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Cigarette taxes and older adult smoking: Evidence from recent large tax increases

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Abstract

While recent evidence casts some doubt, it is generally accepted that the price sensitivity of smoking varies inversely with age. We investigate the responsiveness of older adult smoking using variation from recent historically large cigarette tax increases in the United States. Using data from the Behavioral Risk Factor Surveillance System from 2000 to 2005, we find consistent evidence that higher taxes reduced smoking participation by older adults, especially those who are less educated and live in low-income households. Our findings run contrary to existing evidence which suggests that cessation behavior by older adults is not sensitive to price. Since a large literature suggests smoking cessation even later in life reduces morbidity and increases longevity, our findings may represent substantial gains in health among tax-induced quitters.

JEL classification: I18; I12

Keywords: Taxes and smoking behavior; Economics of smoking; Aging and health

1. Introduction

While recent evidence casts some doubt, it is generally accepted that smoking by youth and young adults is more price sensitive than that of older adults. This consensus is based on empirical evidence and on conventional wisdom that individuals with more experience with an addictive good, like cigarettes, are less sensitive to changes in its price.[1] Despite these findings, adult smoking behavior remains of interest, since a large literature implies that even late-in-life cessation can yield substantial improvements in mortality and morbidity.

Using data from the Behavioral Risk Factor Surveillance System (BRFSS) from 2000 to 2005, we investigate the price-responsiveness of older adult smoking using variation from recent historically large cigarette tax increases in the United States. Given the repeated cross-sectional nature of these data, we estimate standard two-way fixed

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[1] In terms of empirical evidence, see Lewit and Coate (1982), Evans and Farrelly (1998) and Farrelly et al. (2001) who examine the impact of taxes or prices on smoking across adults of various ages. See also Lewit et al. (1981), Chaloupka and Wechsler (1997), Tauras and Chaloupka (1999), Gruber and Zinman (2001) and Carpenter and Cook (2007) who all find evidence of substantial price sensitivity among teenagers, though some studies find evidence to the contrary (c.f., Wasserman et al., 1991; DeCicca et al., 2002).

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effects models that include controls for other state-level smoking policies implemented over this time period. We find consistent evidence that higher taxes reduced smoking participation by adults aged 45–59 and a slightly older group, suggesting that cessation behavior is price responsive. In particular, our findings imply that a $1 increase in the cigarette excise tax leads to a 1.0–1.5 percentage point decrease in daily smoking participation among these groups. Off a base of roughly 18%, this translates into a reduction in smoking prevalence of between 6 and 8%. We also find that tax-induced reductions in smoking are fairly equal for men and women, but are especially large for relatively less-educated, lower-income and less-healthy individuals.

The paper proceeds as follows. The next section reviews existing literature on the impact of price on older adult smoking. In general, most studies rely exclusively on cross-sectional variation in taxes or prices and, to our knowledge, we are first to exploit the recent large tax increases that occurred in the U.S. after 2001. These increases provide ample variation for the identification of tax effects, relative to earlier periods. We also provide motivation for the study, which rests in part on the findings of others which suggest that even later-in-life smoking cessation can yield substantial health improvements. Section 3 discusses our data, paying particular attention to key measures and providing detailed information on our analysis sample. In addition to focusing on the size and breadth of these historically large tax increases, we present evidence which suggests that they were driven by state budget shortfalls, rather than state-specific collective preferences regarding tobacco. Section 4 discusses our empirical strategy which involves a standard two-way fixed effects framework. Section 5 presents our results, along with estimates from several robustness checks, which together imply that older adult smoking participation fell in response to the large post-2001 tax increases. We also present evidence of substantial heterogeneity in the impact of cigarette taxes on older adult smoking and, separately, discuss possible health implications of our findings. Section 6 concludes the paper.

2. Background and motivation

2.1. Previous research on older adult smoking behavior

While economists have focused relatively more on youth smoking in recent times, a few papers provide evidence on the price-responsiveness of older adults, albeit indirectly or as part of examining the impact of price by demographic characteristics that include age.[2] In one of the earliest studies on the impact of excise taxes on smoking behavior, Lewit and Coate (1982) find little evidence that taxes reduce the smoking participation of adults 35 and older, for whom they estimate a price participation elasticity of −0.15 using the 1976 Health Interview Survey. More recent studies consistently find no systematic evidence that price reduces smoking prevalence among older adults. For example, Evans and Farrelly (1998) find a positive, albeit statistically imprecise, effect of tax on the smoking participation of adults at least 40 years old using data from the 1987 National Health Interview Survey. These authors do, however, find evidence that higher taxes reduce the amount smoked, in conditional demand models, though the elasticity is small.[3] Using repeated cross-sectional data from various waves of the National Health Interview Surveys between 1976 and 1993 and employing a two-way fixed effects strategy, Farrelly et al. (2001) similarly find no evidence that higher cigarette taxes affect the smoking behavior of older adults on the extensive margin, reporting a price participation elasticity of −0.02. Like Evans and Farrelly, they define an older adult as an individual at least 40 years old. Unlike Evans and Farrelly, they report a conditional demand elasticity of −0.06, which effectively represents no reduction in smoking on the intensive margin. Both studies, by contrast, find substantial evidence of price-responsiveness among younger smokers, especially those 18–24 years old.[4] More recent evidence also suggests a negligible effect of price on adult smoking. Tauras (2006), using repeated cross-sectional data from the 1990s, finds a small, but precisely estimated,

[2] A number of other studies focus on different aspects of smoking by older adults (c.f., Sloan et al., 2004, 2003; Smith et al., 2001).

[3] The paper's main point is also relevant. Tax increases may induce those who do not quit smoking altogether to substitute into cigarettes which contain more tar or nicotine. More recent work confirms these findings and suggests that smokers extract more nicotine by smoking cigarettes "harder" or longer when taxes increase (Adda and Cornaglia, 2006). We will return to these studies when we describe our smoking measures.

[4] In Evans and Farrelly (1998) the total price elasticity of smoking is −0.80, with a participation elasticity of −0.57 and a conditional demand elasticity of −0.22, while in Farrelly et al. (2001) the authors report an overall elasticity of −0.55, split roughly evenly between participation and conditional demand elasticities. Lewit and Coate (1982) also find relatively greater price-sensitivity with a participation elasticity of −0.74 and a conditional demand elasticity of −0.20 for 20–25-year olds.

effect which translates into a price participation elasticity of −0.12.[5] This author does not, however, estimate models for younger versus older adults, so we cannot assess the consistency of his findings with the general consensus that young adult smoking is more price-elastic than that of older adults.

2.2. The impact of smoking cessation on health

The lack of attention to the impact of taxes on older adult smoking, and the more general lack of focus on older adult smoking, is surprising given a large literature which suggests substantial health gains associated with smoking cessation by older adults. In 1990, the U.S. Surgeon General completed the first comprehensive study of smoking cessation (USDHHS, 1990). Among other things, it concluded that smoking cessation improves immediate and long-term health, including substantial reductions in mortality. Most relevant to our study, it provided evidence that even later-in-life smoking cessation increased longevity. In this regard, one of its key findings suggests that a 50-year old who quits smoking cuts the risk of dying by age 65 in half. Two more recent studies, using better data and methodologies, reach similar conclusions.[6] With respect to mortality, Taylor et al. (2002), using data from the Cancer Prevention Study II, find that individuals who quit enjoy prolonged life spans, relative to those who continue to smoke. The gains in longevity are largest at young ages, but remain substantial at older ones. Focusing on estimates that correspond to our age range, these authors find gains of between roughly 5.5 and 7 years for 45-year olds who quit smoking and 3.5–5.5 years for individuals who quit for good at age 55. While it is conceivable that increased longevity might lead to increased morbidity, Ostbye and Taylor (2004) find that smoking cessation leads not only to increases in years of life, but also in years of healthy life (YHL).[7] In particular, for individuals 60–64 years old, who quit smoking between 3 and 15 years prior, they find reductions in YHL of less than 1 year, relative to individuals who never smoked.[8] These estimates suggest that smoking cessation not only extends life, but also improves its quality by reducing smoking-related illness. To the extent that tax-induced cessation produces similar effects, there is potential for substantial improvement in health.[9]

3. Data

We use data from the BRFSS. The BRFSS is an annual telephone survey of adults 18 years of age and older, across the United States. Two key features of the BRFSS are that it has relatively large sample sizes and, more importantly, is representative of state populations by design. In addition to smoking behavior, the BRFSS contains information on health status, as well as more standard demographic information such as age, race, education and marital status. In what follows, we describe our key dependent and independent variables as well as our analysis sample.

3.1. Smoking measures

We employ two smoking measures. The first equals one if an individual reports smoking cigarettes on all days and zero otherwise, while the second is a more liberal definition: it equals one if an individual reports smoking on at least some days. Both measures are self-reported and are standard in the smoking literature. We do not examine the amount smoked, via number of cigarettes per day, because such information is not available in the BRFSS over this time period.[10] While unfortunate, recent evidence finds that smokers who do not quit altogether in response to higher

[5] He also finds evidence of a relatively small conditional demand elasticity of roughly −0.07.

[6] A number of other recent studies also suggest increased longevity and reduced morbidity (c.f., Doll et al., 1994; Tengs et al., 2001; Riiskinen et al., 2002).

[7] See Diehr et al. (1998) for details, but the main idea is that this measure combines mortality status with measures of health status taken over an individual's life.

[8] The implied period of smoking cessation corresponds most closely to the age group we examine. Individuals aged 60–64 who quit smoking 3–15 years prior were between 45 and 61 years old when they quit, which overlaps the age range of our sample almost perfectly.

[9] The potential endogeneity of smoking cessation may complicate matters for all studies of this type. However, the direction of any bias is unclear. If, for example, individuals who stand the most to gain in terms of health by quitting do indeed quit, then such estimates are biased upwards. Conversely, if individuals who are less healthy tend to quit, then they are underestimates of the true effect of smoking cessation.

[10] Of the years we include in our analysis, this information is available only in 2000.

Table 1
Cigarette tax increases of at least 50 cents per pack, 2000–2005

State	Increase	Resulting tax	Effective date
Arizona	58	118	November 2002
Alaska	60	160	January 2005
Colorado	64	84	January 2005
Connecticut	61	111	April 2002
Maine	100	200	September 2005
Massachusetts	75	151	July 2002
Michigan	50	125	August 2002
Michigan	75	200	July 2004
Minnesota	75	123	August 2005
Montana	52	70	May 2003
Montana	100	170	January 2005
New Jersey	70	150	July 2002
New Jersey	55	205	July 2003
New Mexico	70	91	July 2003
New York	55	111	March 2000
Ohio	70	125	January 2005
Oklahoma	80	103	July 2004
Oregon	60	128	November 2002
Pennsylvania	69	100	July 2002
Rhode Island	75	246	July 2004
Washington	60	142.5	January 2002
Washington	60	202.5	July 2005

Notes: Taxes and increases are denominated in nominal cents per pack of 20 cigarettes.

cigarette taxes gravitate towards longer cigarettes or those with higher tar and nicotine content (Evans and Farrelly, 1998). Even more recent evidence suggests smokers tend to smoke cigarettes "harder" or longer since the amount of cotinine, a by-product of nicotine, in continuing smokers' blood rises when taxes increase (Adda and Cornaglia, 2006). As both sets of authors note, such behavioral responses among continuing smokers may work to offset any health benefits of smoking fewer cigarettes per day.

3.2. Cigarette excise taxes, 2000–2005

We use data on taxes and dates of enactment from Orzechowski and Walker (2005) to construct monthly state cigarette taxes. We merge this information to the BRFSS which contains data on state of residence and date of interview. Using monthly, rather than annual, variation allows us to better identify within-year changes in behavior, which may be especially important in the context of the large tax increases we exploit. In all of our models, we use real cigarette excise tax rates per pack of 20 cigarettes denominated in 2001 dollars.

As alluded to earlier, there were several large tax increases on cigarettes between 2000 and 2005 at the state-level. Table 1 lists all increases of at least 50 cents and their dates of enactment. As can be seen, all but one of these large increases occurred after 2001. Indeed, from January 2002 to December 2005 there were 21 large increases in state cigarette excise tax rates, with an average increase of just over 68 cents per pack. While this list includes many of the "usual suspects" (e.g., Massachusetts, New Jersey and Rhode Island), it contains others not known for high cigarette excise taxes (e.g., Colorado, Montana, Ohio and Oklahoma). Research in public finance suggests that the size and breadth of these tax increases were due to state budget shortfalls following the 2001 recession in the United States (Maag and Merriman, 2003).[11] To the extent that these increases were indeed driven by budgetary concerns, rather than state-specific anti-smoking sentiment, they represent more appropriate variation for estimating the causal effect of taxes.[12]

[11] In a separate paper, these authors also show that relative to the 1991–1992 recession in the U.S., the 2001 recession had much deeper negative impacts on state tax revenues (Maag and Merriman, 2007).

[12] Our preferred specification includes controls for state-specific unemployment rates since previous work suggests that macroeconomic conditions may affect smoking behavior (Ruhm, 2005). As will be seen, their inclusion has no impact on our estimated tax coefficients. Further, there is evidence

Table 2
Smoking participation means, by year

	2000	2001	2002	2003	2004	2005
Smoker on all days	0.196	0.197	0.200	0.194	0.179	0.179
Smoker on some days	0.239	0.247	0.243	0.239	0.229	0.229

Notes: Sample includes all individuals aged 45–59 in the year indicated. "Smoker on some days" refers to individuals who smoke on at least some days.

Over the entire period, and with respect to all increases, the average state excise tax increased from about 42 to roughly 87 cents, more than doubling over this short period. By contrast, the average cigarette tax increased only from 30 to 40 cents over the previous 6-year period, 1994–1999. The difference in real, rather than nominal, increases is even more telling. In real 2001 dollars, average state cigarette tax increased from 35 to 42 cents for the earlier period versus 42–78 cents for our period. This dramatic increase over a relatively short period of time provides us with improved variation to identify the impact of taxes on smoking behavior, relative to earlier periods.

3.3. Analysis sample

The 2000–2005 BRFSS files contain data on 437,264 individuals between 45 and 59 years old.[13] Limiting our sample to respondents with valid smoking and state of residence information results in a slightly smaller set of 435,973 individuals, including 179,025 men and 256,948 women. Given that we include indicators for missing covariates, these figures represent our primary analysis samples, though sample sizes naturally change when we investigate heterogeneity in our main estimates. We also present estimates from models that contain no covariates, other than relevant fixed effects to check whether treating missing covariates as described above affects our estimates.

Focusing on the time pattern of smoking in our sample suggests that the tax increases we exploit may have indeed reduced daily smoking. As can be seen in Table 2, the fraction of daily smokers fell from roughly 0.196 to 0.179 from 2000 to 2005, a reduction of nearly 9%. Moreover, the proportion did not start to decrease until 2003, the year following the beginning of truly large tax increases, as listed in Table 1. Smoking on at least some days follows a similar, if less exaggerated, pattern. While suggestive, rigorous empirical analysis is needed to provide evidence of causality (e.g., this was also a period of substantial anti-smoking policy activity by states), the next section presents our empirical strategy which relies on two-way fixed effects models.

4. Empirical strategy

In what follows, we describe our strategy for identifying the impact of state cigarette taxes on the smoking participation of older adults. It is well understood that cross-sectional estimates of this relationship may be subject to serious omitted variables bias. The particular concern is that unobserved state-level sentiment towards smoking or other state-level heterogeneity may be correlated with the level of cigarette taxes in a particular state, while simultaneously having an independent effect on smoking behavior.

A common approach to dealing with such heterogeneity is the inclusion of state fixed effects in cigarette demand equations. To the extent that the heterogeneity in question is time-invariant, state fixed effects will purge the correlation between the error term and the tax variable, eliminating potential omitted variables bias. In this vein, we estimate standard two-way fixed effects specifications of the following form:

$$S_{\mathrm{ijmt}} = \alpha + \gamma T_{\mathrm{jmt}} + \beta X_{\mathrm{ijmt}} + \delta Z_{\mathrm{jmt}} + \sigma_{\mathrm{j}} + \mu_{\mathrm{m}} + \tau_{\mathrm{t}} + \varepsilon_{\mathrm{ijmt}}$$

Here, i indexes the individual, j represents state of residence, m represents month of interview, and t represents year of interview. σ, μ, and τ represent state, month and year fixed effects, respectively, S represents our smoking participation

that the Master Settlement Agreement led to increased state taxes, but the estimated effect is relatively small (roughly 10 cents) and likely occurred prior to the majority of the recession-driven increases we exploit (Trogdon and Sloan, 2006).

[13] We restrict our sample to individuals less than 65 years old because existing evidence suggests that the health benefits of smoking cessation decline sharply with age (c.f., Taylor et al., 2002).

Table 3
Estimated impact of cigarette excise tax on smoking participation, 2000–2005

	Age 45–59			Age 45–64		
	(1)	(2)	(3)	(1)	(2)	(3)
Smoker on all days						
Real cigarette tax	−0.0148	−0.0139	−0.0139	−0.0102	−0.0101	−0.0098
	(0.0039)	(0.0043)	(0.0044)	(0.0036)	(0.0037)	(0.0036)
	[3.82]	[3.24]	[3.18]	[2.84]	[2.71]	[2.68]
	{−0.31}	{−0.29}	{−0.29}	{−0.22}	{−0.22}	{−0.21}
Dependent mean	0.183	0.183	0.183	0.175	0.175	0.175
N	435,973	435,973	435,973	543,384	543,384	543,384
Smoker on some days						
Real cigarette tax	−0.0168	−0.0150	−0.0146	−0.0121	−0.0117	−0.0110
	(0.0038)	(0.0033)	(0.0031)	(0.0042)	(0.0035)	(0.0032)
	[4.40]	[4.53]	[4.67]	[2.90]	[3.28]	[3.38]
	{−0.28}	{−0.25}	{−0.24}	{−0.21}	{−0.21}	{−0.20}
Dependent mean	0.229	0.229	0.229	0.220	0.220	0.220
N	435,973	435,973	435,973	543,384	543,384	543,384

Notes: Coefficients on real cigarette tax are reported in the first row, standard errors are in parentheses in the second row, absolute values of *t*-statistics are in brackets in the third row and implied price participation elasticities are in curly braces in the fourth row. All coefficients and corresponding standard errors are multiplied by 100. Model (1) includes only state, month and year fixed effects and no other covariates. Model (2) includes state, month and year effects, in addition to the following individual-level covariates: gender, age and its square, race, education, income, marital status and health status, while Model (3), our preferred specification, adds controls for state unemployment rate and separate indicators that equal unity if a state bans smoking from private workplaces and restaurants. All models are weighted using sample weights provided by BRFSS. Though not reported, probit marginal effects are nearly identical in all cases, suggesting that the effects presented are not dependent on model choice. Standard errors are adjusted to account for non-independence of observations in the same state.

measures, T represents state-specific cigarette excise taxes, and Z is a set of other smoking regulations defined at the state-level. In particular, we include separate indicators for whether a state-banned smoking in private workplaces and in restaurants since there was considerable action in these policies over this period.[14] We estimate all models with and without a set of covariates (X) as a robustness check. All reported standard errors are clustered by state of residence to allow for non-independence of observations within the same state.

Implementing this strategy requires sufficient variation in the exogenous variable of interest, cigarette excise taxes. To assess this, we regress our tax variable on a set of state, year and month fixed effects. We then compute the variance inflation factor, which, in this context, is the reciprocal of one minus the R^2 from this auxiliary regression. Conventionally, it is assumed that if variance inflation factor is greater than 10, there is not sufficient independent variation to perform estimation with the set of fixed effects (c.f., Kennedy, 1994, p. 183). However, our auxiliary regression yields an R^2 small enough such that sufficient variation in taxes is implied. In particular, the variance inflation factor is roughly 6.2.

5. Estimates and their implications

5.1. Main estimates

Table 3 presents estimates for two groups of older adults: individuals 45–59 years old and individuals 45–64 years old. The upper panel of Table 3 represents estimates from models of daily smoking and the lower panel represents corresponding estimates from models where the dependent variable equals one if the individual smokes on

[14] The number of states banning smoking at private worksites grew from 1 to 10 and the corresponding number for restaurants increased from 1 to 11. Given the timing of these policies, in conjunction with some evidence that they reduce smoking behavior, we think it is a prudent strategy to estimate tax effects in their presence (Evans et al., 1999; Carpenter, 2006). As will be seen, their inclusion has no impact on our main estimates.

at least some days.[15] In both panels, upper and lower, we present the estimates of γ from three specifications which correspond to the columns of Table 3. The specification reported in column (1) effectively treats cigarette taxes as exogenous, including no covariates other than relevant fixed effects for state, year and month. The specification reported in column (2) adds a set of individual-level covariates, including controls for age, gender, race, education, income, marital status and general health status. With the exception of age, the individual-level covariates are parameterized as a series of indicator variables which insures a flexible functional form. In particular, excluding reference categories we include eight indicators for race, three for education, eight for income, six for marital status and six for health status. The specification reported in column (3) is our preferred specification. It augments the column (2) specification by adding time-varying state-level covariates. These include separate controls for state-level bans on smoking in private workplaces and restaurants, as well as state-specific monthly unemployment rates since previous work suggests macroeconomic conditions affect smoking participation and there may be cyclical variation in cigarette excise tax setting policy (Ruhm, 2005).[16] As discussed, there was a great deal of policy action in these areas over the period in question.[17] As can be seen in Table 3, there is little variation in the estimated effect of cigarette taxes on older adult smoking participation across the three columns, independent of age group or smoking measure.

The first three columns of Table 3 present estimates for individuals aged 45–59. We find consistent evidence that higher taxes reduce daily smoking prevalence for this group. In particular, our estimates imply that a \$1 increase in state cigarette tax rates will reduce daily smoking participation by nearly 1.4 percentage points.[18] Relative to a base of 18.3%, this translates into a nearly 8% reduction in the fraction of daily smokers. While we prefer to present our estimates in this manner, we also present corresponding price participation elasticities since they are a common metric in the literature.[19] Corresponding price participation elasticities range from −0.29 to −0.31, which are large relative to the general consensus and relative to estimates reviewed earlier in the paper (e.g., Evans and Farrelly, 1998; Farrelly et al., 2001). We find similar estimates when focusing on some-day smoking for 45–59-year olds. In particular, we again find that a \$1 increase is linked to roughly a 6% decrease in smoking and price participation elasticities that range from −0.24 to −0.28.[20] While somewhat smaller than the elasticities associated with daily smoking participation, these findings also represent a departure from what is generally accepted in the literature.

Estimates for a slightly larger group, those aged 45–64, are similar, but slightly smaller in magnitude, suggesting less price-sensitivity among those 60–64 years old. With respect to daily smoking, our preferred models (i.e., column (3)) imply a \$1 increase reduces the proportion of daily smokers in this age group by nearly 6%, with corresponding price participation elasticities of just over −0.2. Again, we find similar estimates when focusing on some-day smoking behavior. In this case, a \$1 increase is associated with a 5% reduction and price participation elasticities are again roughly −0.2. Despite their smaller magnitude, relative to the 45–59 age group, these estimates also represent substantially greater price-responsiveness relative to what is generally accepted.[21]

In sum, our main estimates run contrary to the conventional wisdom on older adult smoking in that they imply a greater degree of price-sensitivity than has been found for this age group. As discussed in DeCicca et al. (submitted), price participation elasticities can be interpreted as weighted averages of initiation and cessation elasticities. The intuition is that changes in smoking participation reflect underlying smoking dynamics. For example, a participation elasticity for youth is likely dominated by the effect of price on smoking initiation. Given the age of the group

[15] This is a more liberal definition of smoking participation than daily smoking (i.e., smoking on all days).

[16] We obtained information on state smoking bans from various editions of State Legislative Action on Tobacco Issues (SLATI), a publication of the American Lung Association.

[17] We do not control for bans on smoking in bars, since there were relatively few over this period.

[18] This figure corresponds to the third column of the first row of Table 3.

[19] Our preference is based on the fact that participation elasticities are sensitive to the group-specific fraction of smokers, which is included in the metric's denominator. That said, they avoid the need to select a somewhat arbitrary hypothetical tax increase; in our case, 1\$.

[20] To be clear, the 6% figure is calculated as the percentage point decrease in smoking participation due to a 1\$ increase (here, 1.46 percentage points) relative to a base smoking participation rate of 22.9%. Dividing these two figures yields the 6% figure cited in the text. All percent changes mentioned are calculated similarly.

[21] We also estimated models on a sample of individuals 40 years and older to be more consistent with previous work. These models yield estimates that imply price participation elasticities of −0.22 and −0.19 in "all-day" and "some-day" smoking participation models, respectively. So, unlike existing work, we continue to find evidence of a systematic relationship between taxes and older adult smoking participation.

Table 4
Investigating the robustness of our main estimates

	Age 45–59			Age 45–64		
	(1)	(2)	(3)	(1)	(2)	(3)
Smoker on all days						
Real cigarette tax	−0.0239	−0.0106	−0.0215	−0.0175	−0.0099	−0.0156
	(0.0106)	(0.0039)	(0.0068)	(0.0092)	(0.0036)	(0.0064)
	[2.25]	[2.75]	[3.18]	[1.91]	[2.78]	[2.44]
	{−0.51}	{−0.23}	{−0.50}	{−0.39}	{−0.22}	{−0.38}
Dependent mean	0.183	0.183	0.183	0.175	0.175	0.175
N	435,973	435,973	334,449	543,384	543,384	419,384
Smoker on some days						
Real cigarette tax	−0.0222	−0.0076	−0.0148	−0.0189	−0.0073	−0.0094
	(0.0105)	(0.0042)	(0.0070)	(0.0099)	(0.0041)	(0.0068)
	[2.12]	[1.83]	[2.11]	[1.92]	[1.75]	[1.39]
	{−0.38}	{−0.13}	{−0.28}	{−0.33}	{−0.13}	{−0.18}
Dependent mean	0.229	0.229	0.229	0.220	0.220	0.220
N	435,973	435,973	334,449	543,384	543,384	419,384

Notes: Coefficients on real cigarette tax are reported in the first row, standard errors are in parentheses in the second row, absolute values of *t*-statistics are in brackets in the third row and implied price participation elasticities are in curly braces in the fourth row. All coefficients and corresponding standard errors are multiplied by 100. Estimates reported correspond to our most preferred model from column 3's in Table 3, augmented as follows: Model (1) includes state-specific time trends, Model (2) includes an explicit measure of state anti-smoking sentiment, as discussed in the text, in lieu of state fixed effects, while Model (3) restricts the sample to observations from years 2002 to 2005, the period of greatest tax increases. All models are weighted using sample weights provided by the BRFSS. Though not reported, probit marginal effects are nearly identical in all cases, suggesting that the effects presented are not dependent on model choice. Standard errors are adjusted to account for non-independence of observations in the same state.

we examine, it seems very likely that our relatively large participation elasticities reflect large cessation elasticities.

5.2. Sensitivity analysis

Table 4 presents estimates from three separate robustness checks that build upon our most preferred specification.[22] First, we add state-specific trends in order to account for trends beyond those common to all states. Second, we include an empirical measure of state-level anti-smoking sentiment from DeCicca et al. (2006), in lieu of state fixed effects. This measure is an attempt to explicitly model the sort of heterogeneity that prompts the inclusion of state fixed effects. Third, we restrict our sample to individuals surveyed after 2001 since this corresponds to the beginning of the truly large tax increases, like those listed in Table 1. As seen in Table 4, our main finding continues to hold. Indeed, in two of three cases, estimates from Table 4 are actually somewhat larger than our preferred estimates. For example, focusing on individuals aged 45–59, estimates from models that include state-specific trends imply price participation elasticities of −0.51 and −0.38 in daily and some-day smoking models, respectively, while estimates from models that limit our sample to observations from 2002 to 2005 suggest corresponding elasticities of −0.50 and −0.38. Finally, implied price participation elasticities from models that include the anti-smoking sentiment measure are nearly identical to corresponding estimates from our preferred models.[23] Hence, our finding that higher cigarette taxes reduce smoking participation by older adults is quite robust to reasonable changes in specification and sample.

[22] Recall that estimates from our most preferred specification correspond to column 3 of Table 3.

[23] In addition, we estimate models where the dependent variable equals one if an individual reports *never* being a smoker, as an "anti-test" of sorts. That is, in these age groups, cigarette taxes should not affect never-smoker status since our estimates should be driven by individuals transitioning from current-smoker to former-smoker status. Indeed, when we estimate such models, we find no evidence of a relationship between cigarette taxes and non-smoker status.

Table 5
Allowing for heterogeneity in the impact of cigarette taxes on smoking participation, 2000–2005

	Gender		Education		Income		Health status	
	(1) Men	(2) Women	(3) Low	(4) High	(5) Low	(6) High	(7) Unhealthy	(8) Healthy
Real cigarette tax	−0.0161	−0.0114	−0.0286	−0.0043	−0.0273	−0.0035	−0.0369	−0.0038
	(0.0054)	(0.0048)	(0.0088)	(0.0036)	(0.0102)	(0.0052)	(0.0130)	(0.0049)
	[2.95]	[2.35]	[3.25]	[1.21]	[2.67]	[0.67]	[2.85]	[0.78]
	{−0.31}	{−0.27}	{−0.43}	{−0.12}	{−0.39}	{−0.09}	{−0.54}	{−0.11}
Dependent mean	0.201	0.165	0.259	0.136	0.271	0.146	0.266	0.138
N	179,025	256,948	159,042	276,294	135,047	254,602	75,750	235,525

Notes: Coefficients on real cigarette tax are reported in the first row, standard errors are in parentheses in the second row, absolute values of *t*-statistics are in brackets in the third row and implied price participation elasticities are in curly braces in the fourth row. Coefficients and corresponding standard errors multiplied by 100. Dependent variable equals one if individual reports smoking on all days. Low education is defined as individuals with a high school diploma or equivalent, low income is defined as individuals living in households with income under $35,000 and "Unhealthy" individuals are those that report "fair" or "poor", as opposed to "Healthy" individuals who report "excellent" or "very good"; we drop individuals who report "good" health status, the median category, though estimated elasticities are −0.44 and −0.20 when they are included in the low and high health status models, respectively. All models are weighted using sample weights provided by the BRFSS. Though not reported, probit marginal effects are nearly identical in all cases, suggesting that the effects presented are not dependent on model choice. Standard errors are adjusted for non-independence of observations within states.

5.3. *Heterogeneous tax effects*

Given the relative size of our main estimates, we focus on a natural next question: Is there relevant heterogeneity in the overall tax effect? In Table 5, we present estimates from models of daily smoking for 45–59-year olds that allow the effect of taxes to vary by gender, formal education, household income and general health status. The first is conventional, the second and third are in the spirit of recent empirical work that examine the equity implications of cigarette taxes, and the fourth is of interest since cigarette taxes, via induced cessation, may improve health.[24] While somewhat larger for males, the strong price effect observed in Table 3 continues to obtain for both genders. More interestingly, we find evidence that low-educated individuals, defined as those with a high school education or less, are much more price-sensitive than their more-educated counterparts. In particular, we find that a $1 increase in cigarette tax will decrease the fraction of smokers we label as low-educated by over 10%, while the corresponding figure for relatively more-educated individuals is roughly 3%. These estimated effects translate into price participation elasticities of −0.43 and −0.12, respectively. We also redefine the low education category in two ways—first by adding individuals with more than a high school degree, but less than a bachelor's degree, and, second, we redefine this group as only those individuals with *less* than a high school degree. Though not reported, we find respective price participation elasticities of −0.28 and −0.90. Estimates that compare low and higher income individuals are consistent with these findings. Low-income individuals, defined as those living in households with annual incomes of less than $35,000, are found to quit at a much higher rate in response to higher taxes than their counterparts from higher income households. Here, we find that a $1 increase in tax will reduce the fraction of smokers by about 10% and higher income individuals by only about 2%. Though not reported, we redefine low income as living in a household with less than $20,000 in annual income and find an even greater price participation elasticity of −0.93. These estimates are consistent with Gruber and Koszegi (2004) who report greater price-sensitivity among lower income individuals. As these authors observe, such a pattern may have implications for the regressivity of cigarette taxes. However, as Colman and Remler (2004) note, it is unlikely that even what are considered to be high levels of price sensitivity would result in cigarette taxes being progressive.[25]

[24] Equity concerns may be particularly important in the context of these recent historically large tax increases. See Gruber and Koszegi (2004) and Colman and Remler (2004) for related work.

[25] An additional complication is that we report *participation* elasticities. As such, an elasticity greater than 1 does not necessarily imply an overall reduction in spending on cigarettes. Instead, some individuals quit and reduce their tax burden to zero, while those who remain as smokers may

Finally, we allow the impact of cigarette taxes on smoking behavior to vary by self-reported general health status, where legitimate responses include “excellent”, “very good”, “good”, “fair” and “poor”. In particular, we label an individual as “healthy” if he or she responds that his or her health is “excellent” or “very good” and we label individuals who report “fair” or “poor” health as “unhealthy”, excluding individuals who report the medial category, “good”. We find greater price-sensitivity for “unhealthy” individuals, as defined above. In particular, we find that a \$1 increase in cigarette tax leads to a nearly 13% decrease in smoking participation among this group, which translates into a price participation elasticity of −0.54. By contrast, a \$1 increase only reduces smoking by healthy individuals by about 3% with an implied price participation elasticity of −0.11. When we include individuals who report “good” health in these models, these price participation elasticities become −0.44 and −0.20 for unhealthy and healthy individuals, respectively. Since these estimates may be driven by taxes leading to improved self-reported health, we estimate separate models where the dependent variable equals one if the individual is “healthy” and “unhealthy”, as defined above. Though unreported, we find no systematic evidence that cigarette taxes affect health status, suggesting the tax effects we measure are not driven by this phenomenon.[26]

5.4. Implications for health

Our main estimates, in conjunction with available evidence on the health benefits of smoking cessation, suggest substantial improvements in the health of tax-induced quitters. In what follows, we offer a back-of-the envelope calculation with respect to mortality, taking the findings of Taylor et al. (2002) as given. In 2000, there were roughly 50 million adults aged 45–59 in the United States. Based on our smoking prevalence estimate of roughly 18%, this implies about 9 million daily smokers in this age group. If a \$1 tax increase does indeed cause about 7% of daily smokers to quit, our estimates conservatively imply roughly 600,000 tax-induced quitters. Estimates from Taylor et al. (2002) suggest that individuals who quit smoking at 45 years old can expect to live 5.5–7 years longer than continuing smokers and those aged 55 can expect to live 3.5–5.5 additional years relative to this group. Given the nature of our data, we do not know exact age of cessation, but these ranges imply expected gains of 2.1–4.2 million life years, assuming that 600,000 daily smokers are induced to quit.

While these implied effects seem large to us, we remind the reader of some important caveats. First, the studies on which we base these projections cannot explicitly account for the potential endogeneity of smoking cessation. If individuals who quit smoking have the most to gain in terms of longevity by doing so, then our estimates of additional years of life should be considered upper bounds. Of course, if individuals who quit smoking on their own are, for example, already experiencing declining health, then our estimates might be understated.[27] Second, our estimates suggest that individuals who are less healthy, of lower education and have lower household incomes, are induced to quit by higher cigarette taxes. To the extent that the worse health of these individuals is permanent, our estimates again may represent upper bounds. For example, though they do not focus on the longevity effects of smoking cessation per se, Adda and Lechene (2004) find that smokers in poor health lose an average of 3 years of life compared to poor-health non-smokers. By contrast, these authors find that this difference is larger – roughly 4.5 years – when comparing good-health smokers to similar non-smokers. Third, some of our observed tax-induced quitting behavior may be temporary. While tax-induced quitting may involve less recidivism than quitting on one’s own, it is likely that not all such individuals will remain non-smokers for the rest of their lives. Finally, there may be offsetting behavior such as weight gain that works to reduce the health gains associated with the tax-induced cessation we document (c.f., USDHHS, 1990 and, more recently, Chou et al., 2004; Gruber and Frakes, 2006). All of that said, it remains very likely that our findings imply non-trivial gains in longevity, and health more generally, even if they are overstated to some extent.

well spend more unless they cut back substantially on the amount smoked. Again, we are unable to analyze changes in amount smoked due to lack of information in all but one of our years of analysis.

[26] In particular, we estimated models that allowed the impact of cigarette taxes on health to vary by smoking status as well as more straightforward, reduced-form models.

[27] Taylor et al. (2002) offer another reason why their figures might be underestimates. In particular, some individuals in their comparison group – continuing smokers – may have actually quit over the period in question and this would tend to increase the life expectancy of those labeled as continuing smokers.

6. Conclusion

Contrary to existing work, we find substantial price-sensitivity in the smoking behavior of older adults. In particular, we find that a $1 increase results in a 6–8% reduction in smoking participation among individuals aged 45–59. We also find evidence that these large tax effects are concentrated among older adults who are, on average, less educated, of lower income and less healthy. Our estimates imply smoking cessation is more sensitive to price than is generally accepted in the smoking literature. Since a large literature suggests that even later-in-life cessation yields increases in longevity and reductions in morbidity, our findings may represent substantial health improvements for tax-induced quitters.

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