	Two-Way Fixed Effects (2)	Paired DiD (3)	Placebo DiD (4)	Two-Way Fixed Effects (5)	Two-Way Fixed Effects (6)	Paired DiD (7)	Placebo DiD (8)
Tax (in 1995 dollars)	−0.029		−0.108**		−0.040		−0.079	
	(0.031)		(0.050)		(0.038)		(0.069)	
Marginal effect	[−0.003]		[−0.012]		—		—	
Elasticity	−0.013		−0.074**		−0.021		−0.062	
	(0.014)		(0.035)		(0.020)		(0.055)	
Tax—distance 0–60		−0.021		−0.105**		−0.031		−0.078
		(0.033)		(0.047)		(0.041)		(0.065)
Marginal effect		[−0.002]		[−0.012]		—		—
Elasticity		−0.004		−0.040**		−0.007		−0.034
		(0.006)		(0.018)		(0.010)		(0.028)
Tax—distance 61–120		−0.042		−0.124*		−0.063		−0.100
		(0.043)		(0.063)		(0.054)		(0.089)
Marginal effect		[−0.005]		[−0.014]		—		—
Elasticity		−0.005		−0.021*		−0.008		−0.020
		(0.005)		(0.011)		(0.007)		(0.018)
Tax—distance 121+		−0.091		0.031		−0.077		0.048
		(0.064)		(0.070)		(0.065)		(0.081)
Marginal effect		[−0.010]		[0.003]		—		—
Elasticity		−0.013		0.004		−0.013		0.007
		(0.009)		(0.010)		(0.011)		(0.013)
Mean tax change (1995 $s)	0.408	0.408	0.506	0.506	0.408	0.408	0.506	0.506
Mean tax (1995 $s)	0.536	0.536	0.790	0.790	0.532	0.532	0.790	0.790
Mean of dep. variable	0.164	0.164	0.160	0.160	2.40	2.41	2.22	2.22
Observations	669,383	669,383	210,358	210,358	644,836	644,836	202,356	202,356

Notes: Smokers are defined as everyday smokers. Someday smokers are defined as nonsmokers. The full sample includes waves of the CPS-TUS from September 1995 to January 2007, excluding January and May 2000. The composition of the "Large Tax Change Sample" is detailed in Table A1. Column (2) utilizes all states in the "Large Tax Change Sample" from 2000 to 2007. Column (3) utilizes the state tax increases detailed in Table A1 and all corresponding control states (selection process described in Section 4.2). Column (4) utilizes a variant of the "Large Tax Change Sample" described in Section 4.3. Estimates in columns 1 through 4 are from a logistic regression model of smoking participation. Estimates in columns 5 through 8 are from a GLM with a log link and negative binomial distribution of smoking intensity. Marginal effects on probability from logistic regressions are in brackets. Standard errors (in parentheses) are calculated using the delta method and are constructed allowing for nonindependence (clustering) at the state level. Standard errors of elasticity estimates calculated using the delta method.

*$0.05 < p$ value ≤ 0.10; **$0.01 < p$ value ≤ 0.05; ***p value ≤ 0.01.

to range from −0.25 to −0.15 (Chaloupka and Warner 2000).

In this paper, we revisited the issue of cigarette taxes and adult smoking and extended the literature in two ways. First, we focused on recent, large tax changes, which provide the best opportunity to empirically observe a response in cigarette consumption. Second, we employed an underused, but well-suited methodology, a paired DiD approach. The advantages of the paired DiD approach is that it selects control states using an explicit criteria, as opposed to using all potential control states, and it allows us to explicitly test the validity of the research design. We also provided estimates of associations between cigarette taxes and smoking using a standard, two-way fixed effects approach along with separate estimates by age-groups (18–34, 35–54, 55–74).

Overall, estimates indicate that the association between cigarette taxes and either smoking participation or the average number of daily cigarettes consumed is negative, small, and not usually statistically significant. Tax elasticities with respect to smoking participation and number of cigarettes smoked were typically in the −0.02 to −0.05 range. Importantly, standard errors of estimates rule out cigarette tax elasticities with respect to smoking participation (intensity) among adults greater (more negative) than −0.12 (−0.13). Additionally, we find little evidence that young adult (aged 18–34) smokers are more responsive to tax changes than older adult smokers. For specific age-groups, confidence intervals for tax elasticities are larger because of smaller sample sizes, but still relatively small. Standard errors of estimates rule out cigarette tax elasticities with respect to smoking participation (intensity) greater than −0.21 (−0.22) for those aged 18–34; greater than −0.14 (−0.14) for those aged 35–54; and greater than −0.15 (−0.17) for those aged 55–74.

Finally, estimates from the paired DiD and synthetic control approaches using the entire sample of adults (aged 18–74) are similar to those obtained from the standard fixed-effects design, while placebo analyses suggest that the paired DiD approach was valid. However, standard errors of the placebo estimates could not rule out small spurious tax elasticities of the magnitude found in standard two-way fixed effects analyses. This finding is worrisome because it suggests that even under the best conditions, when treatment and comparison states are well matched on pre-tax smoking means, the assumption that trends in smoking are the same for states that increase taxes and states that do not may be invalid. Slightly diverging trends can result in estimates of the effect of taxes on smoking that are small, statistically significant, and of the same magnitude as some estimates reported in the literature. In short, given current data and methods (quasiexperimental), it is difficult to detect reliably causal estimates of cigarette tax elasticities with respect to smoking that are smaller (less negative) than −0.10.

To summarize, our analysis of the association between cigarette taxes and adult cigarette use suggests that adult smoking is largely unaffected by taxes. At best, cigarette tax increases may have a small negative association with cigarette consumption, although it is difficult to distinguish the effect from zero, and in practical terms implies that it will take very large tax increases, for example, on the order of 100%, to reduce smoking by 5%. This finding raises questions about claims that, at the current time, tax (price) increases on cigarettes will have an important beneficial health impact through reduced smoking.[20] It may be that in a time when the median federal and state cigarette tax is approximately $2.50 per pack, further increases in cigarette taxes will have little effect because the pool of smokers is becoming increasingly concentrated with those with strong preferences for smoking. Alternatively, as cigarette taxes and prices continue to rise, smokers are taking other steps to thwart the impact of the price increase such as switching brands and increasing purchases on the black market. Notably, we rule out border crossing as an important explanation of the small tax elasticities.

20. Though if those younger than 18 are more price responsive than older adults, the health effects of cigarette tax increases could prove meaningful for teens.

APPENDIX

TABLE A1
Treatment States used in Large Sample and Paired DiD Analyses

State Pair	Date of Increase	Pre-period TUS Wave	Post-period TUS Wave	Pre-period Tax	Post-period Tax	Tax Increase
Maine	September 19, 2005	November 2003	May 2006	$1.00	$2.00	$1.00
Montana	January 1, 2005	November 2003	May 2006	$0.70	$1.70	$1.00
Arizona	December 8, 2006	August 2006	January 2007	$1.18	$2.00	$0.82
Oklahoma	January 1, 2005	November 2003	May 2006	$0.23	$1.03	$0.80
Massachusetts	July 24, 2002	February 2002	February 2003	$0.76	$1.51	$0.75
Michigan	July 1, 2004	November 2003	May 2006	$1.25	$2.00	$0.75
Minnesota	August 1, 2005	November 2003	May 2006	$0.48	$1.23	$0.75
Rhode Island	July 1, 2004	November 2003	May 2006	$1.71	$2.46	$0.75
New Jersey	July 1, 2002	February 2002	February 2003	$0.80	$1.50	$0.70
New Mexico	July 1, 2003	June 2003	November 2003	$0.21	$0.91	$0.70
Ohio	July 1, 2005	November 2003	May 2006	$0.55	$1.25	$0.70
Pennsylvania	July 15, 2002	February 2002	February 2003	$0.31	$1.00	$0.69
Colorado	January 1, 2005	November 2003	May 2006	$0.20	$0.84	$0.64
Connecticut	April 3, 2002	February 2002	February 2003	$0.50	$1.11	$0.61
Washington	January 1, 2002	November 2001	February 2002	$0.825	$1.425	$0.60
Oregon	November 1, 2002	February 2002	February 2003	$0.68	$1.28	$0.60
Arizona	November 26, 2002	February 2002	February 2003	$0.58	$1.18	$0.60
Washington	July 1, 2005	November 2003	May 2006	$1.425	$2.025	$0.60
Vermont	July 1, 2006	May 2006	January 2007	$1.19	$1.79	$0.60
New York	March 1, 2000	May 1999	June 2001	$0.56	$1.11	$0.55
Kansas	July 1, 2002 & January 1, 2003	February 2002	February 2003	$0.24	$0.79	$0.55
New Jersey	July 1, 2003	June 2003	November 2003	$1.50	$2.05	$0.55

Notes: Taxes are imposed per package of 20 cigarettes.
Source: Orzechowski and Walker, *Tax Burden on Tobacco* (2009).

TABLE A2
Estimates of the Effect of Cigarette Tax on Smoking: Adults Aged 18–74

	Probability of Daily Smoking				Average Number of Daily Cigarettes			
	Full Sample	Large Tax Change Sample			Full Sample	Large Tax Change Sample		
Age-group 18–74	Two-Way Fixed Effects (1)	Two-Way Fixed Effects (2)	Paired DiD (3)	Placebo DiD (4)	Two-Way Fixed Effects (5)	Two-Way Fixed Effects (6)	Paired DiD (7)	Placebo DiD (8)
Tax (in 1995 dollars)	−0.043*	−0.082*	0.011	0.009	−0.064**	−0.046	−0.031	−0.065
	(0.025)	(0.048)	(0.061)	(0.032)	(0.031)	(0.064)	(0.068)	(0.051)
Marginal effect	[−0.007]	[−0.012]	[0.002]	[0.001]	—	—	—	—
Elasticity	−0.017*	−0.052	0.006	0.004	−0.032**	−0.035	−0.019	−0.037
	(0.010)	(0.031)	(0.030)	(0.015)	(0.016)	(0.050)	(0.040)	(0.029)
Mean tax change (1995 $s)	0.408	0.518	0.696	0.00	0.408	0.518	0.696	0.00
Mean tax (1995 $s)	0.502	0.774	0.595	0.457	0.502	0.774	0.594	0.458
Mean of dep. variable	0.216	0.207	0.203	0.219	2.69	2.42	2.32	2.72
Observations	1,058,480	300,309	343,210	400,317	1,017,331	288,601	330,492	385,087

Notes: Smokers are defined as everyday and someday smokers for the smoking propensity analyses and everyday smokers for the smoking intensity analyses. The full sample includes waves of the CPS-TUS from September 1995 to January 2007, excluding January and May 2000. The composition of the "Large Tax Change Sample" is detailed in Table A1. Estimates in columns 1 through 4 are from a logistic regression model of smoking participation. Estimates in columns 5 through 8 are from a GLM with a log link and negative binomial distribution of smoking intensity. Marginal effects on probability from logistic regressions are in brackets. Standard errors are in parentheses and are constructed allowing for nonindependence (clustering) at the state level. Standard errors of elasticity estimates calculated using the delta method.

*0.05 < p value ≤ 0.10; **0.01 <p value ≤ 0.05; ***p value ≤ 0.01.

TABLE A3
Estimates of the Effect of Cigarette Tax on Smoking by Age

	Probability of Daily Smoking				Average Number of Daily Cigarettes			
	Full Sample	Large Tax Change Sample			Full Sample	Large Tax Change Sample		
	Two-Way Fixed Effects (1)	Two-Way Fixed Effects (2)	Paired DiD (3)	Placebo DiD (4)	Two-Way Fixed Effects (5)	Two-Way Fixed Effects (6)	Paired DiD (7)	Placebo DiD (8)
Age-group 18–34								
Tax (in 1995 dollars)	−0.040	−0.078	0.039	0.058	−0.068	−0.070	0.016	−0.095
	(0.038)	(0.077)	(0.072)	(0.060)	(0.043)	(0.112)	(0.064)	(0.075)
Marginal effect	[−0.006]	[−0.012]	[0.006]	[0.009]	—	—	—	—
Elasticity	−0.016	−0.048	0.018	0.026	−0.034	−0.053	0.009	−0.051
	(0.015)	(0.047)	(0.034)	(0.027)	(0.021)	(0.085)	(0.038)	(0.042)
Mean of dep. variable	0.231	0.229	0.220	0.235	2.29	2.09	1.95	2.34
Observations	338,499	91,065	106,217	127,055	323,302	86,693	101,617	122,651
Age-group 35–54								
Tax (in 1995 dollars)	−0.028	−0.076	−0.040	−0.009	−0.053	−0.039	−0.085	−0.116
	(0.035)	(0.070)	(0.080)	(0.052)	(0.042)	(0.071)	(0.077)	(0.074)
Marginal effect	[−0.005]	[−0.012]	[−0.006]	[−0.002]	—	—	—	—
Elasticity	−0.011	−0.047	−0.020	−0.004	−0.027	−0.031	−0.051	−0.067
	(0.014)	(0.044)	(0.039)	(0.024)	(0.021)	(0.055)	(0.046)	(0.042)
Mean of dep. variable	0.237	0.226	0.222	0.241	3.23	2.89	2.78	3.30
Observations	458,772	132,205	148,807	173,988	439,493	126,712	142,850	166,029
Age-group 55–74								
Tax (in 1995 dollars)	−0.054*	−0.085*	0.083	−0.042	−0.081*	−0.039	0.044	−0.039
	(0.033)	(0.047)	(0.087)	(0.095)	(0.045)	(0.092)	(0.105)	(0.073)
Marginal effect	[−0.007]	[−0.010]	[0.009]	[−0.005]	—	—	—	—
Elasticity	−0.023*	−0.058	0.043	−0.021	−0.041*	−0.031	0.026	−0.023
	(0.014)	(0.032)	(0.045)	(0.048)	(0.023)	(0.073)	(0.063)	(0.043)
Mean of dep. variable	0.161	0.150	0.151	0.162	2.23	2.01	1.99	2.24
Observations	261,209	77,039	88,186	99,274	254,536	75,196	86,025	96,407

Notes: Smokers are defined as everyday and someday smokers for the smoking propensity analyses and everyday smokers for the smoking intensity analyses. The full sample includes waves of the CPS-TUS from September 1995 to January 2007, excluding January and May 2000. The composition of the "Large Tax Change Sample" is detailed in Table A1. Estimates in columns 1 through 4 are from a logistic regression model of smoking participation. Estimates in columns 5 through 8 are from a GLM with a log link and negative binomial distribution of smoking intensity. Marginal effects on probability from logistic regressions are in brackets. Standard errors are in parentheses and are constructed allowing for nonindependence (clustering) at the state level. Standard errors of elasticity estimates calculated using the delta method.

*0.05 < p value ≤ 0.10; **0.01 < p value ≤0.05; ***p value ≤ 0.01.

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