Rising cigarette prices and rising obesity: Coincidence or unintended consequence?

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Abstract:
Economists have begun to debate if the rise in cigarette prices in the U.S. in recent decades has contributed to the nation's rise in obesity, reaching conclusions that are surprisingly sensitive to specification. I show that allowing for the effect to occur gradually over several years leads to the conclusion that a rise in cigarette prices is actually associated with a long-run reduction in body mass index and obesity. This result is robust to the different methodologies used in the literature. I also provide evidence that indirect effects on exercise and food consumption may explain the counterintuitive result.

Keywords: Obesity; Weight; Smoking; Cigarette prices; Cigarette taxes

Article:

1. Introduction

Obesity became a critical public health concern in the late 20th century. Studies link excessive body weight to a variety of adverse health conditions, such as high blood pressure, diabetes, heart disease, and stroke (Strum, 2002). The percentage of people in the U.S. who are classified as overweight (having a body mass index[1] of 25–30) or obese (having a BMI of 30 or greater) rose from 43.3% in 1960 to 66.3% in 2004, while the obesity rate grew from 12.8% to 32.2% during this period (Flegal et al., 1998) and (Ogden et al., 2006). Consequences of obesity include an estimated 112,000 deaths and $117 billion in medical and related costs per year (Flegal et al., 2005) and (U.S. Department of Health and Human Services, 2001). While exact estimates vary, experts in the medical community universally acknowledge that obesity imposes substantial costs on society, both in terms of early mortality and medical expenses.

An even more prominent public health concern in the past half-century has been cigarette smoking, which leads to approximately 438,000 deaths and $167 billion in medical expenses per year (Armour et al., 2005). However, while the prevalence of obesity has been steadily increasing, smoking has declined substantially since the 1970s. According to the Center for Disease Control, the percentage of adults who smoke regularly dropped from 42.4% in 1965 to 20.9% in 2004.[2] This decline, largely the result of the U.S. government's anti-smoking campaign, "has been one of the major public health victories in the second half of the 20th Century" (Gruber and Frakes, 2006, p. 186).

While the temporal correlation between the rise in obesity and drop in smoking may be purely a coincidence, a causal relationship is possible. Medical evidence suggests that cigarettes can both suppress one's appetite and increase one's metabolism; therefore, a person who quits smoking may begin to consume more calories while expending fewer, leading to weight gain (Pinkowish, 1999). Smoking also dulls one's taste buds, meaning that food becomes less appealing. Additionally, smokers attempting to quit may feel the need to put something in their mouths to replace cigarettes, resulting in increased caloric intake ("Weight gain…," 2006). This phenomenon is known as "oral fixation." Not surprisingly, a variety of studies show that people tend to gain weight shortly after quitting smoking U.S. Department of Health and Human Services, 1990 U.S. Department of Health and Human Services, 1990. The health benefits of smoking cessation: a report of the surgeon general.(U.S. Department of Health and Human Services, 1990). As a result, some experts fear that the anti-smoking campaign has had the “unintended consequence” of contributing to the U.S.’ rise in obesity (Chou et al., 2004, p. 581). The stronger the connection between smoking and obesity, the less policies designed to discourage smoking would reduce mortality and medical expenditures. If the effect of smoking on weight is so large that the increase in obesity-related mortality was larger in magnitude than the decrease in smoking-related mortality, then the anti-smoking campaign could have actually worsened public health.

In this paper, I examine the effects of cigarette prices and taxes on body weight, an approach that is appealing for two reasons. First, simply estimating the relationship between smoking and obesity would do little to identify causality. Heavy individuals may use smoking as a method of weight control (Cawley et al., 2004), and unobservable characteristics such as one’s attitude toward health may determine both smoking and body weight. Cigarette prices or taxes are potentially exogenous indicators of smoking prevalence. Second, examining the relationship between cigarette prices and obesity actually provides a relatively direct test of the effect of the anti-smoking campaign on obesity. While the anti-smoking campaign encompassed other aspects, such as clean indoor air laws and information-spreading programs, one of the main mechanisms through which it reduced smoking was by affecting real cigarette prices, which rose by 257% from 1980 to 2005 (Fig. 1).[3] While widespread federal and state excise taxes directly increased the price of cigarettes, the majority of the effect of the anti-smoking campaign on cigarette prices may have been indirect. It is a widely known paradox that cigarette companies responded to the reduction in demand caused by the government’s information-spreading programs by increasing prices.[4] Another aspect of the anti-smoking campaign – tobacco lawsuits – raised prices even further in the late 1990s (Gruber and Frakes, 2006), as cigarette companies attempted to pass their losses on to loyal customers.

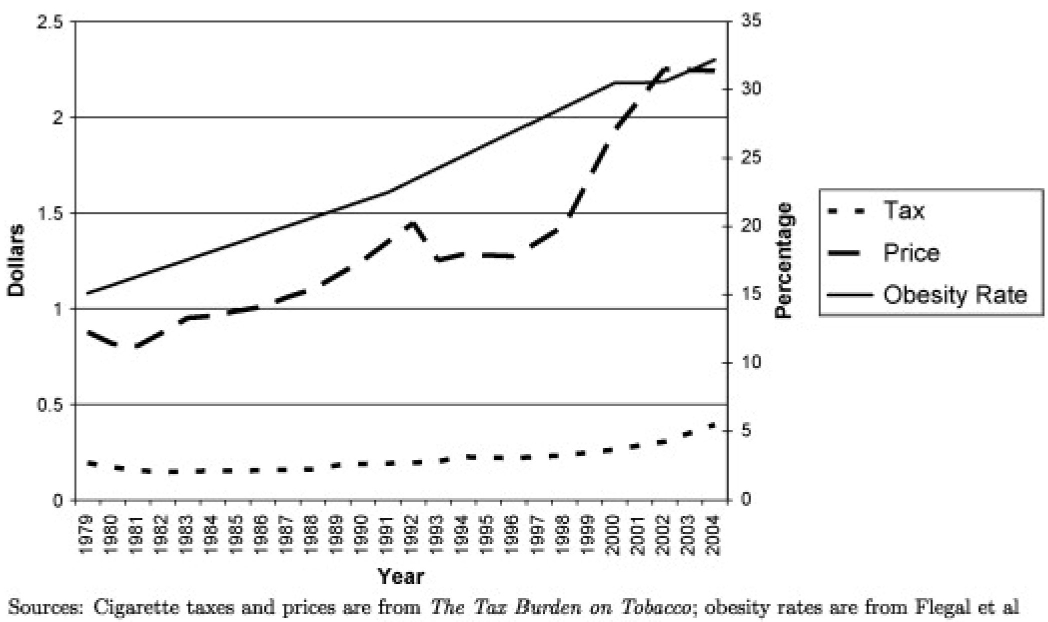

Fig. 1. Trends in average real state cigarette tax rate, average real cigarette price, and obesity.

Economists have recently begun to study the relationship between contemporaneous state cigarette prices or taxes and individual body weight, reaching conclusions that are highly sensitive to specification (Chou et al., 2002), (Gruber and Frakes, 2006), (Rashad et al., 2006) and (Baum, 2009). I show that including lags of prices/taxes causes the different methodologies in the literature to produce the same result: higher cigarette costs are associated with reductions in body mass index (BMI) and obesity in the long run. Specifically, my estimates imply that a permanent $1-per-pack increase in cigarette prices or taxes would eventually decrease average BMI by 0.13–0.59 units and the obesity rate by 1.1–3.6 percentage points.[5] I also conduct a variety of robustness checks, finding that the results are robust to the inclusion of state-specific time trends and a proxy variable for health consciousness, the estimation of both fixed effects and long-differences models, different approaches to using lags, and the use of both National Longitudinal Survey of Young (NLSY) and Behavioral Risk Factor Surveillance System (BRFSS) data. Finally, I examine the mechanisms through which this counterintuitive effect occurs, finding some evidence that people make healthier eating and exercise decisions after a cigarette price increase. Although future research is needed to determine the reason for these healthier habits, possible explanations include increased interest in health, increased confidence in self-control, or a replenished stock of willpower after reducing smoking.

2. Literature review

An increase in cigarette prices would cause a rise in obesity if cigarette prices negatively impact smoking and smoking negatively impacts weight. The first of these two relationships is backed by a number of studies. Most estimates place the price elasticity of demand for cigarettes between −0.3 and −0.5, meaning that a 10% increase in cigarette prices should lead to a 3–5% drop in smoking (Chaloupka, 1999).

The link between smoking and weight, however, is less clear. A 1990 U.S. Surgeon General review of 15 studies found that 58–87% of people who quit smoking gained weight, with the average gain being a modest four pounds (U.S. Department of Health and Human Services, 1990). However, most of these studies followed the individuals for only a short period of time. Caan et al. (1996) showed that the rate of weight increase after quitting slows after six months, and longer-range studies suggest that some or all of the weight gain is temporary (Mizoue et al., 1998) and (Chen et al., 1993). Despite the conflicting evidence, fear of weight gain remains a major reason why people postpone quitting smoking, as evidenced by the existence of pills, lozenges, books, internet programs, and even hypnotic techniques that claim to help people quit smoking without gaining weight.[6] Additionally, most web sites which encourage quitting smoking feel the need to address the issue of potential weight gain and provide suggestions for preventing it.[7]

Four recent economics papers have implemented reduced-form estimation techniques in an attempt to determine the relationship between cigarette prices and weight. Chou, Grossman, and Saffer (CGS) (2004) used pooled individual-level data from the 1984–1999 waves of the Behavioral Risk Factor Surveillance System (BRFSS) matched with an impressive array of state characteristics – including fast food prices, full-service restaurant prices, food at home prices, alcohol prices, number of restaurants, cigarette prices, and clean indoor air laws – in an effort to determine the extent to which these variables explain the rise in obesity. Using state-level cigarette prices from the Tax Burden on Tobacco, they found that a 10% rise in the real price of cigarettes leads to a 0.445% increase in the probability an individual is obese. While they do not explicitly control for the upward trend in BMI over time, the other state-level variables are all strongly correlated with time and should therefore capture much of the effect of time. They use a quadratic functional form for these variables. In the NBER working paper version (2002), CGS also estimate models including a quadratic time trend, which does not affect the signs of their results but does reduce their magnitude.

Gruber and Frakes (GF) (2006) argue that CGS' estimators could be biased because prices may depend on a variety of demand-side characteristics. In addition, GF argue that CGS' use of a quadratic time trend is an incomplete approach to modeling the effect of time on weight; this could affect their results because of the strong upward trend in both weight and cigarette price. GF address these problems by using state cigarette tax rate as a proxy for price and dummy variables for each month–year combination instead of the quadratic time trend. Other more minor alterations include a linear instead of quadratic variable of interest (price/tax), a linear instead of quadratic income effect, the use of a set of dummy variables for gender–age group combinations, the inclusion of state unemployment rate as a regressor, the use of sampling weights, and the exclusion of people 65 years old and above. Although they use the same BRFSS data as CGS (plus three additional survey years), GF find a negative relationship between cigarette tax rate and weight/obesity: a $1.00 rise in taxes lowers BMI by 0.151 and the probability of being obese by 0.015 percentage points. They claim, therefore, that measures taken to curb cigarette smoking do not worsen America's obesity problem. While GF's main results do not include the food and alcohol price, restaurant, and clear indoor air law variables used by CGS, they also run regressions borrowing CGS' data, which contain these variables, and obtain almost identical results. Therefore, omitting these additional state-level variables does not appear to be driving their findings.

While GF's use of taxes and time dummies would generally be expected to improve identification, Chou et al. (2006) reply with an argument that for this particular topic using prices and a quadratic time trend may actually be preferable. They argue that the smoking industry's data – which features overshifting of taxes to consumers and a price elasticity of less than one – is consistent with the Cournot model of oligopoly, in which the error term in the demand function is uncorrelated with optimal price. They also cite a paper (Schneider et al., 1981) which suggests that including excessive controls can lead to misspecified models of the demand for cigarettes and argue that the extensive set of month–year dummies used by GF may have such an effect.

Rashad, Chou, and Grossman (RCG) (2006) use National Health and Nutrition Examination Survey (NHANES) data and an estimation approach similar to GF in that their variable of interest is cigarette tax and they account for the change in weight over time using year dummies. However, they model age, gender, and income similarly to CGS. They find a positive but small effect of cigarette taxes on weight. The fact that RCG's results are in between those of CGS and GF is not surprising since their methodology represents a hybrid of the two approaches.

More recently, and concurrent with this paper, Baum (2009) uses NLSY data and attempts to disentangle correlation and causality by developing a difference-in-difference-type approach in which he interacts cigarette price/tax with a "treatment status" indicator, where a person is considered "treated" if she smoked at least 100 cigarettes in her life by 1992. He considers the effect on the "treatment group" to be the true causal impact, assuming that any correlation between cigarette costs and the weight of people with no smoking history is likely to be the result of endogeneity. He finds that a rise in either prices or taxes increases BMI and obesity, and that the results are similar using both year dummies and a quadratic trend.

In this paper, I contribute to the literature in two main ways. First, the aforementioned papers all focus on the relationship between contemporaneous prices/taxes and weight, but I show that these different methodologies lead to the same robust conclusion when lags of cigarette price/tax are included: in the long run, a rise in prices/taxes is associated with a reduction in BMI and obesity. Second, I provide and test possible explanations for the counterintuitive result, finding some evidence to support the theory that successfully quitting smoking eventually leads to healthier decisions in other areas as well.

3. Cigarette prices and body weight

Despite the popular perception that smoking reduces body weight and that quitting increases it, a more careful analysis reveals that the direction of the effect is theoretically indeterminate. A person's body mass index (B) depends on her food intake (F), level of exercise (E), and metabolism (M), or number of calories she would burn with no physical activity. Therefore,

$$B=f(F,E,M) \tag{1}$$

where $dB/dF > 0$, $dB/dE < 0$, and $dB/dM < 0$.

Reducing smoking may increase BMI because smoking may increase metabolism and reduce food consumption. Smoking directly increases one's metabolism because nicotine is a stimulant (Pinkowish, 1999). Cigarettes may reduce food consumption by suppressing one's appetite. Also, smoking dulls one's taste buds, making food less appealing, possibly causing people to eat less of it. Finally, "oral fixation" may cause people attempting to reduce or quit smoking to substitute food for cigarettes ("Weight gain…," 2006).

Alternatively, reducing smoking has the potential to result in weight loss through psychological phenomena that would lead to healthier eating and exercise habits. First, people who are exogenously induced to smoke less (or quit altogether) may experience a renewed sense of interest in and excitement about their health, leading them to next target other areas, such as eating and exercise. Next, someone who is able to overcome a smoking addiction may gain confidence in her ability to develop healthier habits, and someone who develops a smoking addiction may lose this confidence. A widespread belief among mental health professionals is that low self-esteem contributes to both the development of addictions and the inability to overcome them (Sweet, 2000). Two such addictions are cigarette smoking and compulsive overeating. If an overweight smoker is able to conquer her smoking addiction, she may begin to believe that she is in fact capable of losing weight, especially since making healthier eating and exercise decisions does not require overcoming the strong physical addiction that is present with cigarettes. Third, Ozdenoren et al. (2006) develop a model in which people have a depletable stock of willpower, and fighting against one temptation reduces the amount of willpower left to resist other temptations. Smokers therefore may use most of their stock of willpower resisting smoking, causing them to overeat or not exercise. Curbing one's nicotine addiction may replenish this stock of willpower, leading to healthier eating and exercise decisions. Fourth, smokers may feel that making healthy eating and exercise decisions is pointless if smoking is already dooming them to an early death. Next, people who reduce or quit smoking fear that they might gain weight, so they may develop healthier eating and exercise habits as a precautionary measure. Finally, smoking reduces lung capacity, making exercise more difficult (Hedenstrom et al., 1986). Smokers therefore experience more disutility from exercise than non-smokers and might exercise less. Some empirical evidence suggests that quitting smoking may lead to healthier choices in other areas: Picone and Sloan (2003) found that men who quit smoking began to consume less alcohol.

This discussion implies that smoking should increase metabolism, decrease exercise, and have an ambiguous effect on food consumption. Assuming that a rise in cigarette prices decreases smoking,

$$B=f(F(S(P)),E(S(P)),M(S(P))) \tag{2}$$

where $\mathrm{d}E/\mathrm{d}S < 0$, $\mathrm{d}M/\mathrm{d}S > 0$, $\mathrm{d}S/\mathrm{d}P < 0$, and the sign of $\mathrm{d}F/\mathrm{d}S$ is indeterminate. Therefore,

$$\frac{dB}{dP} = \frac{dB}{dF}\frac{dF}{dS}\frac{dS}{dP} + \frac{dB}{dE}\frac{dE}{dS}\frac{dS}{dP} + \frac{dB}{dM}\frac{dM}{dS}\frac{dS}{dP} \tag{3}$$

the sign of which is indeterminate. Because of this ambiguity, determining the relationship between cigarette prices and weight requires empirical analysis.

4. Data

I obtain data on the body weights and other characteristics of individuals from two data sets: the 1979 cohort of the National Longitudinal Survey of Youth (NLSY) and the 1984–2005 waves of the Behavioral Risk Factor Surveillance System (BRFSS). The NLSY is a panel, tracking 12,686 individuals from 1979–2004. These individuals consist of 6111 randomly chosen U.S. youths, a supplemental sample of 5295 minority and economically disadvantaged youths, and 1280 military youths. All respondents were between 14 and 22 years of age in 1979. The NLSY conducted subsequent interviews each year until 1994, then every two years until 2004. The respondents reported their weight in 1981, 1982, 1985, 1986, 1988, 1989, 1990, 1992, 1993, 1994, 1996, 1998, 2002, and 2004 and their height in 1981, 1982, and 1985. Assuming that heights did not change after 1985, this allows for the construction of BMI and an indicator variable for whether or not the person is obese in 1981, 1982, and 1985 and later. I use only the years starting in 1985 because each person was at least nineteen, meaning that my sample consists only of adults. The NLSY also includes a variety of demographic information, including age, race, gender, marital status, education, and income. Additionally, I utilize a variable from the 1992 survey, in which respondents were asked whether they have smoked at least 100 cigarettes in their life. Although the retention rate of the NLSY was high, not all youths were followed for the duration of the sample, making my data an unbalanced panel.[8] Eliminating all observations before 1985 as well as those with missing data, I am left with a sample size of 10,739 individuals and 110,800 observations.[9]

The BRFSS is a telephone survey of health conditions and risky behaviors conducted by state health departments and the Center for Disease Control. It consists of repeated annual cross sections from 1984 to 2005.

In 1984, only 15 states and 10,613 individuals participated, but the number of states steadily grew to 40 in 1989 and all 50 by 1996. The number of respondents also rapidly increased, reaching 286,941 in 2005. In all survey waves, the BRFSS includes questions on height and weight as well as demographic information. After eliminating observations with missing responses, my BRFSS sample consists of 2,385,565 observations. I also utilize BRFSS data on cigarette smoking, food consumption, and exercise as dependent variables in Section 6. In the 1984–2000 surveys, the BRFSS asked respondents both whether they currently smoke and the number of cigarettes they smoke per day. The 1990–1994 surveys contained self-reported estimates of the frequency with which the respondents consumed a wide variety of foods. The BRFSS used these estimates to calculate an approximation of the number of grams of fat the respondents consumed per day. The 1990–2003 surveys also contained the frequency with which the respondents consumed a narrower range of healthy foods. I sum the BRFSS variables for frequency of consuming fruit, green salad, carrots, and other vegetables to determine the number of times per week the respondent consumed fruit or vegetables. The 1984–2000 surveys asked the respondents a variety of questions regarding physical activity. Based on their answers, the BRFSS developed a binary variable for whether or not they obtained regular and sustained exercise.[10]

I match the NLSY and BRFSS data with state cigarette prices and excise tax rates from The Tax Burden on Tobacco (Orzechowski and Walker, 2005). The book reports average prices in November of each year and gives the dates for all tax changes, which I use to determine monthly tax rates. Cigarette prices are retail, and include both state and federal excise taxes. After 1989, The Tax Burden on Tobacco reported prices both including and excluding generic brands. Following CGS, I use the series excluding generics. Following GF, I match each individual to the tax rate in the month before her interview to allow for a short lag.

State-level cigarette prices are limited because they do not account for smuggling across state lines in order to pay lower taxes. Numerous studies have found evidence of smuggling by documenting a negative relationship between cigarette prices in border states and a state's taxed sales (for examples, see [Baltagi and Levin, 1986], [Baltagi and Levin, 1992], [Chaloupka and Saffer, 1992] and [Coates, 1995]; Keeler et al., 1996; Yurekli and Zhang, 2000). Consumers can also avoid taxes in other ways, such as purchasing cigarettes at an Indian reservation. Hyland et al. (2004) found that 67% of smokers in Erie and Niagra counties in New York, the residents of which all live within 30 miles of at least one Indian reservation, purchase their cigarettes at one. In recent years, purchasing cigarettes over the Internet has become another option. However, in 1999 only 0.3% of California smokers reported typically purchasing their cigarettes online even after a 50 cent per-pack increase in the state cigarette tax (Emery et al., 2002), suggesting that this was not common during my sample periods. Overall, though, the extent of tax avoidance is non-trivial, as Stehr (2005) finds that in 2001 home state taxes were not paid on 13% of cigarettes purchased. When micro-level data is used, neglecting to account for smuggling is likely to bias estimators of cigarette price elasticities toward zero (Lovenheim, 2008). I therefore expect that, if anything, using state-level cigarette prices and taxes will lead to conservative estimates of their effects on weight.

I include additional state-level variables as controls in some regressions. First, I use state unemployment rates from the Bureau of Labor Statistics. Second, I utilize the number of fitness and sports clubs per 10,000 residents from the Economic Census, conducted by the U.S. Census Bureau every five years. For this variable, I use data from the 1987, 1992, 1997, and 2002 censuses and estimate the values in the other years through linear interpolations and extrapolations. My fitness and sports club variable combines health clubs, golf courses, and other members-only sports clubs, since the three are indistinguishable in 1997 and 2002. This information comes from the Economic Census' Census of Service Industries in 1987 and 1992 and the Census of Arts, Entertainment, and Recreation in 1997 and 2002.[11] Finally, as a control for food prices I use the grocery price index from the ACCRA Cost of Living Index (ACCRA COLI), which is constructed based on the prices of 27 grocery items, 24 of which are foods or drinks. The ACCRA COLI prices are city-level, with the number of cities increasing from 230 in 1984 to 324 in 2004. Following Chou et al. (2004), I construct state-level prices by weighting each city by its population, as given by the U.S. Census Bureau.[12] While such an approach suffers from measurement error, these measures should be highly correlated with actual state food prices.

Table 1 reports the weighted summary statistics for both data sets, including descriptions of the variables used. The average BMI is 25.8 in the NLSY and 26.2 in the BRFSS, while the respective obesity rates are 17.5% and 19.1%. 51% of the NLSY sample had smoked at least 100 cigarettes by 1992, while 24% of the BRFSS sample currently smokes, with smokers smoking an average of 19 cigarettes per day. People consume an average of 36 grams of fat per day from the nineteen food and drink categories reported by the BRFSS. 43% of the BRFSS sample obtains regular and sustained exercise. The average cigarette price matched to the NLSY data is $2.57 (in 2002 dollars) and the average cigarette tax is $0.40, while in the BRFSS the mean price and tax are $3.17 and $0.51. These differences are largely the result of the fact that later years are more heavily sampled in the BRFSS than earlier years.

Table 1. Summary statistics.

Variable	**Description**	**Mean and standard deviation**	
		NLSY	**BRFSS**
BMI	Body mass index=weight in kilograms divided by height in meters squared	25.836 (5.264)	26.242 (5.340)
Obese	Binary variable that equals 1 if BMI≥30 kg/m^2 and 0 otherwise	0.175 (0.380)	0.191 (0.393)
Cigarette price	Real cigarette price (in 2002 dollars) in the state	2.571 (0.826)	3.170 (0.956)
Cigarette tax	Real state excise cigarette tax (in 2002 dollars) in the state	0.396 (0.272)	0.509 (0.405)
Income	Real household income (in 2002 dollars) in units of $10,000	3.495 (6.062)	4.395 (2.949)
Race: black	Binary variable that equals 1 if the respondent's race is black and 0 otherwise	0.140 (0.347)	0.090 (0.286)
Race: other	Binary variable that equals 1 if race is neither white nor black	0.027 (0.162)	0.093 (0.291)
Married	Binary variable that equals 1 if the respondent is married	0.573 (0.495)	0.549 (0.498)
Some high school	Binary variable that equals 1 if the respondent's highest grade completed is 9–11	0.087 (0.281)	0.077 (0.266)
High school graduate	Binary variable that equals 1 if highest grade completed is 12	0.433 (0.495)	0.320 (0.466)
Some college	Binary variable that equals 1 if highest grade completed is 13–15	0.229 (0.420)	0.274 (0.446)
College graduate	Binary variable that equals 1 if highest grade completed is at least 16	0.227 (0.419)	0.289 (0.453)
Age	Respondent's age	32.672 (6.387)	47.007 (17.103)
Female	Binary variable that equals 1 if the respondent is female	0.493 (0.500)	0.576 (0.494)
Smoked 100	Binary variable that equals 1 if the respondent has ever smoked at least 100 cigarettes	0.510 (0.500)	–
Smoker	Binary variable that equals 1 if the respondent currently smokes	–	0.244 (0.430) [a]
Cigarettes per day	Average number of cigarettes the respondent smokes per day (smokers only)	–	19.068 (11.217) [a]
Exercise	Respondent's level of exercise, ranging from 0 (lowest) to 3 (highest)	–	0.431 (0.495) [a]
Unemployment rate	Percent unemployment rate in the respondent's county of residence	5.888 (1.633)	5.172 (1.418)
Fitness and sports clubs	Number of fitness centers or sports clubs per 10,000 residents in the state	0.831 (0.163)	0.844 (0.159)
Grams of fat	Grams of fat the respondent consumes per day from the surveyed food types	–	36.364 (24.163) [b]
Fruits and vegetables	Number of times per week the respondent eats fruit or vegetables	–	20.117 (12.335) [c]
Food price	Food price index in respondent's state of residence	–	106.526 (10.811)

[a] Only available from 1984 to 2000.

[b] Only available from 1990 to 1994.

[c] Only available from 1990 to 2003.

5. Empirical analysis

In this section, I show that the different methodologies used in the literature point to the same conclusion if lags of cigarette price/tax are included. I conduct the main analysis using panel data from the NLSY, which allows me to track the response of individuals to price/tax changes over a number of years. I begin by replicating the approaches of CGS and GF, and then add lags to both models. (I do not replicate the approach of RCG since it is a hybrid of the CGS and GF approaches, although results are similar and available upon request.) I also conduct several robustness checks of both methodologies, one of which utilizes the treatment- and control-group approach of Baum (2009). Finally, I examine the sensitivity of the results to different lag selection and the use of BRFSS instead of NLSY data.

There are three reasons to suspect that differentiating between short- and long-run responses may provide insight into the effect of cigarette prices on body weight: cigarette smoking may lag price changes, changes in daily caloric consumption and expenditure patterns may lag changes in smoking, and changes in weight may lag changes in calories consumed or expended. Smoking may lag price changes since both models of myopic and rational addiction predict that the long-run price elasticity of addictive goods is stronger than the short-run elasticity, as people may need a substantial amount of time to successfully quit addictive habits. In the case of cigarettes, Becker et al. (1994) find that the long-run price elasticity is about double the short-run elasticity. Steady-state daily levels of calories consumed and burned may lag smoking because, if people who quit smoking next target other health-related goals such as weight loss, some time may pass before smoking is no longer a threat and they are able to devote their energy to these other goals. Also, increased calorie consumption from oral fixation is likely to be temporary, as it would persist only as long as the cravings for cigarettes last. Additionally, research suggests that the resting metabolic rates of long-term smokers and non-smokers are similar, suggesting that smoking's metabolic-enhancing properties may be temporary (Perkins, 1992). Body weight may lag calorie consumption and expenditure patterns since body weight is a capital stock that depends on calories consumed and burned in all prior periods. Consequently, even if calories consumed and burned per day respond immediately to economic shocks, the effect of these shocks on weight will be gradual. For example, if food prices fall, a person's calorie consumption may rise. She will then begin to gain weight, and this gain will slowly increase (as long as the new eating habits continue) until a new steady-state weight is reached, possibly several years after the price change.

For these reasons, simply examining contemporaneous prices/taxes may not fully capture the total effect. Moreover, it is not clear that neglecting to include lags would impact estimates using the CGS and GF methodologies the same way. The Tax Burden on Tobacco reports the states' average prices in November of each year. Therefore, if only contemporaneous prices are used, most survey respondents were interviewed before prices were measured. In contrast, GF used state tax rates from the end of the month preceding the respondent's interview month. Additionally, tax increases may not be fully passed on to consumers immediately. Timing discrepancies may be particularly important in fixed effects models, such as those used in the literature, because the estimates represent correlations between changes in the regressors and changes in the dependent variable. Also, the inclusion of lags may affect results using the two approaches differently since CGS modeled the effect of time on weight as continuous while GF allowed for a discrete effect.

5.1. Replication

I begin by replicating the approaches of CGS and GF using the NLSY to ensure that the sensitivity to specification of their results is not unique to the BRFSS data that they used. Both CGS and GF use descriptive models to estimate the relationship between cigarette prices/taxes and two different measures of body weight: BMI and whether or not a person is obese. Their specifications are different only in the ways discussed in Section 2. I begin by estimating the cigarette price effect using an approximation of the CGS methodology. I do not include the additional state-level prices, number of restaurants, and clean indoor air laws CGS included as regressors, so this is not an exact replication. However, GF demonstrated that their results were similar both with and without these variables, so I do not expect that omitting them will prevent me from obtaining similar results to CGS. Other covariates are almost exactly the same as those used by CGS.[13] As discussed in the literature review, CGS estimated models both with and without a quadratic time trend. I replicate their model with the quadratic trend. Mimicking CGS, I use a quadratic functional form for cigarette price. My regression equation for the CGS replication is

$$BMI_{ist} = \beta_0 + \beta_1 PRICE_{st} + \beta_2 PRICE^2_{st} + \beta_3 X_{ist} + \beta_4 t + \beta_5 t^2 + \mu_s + \varepsilon_{ist} \quad (4)$$

where *i*, *s*, and *t* are the indexes for individual, state, and time (measured in months), *BMI* is body mass index, *PRICE* is real cigarette price (measured in November of each year), *t* is a measure of time elapsed since the beginning of the BRFSS data set (it equals zero in the first month of 1984, 12 in the first month of 1985, etc.), *X* is a set of individual-specific attributes chosen to reflect those included by CGS, and μ_s is a state fixed effect. I estimate the effect of these covariates on the probability an individual is obese using a linear probability model:

$$OBESE_{ist} = \beta_0 + \beta_1 PRICE_{st} + \beta_2 PRICE^2_{st} + \beta_3 X_{ist} + \beta_4 t + \beta_5 t^2 + \mu_s + \varepsilon_{ist} \quad (5)$$

where *OBESE* is an indicator variable equal to 1 if the individual is obese and 0 otherwise.[14]

I replicate the approach of GF identically, except for the minor differences in marital status and race discussed in footnote 12. My GF regression equations are:

$$BMI_{\mathrm{ist}} = \alpha_0 + \alpha_1 TAX_{st} + \alpha_2 Z_{ist} + \alpha_3 U_{st} + \tau_t + \sigma_s + \varepsilon_{ist} \quad (6)$$

(7)
OBESEist=α0+α1TAXst+α2Zist+α3Ust+τt+σs+ϵist

where *TAX* is real cigarette tax, *U* is the state unemployment rate, σ and τ are fixed effects for state and month–year combination, and *Z* differs from *X* according to the aforementioned differences between the CGS and GF specifications. Table 2 contains a summary of the differences between the CGS and GF methodologies that I use throughout the paper. In all regressions, standard errors are heteroskedasticity-robust and clustered by state.[15]

Table 2. Summary of CGS and GF methodologies.

	CGS methodology	**GF methodology**
Variable of interest	Quadratic price	Linear tax
Time	Quadratic time trend	Month–year dummy variables
Age and gender	Quadratic age and dummy for female	Categories for gender–age combinations
Income	Quadratic	Linear
Sampling weights	No	Yes
Restrictions	None	Drop individuals 65 and older

Table 3 reports the estimates of the coefficients of interest from CGS's and GF's papers (adjusted to reflect 2002 dollars), alongside the results from my replications.[16] Despite the different data sets, the results are quite similar. The CGS methodology produces positive coefficients on price and negative coefficients on price squared, while the GF methodology produces a negative coefficient on tax. For the CGS results, I calculate marginal effects at the sample mean in an effort to assess the magnitude of the effect.[17] Note that, while the coefficients on price estimated in CGS' paper appear large, the negative coefficient on price squared makes the marginal effects more modest—0.8 units BMI and 0.8 percentage points P(Obese). Moreover, their coefficients actually imply slightly negative marginal effects at the average 2005 price level of $4.10.

Table 3. Effect of contemporaneous cigarette prices and taxes on BMI and P(Obese).

	BMI				**P(Obese)**			
	CGS		**GF**		**CGS**		**GF**	
	(1)	**(2)**	**(1)**	**(2)**	**(1)**	**(2)**	**(1)**	**(2)**
Price/tax	0.411 (0.157)***	0.575 (0.122)***	−0.151 (0.073)**	−0.189 (0.090)**	0.032 (0.012)***	0.030 (0.016)*	−0.015 (0.006)**	−0.005 (0.010)
Price/tax squared	−0.071 (0.028)***	−0.073 (0.016)***	–	–	−0.005 (0.002)***	−0.003 (0.002)	–	–
Marginal effect at mean	0.084	0.198 (0.050)***	−0.151 (0.073)**	−0.189 (0.090)**	0.008	0.012 (0.005)**	−0.015 (0.006)**	−0.005 (0.010)
Number of observations	1111074	110800	1381248	110800	1111074	110800	1381248	110800
R^2	0.082	0.110	Not given	0.112	0.042	0.057	Not given	0.059

Notes: (1) indicates results from their paper; (2) replications.*** indicates statistically significant at the 1% level;** 5% level;* 10% level. Standard errors in parentheses. All standard errors are heteroskedasticity-robust and clustered by state.

5.2. Lags of cigarette price/tax

I next add lags to the above models to see if the CGS and GF methodologies produce results that are more similar. I model BMI and obesity status as a function of the independent variables (except for age and time) in the current year as well as the five preceding years. I divide the six years into three two-year periods, and include for each variable its average value over the current and preceding year, the average of the second and third lags, and the average of the fourth and fifth lags. The sum of the coefficients on the three price/tax variables represents the "total effect" of a change in price/tax on weight. The total effects estimated in this section are very similar if I include separate lags for each year, but the estimates are less precise. I account for five years of lags because estimates of the total price/tax effect do not change substantially if more lags are included, as I show in Section 5.5. Also, the fact that the NLSY data contain the independent variables starting in 1979 and the dependent variables starting in 1985 means that my inclusion of five years of lags for the independent variables does not eliminate any waves from my sample. In order to easily interpret the marginal effects, I use a linear functional form in all regressions. Results are similar including the square of each price/tax variable. Also, because of the panel nature of the NLSY, I replace the state fixed effects with individual fixed effects. Results are robust to the use of state effects or both state and individual effects.[18] My regression equations are

$$BMI_{ist}/OBESE_{ist} = \beta_0 + \sum_{j=0}^{2} \beta_{1j}\, \overline{PRICE}_{s,t-2j} + \sum_{j=0}^{2} \beta_{2j}\, \overline{X}_{is,t-2j} + \beta_3 t + \beta_4 t^2 x_i + \varepsilon_{ist} \quad (8)$$

$$BMI_{ist}/OBESE_{ist} = \alpha_0 + \sum_{j=0}^{2} \alpha_{1j}\, \overline{TAX}_{s,t-2j} + \sum_{j=0}^{2} \alpha_{2j}\, \overline{Z}_{is,t-2j} + \sum_{j=0}^{2} \alpha_{3j}\, U_{s,t-2j} + \tau_t + \psi_i + \varepsilon_{ist} \quad (9)$$

where $\overline{PRICE}_{st}$ indicates the average price over years t and $t-1$, $\overline{PRICE}_{s,t-2}$ is the average price over years $t-2$ and $t-3$, $\overline{PRICE}_{s,t-4}$ is the average price over years $t-4$ and $t-5$, and the averages for the control variables are similarly defined.[19] By including the individual fixed effects χ and ψ, the time-invariant gender and race variables are dropped from the sets of controls. I include lags of the control variables because failing to do so could bias the coefficient estimators for the cigarette price lags if lagged cigarette prices are correlated with lagged controls (Ruhm, 2004). Including lags reduces the sample size to 99,877 due to observations with missing values for one or more lags.

The fixed effects estimators are consistent under the assumption that changes over time in unobservable characteristics are uncorrelated with price/tax. This assumption would not be valid if, for example, changes in a state's level of health consciousness influence changes in prices, taxes, and weight. If a state becomes less healthy, demand for cigarettes is likely to increase, which may drive up cigarette prices. However, cigarette taxes would become less politically popular, causing real cigarette taxes to fall. Simultaneously, the prevalence of obesity would increase, even in the absence of a causal effect from prices or taxes. In this case, a spurious positive relationship would exist between prices and weight and a spurious negative relationship would exist between taxes and weight. Similarly, if changes over time in state anti-smoking and anti-obesity policies are correlated, this could also lead to the estimation of a spurious negative relationship between taxes and weight.[20]

Because of these identification concerns, I conduct several robustness checks. First, I estimate these models with linear state-specific time trends. If slow-moving trends in unobservable state characteristics – such as a state's level of health consciousness – affect both prices and weight, including linear state trends will affect my results. However, state trends may not affect my results if changes over time in the sources of endogeneity are sufficiently non-linear. As a second robustness check, I therefore estimate models including both linear state trends and the state's number of fitness and sports clubs, a proxy variable for the state's attitude toward health.[21] Third, I estimate long difference instead of fixed effects regressions, differencing between the current year and six years ago.[22] As discussed by Wooldridge (2002, p. 284), if the explanatory variables are correlated with the error term, then fixed effects and differences estimates have different probability limits. Therefore, if my fixed effects and long differences estimates differ in ways that cannot be attributed to sampling error, it is likely that the estimators are biased.[23]

Table 4 displays the results for regressions using BMI as the dependent variable, while Table 5 displays the results using obesity status. The baseline results are in column (1), while column (2) reports the results including state trends, column (3) reports the results including state trends and fitness/sports clubs, and column (4) reports the long differences results. The total price/tax effects are negative in all sixteen specifications. The magnitudes estimated using the CGS and GF methodologies are not exactly comparable, as cigarette prices may rise by more than $1 when taxes rise by $1 (Sumner, 1981). Using the CGS methodology, a permanent $1 increase in prices is associated with a reduction in BMI of between 0.26 and 0.37 units, and a reduction in P(Obese) of between 1.4 and 2.5 percentage points. Using the GF methodology, a permanent $1 increase in taxes reduces BMI by 0.33 to 0.49 units and P(Obese) by 1.1 to 2.0 percentage points. The total effects are statistically significant at the 5% level in all eight BMI regressions and the 1% level in five. The estimates using obesity status are less precise, likely because the creation of the dependent variable involves converting a continuous variable to discrete at a fairly arbitrary point, and because less than 25% of individuals in the panel change obesity status at least once. Nonetheless, all the CGS estimates are statistically significant at the 10% level or better. The magnitudes of the CGS estimates with state trends and fitness/sports clubs are very similar to those without state trends. Adding state trends and fitness/sports clubs reduces the magnitude of the GF estimates somewhat, but the differences are far from being statistically significant. The long differences estimates are slightly smaller than the fixed estimates, but again the differences are insignificant. I am therefore unable to conclude that the baseline fixed effects estimators – using either the CGS or GF approach – are biased.

Table 4. 6-Year effect of cigarette prices and taxes on BMI.

	CGS				GF			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Price/tax in current year and t-1	−0.100 (0.050)*	−0.070 (0.049)	−0.077 (0.053)	−0.001 (0.042)	−0.139 (0.094)	0.002 (0.094)	−0.027 (0.098)	−0.005 (0.104)
Price/tax in years t-2 and t-3	−0.012 (0.051)	−0.026 (0.049)	−0.019 (0.048)	−0.070 (0.043)	0.149 (0.114)	0.123 (0.103)	0.114 (0.099)	0.088 (0.101)
Price/tax in years t-4 and t-5	−0.253 (0.074)***	−0.246 (0.068)***	−0.257 (0.070)***	−0.184 (0.056)***	−0.498 (0.143)***	−0.493 (0.128)***	−0.527 (0.133)***	−0.411 (0.154)***
Individual fixed effects	YES	YES	YES	NO	YES	YES	YES	NO
State-specific time trends	NO	YES	YES	NO	NO	YES	YES	NO
Fitness and sports clubs	NO	NO	YES	NO	NO	NO	YES	NO
Long differences	NO	NO	NO	YES	NO	NO	NO	YES
Number of observations	99877	99877	99877	43948	99877	99877	99877	43948
R^2	0.864	0.865	0.865	0.021	0.865	0.866	0.866	0.025
Total price/tax effect	−0.365 (0.081)***	−0.342 (0.073)***	−0.352 (0.080)***	−0.255 (0.061)***	−0.489 (0.149)***	−0.368 (0.173)**	−0.440 (0.176)**	−0.329 (0.134)**

Notes: *** indicates statistically significant at the 1% level; ** 5% level; * 10% level. Standard errors in parentheses. All standard errors are heteroskedasticity-robust and clustered by state.

Table 5. 6-Year effect of cigarette prices and taxes on P(Obese).

	CGS				GF			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Price/tax in current year and *t*-1	−0.004 (0.006)	−0.006 (0.006)	−0.007 (0.006)	0.005 (0.006)	−0.003 (0.012)	0.010 (0.012)	0.009 (0.013)	0.007 (0.014)
Price/tax in years *t*-2 and *t*-3	0.002 (0.007)	0.004 (0.007)	0.009 (0.007)	−0.005 (0.006)	0.023 (0.013)*	0.023 (0.012)*	0.023 (0.012)*	0.028 (0.020)
Price/tax in years *t*-4 and *t*-5	−0.019 (0.009)**	−0.023 (0.008)***	−0.025 (0.008)***	−0.014 (0.007)**	−0.040 (0.013)***	−0.044 (0.014)***	−0.047 (0.014)***	−0.053 (0.015)***
Individual fixed effects	YES	YES	YES	NO	YES	YES	YES	NO
State-specific time trends	NO	YES	YES	NO	NO	YES	YES	NO
Fitness and sports clubs	NO	NO	YES	NO	NO	NO	YES	NO
Long differences	NO	NO	NO	YES	NO	NO	NO	YES
Number of observations	99877	99877	99877	43948	99877	99877	99877	43948
R^2	0.670	0.671	0.671	0.004	0.671	0.672	0.672	0.008
Total price/tax effect	−0.021 (0.010)*	−0.025 (0.009)***	−0.023 (0.009)**	−0.014 (0.007)*	−0.020 (0.016)	−0.011 (0.020)	−0.015 (0.021)	−0.018 (0.019)

See notes for Table 4.

Examining the coefficients on the lags reveals that the price/tax effect is largely delayed, explaining the discrepancy between these results and those using only contemporaneous prices/taxes. The coefficients on average price/tax in the current and preceding year are only significant at the 10% level in one of the sixteen regressions, so I cannot conclude that cigarette prices or taxes have any immediate effect on weight. However, the effect of average prices/taxes in the fourth and fifth years is negative, large, and significant in all regressions. Most of the reduction in body weight that results from a rise in cigarette prices therefore does not occur until several years after the price change.

Regarding the methodological debate in the literature, these results suggest that both the approaches of CGS and GF have strengths and weaknesses. The coefficient estimates using the CGS methodology are precise and very similar across specifications, so there is no evidence of bias from unobservable state characteristics. While I am also unable to conclude that the coefficient estimators using the GF methodology are biased, the estimates are much less precise. This supports the argument made by CGS (2006) that models using price and a quadratic trend may be more appropriate for this topic. On the other hand, estimates using the GF methodology appear less sensitive to the inclusion of lags. This may be because prices are measured toward the end of each year, or because the CGS methodology forces the effect of time on weight to be continuous.

5.3. Multicollinearity

Multicollinearity is a potential concern in these models, as the three price/tax variables are highly correlated with each other. The correlation between $\overline{PRICE}_{st}$ and $\overline{PRICE}_{s.t-2}$ is 0.93, the correlation between $\overline{PRICE}_{st}$ and $\overline{PRICE}_{s,t-4}$ is 0.84, and the correlation between $\overline{PRICE}_{s.t-2}$ and $\overline{PRICE}_{s.t-4}$ is 0.92. There is somewhat more variation in tax rates between states over time, as the correlations between $\overline{TAX}_{st}$ and $\overline{TAX}_{s,t-2}$, $\overline{TAX}_{st}$ and $\overline{TAX}_{s,t-4}$, and $\overline{TAX}_{s,t-2}$ and $\overline{TAX}_{s,t-4}$ are 0.89, 0.78, and 0.88, respectively. I assess the extent of the multicollinearity by computing variance inflation factors (VIFs) for each of the price and tax variables in each of the regressions in Section 5.2. The VIF for variable j is defined as $1/(1-R_j^2$, where R_j^2 is the R^2 from the regression of j on the other independent variables. Multicollinearity is generally considered to be severe if $VIF_j > 10$, in which case the other independent variables explain more that 90% of the variation in j (Wooldridge, 2006, p. 99).

In the regressions in Section 5.2, 9 of the 24 VIFs of the price and tax variables are above 10, while 15 are below 10.[24] In the baseline CGS model, the VIFs for the three price variables range from 12.1 to 17.4 (91.8–94.3% of variation explained by the other independent variables). In the CGS models that add state trends, the VIFs rise to between 14.6 to 19.4. After also adding fitness centers, the VIFs are between 14.8 and 22.0. However, multicollinearity is a much less serious problem in the CGS long differences model, as the VIFs are between 2.0 and 3.8. In the GF models, the multicollinearity is also less severe, as the VIFs for the tax variables are below 10 in all regressions. In the baseline model, the VIFs for the tax variables range from 6.1 to 9.5. Even after adding both state trends and fitness centers, this range is 6.9–9.9. In the long differences regressions, the VIFs fall to between 1.9 and 3.2.

In all, then, multicollinearity appears to be a concern in some but not all of the regressions. Multicollinearity does not bias estimators, but does inflate their standard errors (Wooldridge, 2006, pp. 96–97). It is therefore noteworthy that $\overline{PRICE}_{s,t-4}$ is statistically significant even in the models in which the multicollinearity is most severe. This indicates either that the effect is especially strong, or that the improved precision from the large sample offsets the reduced precision from the multicollinearity.

However, a remaining issue is whether the small portion of the total variation in prices and taxes upon which identification is based is unique, in which case drawing general inference based on the results would be inappropriate. Although I am unable to answer this question definitively, the fact that the results are similar in both the models in which the multicollinearity is severe and the models in which it is not suggests that this is unlikely to be a serious concern. Nonetheless, I also estimate long-run effects using two additional models that include only one price/tax variable, which eliminates the concern about multicollinearity from having three price/tax variables. First, I drop $\overline{PRICE}_{st}$ and $\overline{PRICE}_{s,t-2}$ ($\overline{TAX}_{st}$ and $\overline{TAX}_{s,t-2}$), leaving $\overline{PRICE}_{s,t-4}$ ($\overline{TAX}_{s,t-4}$) as the only independent variable of interest. Second, I use average prices/taxes over all six years (*t* to *t*−5) as the only independent variable of interest.

I report the results in Table 6. The coefficient estimates for average price/tax in years *t*−4 and *t*−5 are negative in all four specifications and significant in three, and the magnitudes are generally similar to those reported in Table 4 and Table 5. All coefficient estimates for average price/tax in years *t* to *t*−5 are also negative, and two of the four are significant at the 1% level with a third significant at the 10% level. The magnitudes are similar to the total effects estimates in Table 4 and Table 5. The multicollinearity is indeed less severe, as the VIFs are 6.3 and 10.1 in the CGS regressions and 3.0 and 3.7 in the GF regressions. These are similar to the VIFs in regressions that include only contemporaneous prices or taxes.

Table 6. Multicollinearity robustness checks.

	BMI				Obese			
	CGS		GF		CGS		GF	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Price/tax in years *t*-4 and *t*-5	−0.252 (0.055)***	–	−0.443 (0.132)***	–	−0.016 (0.008)**	–	−0.020 (0.014)	–
Price/tax in years t to *t*-5	–	−0.299 (0.061)***	–	−0.398 (0.138)***	–	−0.015 (0.009)*	–	−0.010 (0.016)
Individual fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Number of observations	104375	99877	104242	99877	104375	99877	104242	99877
R^2	0.862	0.863	0.863	0.864	0.666	0.670	0.666	0.671

See notes for Table 4.

To summarize, there is considerable variation in VIFs among the different specifications in Sections Sections 5.2 and 5.3, and the results do not changes systematically with the extent of the multicollinearity. It therefore does not appear likely that a very small amount of unique variation is driving the results.

5.4. Treatment and control groups

I next reproduce a third methodology used in the literature: Baum's (2009) treatment- and control-group approach. He estimates a difference-in-difference model, classifying individuals as "treated" if and only if they answered yes to a 1992 survey question asking if they had smoked at least 100 cigarettes in their life. This approach is intuitively appealing but suffers from potential limitations. Assuming that cigarette prices only affect weight via one's own smoking behavior, the ideal treatment group would be people whose smoking behavior could potentially be affected by changes in cigarette prices. People who never reached 100 cigarettes only because prices in their state have always been high are incorrectly assigned to the control group, while people who used to smoke but quit and would not start again even if cigarettes were free are incorrectly assigned to the treatment group. Further, even the assumption that cigarette prices only affect weight via one's own smoking behavior may not be valid. People who would never smoke may still be impacted through "spillover" effects: if they have friends or family who quit smoking and change their eating and exercise habits, they themselves may change their eating and exercise habits. Christakis and Fowler (2007) found that people's dietary and exercise decisions appear to be influenced heavily by those of people with whom they associate, suggesting that such a spillover effect may exist.

Despite these possible weaknesses, reproducing Baum's (2009) treatment- and control-group approach serves as a useful robustness check. I do this by estimating (8) and (9), adding interactions of each of the three price/tax variables with treatment status.[25] I report the results in Table 7. The first six rows contain the coefficient estimates for each of the price/tax variables and the interaction terms. The "total effect on the untreated" is the sum of the three coefficients on price/tax, while the "total difference between the effects on the treated and untreated" is the sum of the three coefficients on the interaction terms. The "total effect on the treated" is the sum of the coefficients on price/tax plus the sum of the coefficients on the interaction terms. The "average effect for the entire sample" is the total effect on the treated multiplied by the proportion of the sample who are treated (0.51), plus the total effect on the untreated multiplied by the proportion who are untreated. In the final row, I compute the "average effect for the entire sample, assuming no effect on the untreated." I do this by assuming that the true causal effect on the control group is zero, meaning that the actual causal effect of cigarette prices on weight is captured by the difference between the effects on the treatment and control groups. The average effect assuming no effect on the untreated is therefore this difference multiplied by the proportion of the sample who are treated.

Table 7. Treatment and control groups.

	BMI		**Obese**	
	CGS	**GF**	**CGS**	**GF**
Price/tax in current year and t-1	−0.108 (0.065)	−0.048 (0.138)	−0.003 (0.007)	0.012 (0.016)
Price/tax in years t-2 and t-3	−0.037 (0.061)	0.151 (0.130)	0.003 (0.009)	0.026 (0.016)
Price/tax in years t-4 and t-5	−0.102 (0.087)	−0.471 (0.193)**	−0.007 (0.011)	−0.038 (0.025)
Price/tax in current year and t-1*treated	0.005 (0.075)	−0.149 (0.253)	−0.003 (0.008)	−0.029 (0.024)
Price/tax in years t-2 and t-3*treated	0.062 (0.074)	−0.003 (0.156)	−0.001 (0.011)	−0.010 (0.024)
Price/tax in years t-4 and t-5*treated	−0.329 (0.082)	−0.174 (0.269)	−0.022 (0.012)*	−0.008 (0.037)
Individual fixed effects	YES	YES	YES	YES
Number of observations	87619	87619	87619	87619
R^2	0.862	0.863	0.666	0.667
Total effect on the untreated	−0.246 (0.093)**	−0.368 (0.278)	−0.007 (0.011)	−0.0002 (0.026)
Total difference between the effects on the treated and untreated	−0.262 (0.065)***	−0.326 (0.278)	−0.026 (0.008)***	−0.042 (0.042)
Total effect on the treated	−0.508 (0.087)***	−0.694 (0.162)***	−0.032 (0.011)***	−0.042 (0.027)
Average effect for the entire sample	−0.380 (0.084)***	−0.534 (0.147)***	−0.020 (0.010)*	−0.022 (0.016)
Average effect for the entire sample, assuming no effect on the untreated	−0.134 (0.033)***	−0.166 (0.142)	−0.013 (0.004)***	−0.021 (0.021)

See notes for Table 4.

In all four specifications, the effect on the control group is negative but the effect on the treatment group is considerably more negative. The difference between the effects on the treatment and control groups is negative and substantial in all four regressions and statistically significant in both CGS regressions. The total effect on the treated is negative and large in all four regressions and significant at the 1% level in three. The average effect for the entire sample is similar to the total effects estimated in Table 4, Table 5 and Table 6. Assuming that the true causal effect on the untreated is zero, the average effect for the entire sample is more modest but still economically meaningful: a $1 increase in cigarette prices leads to long-run decreases in BMI and P(Obese) of 0.134 units and 1.3 percentage points, while a $1 increase in cigarette taxes leads to long-run decreases in

BMI and P(Obese) of 0.166 units and 2.1 percentage points. These estimated effects on P(Obese) imply 4% and 6.5% reductions in obesity, relative to the 2004 obesity rate of 32.2%. Using the costs of obesity discussed in the introduction, these reductions imply savings of 4,480 and 7,280 lives and $4.68 and $7.61 billion per year.

There are four possible explanations for the negative effect of cigarette prices/taxes on people who have never smoked 100 cigarettes. First, it could be simply the result of sampling error, as three of the four estimated total effects on the untreated are insignificant and the fourth is only significant at the 5% level. In particular, the effects on the untreated are small and insignificant in both obesity regressions. Second, it could be driven by the people falsely assigned to the control group who never began smoking only because prices were high. Third, it could be due to spillover effects on the eating and exercise habits of people who would never smoke. Fourth, it could reflect unobserved heterogeneity. If the second or third explanations are true, then the effect on the untreated is still causal, and the overall causal effect of cigarette prices/taxes on the weight of the entire population is best represented by the "average effect for the entire sample" row of Table 7. If, however, the fourth explanation is true, then the effect on the untreated is not causal, and the overall causal effect is given by the "average effect for entire sample, assuming no effect on untreated" row. Either way, the causal effect is negative.

Combining Table 4, Table 5, Table 6 and Table 7, all available evidence suggests that the long-run causal effect of cigarette prices on weight is negative and economically meaningful, although future research is necessary to determine the exact magnitude.

5.5. Sensitivity analysis

I next relax the assumption that the entire effect occurs within six years and estimate the baseline individual fixed effects models in (8) and (9) including one additional two-year lag, two additional two-year lags, and three additional two-year lags of the independent variables. These approaches allow the effects to occur over eight years, 10 years, and 12 years, respectively. Adding one additional two-year lag eliminates the year 1985 from the sample, reducing the sample size to 87,774, while adding two additional two-year lags eliminates 1986 as well, reducing the sample size to 76,656, and adding three additional two-year lags eliminates 1988 and 1989, reducing the sample size to 58,856. I present the results in Table 8 and Table 9. The total effects are less precisely estimated, both because of adding the extra variables and reducing the sample size, but the magnitudes are statistically indistinguishable from the 6-year effects.

Table 8. 8-, 10-, and 12-Year effects of cigarette prices and taxes on BMI.

	CGS			GF		
	8-Year	10-Year	12-Year	8-Year	10-Year	12-Year
Price/tax in current year and t-1	−0.102 (0.051)*	−0.076 (0.054)	−0.049 (0.061)	−0.057 (0.113)	−0.061 (0.101)	−0.034 (0.093)
Price/tax in years t-2 and t-3	−0.033 (0.055)	−0.061 (0.055)	−0.076 (0.063)	0.058 (0.105)	−0.003 (0.091)	0.026 (0.100)
Price/tax in years t-4 and t-5	−0.252 (0.068)***	−0.147 (0.057)**	−0.163 (0.076)**	−0.287 (0.138)**	−0.204 (0.132)	−0.187 (0.148)
Price/tax in years t-6 and t-7	0.030 (0.068)	−0.175 (0.072)**	−0.134 (0.068)*	−0.306 (0.196)	−0.529 (0.204)**	−0.452 (0.276)
Price/tax in years t-8 and t-9	–	0.178 (0.094)*	0.150 (0.119)	–	0.395 (0.176)**	0.517 (0.184)***
Price/tax in years t-10 and t-11	–	–	−0.018 (0.100)	–	–	−0.404 (0.262)
Number of observations	88266	77844	58856	87774	76656	58602
R^2	0.872	0.879	0.892	0.873	0.881	0.893
Total price/tax effect	−0.356 (0.120)***	−0.281 (0.146)*	−0.289 (0.215)	−0.593 (0.175)***	−0.402 (0.221)*	−0.534 (0.332)

See notes for Table 4.

Table 9. 8-, 10-, and 12-Year effects of cigarette prices and taxes on P(Obese).

	CGS			GF		
	8-Year	10-Year	12-Year	8-Year	10-Year	12-Year
Price/tax in current year and t-1	−0.002 (0.006)	−0.001 (0.006)	0.003 (0.007)	0.011 (0.013)	0.014 (0.013)	0.023 (0.015)
Price/tax in years t-2 and t-3	0.003 (0.007)	0.001 (0.007)	0.004 (0.007)	0.012 (0.016)	0.007 (0.017)	0.006 (0.020)
Price/tax in years t-4 and t-5	−0.020 (0.008)**	−0.013 (0.008)*	−0.015 (0.010)	−0.010 (0.017)	−0.014 (0.019)	−0.014 (0.021)
Price/tax in years t-6 and t-7	0.008 (0.009)	−0.006 (0.010)	−0.003 (0.011)	−0.047 (0.017)***	−0.047 (0.015)***	−0.041 (0.020)**
Price/tax in years t-8 and t-9	–	0.010 (0.011)	0.006 (0.012)	–	0.004 (0.024)	0.008 (0.023)
Price/tax in years t-10 and t-11	–	–	−0.003 (0.014)	–	–	−0.020 (0.029)
Number of observations	88266	77844	58856	87774	76656	58602
R^2	0.688	0.704	0.736	0.689	0.706	0.738
Total price/tax effect	−0.012 (0.015)	−0.009 (0.019)	−0.007 (0.029)	−0.035 (0.016)**	−0.035 (0.021)	−0.036 (0.027)

See notes for Table 4.

The results regarding timing are mixed. In the CGS 8-year regressions, the coefficient on the additional price variable is insignificant and close to zero. In the GF regressions, however, the effect of tax in years t−4 and t−5 estimated in the preceding subsection appears to "spread out" over all years between t−4 and t−7. In the CGS 10-year regression for BMI, the effect of prices in years t−6 and t−7 is negative but offset by a positive effect in years t−8 and t−9, while in the regression for obese both new price variables are highly insignificant. In the GF 10-year regression for BMI, a negative estimate of the coefficient on tax in t−6 and t−7 is partially offset by a positive coefficient estimate on tax in t−8 and t−9, while in the obese regression the coefficient on t−8 and t−9 is essentially zero. In both CGS 12-year regressions, the coefficient on price in t−10 and t−11 is practically zero, while in the GF 12-year regressions the coefficient on tax in t−10 and t−11 is negative but the total effects remain similar to those estimated using shorter lag lengths.

In short, the total effects estimated previously appear robust to the use of longer lags. However, the mixed results in Table 4, Table 5, Table 6, Table 7, Table 8 and Table 9 regarding timing prevent a more specific conclusion than that the effect appears substantially delayed.[26]

5.6. Robustness check: BRFSS data

I next attempt to ensure that the discrepancy between the results using contemporaneous and lagged prices/taxes is not due simply to the use of different data sets by estimating variations of (8) and (9) using the BRFSS data. While the increased sample size of the BRFSS is appealing, the repeated cross-sectional nature of the data means that I am unable to estimate individual-level models including lags of the control variables. I therefore model BMI and obesity status as a function of current and lagged prices/taxes but only contemporaneous values of the other independent variables. Such an approach poses two problems. First, since I match the individual characteristics to prior prices/taxes in the states where individuals currently live, there is measurement error for people who have recently moved. However, this should bias my estimators toward zero. Second, including lags of state cigarette price/tax without also including lags of the controls may result in biased estimators if the lagged values of the controls are correlated with the lagged values of cigarette price/tax (Ruhm, 2004).

To address this second problem, I also aggregate the data to the state level and model a state's average BMI and obesity rate as a function of its values of the independent variables over the current and preceding five years. Recall that GF, but not CGS, used sampling weights. Also, CGS included all ages while GF excluded individuals age 65 and older. In order to make the values of the control variables the same in both regressions, when aggregating the variables I elect not to use sampling weights and to drop individuals age 65 and older. In the regressions, I weight each observation by the number of respondents in the survey from that particular state and year. Results are robust to other methods of weighting and aggregating. Including five lags eliminates all observations prior to the sixth survey in which a state participated. For the 15 states that participated in all surveys, this means that they are dropped prior to 1989. States that began participating after 1984 are dropped for even more waves. Ultimately, my sample in the state-level regressions consists of 706 observations.

Columns (1) and (2) of Table 10 report the results from the individual- and state-level BRFSS regressions, respectively. The total effects are negative and significant at the 1% level in all regressions, and the magnitudes are comparable to those estimated using the NLSY data. While the delay before impact is less pronounced in these regressions, in all of them the majority of the effect occurs two years or more after the price/tax change. The discrepancy between the results using contemporaneous and lagged prices/taxes therefore does not appear to be due to differences between the BRFSS and NLSY data.

Table 10. BRFSS regressions with lagged independent variables.

	BMI				**P(Obese)**			
	CGS		**GF**		**CGS**		**GF**	
	(1)	**(2)**	**(1)**	**(2)**	**(1)**	**(2)**	**(1)**	**(2)**
Price/tax in current year and t-1	−0.028(0.031)	−0.034 (0.044)	−0.174 (0.068)***	−0.084 (0.064)	−0.001 (0.002)	−0.002 (0.003)	−0.010 (0.005)**	−0.005 (0.005)
Price/tax in years t-2 and t-3	−0.096 (0.032)***	−0.109 (0.047)**	−0.043 (0.048)	−0.062 (0.075)	−0.007 (0.002)	−0.008 (0.003)**	−0.007 (0.004)*	−0.007 (0.006)
Price/tax in years t-4 and t-5	−0.122 (0.031)***	−0.092 (0.043)**	−0.175 (0.090)*	−0.200 (0.097)**	−0.007 (0.003)***	−0.007 (0.004)*	−0.013 (0.008)*	−0.016 (0.007)**
Number of observations	2385565	706	1940169	706	2385565	706	1940169	706
R^2	0.091	0.975	0.114	0.977	0.048	0.962	0.052	0.965
Total price/tax effect	−0.245 (0.045)***	−0.234 (0.059)***	−0.391 (0.116)***	−0.343 (0.099)***	−0.016 (0.004)***	−0.017 (0.005)***	−0.029 (0.008)***	−0.028 (0.007)***

Notes: (1) individual data; (2) aggregated data. See other notes for Table 4.

6. Smoking, exercise, and food consumption

I next attempt to determine the reason for the negative effect of cigarette prices on weight by estimating their effect on the smoking, exercise, and food consumption variables discussed in Section 4. If I find that cigarette price increases lead to healthier exercise decisions but not healthier eating decisions, then the effect of smoking on lung capacity is likely driving the results in Section 5. If I find that both eating and exercise decisions become healthier after a price/tax increase, then one or more of the other explanations provided in Section 3 is necessary. Also, comparing the timing of the effect of cigarette prices on smoking to the timing of their effect on exercise and food consumption provides insight into why the effect is delayed. I use only the BRFSS in this section since the NLSY data on smoking, exercise, and food consumption are limited.

Since I am no longer concerned in this section with reconciling the literature, I use a single methodology that is similar to the CGS approach, except I use year fixed effects instead of the quadratic time trend. I elect to use price instead of tax since price measures contain more information than tax rates, and my estimators using price in Section 5.2 showed no evidence of being biased.[27] I also continue to include three two-year lags of cigarette price, making my regression equation

$$Y_{ist} = \beta_0 + \sum_{j=0}^{2} \beta_{1j} \overline{PRICE}_{s,t-2j} + \beta_2 X_{ist} + \tau_t + \sigma_s + \varepsilon_{ist} \tag{10}$$

where Y represents one of five dependent variables: whether or not the individual currently smokes, how many cigarettes she currently smokes per day if she smokes, whether or not she obtains regular and sustained exercise, the number of grams of fat she consumes per day, and the number of times she consumes fruit or vegetables per week. In the food consumption regressions, I also estimate models including grocery price as a control to help ensure that cigarette price is not simply acting as a proxy for food price.

Limited data quality and availability for the dependent variables is a potential problem in this section. All measures of smoking, exercise, and food consumption are self-reported and may therefore suffer from measurement error, both from limited memory and a desire to impress the interviewer. However, if people with unhealthy habits exaggerate while people with healthy habits report accurately, thereby compressing the distribution of the dependent variables, then my results in this section should be understatements. Perhaps more importantly, data on smoking, exercise, and food consumption are not available for many of the BRFSS observations. In particular, number of cigarettes smoked per day by smokers is available for only 9% (213,191 observations) of the full BRFSS sample from column (1) of Table 10, while fat consumption is available for only 2.5% (58,702). Likely because of the reduced sample size, including state trends does not change the sign of the coefficient estimates in this section but inflates the standard errors to the point where the magnitudes are uninformative in some cases. I therefore do not include state trends in any of the regressions in this section. Also, long difference estimation is impossible since the BRFSS is not a panel, and the treatment- and control-group approach of Baum (2009) is also not feasible in the BRFSS. I therefore estimate state fixed effects models and state fixed effects models including fitness and sports clubs. To provide a baseline, I also estimate random effects models.

Table 11 reports the results for the regressions with whether or not the individual currently smokes and number of cigarettes smoked per day by smokers as the dependent variables. Columns (1), (2), and (3) contain the results from the random effects, fixed effects, and fixed effects with fitness clubs models, respectively. Cigarette prices have the expected effect on both variables, but the timing of their effect is different. While some people seem to quit smoking soon after a price increase, about half of the total effect on quitting occurs four or more years afterwards. However, the effect of price changes on the intensity of smoking among smokers is considerably less gradual, with the majority of the effect occurring in the first two years. Many smokers therefore may respond to price increases by gradually smoking less until they are finally able to quit altogether, possibly years after the price change. The results for smoking status and number of cigarettes smoked per day imply a long-run price elasticity of around −0.7, which is similar to Becker, Grossman, and Murphy’s (1994) estimated long-run elasticity of −0.75.[28] The results for both dependent variables are very similar using random effects, fixed effects, and fixed effects with fitness and sports clubs.[29]

Table 11. Effect of cigarette price on smoking.

	P(Smoker)			Cigarettes per day		
	(1)	(2)	(3)	(1)	(2)	(3)
Price in current year and t-1	−0.018 (0.011)	−0.020 (0.011)*	−0.020 (0.011)*	−1.181 (0.287)***	−1.113 (0.297)***	−1.148 (0.266)***
Price in years t-2 and t-3	−0.007 (0.010)	−0.007 (0.010)	−0.007 (0.010)	−0.021 (0.386)	0.006 (0.386)	−0.012 (0.391)
Price in years t-4 and t-5	−0.026 (0.010)***	−0.028 (0.010)***	−0.027 (0.011)**	−0.333 (0.371)	−0.294 (0.378)	−0.274 (0.420)
State random effects	YES	NO	NO	YES	NO	NO
State fixed effects	NO	YES	YES	NO	YES	YES
Fitness and sports clubs	NO	NO	YES	NO	NO	YES
Number of observations	957639	957639	957639	213191	213191	213191
R^2	0.058	0.058	0.058	0.092	0.092	0.092
Total price effect	−0.050 (0.011)***	−0.055 (0.012)***	−0.054 (0.013)***	−1.536 (0.268)***	−1.401 (0.288)***	−1.434 (0.311)***

See notes for Table 4.

In Appendix A, I use these results together with those from Section 5 to perform a back-of-the-envelope calculation of the implied effect of smoking on steady-state daily net caloric intake if we assume that cigarette prices only impact weight through their effect on an individual’s own smoking habits. In other words, this assumes that there are no spillover effects. I use two of the estimates from Section 5. First, I use the estimate from the BRFSS individual-level CGS regression reported in the first column of Table 10, because this is the regression that most closely resembles the smoking regressions used to calibrate other parameters as it uses the same data and price instead of tax. Second, I use the estimate (of the sample-wide impact assuming no effect on never-smokers) from the NLSY treatment-and-control group regression from the first column of Table 7. I use this estimate as well because it represents the portion of the effect that is most likely to occur via changes in one’s own smoking behavior. I calculate that the average individual who quits smoking eventually reduces her net daily caloric intake by 245 using the first of these estimates and 134 using the second. Neither effect is impossible, but the second seems particularly plausible as it could be explained by relatively small changes in behavior. 134 calories is fewer than the 150 calories in a one ounce single-serving bag of Lay’s Classic Potato

Chips, and also fewer than the 165 calories in three Oreo cookies. On the exercise side, a 170-pound individual would burn 134 calories in just 13 min of leisurely bike riding.[30]

I next examine whether the change in net caloric intake is driven by changes in exercise, eating, or both. Table 12 displays the results for the exercise regressions. In all regressions, a rise in cigarette prices increases the probability of obtaining regular and sustained exercise, and the total effect is significant at the 5% level or better. While there appears to be some immediate response, the majority of the effect is delayed until at least four years after the price change, consistent with the results for BMI, obesity, and smoking. The total effects and the magnitudes of the coefficient estimates on each price/tax variable are stable across specifications.

Table 12. Effect of cigarette price on exercise.

	Exercise		
	(1)	**(2)**	**(3)**
Price in current year and t-1	0.007 (0.005)	0.008 (0.005)	0.009 (0.005)*
Price in years t-2 and t-3	0.010 (0.021)	0.006 (0.021)	0.004 (0.021)
Price in years t-4 and t-5	0.033 (0.021)	0.032 (0.022)	0.035 (0.024)
Number of observations	968099	968099	968099
R^2	0.041	0.041	0.041
State random effects	YES	NO	NO
State fixed effects	NO	YES	YES
Fitness and sports clubs	NO	NO	YES
Total price effect	0.050 (0.017)***	0.046 (0.019)**	0.048 (0.020)**

See notes for Table 4.

I report the results for fat intake in Table 13. Data availability is a severe problem in this analysis, as fat consumption information is available in only 57 state-year cells (spanning 30 states). I therefore only include contemporaneous cigarette prices in the grams of fat regression. In the left half of the table, I report the results without including grocery price as a control. In the random effects regression without grocery prices, I find that a $1 increase in cigarette price decreases fat consumption by a statistically significant 4.3 grams per day. The standard error nearly doubles when fixed effects are added, and cigarette price becomes insignificant. However, the magnitudes actually becomes slightly larger, and a Hausman test fails to reject the null hypothesis that the random effects estimator is consistent. Adding fitness clubs makes little difference in the estimate. In the right half of the table, I add the grocery price control. In all three regressions, the estimates are well within the 95% confidence intervals of the corresponding estimates without grocery prices, so I find no evidence that not controlling for food prices biases the coefficient estimator for cigarette price.

Table 13. Effect of cigarette price on grams of fat consumed.

	No food price control			Food price control		
	(1)	(2)	(3)	(1)	(2)	(3)
Contemporaneous price	−4.167 (1.693)**	−5.296 (3.197)	−5.387 (3.380)	−5.379 (1.936)***	−3.320 (2.064)	−3.373 (2.216)
State random effects	YES	NO	NO	YES	NO	NO
State fixed effects	NO	YES	YES	NO	YES	YES
Fitness and sports clubs	NO	NO	YES	NO	NO	YES
Number of observations	68702	68702	68702	67584	67584	67584
R^2	0.111	0.111	0.111	0.114	0.114	0.114

See notes for Table 4.

I display the results for fruit and vegetable consumption in Table 14. The number of observations is considerably larger than for fat intake, so I include the three cigarette price variables. The total effect of cigarette prices on fruit and vegetable consumption is positive in all six regressions. A $1 increase in cigarette price is associated with the consumption of an additional 0.71–0.94 servings of fruits and vegetables per week. Although the estimation is imprecise, cigarette price is statistically significant at the 10% level or better in four of the six regressions, including both fixed effects regressions with fitness clubs. The results are similar both with and without food price controls. The entire effect appears delayed until four or five years after the price change.

Table 14. Effect of cigarette price on fruits and vegetables consumed.

	No food price control			Food price control		
	(1)	(2)	(3)	(1)	(2)	(3)
Price in current year and t-1	−0.018 (0.207)	−0.024 (0.208)	0.025 (0.224)	−0.001 (0.206)	−0.008 (0.206)	−0.064 (0.224)
Price in years t-2 and t-3	−0.181 (0.461)	−0.185 (0.467)	−0.142 (0.452)	−0.117 (0.473)	−0.122 (0.480)	−0.019 (0.458)
Price in years t-4 and t-5	0.912 (0.556)	0.919 (0.562)	1.056 (0.541)*	1.000 (0.560)*	1.004 (0.566)*	0.941 (0.537)*
State random effects	YES	NO	NO	YES	NO	NO
State fixed effects	NO	YES	YES	NO	YES	YES
Fitness and sports clubs	NO	NO	YES	NO	NO	YES
Number of observations	1004302	1004302	1004302	1004302	1004302	1004302
R^2	0.072	0.072	0.072	0.061	0.061	0.061
Total price effect	0.714 (0.489)	0.709 (0.519)	0.942 (0.443)**	0.878 (0.478)*	0.874 (0.511)*	0.857 (0.430)*

See notes for Table 4.

The fact that a rise in cigarette prices is associated with a drop in fat consumption but a rise in fruit and vegetable consumption provides evidence that people may make healthier eating decisions after a cigarette price increase. However, the imprecise estimates suggest that further study is necessary. Regarding timing, the results from Sections Sections 5 and 6 imply that the effect of cigarette prices on eating, exercise, and body weight is more gradual than their effect on smoking. Therefore, smoking appears to lag cigarette prices, and eating and exercise decisions appear to lag smoking. I find no evidence, however, that body weight substantially lags eating and exercise decisions.

7. Conclusion

Despite mixed evidence, the fear that the anti-smoking campaign may have contributed to America's rise in obesity has become widespread in recent years. I test this theory by examining the impact of state cigarette prices and excise taxes on body weight. I find that, in the long run, a rise in cigarette prices/taxes is actually associated with a decrease in both BMI and obesity using the different methodologies employed in the literature. In addition, this result is robust to the inclusion of linear state-specific time trends and a proxy variable for state healthiness, the use of both fixed effects and long differences models, alternative specifications designed to reduce multicollinearity, different lag lengths, and both NLSY and BRFSS data. I also find preliminary evidence that indirect effects on eating and exercise may explain this counterintuitive result.

This paper suffers from caveats that provide directions for future research. First, although the sign of the estimated effect is consistently negative, the magnitude is somewhat sensitive to specification. Specifically, in the long run a $1 increase in cigarette prices or taxes reduces average BMI by between 0.13 and 0.59 units and the obesity rate by between 1.1 and 3.6 percentage points. Further research is necessary to determine the magnitude more precisely. Second, future work should use a broader range of data to further examine the mechanisms through which cigarette prices affect weight, as the estimated effects of state cigarette prices on BRFSS measures of exercise and eating are relatively imprecise. Third, future research should examine the reason for the effects on food consumption and exercise. I propose as possible explanations that successfully reducing or quitting smoking could lead to a newfound enthusiasm in one's health, improved confidence in one's ability to make healthy decisions, or a replenished stock of willpower, but I am unable to specifically test these hypotheses. Next, further study is necessary to determine more precisely the timing of the effects on food consumption and exercise. I propose as possible explanations that successfully reducing or quitting smoking could lead to a newfound enthusiasm in one's health, improved confidence in one's ability to make healthy decisions, or a replenished stock of willpower, but I am unable to specifically test these hypotheses. Next, further study is necessary to determine more precisely the timing of the effects of cigarette prices on smoking, eating, exercise, and body weight. Also, estimating a structural model where cigarette prices affect smoking, food consumption, and exercise, which in turn affect BMI, may prove insightful. However, such an approach would require the finding of at least two additional valid instruments as well as strong assumptions about the timing of the various effects. Additionally, it is unclear whether the effects I find come from people quitting smoking, people smoking less, people not starting to smoke who would have if prices were lower, people who begin to make healthier decisions after their family and friends who quit smoking begin to make healthier decisions, or some combination of the four. The large magnitudes suggest that a combination of the four is likely. Finally, I make no effort to determine if and how the socially optimal cigarette tax rate would change after accounting for the obesity effect.

In the end, my results provide evidence against the theory that America's anti-smoking campaign has had the unintended consequence of contributing to the country's growing obesity rate. If anything, the anti-smoking campaign may have actually had the unintended benefit of limiting this growth.

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Appendix A. Effect of smoking on net caloric intake

In this appendix, I perform a back-of-the-envelope calculation of the effect smoking would have to have on caloric intake or calories burned from exercise in order to produce the estimated effect of cigarette price on BMI. Define N as net daily calorie consumption, or the difference between an individual's calories consumed and burned. Increasing food intake increases N, while additional exercise lowers N. Setting C = number of cigarettes smoked per day and B = body mass index, the effect of smoking on net caloric intake that is implied by the estimated effect of cigarette prices on BMI can be expressed as

$$\frac{dN}{dC} = \frac{dN}{dB}\frac{dB}{dC} = \frac{dB/dC}{dB/dN} \tag{11}$$

Assuming cigarette prices (P) only affect an individual's weight through her smoking habits, the impact of cigarette price on BMI can be expressed as

$$\frac{dB}{dP} = \frac{dB}{dC}\frac{dC}{dP} \tag{12}$$

Solving for dB/dC yields

$$\frac{dB}{dC} = \frac{dB/dP}{dC/dP}. \tag{13}$$

Average cigarettes smoked per day is equal to the product of the smoking rate (S) and the average number of cigarettes smokers smoke per day ($\overline{C_S}$), so

$$\frac{dC}{dP} = \frac{d(S * \overline{C_S})}{dP} = S\frac{d\overline{C_S}}{dP} + \overline{C_S}\frac{dS}{dP} \tag{14}$$

Therefore,

$$\frac{dB}{dC} = \frac{dB/dP}{S(d\overline{C_S}/dP) + \overline{C_S}(dS/dP)}. \tag{15}$$

and

$$\frac{dN}{dC} = \frac{dB/dP}{(dB/dN)(S(d\overline{C_S}/dP) + \overline{C_S}(dS/dP)}. \tag{16}$$

I calibrate the parameters as follows. For (dB/dP), I use two of the estimates from Section 5. First, I use −0.245, which is the estimate from the BRFSS individual-level CGS regression reported in the first column of Table 9. I use this regression because it is the one that most closely resembles the smoking regressions that will be used to calibrate other parameters (which use the BRFSS data and price instead of tax). Second, I use −0.134, the estimated effect assuming no effect on never-smokers from the NLSY CGS regression using treatment and control groups from the first column of Table 9. I use this estimate as well because it represents the portion of the effect that we can be most confident occurs via changes in one's own smoking behavior. This is appropriate here because (16) assumes that cigarette prices only affect an individual's weight by affecting her own smoking behavior. I use the sample mean smoking rate of 0.244 for S, and the mean number of cigarettes smoked by smokers of 19.068 for $\overline{C_S}$. For $d\overline{C_S}/dP$ and dS/dP, I use estimates from the smoking regressions in Table 11 that include state fixed effects and fitness and sports clubs, 1.434 and 0.054. I set dB/dN = 0.01385 based on the estimates of Cutler et al. (2003).[31]

I compute dN/dC = −12.8 when dB/dP = −0.245 and dN/dC = −7.0 calories when dB/dP = −0.134. Therefore, for the entire effect estimated in the BRFSS individual-level CGS regression to occur through an individual's own smoking habits, a reduction in smoking of one cigarette per day would have to be associated with 12.8 fewer calories consumed per day in the long run. Similarly, for the entire effect estimated in the NLSY CGS regression using treatment and control groups to occur through an individual's own smoking habits, one less cigarette per day would have to be associated with 7.0 fewer calories consumed per day. These two estimates imply that if an individual who smokes the sample mean of 19.068 cigarettes per day quits entirely, eventually she will reach a steady state in which she consumes 245 fewer calories per day or 134 fewer calories per day, respectively.[32]

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Notes

1. Body mass index (BMI) = weight in kg/height in m^2. BMI is a commonly used measure of a person's weight relative to her medical optimum because it accounts for height.
2. The 1965 number reflects data compiled by the Centers for Disease Control and Prevention, Office on Smoking and Health from the 1965 National Health Interview Survey. The 2004 number is from Centers for Disease Control and Prevention (2005).
3. The percentage was calculated using data from *Tax Burden on Tobacco* by Orzechowski and Walker (2005).
4. Becker et al. (1994) attributed this paradox to the addictive nature of cigarettes, arguing that cigarette companies may keep prices below their short-run profit-maximizing level in an effort to increase longer-range profits by creating more addicts. Foreseeing a sizeable drop in future demand, they may have raised prices to closer to the short-run profit-maximizing level.
5. For the duration of this paper, cigarette prices or taxes refer to the price/tax per pack, or twenty cigarettes.
6. I-Quit ™ is an example of a pill, while Commit ™ is an example of a lozenge. Books include T he How to Quit Smoking and Not Gain Weight Codebook by Mark Donkersloot, *How to Quit Smoking Without Gaining Weight* by the American Lung Association, and *The I-Quit Smoking Diet/the Revolutionary 21-Day Plan that Lets You Quit Smoking Without Gaining Weight* by Janice Alpert. For an example of an internet program see http://www.burnthefatblog.com/quit_smoking.html. Visit http://www.nosmoke4u.homestead.com/ for an example of a hypnotic technique.
7. Several examples are http://www.coolnurse.com/smoking_quit.htm, http://quitsmoking.about.com/od/weightgain/a/weightgainquit.htm, http://www.surgeongeneral.gov/tobacco/consquits.htm, http://www.cancer.org/docroot/PED/content/PED_10_13X_Guide_for_Quitting_Smoking.asp, and http://www.quitsmokingsupport.com/withdrawal1.htm.
8. Since death due to smoking- or obesity-related diseases is rare in the NLSY's age range, it is unlikely that attrition would bias my estimator of the effect of cigaratte prices/taxes on weight. Accordingly, researchers using the NLSY to study the determinants of body weight generally do not address the issue of attrition (Lakdawalla and Philipson, 2002) and (Anderson et al., 2003).
9. Many values of income are missing in the NLSY, which would lead to a substantial reduction in sample size in the regressions which include several lags. I therefore impute these missing values of income by averaging the first non-missing income values before and after the missing period. If all income values are missing for two years before and after the current period, I drop the observation.
10. In both data sets, I calculate BMI using respondents' self-reported weight and height. Self-reported weight and height are potentially problematic since people tend to underreport their weight and, to a lesser extent, exaggerate their height. Some economists in the obesity literature have employed a correction for self-reported BMI developed by Cawley (1999). They use the NHANES, which includes both actual and self-reported weight and height, to estimate actual BMI as a function of self-reported BMI and a variety of demographic characteristics. Researchers have generally found that the correlation between actual and self-reported BMI is very high, and that correcting for measurement error does not substantially alter the coefficient estimates in regressions (Cawley, 1999) and (Lakdawalla and Philipson, 2002). CGS and Baum (2009) employ this correction, GF's main results do not, and RCG's data include actual height and weight. GF show that their specification changes lead to virtually identical results using both their data and the data they borrowed from CGS, which contain corrected BMI. Therefore, employing Cawley's correction appears to make no difference in the cigarette price/obesity literature, and I elect not to use it in this paper. The food and exercise variables likely also suffer from measurement error, but one would expect them to be highly correlated with actual measures.
11. I include only fitness and sports clubs subject to federal income tax since data for other places is not comparable across the sample period.
12. The cities included in the ACCRA COLI change somewhat from year to year. To ensure that the same cities are included each year, I drop cities for which prices are not reported in more than four years and impute the remaining missing prices by averaging the prices from the preceding and following years. Also, to make the variable annual I use the prices from the second quarter of each year.

13. The NLSY data contains more limited classifications of race and marital status than the BRFSS. My categories for race are black, white, and other, while for marital status I classify people as either married or single.
14. Both CGS and GF use a linear probability model; my results are robust to the use of probit models. Also, although CGS do not use sampling weights, I elect to use them in my NLSY replications of their approach because the NLSY oversamples minorities and economically disadvantaged individuals. Results are similar without sampling weights.
15. In unreported regressions, I drop pregnant women as well as the top and bottom percentiles of the BMI distribution. The results remain very similar.
16. CGS used 1982–1984 dollars while GF used 2002 dollars. I divide CGS' coefficient estimates on price by 1.794 and price squared by 1.794 squared to convert them to 2002 dollars, and I continue to use 2002 dollars throughout the paper.
17. CGS do not report marginal effects, but I calculate them using their sample mean. I do not report standard errors since calculating them would require the covariance of the coefficient estimates on price and price squared.
18. Since the independent variables of interest are state-level, including individual instead of state fixed effects would only improve the consistency of the estimates if unobservable individual characteristics are correlated with state-level prices or taxes. This seems unlikely, but would be possible if, for example, moving patterns are endogenous.
19. I impute the independent variables for years not included in my sample (1987, 1991, 1995, 1997, 1999, 2001, and 2003) by taking the average of the years immediately before and after the skipped year. For example, BMI and obesity status in 1998 are functions of the independent variables in 1998, 1997, 1996, 1995, 1994, and 1993. Since 1997 and 1995 are not surveyed, I impute the values of the independent variables in those years. Since I obtain prices and taxes from a data source that includes all years, I do not have to impute their values. However, I assume that the respondents live in the same state that they did in the year after the skipped year.
20. Smoking and obesity policies do seem to be correlated with economic variables. DeCicca et al. (2008) find that controlling for a state's anti-smoking sentiment – which affects cigarette taxes – mitigates the impact of cigarette prices on youth smoking, while Cawley and Liu (2008) find that state socioeconomic conditions and political climate are determinants of government action addressing childhood obesity.
21. I suspect that changes in fitness and sport clubs proxy for changes in health consciousness because, in fixed effects regressions using the BRFSS (available upon request), I observe a very large negative association between these clubs and both smoking and food consumption. Since there is little reason to suspect that these effects are causal, it appears that fitness clubs proxy for unobservable state health attributes.
22. For example, by long differencing BMI becomes the difference between BMI in year t and BMI in year $t-6$, average cigarette price over years t and $t-1$ becomes the difference between average cigarette price over years t and $t-1$ and average cigarette price over years $t-6$ and $t-7$, and so on.
23. In unreported regressions, I also estimate dynamic panel data models that include the lag of the dependent variable. Results are similar and available upon request.
24. Note that, even though I obtained 48 estimates of price/tax coefficients in Section 5.2, there are only 24 unique VIFs, as the VIF for an independent variable is the same regardless of whether the dependent variable is BMI or obesity status.
25. Baum uses only the NLSY waves starting in 1992. I use all waves in order for the results to be comparable to those in Table 4, Table 5 and Table 6, and also to maximize the sample size, which is especially important given the multicollinearity and number of price/tax parameters estimated. Results are similar if I restrict the sample to 1992 and later, although the level of statistical significance is reduced in some cases, likely because of the limited sample size.
26. In unreported regressions, I also considered a rational addiction framework by including lead prices and taxes. However, their coefficients were small and insignificant.
27. In the regressions in this section (available upon request), the signs of the coefficient estimates are the same using tax, but the estimates are imprecise and some of the magnitudes are unreasonably large.

28. This calculation is based on the sample means for the smoking variables and the sample mean cigarette price during the years included in the smoking regressions (1984–2000) of $2.49.
29. An alternative approach to incorporating smoking would be to estimate both the long-run effects of cigarette price on smoking and smoking on weight using an instrumental variables model. However, data limitations prevent such an analysis. If cigarette prices affect smoking gradually and smoking affects weight gradually, one would need to use lags of cigarette price as instruments for lags of smoking, and neither data set used in this paper allow for such an approach. The NLSY only collects smoking information in three survey waves, while the BRFSS is not a panel and therefore does not include individuals' smoking status in previous years, preventing a regression of lagged smoking status on lagged prices. Further, even if this approach were possible, if cigarette prices affect the weight of non-smokers through spillover effects, price would not be a valid instrument for smoking.
30. The nutrition information on Lay's Classic Potato Chips is from http://www.fritolay.com/our-snacks/lays-classic-potato-chips.html, while the information on Oreo cookies is from http://www.calorie-counter.net/calories-in-oreo-cookies.htm. I calculate the calories burned from bike riding using the calculator on http://primusweb.com/fitnesspartner/jumpsite/calculat.htm.
31. Cutler et al. use results from medical literature to develop a model of how steady-state weight responds to changes in caloric intake or expenditure. They estimate that a 10–12-pound increase in steady-state weight is associated with an increase in daily net caloric balance of 100–150. This implies a pound-to-calorie ratio of between 0.1 and 0.08, the midpoint of which is 0.09. One unit of BMI is approximately 6.4 pounds at the sample mean height in both the NLSY and BRFSS, so the BMI-to-calorie ratio is about 0.01385.
32. This analysis assumes that in the long run smoking only affects weight through eating and exercise—in other words, there is no long-run impact on metabolism. Evidence that the metabolic effect of smoking is temporary (Perkins, 1992) suggests that this assumption is reasonable.