Volume Title: Risky Behavior among Youths: An Economic Analysis

Volume Author/Editor: Jonathan Gruber, editor

Volume Publisher: University of Chicago Press

Volume ISBN: 0-226-31013-2

Volume URL: http://www.nber.org/books/grub01-1

Publication Date: January 2001

Chapter Title: Youth Smoking in the United States: Evidence and Implications

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Chapter URL: http://www.nber.org/chapters/c10687

Chapter pages in book: (p. 69 - 120)

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Youth Smoking in the United States
Evidence and Implications

Jonathan Gruber and Jonathan Zinman

One of the most striking trends in the behavior of youths in the United States during the 1990s has been the increased incidence of smoking. After steadily declining over the previous fifteen years, youth smoking began to rise precipitously in 1992. By 1997, smoking by teenagers in the United States had risen by one-third from its 1991 trough. This trend is particularly striking in the light of the continuing steady decline in adult smoking in the United States. Indeed, today we are in the alarming position of having a youth-smoking rate that is roughly 50 percent greater than the smoking rate of adults.

This striking time trend has motivated substantial public-policy interest in youth smoking, highlighted by the recent unsuccessful attempt of the Clinton administration to pass a comprehensive tobacco regulation bill that had the ostensible main purpose of reducing youth smoking. This public-policy interest arises out of concern that youths are not appropriately recognizing the long-run implications of their smoking decisions. Indeed, young smokers clearly underestimate the likelihood that they will still be smoking in their early twenties and beyond. For example, among

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The authors are grateful to James Berry, Cristian Gonzalez, Jeffrey Hoffner, and Choonsiang Tan for outstanding research assistance, and to seminar participants at the NBER, the University of Pennsylvania, and the University of Chicago for helpful comments. Gruber acknowledges financial support from the National Institute on Aging and the National Science Foundation; Zinman acknowledges support from MIT's John Castle Fellowship, the Harry S. Truman Scholarship Foundation and the Social Science Research Council's Program in Applied Economics.

high school seniors who smoke, 56 percent say that they will not be smoking five years later, but only 31 percent of them have in fact quit five years hence. Moreover, among those who smoke more than one pack per day, the smoking rate five years later among those who stated that they would not be smoking (74 percent) is actually *higher* than the smoking rate among those who stated that they would be smoking (72 percent) (DHHS 1994).

If youth smoking leads to adult smoking, particularly in a manner that is underappreciated by the young smokers themselves, it can have drastic implications for the health of the U.S. population. Smoking-related illness is the leading preventable cause of death in the United States, and smokers on average live 6.5 (males) to 5.7 (females) fewer years than those who never smoked (Cutler et al. 1999). Thus, it is critical to understand the role that public policy can play in deterring youth smoking.

Yet, despite this interest and concern, we do not very well understand either the determinants or the implications of youth-smoking behavior. This paper attempts to remedy these deficiencies in our understanding by providing new evidence on four aspects of youth smoking.

The first is the correlation between background characteristics such as race, sex, education, family structure, and work behavior and the decision to smoke (or how much to smoke conditional on smoking). We explore how well smoking behavior can be explained both by clearly exogenous background characteristics and by potentially endogenous attitudine variables. And we assess how these relations have changed over time as youth smoking has risen. Our key findings here are that background characteristics explain only a small share of the decision to smoke and that smoking participation is not simply concentrated among the most disadvantaged youths; indeed, increasingly over time, youth smoking is taking place among white, suburban youths with college-educated parents and good grades.

The second is an assessment of the extent to which changes in background characteristics or changes in attitudes toward smoking can explain the precipitous recent rise in youth smoking. In short, we find that neither plays an important role in explaining this rise; background characteristics can explain at most 10 percent of it.

The third is an understanding of the role that public policy can play in deterring youth smoking. We provide a comprehensive analysis of the effect of prices and other public policies on youth smoking in the 1990s, using three different data sets with information on youth smoking to assess the robustness of our findings.

We find that the most important policy determinant of youth smoking, particularly among older teens, is price. There is a statistically significant and quantitatively large response of smoking by older teens to prices in all three data sets, although the estimated price elasticity varies significantly. On the other hand, price does not appear to be an important determinant of smoking by younger teens. There is little consistent evidence of other public policies meant to reduce youth smoking having a robust effect, although there is some suggestion that restrictions on youth purchase of cigarettes reduce the quantity of cigarettes smoked. And we find that black youths and youths with less-educated parents are much more responsive to price than are white teens and teens with more-educated parents, suggesting a strong correlation between price sensitivity and socioeconomic status.

The final part of this paper then builds on these findings to assess the long-run implications of youth smoking and in particular to forecast what the recent rise in youth smoking bodes for future smoking in the United States. We do so in two ways. First, we pursue a cohort analysis, examining what the historical record tells us about the implications of higher rates of youth smoking for the adult smoking of those same cohorts as they age. Second, we use the Vital Statistics Natality data to examine the extent to which policy interventions when individuals are young determine their smoking decisions later in life. Both approaches yield similar results: there are significant intertemporal linkages between youth smoking and adult smoking, with each percentage point of additional smoking by youths translating into only 0.25–0.5 percentage points more smoking by those youths as adults. This finding suggests that there will be a significant rise in future adult smoking because of the 1990s experience. Rough calculations suggest that, even if this rise in youth smoking is transitory (owing to significant recent price increases), the adult-smoking rate for this cohort will rise by 8–16 percent and that at least 1.6 million total life years will be lost by this cohort.

The paper proceeds as follows. Section 2.1 provides background on trends in youth smoking and on previous work in this area. Section 2.2 presents our cross-sectional analysis of demographic determinants of smoking decisions and what this analysis implies for explaining time trends. Section 2.3 explores the role of price and other public policies. Section 2.4 then turns to the intertemporal implications of youth smoking. Section 2.5 concludes.

2.1 Background

2.1.1 Youth Smoking: Where It Has Been, Where It Is Going, and Why We Should Care

The time-series trends in youth smoking are depicted in figures 2.1 and 2.2. Figure 2.1 shows the trend since the 1970s for the three available surveys of seniors: the Monitoring the Future (MTF) survey, which has surveyed high school seniors since 1976 but eighth and tenth graders only since 1991; the National Health Interview Survey (NHIS); and the Na-

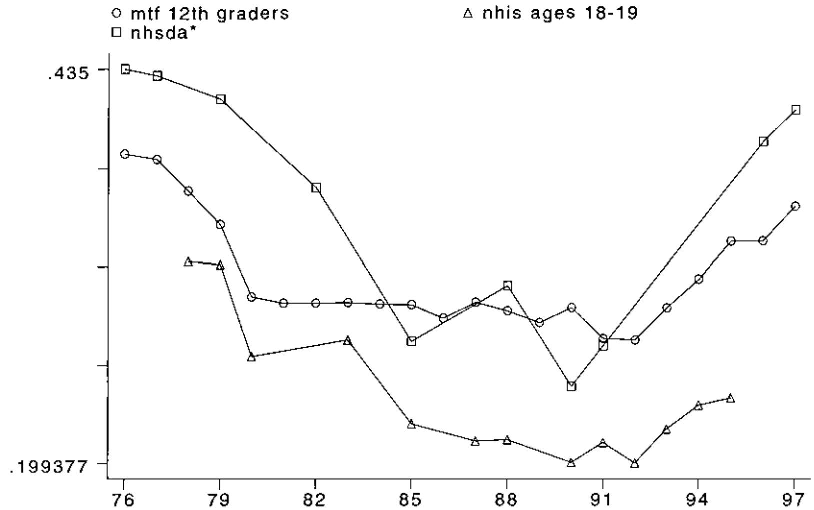

Fig. 2.1 Smoking participation: Older youths time series
Note: NHSDA data are for persons age 17–19 through 1991 and age 18–20 in 1996–97.

tional Household Survey of Drug Abuse (NHSDA). The latter two surveys are household surveys; from them we use data on older teens.[1] There is considerable uncertainty over the relative value of in-school versus household surveys for collecting smoking information; the latter have the advantage of collecting information on dropouts but the disadvantage that youths may be less willing to give honest answers when their parents may overhear. Despite these differences in sources, however, all three surveys show the same basic trend: large declines over the late 1970s, flattening and slow declines in the 1980s, and a steep rise in the 1990s.

Figure 2.2 focuses on the trend for the 1990s for all high school youths, using data from the MTF survey and the Youth Risk Behavior Survey (YRBS), which collected data for 1991, 1993, 1995, and 1997 on a large sample of ninth to twelfth graders. For both data sets, there are dramatic increases in the 1990s. In the MTF data, there is an increase of 7.2 percentage points, or 35 percent; in the YRBS, the increase starts from a higher base, but the increase is larger, at 8.7 percentage points, so that the percentage increase is also about one-third.

This dramatic upswing in youth smoking is a concern because smoking as a youth has been strongly correlated with smoking as an adult. Table 2.1 shows tabulations from the 1992 NHIS and the 1995 NHIS on the age of initiation of smoking by current or former adult smokers. This table reveals that 42 percent of current or former adult smokers started before

1. In particular, from the NHIS, we use eighteen- to nineteen-year-olds, and, from the NHSDA, we use seventeen- to nineteen-year-olds through 1991 and eighteen- to twenty-year-olds for 1996–97.

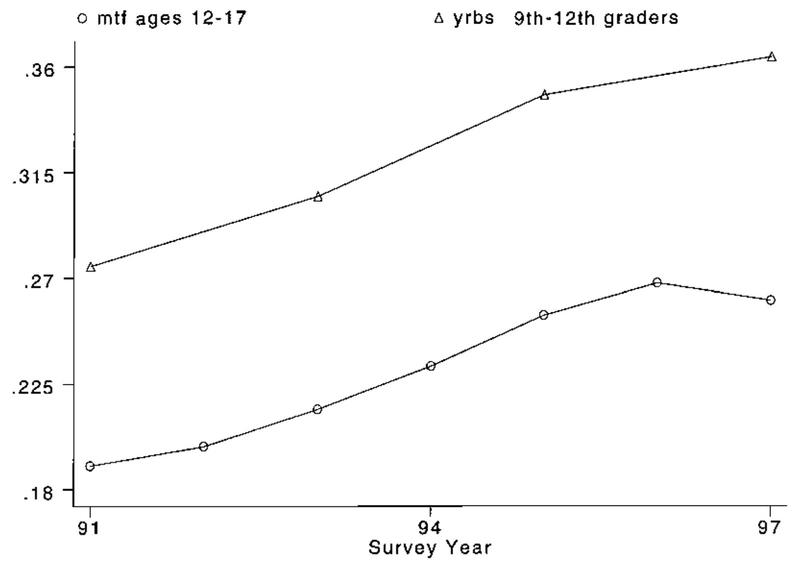

Fig. 2.2 Youth-smoking participation in the 1990s

Table 2.1 Percentage of Those Who Ever Smoked Who Began Smoking at Each Age

Age	% Starting at Age	Cumulative %	Age	% Starting at Age	Cumulative %
6	.47	.47	19	4.47	79.35
7	.79	1.26	20	5.48	84.82
8	1.05	2.31	21	3.21	88.03
9	1.24	3.55	22	1.76	89.79
10	2.67	6.22	23	1.05	90.83
11	1.85	8.08	24	.85	91.68
12	6.50	14.57	25	1.47	93.16
13	6.63	21.21	26	.39	93.54
14	8.81	30.01	27	.42	93.97
15	11.52	41.53	28	.40	94.37
16	13.53	55.06	29	.19	94.56
17	8.73	63.79	30	.98	95.53
18	11.09	74.88			

Source: NHIS smoking supplements, 1992 and 1995.

their sixteenth birthday and that 75 percent started before their nineteenth birthday. Conversely, of those smoking a pack a day as high school seniors in the MTF survey, 87 percent are smoking five years later. Even among those smoking one to five cigarettes per day, 70 percent are smoking five years later.

If youth smoking is a strong determinant of adult smoking, then the long-run secular decline in adult smoking may be reversed. Of course, these facts do not prove that the current upswing in youth smoking will lead to higher long-run adult-smoking rates, for two reasons. First, it is difficult to distinguish causality from these intertemporal correlations; smoking later in life may not be a consequence of youth smoking for adults in the past, but, rather, smoking at both points in life may simply arise from intertemporal correlation in tastes for this activity. This suggests a natural test for the causality of this relation between youth smoking and adult smoking, which is to assess whether exogenous shifts in youth smoking affect the smoking of those same individuals later in life. This is the exercise that we will take up in section 2.4 below.

Second, however, there may also have been a structural shift in the nature of youth smoking. New young smokers today may be different from new young smokers in the past—in particular, with a greater resolve to quit—and therefore this intertemporal correlation may be broken. We do find some evidence for this view (which we present below) in that there is a relative rise in the smoking rates of more advantaged youths in the 1990s. But these changes are modest relative to the enormous rise in smoking within virtually every identifiable subgroup in our data.

2.1.2 Previous Literature on Youth Smoking

There is a substantial literature on the background characteristics of youths that are most closely correlated with smoking decisions. This literature is reviewed in DHHS (1994), which provides a comprehensive overview of the state of knowledge to that point. Many of the conclusions in that chapter are echoed and updated in the discussion presented below as they draw on the same data that we use.

One interesting additional piece of data that does not come from the data that we use below is the brand preferences of youths relative to those of adults. In 1993 (the latest year for which data are available for adults and teens), smoking among adults was relatively dispersed across many brands, with the top three brands (Marlboro, 24 percent; Winston, 6.7 percent; Newport, 4.8 percent) accounting for only 35 percent of the total cigarettes smoked. But smoking among youths was much more concentrated, with the top three brands (Marlboro, 60 percent; Camel, 13.3 percent; Newport, 12.7 percent) accounting for 85 percent of cigarettes smoked.

There is also a sizable literature on the responsiveness of youth smoking to prices and other public policies. The early work on the price elasticity of youth smoking was cross-sectional in nature. This work generally found quite strong effects of prices on youth smoking. While there is some variation, a representative estimate that is frequently cited is Chaloupka and Grossman's (1996) estimate of a participation elasticity of −0.675 and a total smoking elasticity of −1.313. Similar estimates are found in Lewitt, Coate, and Grossman (1981) and Lewitt and Coate (1982), although the result is disputed by Wasserman et al. (1991).

This literature has been strongly criticized, however, by DeCicca, Kenkel, Mathios (1998) and Evans and Huang (1998), who point out that, in cross-sectional data, it is impossible to disentangle price and policy effects from other underlying long-run determinants of smoking attitudes. For example, as they note, taxes are traditionally the lowest in the tobacco-producing states, where smoking is also the highest, and it is difficult to disentangle causality in that relation. These two papers take different approaches to solving this problem. DeCicca, Kenkel, and Mathios pursue a strategy of focusing on smoking initiation, which compares changes in smoking rates to changes in price within a cohort, and they find no significant price effect. Evans and Huang, on the other hand, use repeated cross sections of youths and include state fixed effects to control for fixed-state tastes toward smoking, and they find a significant participation price elasticity of −0.5 over the period 1985–92 (using repeated cross sections of the restricted MTF data discussed below).

Both these approaches have weaknesses. The DeCicca, Kenkel, and Mathios (1998) methodology excludes the responsiveness of quitting to price increases; ultimately, it is the level of youth smoking that is the concern, not just initiation. Evans and Huang do consider the overall level of smoking, but they do not include the other controls for state smoking regulations that are deemed quite important by DeCicca, Kenkel, and Mathios. Moreover, neither paper focuses on the period of most interest, the 1990s.

A smaller literature has studied the effect of other antismoking policies on youth smoking. DeCicca, Kenkel, and Mathios (1998) include in their model measures of state access restrictions on youth tobacco purchase and of restrictions on smoking in public places and find no effect on smoking. Chaloupka and Grossman (1996) include a variety of measures of access restrictions and clean-air regulations; they find no (actually wrong-signed) effects of the former but fairly strong negative effects of the latter in their cross-sectional study. Another cross-sectional study by Chaloupka and Pacula (1998) focusing on youth-access-restriction enforcement does find some evidence that more tightly enforced youth-access restrictions lower youth smoking. But these cross-sectional studies once again suffer from the fact that the legislation and enforcement of youth-access restrictions may be correlated with fixed underlying attitudes toward smoking. Two interesting case studies of communities that implemented tough youth-access restrictions found mixed results, with Jason et al. (1991) finding substantial (50 percent) declines in youth smoking in Woodridge, Illinois, and Rigotti et al. (1997) finding very limited effects on sales to youths and on youth smoking in several Massachusetts communities.

To summarize, the literature on both prices and policies has produced somewhat mixed conclusions, particularly the limited literature that has attempted to control for unobserved state characteristics. Moreover, another limitation of most of the work on price responsiveness is that it has focused on either only one cohort or only one age group of teens. In fact, as we document at length below, there is considerable heterogeneity among the teen population in terms of responsiveness to policy variables.

2.2 Determinants of Youth Smoking

2.2.1 Data and Empirical Strategy

For this part of the analysis, we will be using the public-use data set from the Monitoring the Future (MTF) survey, a large, nationally representative cross section of high school seniors. This survey contains information on a rich set of background characteristics and attitudine variables, making it an excellent source for trying to model the determinants of youth smoking comprehensively. These public-use data do have two drawbacks, however. First, they are only for high school seniors, which means that we cannot explore how these background characteristics affect smoking decisions by younger teens. Second, they do not contain very detailed location information, which means that we cannot explore the role of state-level policies. We address both these issues in our policy analysis below. We use data from the MTF surveys over the period 1976–97.

We will estimate simple cross-sectional models of smoking, using two different dependent variables: a dummy for smoking participation (defined as smoking any cigarettes over the previous month) and the number of cigarettes smoked conditional on smoking at all. For the former, we will estimate linear-probability models for ease of coefficient interpretation; our results are very similar if we use probit or logit models instead. For the latter, the results are somewhat difficult to interpret since factors that affect the participation decision can have composition effects on the conditional number of cigarettes smoked.

2.2.2 Cross-Sectional Results for 1997

We begin by estimating cross-sectional models for 1997, the last year in our sample. We include a broad set of variables in our base specification that are plausibly exogenous to the smoking decision. Our basic results for 1997 are presented in tables 2.2 (participation) and 2.3 (conditional intensity). There are a number of findings of interest: The single most important determinant of smoking is race: blacks and those of "unknown race" (likely other nonwhites) smoke much less. Those living with fewer than two parents are more likely to smoke. Males are less likely to smoke (but smoke more when they do smoke); in the raw data, males are more likely

Table 2.2 1997 MTF Public-Use Regression Results: Participation

Covariate	Coeff.	Covariate	Coeff.
SMSA	−.0276 (.0102)	Age < 18	.0126 (.00746)
Number of siblings	−.0015 (.00403)	GPA (normalized by regional annual average)	−.265 (.0197)
Race (1 = black)	−.30 (.0118)	Married (omitted = unmarried	−.0368 (.0274)
Race unknown (captures Latino etc. + no response)	−.141 (.00891)	Engaged (omitted = unmarried	.0689 (.016)
Parents < 2 (omitted = two-parent household)	.0209 (.00862)	Separated/divorced (omitted = unmarried)	.045 (.0414)
Sex (1 = male)	−.0122 (.00769)	College-prep program (omitted = general high school program)	−.0233 (.0091)
Grew up in rural area (omitted = 'grew up mostly in suburbs')	−.0357 (.0139)	Vocational-prep program (omitted = general high school program)	.0269 (.014)
Grew up in a town (omitted = grew up mostly in suburbs)	.0178 (.0116)	No plan to attend college (omitted = plan to attend college)	.0741 (.0142)
Grew up in city (omitted = grew up mostly in suburbs)	−.0307 (.0109)	Probably will not attend college (omitted = plan to attend college)	.0418 (.0139)
Father's education < high school (omitted = high school education)	−.0082 (.0133)	Probably will attend college (omitted = plan to attend college)	.0175 (.0101)
Mother's education < high school (omitted = high education)	−.0318 (.0136)	Mom worked (omitted = mom never worked when growing up)	.00753 (.0101)
Father has some college (omitted = high school education	.0076 (.0117)	Works (1 = yes, 0 = no)	−.0177 (.0124)
Mother has some college (omitted = high school education)	.0232 (.011)	Job hours per week	.00224 (.000559)
Father college graduate (omitted = high school education)	.0248 (.0108)	$/week from job	.000458 (.000109)
Mother college graduate (omitted = high school education)	.0373 (.0103)	$/week from other sources	.000665 (.000131)
Northeast region (omitted = Midwest region)	.0134 (.0108)	Religion important	−.00545 (.00451)
South region (omitted = Midwest region)	−.0263 (.0099)	Attend religious services regularly	.000242 (.00439)
West region (omitted = Midwest region)	−.0546 (.0127)	Days missed school	.0161 (.001)
Adjusted R^2	.1125	*N*	15,525

Note: Regressions estimated using 1997 cross section of MTF data by linear-probability model; mean of dependent variable is 0.353. Standard errors are given in parentheses. SMSA = standard metropolitan statistical area.

Table 2.3 **1997 MTF Public-Use Regression Results: Conditional Intensity**

Covariate	Coeff.	Covariate	Coeff.
SMSA	−.143 (.303)	Age < 18	.318 (.234)
Number of siblings	−.0114 (.127)	GPA (normalized by regional annual average)	−4.35 (.595)
Race (1 = black)	−5.61 (.492)	Married (omitted = unmarried	1.55 (.944)
Race unknown (captures Latino etc. + no response)	−2.57 (.33)	Engaged (omitted = unmarried	1.55 (.443)
Parents < 2 (omitted = two-parent household)	1.1 (.265)	Separated/divorced (omitted = unmarried)	5.29 (1.19)
Sex (1 = male)	.867 (.244)	College-prep program (omitted = general high school program)	−1.02 (.285)
Grew up in rural area (omitted = 'grew up mostly in suburbs')	−.394 (.418)	Vocational-prep program (omitted = general high school program)	.915 (.409)
Grew up in a town (omitted = grew up mostly in suburbs)	.761 (.359)	No plan to attend college (omitted = plan to attend college)	1.77 (.419)
Grew up in city (omitted = grew up mostly in suburbs)	−.994 (.341)	Probably will not attend college (omitted = plan to attend college)	.776 (.416)
Father's education < high school (omitted = high school education)	−.569 (.417)	Probably will attend college (omitted = plan to attend college)	−.279 (.321)
Mother's education < high school (omitted = high education)	−.059 (.441)	Mom worked (omitted = mom never worked when growing up)	−.0113 (.321)
Father has some college (omitted = high school education	−.312 (.367)	Works (1 = yes, 0 = no)	−2.2 (.419)
Mother has some college (omitted = high school education)	.25 (.342)	Job hours per week	.0837 (.0177)
Father college graduate (omitted = high school education)	−.188 (.334)	$/week from job	.0135 (.00347)
Mother college graduate (omitted = high school education)	−.042 (.321)	$/week from other sources	.0209 (.00394)
Northeast region (omitted = Midwest region)	.205 (.322)	Religion important	.2 (.142)
South region (omitted = Midwest region)	−1.06 (.312)	Attend religious services regularly	−.419 (.138)
West region (omitted = Midwest region)	−2.48 (.405)	Days missed school	.187 (.028)
Adjusted R^2	.139	*N*	5,450

Note: Regressions estimated using 1997 cross section of MTF data by linear-probability model; mean of dependent variable is 7.329. Standard errors are given in parentheses. SMSA = standard metropolitan statistical area.

to smoke, but this appears to be reversed by the inclusion of variables such as work status and family structure. Those living in suburbs (the omitted location group) are likely to smoke more. Those with more highly educated parents are *more* likely to smoke, with no clear pattern for the intensity of smoking. There are lower smoking rates in the West. Academic performance and trajectory appear to be very significant. Current GPA (relative to year/region normalization) has huge significant effects (with better students smoking less), and students in college-preparatory programs and those planning on attending college smoke significantly less across the board. Those who are married are less likely to smoke, but those who are engaged or divorced/separated are more likely to smoke. Those who work are less likely to smoke, but smoking rises quite strongly with hours worked and with income. Those who are absent from school more frequently are more likely to smoke. Overall, the model has modest explanatory power, with an R^2 of 0.11, which is fairly high for a linear-probability model.

The key findings of these regressions are that smoking is not as purely concentrated in disadvantaged youths as it is concentrated in low-socioeconomic-status adults: smoking is much lower among minorities than among whites; it is more likely in the suburbs than in either the city or rural areas; and it is positively correlated with parents' education. On the other hand, youth smoking is much more likely among those with worse academic performance, those who miss more school, and those who do not plan to go to college. There also appear to be strong income effects on smoking: more hours worked and more income lead to more smoking among high school seniors. An interesting question is whether the positive association with indicators of advantage such as suburban dwelling or college-educated parents are also picking up unmeasured aspects of income.

2.2.3 Can We Explain the Time Series?

In trying to explain the dramatic increase in smoking in the 1990s, the first step is to assess whether there were changes in background characteristics that might play an important role. We do this by taking our cross-sectional models and using the estimated relations to predict smoking in each year, given the values of the X's in each year. If changes in background characteristics are explaining the time-series pattern, then this predicted series should mimic actual smoking behavior.

We present the results from doing so in figure 2.3. We use here a cross-sectional model estimated in 1985–86 to predict smoking in each year so that we can do out of sample predictions on both the steep decline in the 1970s and the steep increase in the 1990s; we come to the issue of coefficient stability next. In fact, we find that this model can predict very little of either the decline in the 1970s or the rise in the 1990s. The predicted series

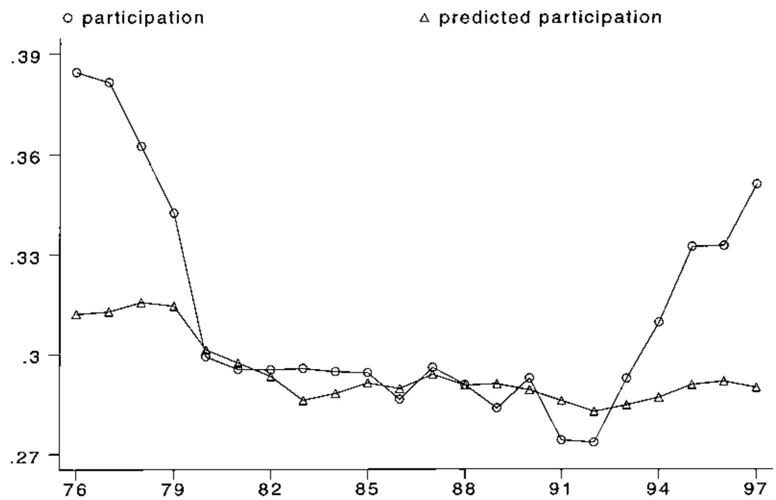

Fig. 2.3 Smoking participation: predicted vs. actual
Note: p-values were generated using 1985–86 coefficients.

does rise very slightly in the 1990s, by 0.7 percentage points, but this is less than 10 percent of the 7.8 percentage point rise in smoking by high school seniors over this period. Thus, the steep increase in smoking participation of the 1990s is not explained by changes in background characteristics.

Figure 2.4 shows the same exercise for smoking intensity. Here, the model predicts somewhat better the changes over time, but this is perhaps not surprising since the changes are much more modest.

2.2.4 Changes in the Estimated Relations

One issue of importance is whether it is very inappropriate to apply constant coefficients to the estimated relations between smoking and background characteristics because the relations are changing over time. This is a particularly interesting question owing to our inability to find the expected negative relation between socioeconomic status and smoking in the recent cross section. Is this because over the 1990s smoking has become more of a "yuppie phenomenon"?

We investigate this by examining the changes in the coefficients in our cross-sectional model over time. Table 2.4 shows changes in these coefficients for smoking participation, first from 1976 to 1986, then from 1986 to 1997. The second column shows the coefficients for 1976, the third column for 1986, and the fourth column for 1997; the fifth column shows the change from 1976 to 1986 (the 1986 coefficient minus the 1976 coefficient) and a *t*-statistic on that difference; the next column does the same for 1986–97; and the final column does the same for the entire period 1976–97.

The results show substantial stability in the characteristics of smokers

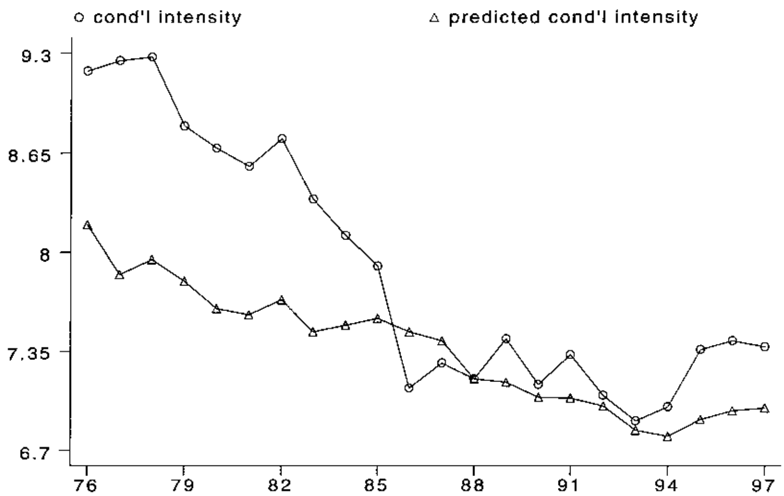

Fig. 2.4 Smoking intensity: predicted vs. actual
Note: p-values were generated using 1985–86 coefficients.

over this twenty-one-year period; only eleven of the thirty-five coefficients change significantly. But there is a striking and consistent story from the coefficients that do change: smoking is rising much more among "advantaged" youths, particularly in the 1990s. There is a significant rise in smoking among whites relative to nonwhites, among those students who do not work relative to those students who do, among those students who live in suburban settings relative to those who live in town or urban settings, among those whose mothers are college educated relative to those whose mothers are not, and among religious students. Perhaps the most striking pattern is the association of high relative GPA and smoking: while smoking fell significantly for high-GPA students relative to low-GPA students between 1976 and 1986, it then rose even more over the next decade for the high-GPA strata.

Thus, smoking is indeed rising significantly among advantaged youths relative to their disadvantaged counterparts. But it is worth noting that these relative changes are completely swamped by the secular increases among all groups. To confirm this point, in figure 2.5 we graph smoking rates over time for four comparison groups: men and women; whites and blacks; suburban and urban dwellers; and high- (top 25 percent) and low- (bottom 25 percent) GPA students. For every group, smoking rates rise precipitously in the 1990s; the only group for which this rise appears modest is low-GPA students. Thus, we conclude that, while there was a shift toward more smoking by advantaged students over the past ten to twenty years, that shift was swamped in the past decade by other factors that are affecting all students.

Table 2.4 Smoking-Participation Coefficient Stability, MTF Public-Use Data

Covariate	1976 (1)	1986 (2)	1997 (3)	1986–76 (4)	1997–86 (5)	1997–76 (6)
SMSA	−.041	−.034	−.028	.007	.006	.013
	(.01)	(.01)	(.01)	(.014)	(.014)	(.014)
Age[a]	.017	−.003	.013	−.02	.016	−.004
	(.009)	(.008)	(.007)	(.012)	(.011)[a]	(.011)[a]
Black	.025	−.2	−.31	−.226	−.1	−.326
	(.013)	(.012)	(.012)	(.018)	(.017)	(.018)
Other nonwhite	−.011	−.075	−.134	−.065	−.059	−.124
	(.013)	(.011)	(.01)	(.017)	(.015)	(.016)
Works 1/0	.009	.001	−.018	−.008	−.019	−.027
	(.012)	(.012)	(.012)	(.017)	(.017)	(.017)
Job hours per week	.003	.002	.002	−.0009	.0004	−.0004
	(.0004)	(.001)	(.001)	(.0007)	(.0008)	(.0007)
Job income per week (/100)	.04	.03	.05	−.01	.02	.00
	(.001)	(.01)	(.01)	(.01)	(.02)	(.02)
Other income per week (/100)	.00	.04	.07	.04	.02	.07
	(.01)	(.02)	(.01)	(.02)	(.02)	(.02)
Male	−.063	−.067	−.012	−.004	.055	.051
	(.008)	(.007)	(.008)	(.011)	(.011)	(.011)
Grew up in rural area	−.028	−.039	−.036	−.011	.003	−.008
	(.013)	(.013)	(.014)	(.019)	(.019)	(.019)
Grew up in a town	.001	−.035	−.018	−.036	.017	−.02
	(.011)	(.011)	(.012)	(.016)	(.016)	(.016)
Grew up in a city	.024	−.012	−.031	−.035	−.019	−.055
	(.011)	(.01)	(.011)	(.015)	(.015)	(.015)

Father's education < high school	.01	−.019	−.008	−.029	.011	−.018
	(.011)	(.011)	(.013)	(.016)	(.017)	(.017)
Mother's education < high school	−.023	.001	−.032	.023	−.033	−.009
	(.011)	(.011)	(.014)	(.016)	(.018)	(.018)
Father has some college	−.007	.003	.008	.009	.005	.014
	(.014)	(.011)	(.012)	(.018)	(.016)	(.018)
Mother has some college	.007	.002	.023	−.004	.021	.017
	(.013)	(.01)	(.011)	(.016)	(.015)	(.017)
Father college graduate	.021	.013	.025	−.009	.012	.003
	(.012)	(.01)	(.011)	(.016)	(.015)	(.016)
Mother college graduate	−.02	.009	.037	.028	.029	.057
	(.013)	(.01)	(.01)	(.016)	(.014)	(.016)
GPA	−.282	−.203	−.265	.08	−.063	.017
	(.02)	(.018)	(.02)	(.027)	(.027)	(.028)
Northeast	.022	.009	.014	−.013	.005	−.008
	(.01)	(.01)	(.011)	(.014)	(.015)	(.015)
South	−.007	−.029	−.026	−.022	.003	−.019
	(.01)	(.009)	(.01)	(.014)	(.014)	(.014)
West	−.11	−.106	−.055	.004	.051	.056
	(.011)	(.011)	(.013)	(.015)	(.017)	(.017)
Mother ever worked	.007	.016	.008	.005	−.008	−.004
	(.008)	(.008)	(.01)	(.012)	(.013)	(.013)
Fewer than 2 parents	−.001	.022	.021	.022	−.001	.022
	(.01)	(.008)	(.009)	(.013)	(.012)	(.013)
Married	.039	−.095	−.037	−.134	.058	−.077
	(.025)	(.025)	(.027)	(.036)	(.037)	(.037)
Engaged	.054	.053	.069	−.001	.016	.015
	(.015)	(.015)	(.016)	(.021)	(.022)	(.022)

(*continued*)

Table 2.4 (continued)

Covariate	1976 (1)	1986 (2)	1997 (3)	1986–76 (4)	1997–86 (5)	1997–76 (6)
Separated/divorced	.066	.004	.045	−.062	.042	−.022
	(.058)	(.042)	(.041)	(.071)	(.059)	(.071)
College-prep program	−.038	−.061	−.023	−.023	.038	.015
	(.01)	(.009)	(.009)	(.013)	(.013)	(.013)
Vocational-prep program	.032	.023	.027	−.009	.004	−.005
	(.011)	(.012)	(.014)	(.016)	(.018)	(.018)
No plan to attend college	.093	.073	.074	−.02	.001	−.019
	(.012)	(.012)	(.014)	(.017)	(.019)	(.019)
Probably will not attend college	.044	.042	.042	−.003	0	−.003
	(.012)	(.012)	(.014)	(.017)	(.018)	(.019)
Probably will attend college	.025	.004	.017	−.021	.013	−.008
	(.011)	(.01)	(.01)	(.014)	(.014)	(.015)
Days missed school	.019	.016	.016	−.003	0	−.003
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Attend religious services regularly	−.035	−.029	0	.006	.029	.036
	(.004)	(.004)	(.004)	(.006)	(.006)	(.006)
Religion important	−.029	−.014	−.005	.016	.008	.024
	(.005)	(.004)	(.005)	(.006)	(.006)	(.006)
Adjusted R^2	.123	.112	.113	.127	.117	.119
N	16,078	15,310	15,525	31,388	30,835	31,603
Joint F-test of stability interactions				[b]	[b]	[b]

[a] The MTF data provide only whether younger than 18 or 18 and older in 1997, year of birth in 1976 and 1986. We construct a measure to make this consistent across the 1976 and 1986 data, but the mean and distribution differ substantially from 1997. Accordingly, the stability tests for age in cols. 5 and 6 are probably inaccurate.

[b] Probability $> F = 0$.

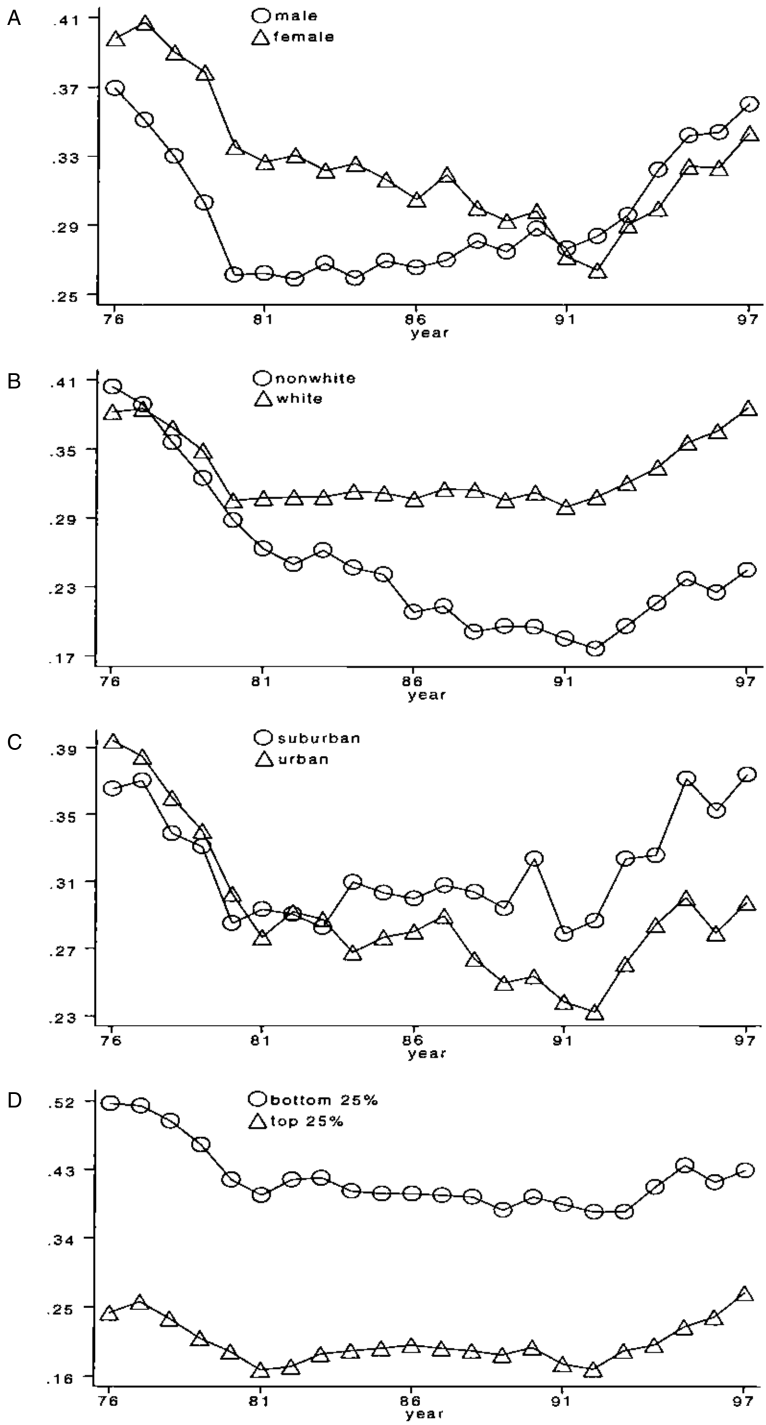

Fig. 2.5 Smoking participation by subgroup: *A*, gender; *B*, race; *C*, urban vs. suburban; *D*, GPA

2.2.5 Adding Attitude Variables

The MTF data have a variety of other attitude variables that may potentially be correlated with smoking decisions but are also potentially endogenous to the same external factors that are causing smoking to increase. While we have excluded these variables from our basic model, given the failure of changes in background characteristics to explain the time trend in smoking participation, it is of interest to know whether changes in attitudes can explain this trend. We have therefore augmented our basic models by including the host of attitude variables that may be relevant for smoking decisions: Do you disapprove of those who smoke one or more packs per day? How often do you see antismoking ads? Does smoking make a guy/girl your age look cool, insecure, independent, conforming, mature, or trying to look mature and sophisticated? Do you prefer to date a nonsmoker? Has the harm from smoking been exaggerated? Does smoking reflect bad judgment? Do you mind being around others who smoke? Is smoking a dirty habit? How much of a physical risk does smoking pose? How severe are the consequences of smoking for students in your school?

The full set of these variables is available only since 1989, so, for this exercise, we focus on the period from 1989 to 1997. Over this period, somewhat surprisingly, there do not appear to be any clear trends in attitude variables. There is a slight rise in the share of teens exposed to counteradvertising in 1996–97, although this variable has no effect on smoking decisions in our cross-sectional models.

Perhaps reflecting this lack of trends, adding these variables to our model does not improve its predictive performance. Indeed, as figure 2.6 shows, the model with attitude variables actually predicts declining smoking in 1996 and 1997, when smoking continues to rise. So changing attitudes, at least as reported in the MTF survey, do not appear to explain the rise in smoking in the 1990s either.

2.2.6 The Role of Other Substances

One interesting question is what role other risky behaviors, and in particular, the use of other substances, are playing in this time-series pattern. We explore this issue in figure 2.7, which shows, along with smoking participation, the use of alcohol, marijuana, and other drugs; in each case, *participation* is defined as any use over the past thirty days, analogously to smoking. In fact, in all three cases, the time trend looks very much like that for smoking, with declines until 1991 and increases thereafter. The correspondence is particularly striking for marijuana, although use drops off some in 1997.

Interpreting these correlated trends is difficult as there are at least three possibilities. The least likely is that there have been exogenous shifts in the use of these other substances that have led to increased smoking; although

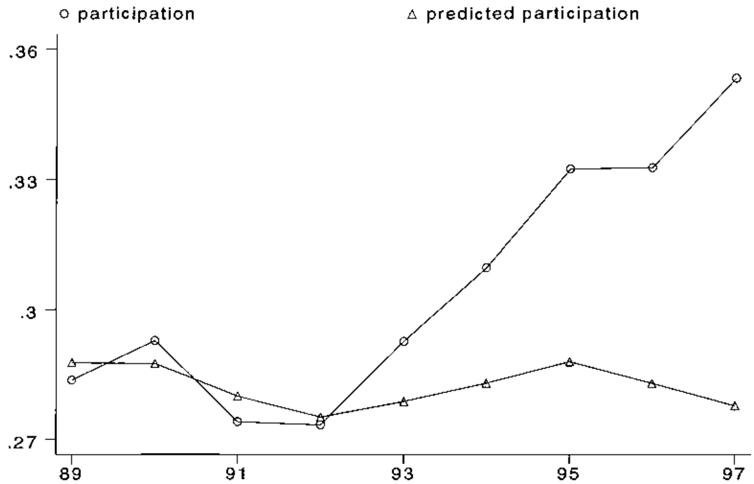

Fig. 2.6 Predicted vs. actual participation with attitude variables
Note: p-values were generated using 1989–90 coefficients.

there is no statistically convincing evidence, most work on pathways of substance use suggests that smoking is a pathway to the use of other substances, not the reverse. A second possibility is, therefore, the reverse: that the rise in smoking in the 1990s led to more use of these other substances. We present evidence below that falling cigarette prices in the 1990s can explain 30 percent or more of the trend in smoking by highschool seniors over this time period. And there are a number of studies that suggest that smoking is complementary with the use of these other substances (Chaloupka et al. 1998; Dee 1999). Thus, at least part of these trends in other substances may have been driven by falling cigarette prices.

But it seems unlikely that this can explain all these strong trends, particularly for marijuana. A third alternative is, therefore, that there were other shifts in tastes that led to greater use of all these substances. One piece of suggestive evidence for this view is the lack of correspondence between the time trends in the use of cigarettes and the use of these other substances in the 1980s: the use of other substances fell much more than did smoking. These trends cast some doubt on either of the two alternatives suggested above and favor instead a shift in taste toward all substance use in the 1990s.

2.3 Prices and Other Public Policies

Given the failure of background characteristics to explain the trend in youth smoking in the 1990s, a particularly important question is whether price movements can explain this upsurge. In fact, as figure 2.8 documents,

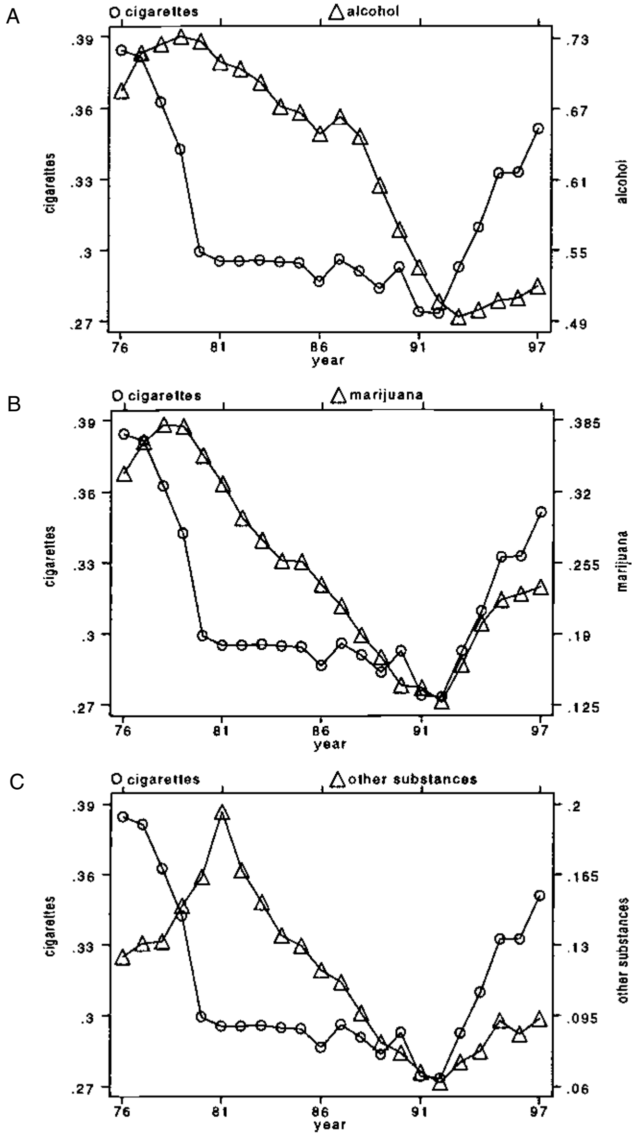

Fig. 2.7 Other substances, participation vs. smoking: *A*, alcohol; *B*, marijuana; *C*, other drugs

there was a substantial decline in prices in the early 1990s, corresponding precisely to the timing of the increase in youth smoking. Of course, youth smoking continued to increase even after prices began to rise again, but the most precipitous increases in youth smoking were over the period 1992–94, when prices were falling. Moreover, recently released data for

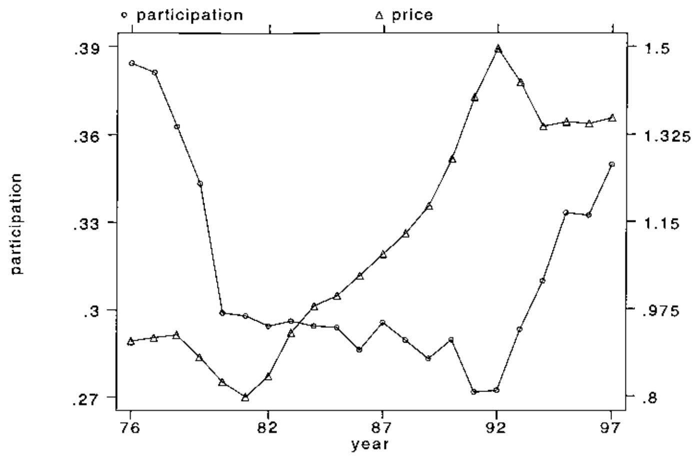

Fig. 2.8 Smoking participation vs. cigarette price

1998 reveal both a sharp increase in prices (by 10 percent) and the first decline in youth smoking of the decade, as smoking among high school seniors fell by 1.4 percentage points (3.8 percent). So the question naturally arises, Can price movements explain the time-series trend?

This ties into a more general question of the role that public policy can play in determining youth-smoking behavior. A primary determinant of price movements for cigarettes is excise-tax policy. Moreover, there are a variety of other policy tools that state and federal policy makers can pursue in an attempt to reduce youth smoking. Are these successful, and what do they suggest for future policy directions? We address these questions below.

2.3.1 Data

The public-use data from the MTF survey are not suitable for this exercise because they do not contain information on state of residence and they are not available for teens other than high school seniors. We therefore searched for data sets that had available data on smoking for repeated cross sections of teens of all ages over the 1990s as well as state identifiers so that cigarette prices and taxes could be matched to the youths. Three data sources meet this criterion, and all three are used.[2] The first, and best,

2. A fourth data set that could have been used here is the National Household Survey of Drug Abuse (NHSDA), but this was excluded for two reasons. First, the quality of these data before the mid-1990s is quite suspect owing to the use of in-home surveys without computer assistance that could suffer from bias owing to observation by parents; indeed, these data do not appear to show an increase in smoking among teens through the mid-1990s, while the more respected MTF and YRBS surveys both do. Second, there is no public-use or even private-use version of the NHSDA data available with state identifiers; only selected researchers can access these data.

source for our purposes is a restricted-use sample of the MTF data that includes information on smoking behavior, race, age, sex, and state of residence for eighth, tenth, and twelfth graders, from 1991 through 1997. We focus on 1991 as the starting point for the analysis for three reasons: this is the last year before teen smoking began to rise; this is the year in which the eighth- and tenth-grade MTF data become available; and this is the year in which the YRBS data (described next) became available.

The second data source is the YRBS data graphed earlier. These data provide information on smoking and a limited set of background characteristics for 1991, 1993, 1995, and 1997 for a sample of ninth to twelfth graders. The MTF and YRBS data are comparable in that they provide nationally representative, in-school surveys of youths. As noted earlier with reference to figure 2.2 above, they suggest different levels of smoking among teens but similar trends. The sample sizes of these surveys are also comparable. But the MTF data have the strong advantage of being a more complete survey over this sample period; the survey includes data on thirty-five states in every year from 1991 to 1997 and a total of 277 state-year pairs over this time period. On the other hand, the YRBS is a survey that is phasing into coverage of the entire nation, with only ten states in the survey in every year and only 102 state-year pairs over this time period.[3] As a result, while from 1991 to 1997 there are fifty-nine tax changes to be studied in the MTF data, there are only fourteen in the YRBS data. Thus, the results that we obtain with the MTF data are more robust to the specification check pursued below, and we will rely on them as our primary estimates.

The third data source is the Vital Statistics Natality detail files. These data are a census of birth certificates for the United States, with approximately 4 million observations per year. The data contain information, since 1989, on the smoking behavior of the mother during pregnancy, and they are available for virtually every state in every year from 1991 on, providing even more variation than the MTF survey (seventy-three tax changes over this time period).[4] But these have the disadvantage of being focused solely on one particularly select group of teens, those having children before their nineteenth birthday. Owing to the enormous size of the Natality database (over 300,000 teen mothers per year for our seven years of analysis), we do not analyze micro data on smoking rates by mothers. Rather, we group these data into state $\times$ year $\times$ age cells and analyze cell mean rates of smoking and conditional smoking intensity where the regressions are weighted by cell size.

3. All our YRBS means and estimates are weighted using weights designed to reproduce national representativeness.

4. Smoking data are not available for California, Indiana, and South Dakota for any year and for New York for 1991–93; smoking data for New York City, but not the remainder of the state, are available beginning in 1994.

Table 2.5 Means of MTF, YRBS, and Natality Price-Regression Samples

	Any Smoking	Cigarettes/ Day When Smoking	Real Price	Real Excise Tax
MTF Data, 1991–97:				
12th grade (N = 91,567)	.309	7.21	1.39	.21
	(.462)	(8.87)	(.17)	(.10)
8th–10th grade (N = 213,527)	.217	5.42	1.38	.21
	(.412)	(8.38)	(.17)	(.10)
8th–12th grade (N = 336,665)	.246	6.13	1.39	.21
	(.431)	(8.63)	(.17)	(.10)
YRBS data, 1991, 1993, 1995, 1997:				
12th grade (N = 14,346)	.358	6.06	1.28	.21
	(.479)	(6.13)	(.15)	(.10)
9th–11th grade (N = 38,932)	.315	5.15	1.28	.21
	(.464)	(5.70)	(.15)	(.10)
9th–12th grade (N = 53,278)	.326	5.42	1.28	.21
	(.469)	(5.85)	(.15)	(.10)
Natality data, 1991–97:				
17–18 years old (N = 666)	.180	10.23	1.23	.19
	(.075)	(1.31)	(.14)	(.10)
13–16 years old (N = 1,319)	.127	9.21	1.22	.19
	(.071)	(1.70)	(.14)	(.10)
13–18 years old (N = 1,985)	.164	9.93	1.22	.19
	(.078)	(1.51)	(.14)	(.10)

Note: Authors' tabulations of MTF, YRBS, and Natality data. All prices and taxes in 1982 dollars. Micro data for MTF and YRBS; cell-level data for Natality, as described in the text, with means weighted by cell count. "Cigarettes per day when smoking" is cigarettes per day smoked on days when smoking. Standard deviations are given in parentheses.

The means for all three data sets are presented in table 2.5. We consider two measures of smoking: *participation,* defined as any smoking over the past month, and *conditional intensity.*[5] The latter measure has the difficulty as a dependent variable that, if there are policy effects on participation, there could be sample-selection bias to the effects on conditional intensity; for example, if higher prices reduce smoking participation and those who quit are low-intensity smokers, then higher prices could be associated with higher intensity among those who remain smokers through this composition effect.

As noted above, smoking rates are somewhat higher in the YRBS than in the MTF data sets; for high school seniors over this time period, participation rates are 31 percent in the MTF and 36 percent in the YRBS. Smoking rates are much lower for teen mothers; for seventeen- to eighteen-

5. In the MTF and YRBS data, conditional intensity is measured in intervals, and we use the midpoint of each interval for intensity. In the Natality data, the intensity question is continuous.

year-old teen mothers, the smoking rate is only 18 percent. However, smoking intensity for high school seniors is higher in the MTF survey, averaging 7.2 cigarettes per day, compared to 6.1 cigarettes per day in the YRBS. Intensity is even higher for teen mothers, averaging over 10 cigarettes per day for seventeen- to eighteen-year-old mothers. Thus, smoking among teen mothers appears less frequent but more intense when these women are participating. Note that this is not just a male/female difference; smoking participation among males and females is very similar in the YRBS and MTF surveys, and intensity is actually somewhat higher for males.

Smoking is less frequent, and smoking intensity lower, for younger teens in all three surveys. In the MTF and YRBS surveys, the full sample results are weighted more closely to the results for younger teens since the samples of younger teens are much larger; in the Natality data, they are weighted toward older teens since there are so many more births to older teens in the data.

The key independent variables to be used in the analysis are state-level measures of prices, taxes, and other policies. Prices as of November of each year for each state are provided in Tobacco Institute (1998), and a monthly series can be constructed from information on taxes given in that volume as well. The MTF and YRBS surveys are both conducted in the spring, so we use an average of the prices from November of year $t - 1$ and November of year t as our price measure and the tax rate as of February as our tax measure. For the Natality data, we know the actual month of the birth, so we use the tax rate from that month of birth.

The two dimensions of antismoking policy that we explore are clean-air regulations and youth-access restrictions. Clean-air regulations, which are described in substantial detail in Jacobson and Wasserman (1997), are laws that restrict smoking in certain public areas. We constructed a comprehensive database of such laws using information from the state legislative records, Coalition on Smoking OR Health (various years), and the Centers for Disease Control website (http://www2.cdc.gov/nccdphp/osh/state/). While there are a large number of such laws, we categorize them into five categories: restrictions on private workplaces, public (e.g., state and local government) workplaces, restaurants, schools, and other public places (e.g., elevators, public transportation, theaters).

Youth-access restrictions are laws designed to limit youth purchases of tobacco products since, while youth smoking is legal, selling cigarettes to youths is not. As reviewed in Jacobson and Wasserman (1997), states have therefore endeavored to implement barriers to youth access to cigarette purchase along various dimensions. Categorizing these state efforts is difficult as there are a large number of different regulatory tools, and states enforce them with varying degrees of rigor. We therefore rely on the expert opinion of a panel convened by the National Cancer Institute (NCI) to evaluate state laws limiting youth access to cigarettes (Alciati et al. 1998). This panel reviewed a wide variety of state laws in this area and formed an index to capture their overall "bite" in limiting youth access. Its members considered nine categories of state regulation and provided a score within each, which is aggregated into a total index. Their index is available for 1993–96; we have followed their rules, in consultation with them, to use state laws to extend the data back to 1991 and forward to 1997. We did augment their index by adding some finer disaggregation of categories and by correcting some inconsistencies with actual legislation. We also added three additional categories that they did not consider: advertising restrictions; licensing of retailers; and penalties on minors themselves for tobacco purchase. The index also reflects state efforts to comply with the recent Synar amendments proposed by the Food and Drug Administration.

We describe in more detail how this index was created in the appendix. Appendix table 2A.1 also shows means for the MTF data of the clean-air and youth-access-index variable. The average value of our access index is roughly 12 (where the maximum possible value is 26); about half of students were subject to restrictions on smoking in private workplaces, whereas restrictions in restaurants, government work sites, schools, and other sites were more common.

The other frequently discussed public-policy intervention meant to reduce youth smoking is counteradvertising. While this is a major focus of very recent discussions, over the time period studied in this paper (ending in 1997) there was very little counteradvertising in most states.[6]

2.3.2 Empirical Strategy

For all three data sets, we pursue a similar estimation strategy, considering the effect of prices and public policies on smoking in the following regression framework:

$$(1) \quad \text{SMOK}_{ijt} = \alpha + \beta\text{PRICE}_{jt} + \delta\text{ACCESS}_{jt} + \gamma\text{CLNAIR}_{jt} + \eta X_{ijt} + \lambda S_j + \nu T_t + \varepsilon_{ijt},$$

where i indexes individuals, j indexes states, t indexes years, SMOK is a measure of smoking (participation or conditional intensity), PRICE is the price per pack of cigarettes (inclusive of taxation), ACCESS is the index of access restrictions, CLNAIR is a set of dummy variables for clean-air regulations, X is a set of individual control variables (which varies by data set), S is a set of state dummies, and T is a set of year dummies.

6. Based on conversations with experts at the Office on Smoking and Health at the Centers for Disease Control. The available data suggest that only a few states had major programs in place by 1997 and that the spending on those programs was fairly constant over our time period.

By including a complete set of state fixed effects, this regression surmounts any problems with fixed differences across states in both the level of prices and the propensity to smoke, for example, owing to tobacco-production intensity. However, two potential concerns remain with the interpretation of the price coefficient in this specification. First, if tobacco companies are doing any state-specific pricing, then prices may be endogenous to smoking levels. While 80 percent of the variation in prices within states over time is driven by tax changes (Gruber and Koszegi 2000), there remains 20 percent that is possibly demand driven. We therefore instrument prices with the tax rate in the state to provide identification solely from tax-induced price movements. All estimates presented below are from such instrumental-variables models.

Second, the identifying assumption of this estimator is that within-state changes in taxation (and other public policies) are not themselves determined by youth-smoking behavior. It is plausible that tax policy is set as a function of smoking in a state, with revenue-maximizing legislators and an inelastically demanded good. Since youths smoke only about 2 percent of the total number of cigarette packs smoked annually, it is doubtful that youth smoking per se is driving tax policy. But it is possible that youth smoking is correlated with some of the same factors that drive adult smoking and possibly therefore with tax setting. With this short panel it is difficult to address this concern definitively, but we discuss an attempt to do so below.

Another important estimation issue is that we are using a large number of observations in each of these data sets but really have variation in our key variables only across state and year cells. As a result, all regressions are estimated with the standard errors corrected for within-state-year-cell correlations in the error terms.

2.3.3 Results—MTF Data

We begin the analysis by focusing on the MTF data, the MTF being, as discussed above, the highest-quality source of nationally representative data. We also start with high school seniors, to parallel most previous work in this area.

The results of estimating equation (1) for high school seniors are presented in the first two columns of table 2.6. The most important finding is that there is a negative and statistically significant effect of prices on smoking participation. The implied elasticity at the sample mean is −0.67. The effect on conditional intensity is negative but insignificant, implying a small elasticity of conditional intensity of −0.06. As noted earlier, it is difficult to interpret these estimates as the pool of smokers is changing. In particular, it seems likely that those who quit smoking as the price rises have the lowest ex ante intensity, which would lead to a positive composition bias to the estimates.

Table 2.6 **Effect of Price and Regulations on Youth Smoking in the MTF Data**

	12th Graders		8th & 10th Graders		8th–12th Grader	
	Participation	Cigarettes/Day	Participation	Cigarettes/Day	Participation	Cigarettes/Day
Price	−.148	−.310	−.033	−.013	−.055	−.129
	(.078)	(2.388)	(.035)	(1.243)	(.034)	(1.132)
	[−.666]	[−.059]	[−.21]	[−.003]	[−.311]	[−.029]
Access index/100	.084	−3.48	.033	−5.520	.066	−5.22
	(.106)	(2.76)	(.060)	(1.640)	(.056)	(1.49)
Private workplace	−.041	.462	−.006	1.464	−.021	1.045
	(.028)	(.589)	(.017)	(.489)	(.017)	(.348)
Government workplace	.022	−1.128	−.019	−.813	−.001	−.834
	(.026)	(.517)	(.015)	(.394)	(.013)	(.251)
Restaurant	.032	2.166	.012	.868	.016	1.318
	(.030)	(.783)	(.017)	(.615)	(.015)	(.483)
Schools	.050	.931	.044	.788	.040	.645
	(.030)	(.915)	(.018)	(.553)	(.015)	(.392)
Other	−.080	−2.791	−.032	−1.424	−.038	−1.617
	(.041)	(1.234)	(.020)	(.775)	(.019)	(.621)
Male	.016	1.235	−.009	.926	−.001	1.041
	(.004)	(.115)	(.003)	(.085)	(.003)	(.069)
Nonwhite	−.153	−1.908	−.076	−.436	−.1	−.962
	(.007)	(.171)	(.004)	(.119)	(.005)	(.115)
Grade 8			−.088	−2.746	−.185	−3.815
			(.018)	(.576)	(.014)	(.538)

(*continued*)

Table 2.6 (continued)

	12th Graders		8th & 10th Graders		8th–12th Grader	
	Participation	Cigarettes/Day	Participation	Cigarettes/Day	Participation	Cigarettes/Day
Grade 10					−.098	−1.055
					(.019)	(.632)
Grade 8, age ≤ 13			−.027	.217	.03	.181
			(.018)	(.573)	(.014)	(.519)
Grade 8, age ≥ 14			.026	1.829	.083	1.8
			(.018)	(.572)	(.014)	(.515)
Grade 10, age ≤ 15			−.037	−1.211	.019	−1.334
			(.003)	(.104)	(.019)	(.625)
Grade 10, age ≥ 16					.055	−.119
					(.019)	(.627)
Grade 12, age ≤ 17	−.013	−.546			−.012	−.512
	(.003)	(.113)			(.003)	(.112)
No. of observations	106,539	32,868	230,126	49,927	336,665	82,795

Note: Standard errors (corrected for state/year clustering) are given in parentheses, standard errors in square brackets. All regressions also include the full set of state and year fixed effects.

We obtain much less convincing evidence for the role of other policies, however. There is a negative effect of access restrictions on the quantity of cigarettes smoked, but the coefficient is not significant. The only clean-air restrictions for which there are significant negative effects are for restrictions on government workplaces (in terms of conditional quantity smoked) and for restrictions on other sites (for both participation and quantity smoked). It seems highly unlikely that there is a true causal effect of restrictions in government work sites on youths; it is perhaps more plausible that restrictions on other sites, such as public transportation, might matter.

Does this significant price effect suggest that we can explain the time-series movements by the price decline of the early 1990s? From 1991 through 1997, the price of cigarettes fell by fourteen cents. At our estimated coefficient on participation, this explains 26 percent of the 8 percentage point rise in smoking for high school seniors over this time period. Thus, price is playing an important role but not the dominant one.

The next four columns of table 2.6 investigate the effect of prices and policies on younger smokers (eighth and tenth graders). Interestingly, there is little effect of price on the smoking of younger teens. The coefficients on both participation and intensity are insignificant for eighth to tenth graders and, as a result, for the full sample of eighth to twelfth graders. Over the full sample, the price elasticity for participation is only −0.31, with a conditional-intensity elasticity of −0.03. This casts further doubt on the role of price as the primary determinant of the time-series trend since the trends in smoking are quite similar for eighth to tenth graders and for high school seniors. On the other hand, even though younger smokers are less price sensitive, their estimated price sensitivity is still nontrivial; it is similar, for example, to the elasticity of smoking participation estimated for adults by Evans, Ringel, and Stech (1999).

The effects of other policies on the smoking of eighth to tenth graders are more interesting than are their effects on the smoking of high school seniors. We now estimate a highly significant effect of youth-access restrictions on the conditional quantity of cigarettes consumed by younger teens, which is not subject to selection bias owing to the insignificant effects on participation. This coefficient suggests that moving from the lowest to the highest value of this index would lower smoking intensity by 1.38 cigarettes per day, or 25 percent. This is interesting because it is indeed possible that, by raising the hassle costs of obtaining cigarettes, access restrictions do not deter youths from smoking at all but rather limit the extent to which they do smoke. We also again obtain negative effects of government-work-site restrictions on smoking intensity and negative effects of other clean-air restrictions on both intensity and participation.

There is a paucity of control variables available in these restricted MTF data. We do find that smoking rises with grade. The age variables are defined only conditional on grade (owing to restrictions in the MTF data), but they have the expected pattern: older children within each grade smoke more. Nonwhite youths are much less likely to smoke, and there is a positive effect of being male among high school seniors but a negative effect among eighth and tenth graders, with the result that, for the full sample, the effect is insignificant. As we showed above, the positive effect for high school seniors of being male becomes negative when other covariates available in the public-use data are included.

2.3.4 YRBS and Natality Data

As emphasized above, a key advantage of our analytic strategy is that we have brought several data sets to bear on this question in order to analyze the most consistent patterns of effects of public policy on smoking. In this spirit, tables 2.7 and 2.8 replicate the results for the MTF data in the YRBS and Natality data, once again for older teens (seniors in the YRBS, seventeen- to eighteen-year-olds in the Natality data), younger teens (ninth to eleventh graders in the YRBS, thirteen- to sixteen-year-olds in the Natality data), and overall.

The most strikingly consistent finding across all three data sets is the negative effect of prices on smoking by older teens. In the YRBS, the elasticities are enormous: there is an elasticity of −1.5 on participation *and* an elasticity of −1.5 on conditional intensity. In the Natality data, the elasticities are more modest, with an elasticity of participation of −0.38 and an elasticity of conditional intensity of −0.15. It is perhaps not surprising that the elasticity is smaller for teen mothers than for other groups given that the very fact that these women are smoking reveals their insensitivity to information about the hazards posed by smoking to newborns. This smaller elasticity is not due to the gender composition of the sample. In both the MTF and the YRBS data, we estimate very similar elasticities for males and females; the elasticities are somewhat higher for males in the MTF and somewhat higher for females in the YRBS.

Moreover, there is a consistent finding of a much smaller effect of prices on young smokers. In the YRBS data, the elasticity of participation is wrong signed, and the elasticity of conditional intensity insignificant, for ninth to eleventh graders. In the Natality data, both coefficients are right signed but insignificant.

Why might we be finding that older teens are more price sensitive? There are several possible explanations. One is that smoking means different things at different ages. Younger teens may view participation as pure experimentation, which is less well described by economists' models of addictive behavior (e.g., Becker and Murphy 1988) and which is as a result less sensitive to such economic factors as price. But, by the time these youths have become high school seniors, they have completed their experimentation phase, and smoking follows expected relations with price and other economic factors. This type of story is consistent with the fact that

Table 2.7 **Effect of Price and Regulations on Youth Smoking in the YRBS Data**

	12th Graders		9th–11th Graders		9th–12th Graders	
	Participation	Cigarettes/Day	Participation	Cigarettes/Day	Participation	Cigarettes/Day
Price	−.429	−7.462	.103	−.912	−.032	−2.228
	(.200)	(3.461)	(.134)	(1.847)	(.103)	(1.937)
	[−1.534]	[−1.576]	[.419]	[−.227]	[−.126]	[−.526]
Access index/100	−.060	.461	−.098	−.804	−.098	−.316
	(.169)	(3.985)	(.092)	(2.223)	(.088)	(2.308)
Private workplace	.006	−2.723	.064	1.146	.051	.905
	(.047)	(1.742)	(.046)	(.552)	(.042)	(.476)
Government workplace	−.075	−.168	−.088	−1.800	−.087	−1.971
	(.032)	(1.459)	(.037)	(.473)	(.034)	(.396)
Restaurant	−.162	−1.435	−.006	.383	−.050	−.447
	(.028)	(1.466)	(.025)	(.627)	(.025)	(.783)
Schools	.006	−.578	.008	.517	.008	.578
	(.060)	(1.161)	(.045)	(.567)	(.034)	(.533)
Other	.012	6.164	−.015	.775	.002	1.842
	(.065)	(2.260)	(.054)	(.813)	(.048)	(.950)
Male	.002	1.283	−.001	.846	.000	.984
	(.014)	(.268)	(.010)	(.164)	(.008)	(.146)
White	.048	.585	.046	−.148	.044	.071
	(.028)	(.634)	(.014)	(.506)	(.012)	(.382)
Black	−.206	−3.16	−.128	−2.69	−.149	−2.73
	(.033)	(.621)	(.018)	(.541)	(.015)	(.417)
Hispanic	−.013	−2.10	.003	−2.09	−.002	−2.02
	(.030)	(.627)	(.016)	(.448)	(.014)	(.354)
Grade 10			−.055	.261	−.055	.266
			(.010)	(.366)	(.011)	(.363)

(*continued*)

Table 2.7 (continued)

	12th Graders		9th–11th Graders		9th–12th Graders	
	Participation	Cigarettes/Day	Participation	Cigarettes/Day	Participation	Cigarettes/Day
Grade 11			−.076	.289	−.075	.379
			(.017)	(.455)	(.017)	(.452)
Grade 12					−.092	.103
					(.022)	(.551)
Age 15	−.052	12.28	.079	1.04	.079	1.00
	(.211)	(6.70)	(.013)	(.369)	(.013)	(.370)
Age 16	−.044	−14.86	.146	1.39	.146	1.36
	(.161)	(4.93)	(.016)	(.443)	(.016)	(.444)
Age 17	−.106	−15.50	.191	2.28	.191	2.13
	(.145)	(4.62)	(.020)	(.542)	(.020)	(.544)
Age 18	−.086	−14.95	.244	2.71	.218	2.53
	(.144)	(4.66)	(.035)	(.772)	(.023)	(.618)
Father a high school graduate	.014	−.349	−.007	−.384	−.002	−.362
	(.022)	(.431)	(.015)	(.252)	(.012)	(.215)
Father has some college	.008	−1.07	−.035	−.488	−.025	−.660
	(.027)	(.549)	(.018)	(.410)	(.014)	(.332)
Father college graduate	−.004	−1.26	−.055	−.905	−.042	−.971
	(.024)	(.493)	(.019)	(.305)	(.015)	(.232)
Mother a high school graduate	−.058	−.701	−.014	−.640	−.027	−.686
	(.024)	(.403)	(.017)	(.416)	(.015)	(.339)
Mother has some college	−.031	−.382	−.030	−.935	−.030	−.751
	(.030)	(.508)	(.013)	(.429)	(.015)	(.326)
Mother college graduate	−.044	−.785	−.038	−.908	−.041	−.891
	(.027)	(.446)	(.017)	(.423)	(.015)	(.338)
No. of observations	14,346	4,429	38,932	11,368	53,278	15,797

Note: Standard errors (corrected for state/year clustering) are given in parentheses, standard errors in square brackets. All regressions also include the full set of state and year fixed effects.

Table 2.8 Effect of Price and Regulations on Youth Smoking in the Natality Data

	17–18-Year-Olds		13–16-Year-Olds		13–18-Year-Olds	
	Participation	Cigarettes/Day	Participation	Cigarettes/Day	Participation	Cigarettes/Day
Price	−.055	−1.209	−.025	−.436	−.047	−1.003
	(.018)	(.527)	(.018)	(.638)	(.016)	(.440)
	[−.376]	[−.145]	[−.240]	[−.058]	[−.353]	[−.124]
Access index/100	.023	−1.771	−.006	−1.010	.013	−1.485
	(.026)	(.718)	(.024)	(1.106)	(.023)	(.651)
Private workplace	.009	.037	.013	.555	.011	.134
	(.007)	(.329)	(.007)	(.466)	(.005)	(.254)
Government workplace	.000	.014	−.005	.180	−.002	.021
	(.003)	(.109)	(.004)	(.160)	(.003)	(.089)
Restaurant	−.012	−.493	.002	−.972	−.007	−.597
	(.004)	(.233)	(.005)	(.355)	(.004)	(.193)
Schools	.002	−.195	.006	−.613	.004	−.316
	(.004)	(.175)	(.005)	(.220)	(.004)	(.148)
Other	−.003	.231	−.011	.299	−.006	.300
	(.005)	(.238)	(.006)	(.327)	(.004)	(.205)
% white	.179	2.16	.131	−.839	.171	−.016
	(.093)	(2.67)	(.056)	(2.59)	(.046)	(1.61)
% black	−.263	2.09	−.139	−2.40	−.203	−2.42
	(.096)	(2.80)	(.052)	(2.39)	(.044)	(1.49)
% Hispanic	−.255	−1.90	−.238	.34	−.238	−1.15
	(.044)	(1.64)	(.028)	(1.13)	(.025)	(.896)

(*continued*)

Table 2.8 (continued)

	17–18-Year-Olds		13–16-Year-Olds		13–18-Year-Olds	
	Participation	Cigarettes/Day	Participation	Cigarettes/Day	Participation	Cigarettes/Day
Age 14			.017	−.158	.009	−.187
			(.004)	(.366)	(.004)	(.363)
Age 15			.024	.317	.009	.213
			(.005)	(.354)	(.005)	(.349)
Age 16			.033	.734	.011	.557
			(.007)	(.375)	(.006)	(.354)
Age 17					0.14	.912
					(.007)	(.368)
Age 18	.003	.502			.019	1.362
	(.002)	(.055)			(.007)	(.376)
No. of observations	1,319	1,189	666	666	1,985	1,855

Note: Standard errors (corrected for state/year clustering) are given in parentheses, standard errors in square brackets. All regressions also include the full set of state and year fixed effects.

younger teens who smoke consume a smaller quantity of cigarettes and with the evidence (presented below) that the demographic correlates of socioeconomic disadvantage (race and parents' education) lead to higher price sensitivity for high school seniors but not for younger teens.

Alternatively, younger teens may be pursuing smoking in order to be accepted by a peer group, and, by the time they are high school seniors, they have been accepted into the group. If smoking as a younger teen regardless of price is required to gain acceptance to a peer group but, once within the group, peer effects have their usual multiplier effect on price elasticities, then this would yield low price elasticities on younger teens and higher ones on older teens. Finally, it may simply be that teens using their own money are more price elastic than are those who rely on money from parents (obtained either complicitly or illicitly).

In contrast to the robust and significant effect of prices on youth smoking, however, we obtain much more mixed evidence on a role for other public policies. There is no public-policy variable other than price that is significant for either age group in all three data sets or even in both the data sets representing the full teen population (the MTF and the YRBS). The most robust finding appears to be for the effect of youth-access restrictions on the quantity of cigarettes smoked, which is negative for both younger teens and high school seniors and significant for the latter in the Natality data; the magnitudes of the effects for teen mothers are much smaller than for all teens in the MTF. We also find negative effects of clean-air regulations for restaurants (which are significant for participation by high school seniors in the Natality and YRBS data) and of clean-air regulations for other sites such as public transportation (which are highly significant in the MTF and are negative and marginally significant for younger teen participation in the Natality data).

The coefficients on the covariates in the YRBS generally conform to expectations. There is little effect of sex and an enormous negative effect of race on smoking rates. Smoking rates fall with grade (conditional on age) but rise strongly with age so that, on average, smoking is rising with grade as well. Echoing the findings presented above using MTF micro data, there is little effect of parents' education on smoking, at least for high school seniors; there is some evidence that having more-educated parents leads to less smoking for eighth and tenth graders.[7] There are very few covariates in the Natality data, but they do confirm that smoking rises with age and is much higher among whites.[8] One interesting difference between the Natality and the YRBS data sets is that smoking among Hispan-

7. Parents' education is not available in the 1991 YRBS and is missing for a number of respondents in other years, so the omitted category here is either parents who are high school dropouts or parents for whom education is missing.

8. Note that the covariates here are cell means, e.g., percentage white in the age-year-state cell.

ics is much lower in the Natality data but is only marginally lower in the YRBS. Unfortunately, owing to the restricted nature of the MTF data, we cannot bring that evidence to bear on racial distinctions; the data report only whether the youth is white or nonwhite.

In summary, there are four conclusions to be drawn from these basic results. First, there is a sizable and significant negative effect of price on smoking by high school seniors, particularly for the decision to participate. This finding is robust to all three data sets. We estimate elasticities that range from −0.38 (Natality) to −1.5 (YRBS), but the most reliable estimate is probably the elasticity of −0.66 from the MTF data. Second, however, we find that there is no effect of price on younger teens so that, in the aggregate, the price effects on teen smoking are weak, with overall price elasticities for teens ranging from −0.13 (YRBS) to −0.35 (Natality). Third, there is some suggestion that laws that restrict youth access to tobacco products reduce the intensity of youth smoking but not smoking participation. Finally, there is little consistent evidence that clean-air restrictions matter for youth-smoking decisions.

We have also considered specification tests to address two potential concerns about this exercise. The first is that, for two of our three data sets, we have data only on students, not on high school dropouts. This may lead to a biased estimate of the aggregate teen elasticity if dropouts are differentially price sensitive, but, since the quoted statistics on teen smoking come from the in-school surveys used here, these are the relevant data for trying to explain time trends. More perniciously, however, if high school dropout rates are somehow correlated with tobacco taxation, then there could be a sample-selection bias to our estimates. We have included in the regressions reported here data on dropout rates by state and year. In no case did including these control variables much change our coefficient on price, nor did the variables themselves enter significantly in our regressions. So this suggests no bias from selection on who remains in school as taxes change.

Another more serious concern, mentioned earlier, is that excise taxes may be endogenous, if not directly to youth smoking, then to aggregate cigarette consumption, which may in turn be correlated with youth-smoking decisions (either positively, through adult and peer effects, or negatively, through teen contrariness). This general endogeneity concern is impossible to address perfectly, in particular given the very short panel of data with which we are working. One approach to addressing the specific concern that our finding is driven by an omitted correlation of youth and adult smoking is to include directly in the regression a control for aggregate cigarette consumption in that state in the previous year. Once again, the results are very robust (with the exception of a decline in the participation elasticity estimate to −1.2 in the YRBS data), and the coefficients on lagged packs per capita themselves are generally insignificant. Thus, it appears that correlations between aggregate consumption and both tax setting and youth smoking cannot explain our findings.

2.3.5 Heterogeneity

The analysis thus far has considered youth smoking as a simple aggregate and has not explored the heterogeneity in policy effects across different groups of youths. But there are considerable differences across youths in their underlying propensity to smoke. Most noticeable are racial differences, and the YRBS suggests some differences by parents' education as well, at least for younger teens. In this section, we explore the heterogeneity in the price responsiveness of young smokers. In particular, we assess whether socioeconomically disadvantaged youths are more responsive to prices, suggesting a cross-elasticity between price and income.

The results for a racial decomposition of smoking responsiveness are presented in table 2.9. For the MTF sample, we can compare only white and nonwhite youths since this is the only racial distinction available in these restricted data. In the YRBS and Natality data, we can compare white and black youths more specifically. When we have estimated models for whites and all nonwhites in these other data sets, the results are similar but more muted than those for whites and blacks.

The results for the MTF and YRBS data for high school seniors are striking: there is much higher price responsiveness among blacks than among whites. In the MTF, the price elasticity of participation for white high school seniors is only −0.35 and is insignificant, and there is a positive coefficient on conditional intensity. But for black high school seniors the elasticity of participation is an enormous and statistically significant −2.32, and there is a significant elasticity of conditional intensity of −2.03 as well. In the YRBS, the results are even more extreme, with an elasticity of −0.63 for white smoking participation and an unreasonable elasticity of −9.3 for blacks; this implausibly large estimate likely reflects the effect of examining a small number of tax changes in only a subsample of the data. In the Natality data, on the other hand, the results are reversed: the price elasticity for whites is slightly larger than for the full sample, and there is no price responsiveness of participation among blacks (although there is a large negative effect on conditional intensity).

For younger teens, there is a much less clear racial pattern. There are no significant elasticities for either whites or blacks in the MTF or YRBS data. For the Natality data, the elasticities are once again significant for whites and wrong signed for blacks.

One explanation for this higher price sensitivity among black youths is lower income. A number of articles have found price elasticities for adult smokers that fall with income (e.g., Evans, Ringel, and Stech 1999). If the same is true for teens, then the lower incomes of black high school seniors may explain their increased responsiveness. Unfortunately, none of these

Table 2.9 **Price-Coefficient Heterogeneity by Race**

	Older Teens		Younger Teens		All Teens	
	Participation	Cigarettes/Day	Participation	Cigarettes/Day	Participation	Cigarettes/Day
MTF data:						
Whites	−.091	.721	−.054	−1.611	−.057	−.848
	(.010)	(2.637)	(.047)	(1.214)	(.041)	(1.225)
	[−.350]	[.130]	[−.300]	[−.393]	[−.277]	[−.181]
Nonwhites	−.323	−7.690	.025	4.962	−.039	2.417
	(.163)	(3.749)	(.050)	(2.843)	(.045)	(2.395)
	[−2.324]	[−2.03]	[.226]	[1.488]	[−.327]	[.691]
YRBS data:						
Whites	−.198	−13.70	.083	.470	.026	−3.563
	(.271)	(4.554)	(.177)	(2.326)	(.123)	(2.344)
	[−.628]	[−2.662]	[.303]	[.106]	[.092]	[−.775]
Blacks	−1.187	−22.78	−.132	12.48	−.369	11.24
	(.485)	(20.50)	(.372)	(12.44)	(.351)	(10.81)
	[−9.259]	[−8.248]	[−.874]	[4.958]	[−2.530]	[4.393]
Natality data:						
Whites	−.079	−.934	−.060	.307	−.079	−.639
	(.023)	(.556)	(.023)	(.682)	(.021)	(.453)
	[−.412]	[−.109]	[−.385]	[.040]	[−.433]	[−.076]
Blacks	.026	−3.357	.033	−2.809	.028	−3.256
	(.017)	(1.286)	(.019)	(2.113)	(.015)	(1.144)
	[.534]	[−.539]	[1.115]	[−.494]	[.671]	[−.539]
YRBS data, parents' education:						
Mother & father high school	−1.266	−2.036	.79	10.806	.207	4.464
dropouts or graduates	(.583)	(8.497)	(.369)	(5.889)	(.206)	(5.4)
	[−4.387]	[−.401]	[2.721]	[2.514]	[.715]	[.103]
Mother & father have some	−.067	−10.068	.228	−1.432	.157	−3.285
college or are college graduates	(.238)	(4.159)	(.231)	(3.144)	(.188)	(2.353)
	[−.236]	[−2.393]	[.956]	[−.390]	[.645]	[−.874]

Note: Coefficient on price from regressions in the MTF (first panel), YRBS (second and fourth panels), and Natality (third panel) data. Regressions include all the controls shown in tables 2.2–2.5 above, including the full set of state and year fixed effects. Standard errors (corrected for state/year clustering) are given in parentheses, price elasticity in square brackets.

data sets contain information on income. But the YRBS data do have an excellent proxy for permanent income: parents' education.

In the final panel of table 2.9, we therefore present results that divide the YRBS sample into those whose mother and father are high school dropouts or graduates and those whose mother and father have some college or are college graduates. There is a striking difference across these groups for high school seniors: the elasticity of participation is −4.4 for the low-education group and is only −0.2 for the high-education group (and is highly insignificant for the latter). This is offset to some extent by a very large conditional-intensity elasticity for the high-education group. But, overall, there is a clear negative correlation of price responsiveness and socioeconomic status measured this way. Once again, however, there is no clear relation for younger teens; the elasticity of participation is actually positive and significant for younger teens with less-educated parents and is positive and insignificant for younger teens with more highly educated parents.

Taken together, the results presented in table 2.9 suggest two important conclusions. First, for high school seniors, there is a strong cross-elasticity between price and income. Lower-income groups, whether measured racially or by parents' education, are much more price sensitive. Moreover, the fact that the results by race for teen mothers are reversed is consistent with the fact that, while white teens are much more advantaged than black teens as a whole, among teen mothers blacks actually have a higher median income.[9] Second, there continues to be evidence that the smoking decisions of younger teens are determined primarily by noneconomic factors. Not only are younger teens not price sensitive, but there is also no pattern of increased relative sensitivity with income, as proxied by either race or parents' education.

2.4 Intertemporal Correlation in Youth Smoking

While the previous discussion has suggested that seniors are responsive to the price of cigarettes, it does not resolve the more important question: the long-run intertemporal implications of youth smoking. That is, what does rising youth smoking today imply for the adult smoking rate in the future? If shifts in youth smoking imply long-run increases in adult smoking, then we are headed toward a substantial reversal in the downward trend in smoking in the United States.

There are two extreme possibilities for thinking about the linkage between youth smoking and adult smoking. At one extreme, which we label

9. Specifically, in 1997, median family income for white teens was $47,000, while it was only $25,000 for black teens. At the same time, median income was $3,000 among white teen mothers and $4,300 among black teen mothers.

Table 2.10 Age of Initiation versus Current Smoking

	Start at Age 12–14	Start at Age 15–17	Start at Age 18–20	Start at Age 21–25
Those age 28+:				
Smoke now?	.52	.49	.46	.52
Smoke every day?	.45	.41	.36	.42
Cigarettes per day	19.6	18.1	15.8	16.7
Those age 33+:				
Smoke now?	.49	.46	.43	.49
Smoke every day?	.42	.38	.35	.40
Cigarettes per day	20.4	18.6	16.3	16.8
Those age 38+:				
Smoke now?	.45	.43	.40	.46
Smoke every day?	.40	.36	.33	.38
Cigarettes per day	21.2	19.1	16.8	17.4

Note: Authors' tabulations of the 1992 NHIS and the 1995 NHIS. Each cell shows either the proportion smoking or cigarettes per day for the group indicated in the column heads at the age denoted for each panel.

the *public-health view,* all that matters for adult smoking are youth-smoking decisions. Since almost all smokers start as youths, if we could end youth smoking, we would end adult smoking. At the other extreme, which we label the *delayed-initiation view,* there is a fixed predisposition to experiment with cigarettes, and setting up barriers to youth smoking simply delays the period of experimentation until after the teen years. Under this view, reducing teen smoking has little effect on long-run adult smoking.

Some casual evidence on this issue is provided in table 2.10. This table presents cross-tabulations of the odds of smoking at older ages against the age of initiation, using data from the 1992 NHIS and the 1995 NHIS. We find that, as the public-health view would suggest, initiation at twelve to fourteen is worse than initiation in later teen years in terms of the subsequent likelihood of smoking. On the other hand, initiation at twenty-one to twenty-five appears to have similar implications for later smoking participation, if not intensity, as does initiation at twelve to fourteen. This is not simply an artifact of the feature of the data that, at any age past twenty-five, ages twenty-one to twenty-five are closer than are ages twelve to fourteen. As the age group used for this table ages, the relative relations persist; indeed, for those age thirty-eight and older, the odds of smoking participation are higher for those who started at ages twenty-one to twenty-five than they are for those who started at ages twelve to fourteen.

Of course, this evidence does not provide definitive evidence for either view described above since it may simply represent individual heterogeneity; the set of persons who begin smoking after age twenty may intend more to continue smoking at later ages. But it is nevertheless suggestive of the merit of the delayed-initiation view.

In this section, we take two approaches to trying to assess more rigorously the extent to which more youth smoking will translate into increased adult smoking. The first, and most direct, is to examine whether shifting patterns of smoking across cohorts of youths are reflected in the smoking rates of those cohorts as adults. This approach will yield an estimate of the relation between rising youth smoking and rising adult smoking that is free of individual heterogeneity bias; this is akin to using cohort dummies as instruments. But, while we attempt several approaches below, this method may not be able to disentangle general time-series effects from true cohort shifts in smoking propensities.

The second method is therefore to assess the implications for adult smoking of differential taxes on youths. That is, if there are two adults who face the same tax regime today but who faced different tax regimes as teens, by how much does their smoking differ? This approach has the advantage that it best approximates the experiment of interest, which is exogenously to induce some groups of teens to smoke and others not to smoke. But it has the disadvantage that one can infer the implications of our findings for the long-run implications of youth smoking only indirectly. To the extent that this indirect inference yields results similar to those yielded by the first method, then these provide two reinforcing approaches to estimating this important intertemporal correlation.

The only paper of which we are aware that attempts to carry out an exercise of this nature is Glied (1999), which examines the effect of cigarette taxes in the state where the smoker was fourteen on later smoking, using the National Longitudinal Survey of Youth (NLSY), and which finds no effect of youth taxes. But Glied's relatively small sample does not permit the inclusion of fixed effects for either state of birth or state of residence; also, the standard errors on her estimates are too large to rule out relatively sizable effects of youth taxes on later smoking.

2.4.1 Intertemporal Correlation across Cohorts

We first consider the extent of intertemporal correlation in smoking across cohorts, in two ways. First, we use data on smoking by high school seniors from the MTF data matched to the smoking of this same cohort ten years later, as twenty-seven- to twenty-eight-year-olds, from the Behavioral Risk Factor Surveillance System (BRFSS) data, which provide an annual survey of smoking rates for a large representative sample of the U.S. population. We match data from the period 1976–87 on smoking of high school seniors to data from 1986–97 on the smoking of this same cohort as twenty-seven-to twenty-eight-year-olds. We focus on young adults because this maximizes the number of cohort comparisons that we can make, given that the MTF data are not available before 1976.

The results of doing so are presented in the first panel of table 2.11. We first show a simple bivariate regression of the smoking of twenty-seven-to twenty-eight-year-olds on the smoking of this cohort as high school seniors

Table 2.11 Intertemporal Correlations across Cohorts in Smoking Behavior

	No Controls	Time Trends	Smoking among 42–43-Year-Olds	Time Trends & 42–43 Smoking
BRFSS 27–28-year-olds (N = 12, 1986–97):				
MTF high school senior smoking rate in year $t - 10$	.648 (.088)	.397 (.157)	.781 (.273)	.500 (.298)
Time trend		−.0030 (.0016)		−.0029 (.0017)
42–43-year-old smoking rate			−.202 (.389)	−.148 (.354)
NHIS 27–28-year-olds (N = 36, 1950–95):				
NHIS 17–18 smoking rate in year $t - 10$	.673 (.482)	1.185 (.131)	.067 (.179)	.350 (.127)
Time trend		−.0058 (.0002)		−.0044 (.0007)
42–43-year-old smoking rate			1.082 (.056)	.485 (.100)

Note: Standard errors are given in parentheses. Regressions in the first panel match data from the BRFSS on 27–28-year-olds and 42–43-year-old controls for 1986–97 to corresponding MTF data from 10 years earlier; second panel matches backcast data from the NHIS for 27–28- and 42–43-year-olds to data from 10 years earlier on 17–18-year-olds. First column is just bivariate regression of 27–28-year-old smoking rates on 17–18-year-old smoking rates; second column includes linear time trend; third column includes contemporaneous smoking rate of 42–43-year-olds; and final column includes both time trends and 42–43-year-olds.

ten years earlier. We find a very strong correlation, with a coefficient of 0.65. Of course, this finding may reflect simple secular trends in the data; if smoking is declining over time, even in the absence of any within-cohort correlation one will find that cohorts born later smoke less both as teens and as adults. We attempt to control for this in two ways in table 2.11. First, we include a linear time trend. This time trend is marginally significant, indicating a secular decline in smoking of 0.3 percentage points per year for this population over our twelve-year sample, and the coefficient on the lagged youth-smoking rate falls to 0.4. Second, we include the smoking rate in each year of forty-two to forty-three-year-olds as a proxy for trends in adult smoking that should not be determined by the smoking of high school seniors ten years earlier. This additional control is insignificant, and, when it is included with the time trend, the coefficient on lagged youth smoking actually rises to 0.5.

The disadvantage of this approach is its relatively narrow historical coverage. We therefore next turn to data from the NHIS, which in several years (1978, 1979, 1980, 1987, and 1988) asked current and former smokers the age at which they began smoking (as well as when they quit if they are former smokers). This allows us to calculate by cohort, not only their current smoking rate in the survey year, but their smoking rate when they were high school seniors (ages seventeen to eighteen) as well as when they were twenty-seven to twenty-eight and forty-two to forty-three.[10] We can then draw comparisons between the smoking rate of high school seniors and the smoking rate of those same youths as they age, but over a much larger historical range. We restrict the data to those persons age sixty and younger to minimize any bias to this exercise through the differential mortality of smokers. As a result, the earliest cohort comprises those who were sixty in 1978, or high school seniors in 1936 and forty-two- to forty-three-year-olds (our "control" group) in 1960. Data are available on adult smoking from these look-back surveys through 1988 and then from cross-sectional NHIS data sets through 1995. Thus, we can model the smoking of twenty-seven- to twenty-eight-year-olds on their smoking rates as seventeen- to eighteen-year-olds, and include forty-two- to forty-three-year-olds as a control, from 1960 through 1995 (thirty-six observations).

The results of doing so are shown in the next panel of table 2.11. Once again, when one simply examines the correspondence between the smoking of these cohorts as youths and their smoking as young adults, the correlation is quite strong, with a coefficient that is very similar to the first column of the top panel. But, once again, when we control for time trends, the coefficient falls, and here it falls farther when we control as well for the smoking of forty-two- to forty-three-year-olds (to capture general trends in taste for smoking). The final column reveals an intertemporal correlation of 0.35, a time trend of −0.44 percentage points per year over this period, and very significant positive effect of the smoking of older adults.

Thus, the findings from this first exercise suggest that higher smoking rates among youths translate in a significant way to the smoking rates of adults. The final estimates are similar across these data sets, suggesting an intertemporal correlation of 0.35 to 0.5. On the other hand, the fact that this estimate is significantly smaller than 1, even observing cohorts ten years later, suggests that the pure public-health view is not appropriate; there is more to reducing adult smoking than simply stopping youths from smoking.

2.4.2 Youth Taxes and Adult Smoking

The advantage of the cohort approach is that it yields relatively straightforward estimates of the intertemporal correlation across cohorts. The disadvantage is that it does not definitively prove that there is an important

10. For most years, we can backcast from more than one NHIS (e.g., for the cohort that was seventeen years old in 1940, we can use those who were fifty-five in 1978, fifty-six in 1979, and fifty-seven in 1980); in those cases, we average the smoking rates for each cohort that we obtain from the various years of the NHIS to reduce measurement error in the backcasted smoking rates. The NHIS also asks about age of initiation in 1992 and 1995 but not about age of quitting for former smokers, so this backcasting exercise is not possible in those years.

intertemporal correlation since there may be underlying time-series trends that cannot be captured by our controls. We therefore attempt the second approach laid out above, examining the effect of taxes on youths on their smoking as adults. Once again, the motivation is to use variation in taxes that individuals faced as youths, conditional on the tax environment in which they currently live, to provide exogenous variation in youth smoking for the purposes of assessing intertemporal correlations. There is significant variation in youth taxes, conditional on current taxes, even for nonmovers, owing to changes in state tax policy over time.

We use the Natality data used earlier—which, in addition to other strengths, also have information on the state of birth of these mothers—to assess the effect of the tax rate that teens faced on their smoking as adults. The regressor of interest here is the average tax rate in the teens' state of birth during the years when they were fourteen to seventeen years old. Of course, state of birth is not the ideal measure for this exercise since some individuals move between birth and the teen years. However, in the 1990 census, 74 percent of thirteen- to seventeen-year-olds lived in their state of birth, so this is a reasonable proxy for state of residence as a teen. Moreover, we (crudely) correct the estimates for mobility by using information on state of birth and state of residence. If the mother's state of birth is the same as her current state of residence, we assume that she was in that state as a teen. If not, we assume that she moved only from the state of birth to the state of residence (making no other moves) and assign her a weighted average of the tax rates in the two states when she was a teen. These weights come from tabulations from the NLSY, which was used to compute, for movers of a given age, at what age they moved; this provides a means of averaging the state of birth and the state of residence to reflect, given current age, the odds of moving before age fourteen.[11] In practice, this correction has little effect on the estimates; for the most reliable estimates, we will use nonmovers only to mitigate this measurement-error concern. We focus on women age twenty-four and older to allow for a sufficient lag to separate current and teen tax rates.

Since estimating this model on the 15 million observations in the micro data is impractical, we first convert the eight years of Natality data into a set of year of birth × year of survey × state of birth × state of residence cells. We then use the means of smoking rates in these cells to estimate models of smoking today on the tax rates in both the current state of residence and the state of birth, including fixed effects for each of these sets of states (residence and birth), for year of birth, for year of the survey, and for age. We also control for the racial composition of the cell and the share of the cell that are high school dropouts, are high school grad-

11. We are grateful to Phil Levine for providing us with these estimates from the NLSY.

uates, or have at least some college. All regressions are weighted by the cell counts.

The primary dependent variable is the average number of cigarettes smoked by women giving birth in the cell, incorporating zeros. We then decompose this effect into the effect on smoking participation and the effect on conditional smoking intensity. On average in the sample (as is shown in the bottom row of the regression in table 2.12), women who give birth smoke roughly 1.95 cigarettes per day. This consists of a participation rate of 15.8 percent and conditional cigarette consumption among those who do smoke of 12.85. These smoking rates can be compared to the full population of twenty-four- to forty-five-year-old females over this period, where smoking rates were 26 percent and conditional cigarettes per day were seventeen,[12] women giving birth smoke less than the typical adult, but smoking is still distressingly common in this population.

Table 2.12 shows the results that include both the contemporaneous tax and the teen tax along with the control variables described above. For total cigarettes smoked, we find an elasticity of −0.46.[13] This is almost identical to the overall elasticity of cigarettes smoked for adults estimated in either aggregate data (Becker, Grossman, and Murphy 1994) or micro data (Evans, Ringel, and Stech 1999). This effect is then decomposed into a large negative effect on participation, with an elasticity of −0.6, and a small positive effect on conditional intensity. As discussed earlier, this wrong-signed effect may be the result of sample selection into who remains a smoker as the tax changes. But the elasticity here is small in any case.

There is also a strong negative effect of the tax as a teen. The overall elasticity is −0.19, which is over 40 percent as large as the effect of current taxes. This arises from a participation elasticity of −0.06 and a negative conditional-intensity elasticity.

One problem noted above is that the tax rate is assigned with some error since we know only birthplace and not the state of residence as a teen. To mitigate this measurement error, in the second set of columns in table 2.12 we use only the sample of nonmovers, for whom we can presume that the state of both birth and current residence is the state in which the mother resided as a teen. For this sample, the effect of both current taxes and taxes as a teen is somewhat larger, and the effect of youth taxes is somewhat larger, with the result that the overall elasticity with respect to youth taxes is −0.22 and the participation elasticity −0.078.

These findings clearly provide evidence for the addictive nature of smoking: if one exogenously shifts women early in life to nonsmoking status with higher taxes, they will smoke less later in life as a result. But their

12. Authors' tabulations from the 1989–97 BRFSS data.

13. Even though we are using tax rates as the regressor here, we show price elasticities, assuming one for one pass-through of taxes to prices.

Table 2.12 The Effect of Current and Teen Taxes on the Smoking of Pregnant Women

	All			Nonmovers only		
	Cigarettes Smoked	Participation	Cigarettes/Day If Smoking	Cigarettes Smoked	Participation	Cigarettes/Day If Smoking
Current tax	−.810	−.080	.536	−.970	−.086	.327
	(.033)	(.002)	(.131)	(.064)	(.004)	(.186)
	[−.455]	[−.552]	[.046]	[−.513]	[−.559]	[.028]
Teen tax	−.480	−.013	−.678	−.598	−.017	−.500
	(.061)	(.004)	(.242)	(.111)	(.007)	(.324)
	[−.188]	[−.061]	[−.040]	[−.221]	[−.078]	[−.030]
% high school dropout	5.73	.390	4.66	3.98	.256	7.26
	(.038)	(.002)	(.134)	(.228)	(.017)	(.677)
% high school graduate	2.93	.214	1.90	3.59	.255	3.94
	(.018)	(.001)	(.079)	(.136)	(.010)	(.409)
% some college	1.56	.121	.797	2.84	.260	.902
	(.021)	(.001)	(.094)	(.166)	(.012)	(.512)
% white	.838	.022	2.58	2.31	.102	6.19
	(.045)	(.003)	(.189)	(.347)	(.026)	(1.14)
% black	−.047	.015	−1.87	2.72	.210	1.47
	(.049)	(.003)	(.205)	(.361)	(.027)	(1.18)
% Hispanic	−2.71	−.178	−3.17	−3.20	−.218	−6.11
	(.042)	(.003)	(.181)	(.258)	(.019)	(.844)
No. of observations	337,690	338,167	182,379	9,941	9,947	8,945
Mean of dependent variable	1.95	.158	12.85	2.07	.167	12.86

Note: Standard errors are given in parentheses, price elasticities in square brackets. Coefficients are those from regressions that also include the full set of dummy variables for state of birth, state of residence, year of birth, year of survey, and age.

magnitudes are difficult to interpret in a vacuum. To do so, we can compare the elasticity of adult smoking with respect to youth taxes to the earlier estimates of the elasticity of youth smoking with respect to youth taxes. Such a comparison is not fully direct since these women were youths during the period 1957–85 and our estimates pertain to the 1990s, but these youth elasticities nevertheless provide a sensible benchmark. In the MTF data, over all youths (since this exercise compares average youth taxes to adult smoking), the elasticity of smoking participation with respect to price is −0.31. We find here that the elasticity of participation as an adult with respect to youth taxes is −0.078, or 25 percent as large. Thus, these results imply that there is an intertemporal correlation coefficient of −0.25.

Thus, we conclude that there is evidence for both hypotheses about the potential effect of youth smoking. Youth smoking is clearly an important determinant of adult smoking, with an intertemporal correlation of from −0.25 to −0.5, and our second piece of evidence suggests in particular that the taxes that youths face clearly have an important effect on the decision to smoke many years later. But youth smoking is by no means the sole, or even the primary, determinant of smoking later in life; indeed, the taxes that smokers face as adults are significantly more important than the taxes that they faced as youths.

2.5 Conclusions

The 1990s is a decade that has produced a very mixed track record with respect to risky behaviors among youths. While teen births and crime rates are steeply down (Levine, chap. 4 in this volume; Levitt and Lochner, chap. 7 in this volume), we have shown here that rates of substance use, and particularly smoking rates, are rising. The increase in smoking rates is particularly vexing given the expected, widely postulated intertemporal correlation between the decisions of youths to smoke and their subsequent smoking as adults, with the corresponding costly effects on health.

We have attempted to investigate several aspects of the youth-smoking question in this paper in an effort to advance our understanding of what drives these important decisions. We report four findings of interest. First, smoking participation is not simply concentrated among the most disadvantaged youths; indeed, increasingly over time, youth smoking is taking place among white, suburban youths with college-educated parents and good grades. Second, we show that neither changes in demographic characteristics nor changes in attitudes toward smoking can explain the striking increase in smoking rates in the 1990s.

Third, we find that the single greatest policy determinant of youth smoking is the price of cigarettes. We consistently estimate across several data sets that older teens are very sensitive to the price of cigarettes, with a central price-elasticity estimate of −0.67. This estimate implies that the sharp reduction in cigarette prices in the early 1990s can explain roughly 26 percent of the increase in smoking over the subsequent six years. Moreover, this price sensitivity rises for more socioeconomically disadvantaged groups such as blacks or those with less-educated parents.

At the same time, we find that younger teens are not sensitive to prices on average, nor is there any relation between price sensitivity and socioeconomic status for younger teens. These findings suggest important heterogeneity in the teen population. Younger teens appear to be price-insensitive experimenters who evolve into more price-sensitive smokers by their older teen years. An important priority for future work in this area is to understand the evolution of smoking between the younger and the older teenage years.

These findings also hold out little hope for other policies as a means of reducing youth smoking. We do find some evidence that policies that restrict youth access to cigarettes reduce the quantity of cigarettes smoked by the youths so affected, but this finding is not nearly as robust as the price relation. There is no consistent evidence that restrictions on smoking in public places decrease smoking.

Finally, the results imply that this rise in youth smoking will have important implications for the long-run stock of smokers in the United States. Evidence from two different approaches, examining the intertemporal correlation across cohorts and modeling the effect of youth taxes on adult smoking, suggests that between 25 and 50 percent of the rise in youth smoking will persist into adulthood. Over this period, smoking rose by 8 percentage points for high school seniors in the MTF survey. This implies a long-run rise in the adult-smoking rate of 2–4 percentage points. Compared to the current adult-smoking rate of 25 percent, this is a rise of 8–16 percent, a nontrivial increase. Of course, whether this recent rise will persist into adulthood in the manner suggested by past cohort shifts is unclear. The technology for quitting smoking has improved dramatically in recent years, and these youths are moving into workplaces that almost universally ban smoking, raising significantly the hassle costs of maintaining a habit. But the historical record speaks clearly, which should indicate a very significant rise in adult smoking going forward.

On the other hand, the recent decline in youth smoking in the face of modest price increases suggests that this may cause, not a permanent upward shift in adult smoking, but perhaps a "bulge" in smoking rates across cohorts. The prices of cigarettes rose by roughly 30 percent over the course of 1999, as a result of a shifting forward by tobacco manufacturers of the costs of settling their state lawsuits. Using the estimates presented here, this price rise should cause a 20 percent decline in youth smoking, which would almost fully undo the rise from 1991 to 1997.

Even if the rise from 1991 to 1997 was a transitory one, however, the long-run health consequences could be substantial. A 2–4 percentage point rise in smoking for this seven-year cohort, along with a somewhat reduced increase for the 1998 cohort of high school seniors (as prices began to rise), implies 477,000–954,000 more adult smokers. Of course, some of these adults will then quit in their adult years, and those who quit sufficiently before the age of greatest medical risk from smoking (age sixty on) can substantially reduce their mortality risk. On the basis of the NHIS data for 1987–88 on age of initiation and age of quitting, we can estimate that, of those who started smoking as youths and are still smoking at age thirty-five, 45 percent will quit by age sixty. So a conservative estimate is that 263,000–525,000 additional persons will have their lives shortened owing to increased smoking.

As noted above, smoking throughout one's life shortens life expectancy by 6.5 years for men and 5.7 years for women. Taking a simple average across men and women, this implies that the rise in youth smoking will cause a reduction of 1.6–3.2 million life years, even if this rise is totally undone. At a value of $100,000 per life year (Cutler and Richardson 1997), and discounted at a real 3 percent rate from age sixty-nine (typical life expectancy for smokers) to age nineteen, this is a forgone value of life years in today's dollars of $36–$73 billion. Once again, this is a vast oversimplification as both quitting technologies and the mortality effects of smoking are evolving rapidly over time. But it suggests the importance of even a potentially transitory rise in youth smoking for the health of the U.S. population.

Overall, these results imply that policy makers should be concerned about rising youth smoking; even if there is not a one-for-one translation into higher adult-smoking rates, the health implications can be enormous. And this concern should lead policy makers to consider cigarette taxes as the most effective means of reducing youth smoking. Of course, with youths smoking only about 2 percent of cigarette packs, taxes are a very blunt instrument with which to address youth-smoking issues. Thus, there are a host of additional issues that must be considered in deriving the optimal cigarette tax beyond considerations of youth smoking; Chaloupka and Warner (in press), Evans, Ringel, and Stech (1999), and Gruber and Koszegi (2000) provide further discussions of these factors. But the results presented here suggest that consideration of optimal cigarette-tax policy must include the very strong effect that taxes have on teen smoking.

Appendix

Youth-Access Index

Our Youth-Access Index (YAI) is based on the National Cancer Institute's (NCI) decision criteria for rating state youth-access laws. The NCI's criteria include nine categories: minimum age of purchase; packaging; clerk intervention; photo identification; vending-machine availability; free distribution; graduated penalties; random inspections; and statewide enforcement. For each category, a score is granted on a scale of 1–4 or 1–5 as a function of the stringency of state regulation in that area. For example, states get a score of 0 if the minimum age is younger than eighteen; a score of 3 if the minimum age is eighteen but there is no requirement of signposting and/or there is no specific penalty for failure to post a sign; a score of 4 if the minimum age is eighteen with specific signposting requirements and penalties for failure to post; and a score of 5 if there is a minimum age older than eighteen and there are posting/penalty provisions. These scores are then summed across categories to get a total access index score. Then states' points are reduced by two points (to a minimum score of 0) if they allow their state regulation to preempt a stricter local ordinance.

While the general framework of the two indexes is the same, the YAI contains several variations in order to describe state tobacco laws in more detail. The largest difference is the inclusion of three categories in addition to the nine utilized by the NCI—advertising, licensing, and restrictions on minors. Points are awarded for advertising restrictions on a scale between 1 and 4. A state earns a score of 1 for minimal limitations (no advertising on school buses etc.) and a score of 4 for a ban on all tobacco advertisements. Including licensing in the YAI captures the extent to which retailers, vendors, and wholesalers are regulated by state agencies. Maximum licensing requirements (applicable to retailers, vendors, and wholesalers) received a score of 4, while states mandating only wholesale licenses received a score of 1. The restrictions-on-minors category encompasses laws relating to the underage purchase, possession, and use of tobacco. Those states outlawing these actions but implementing no penalties for violating the laws received a score between 0 and 1. The highest possible score, 4, is given to states outlawing the purchase, possession, and use and implementation of graduated penalties.

The YAI also allows for more point levels under each category than does the NCI index to create a finer gradation between the stringency of various laws. For example, one problem with the minimum-age categorization noted above is that some states mandate signage at the point of purchase while others mandate signage but not at the point of purchase; we awarded the latter group of states a score of 3.5 instead of 4. This affected twenty-one states overall. Similar half-point steps were added to the scoring of each of the nine original NCI categories. The purpose of this variation from the NCI index was to distinguish more clearly between the stringency of varying state requirements.

In several instances we also altered scoring decisions made by the NCI in the final computation of state scores. After extensive investigation of state laws and statutes, several inconsistencies were discovered between the laws and the NCI point allotment. For example, on consultation with NCI representatives, it was revealed that Connecticut received a score of 2 for the vending-machine category in 1996. The justification for this score was that a 1996 law added new restrictions. However, certain sections of the law did not take effect until after the time period of the NCI study. Since the law had already passed, however, the NCI awarded points to reflect that fact. For this project, however, this point assignment was inappropriate. Credit for laws was awarded only after those laws came into effect. Therefore, the YAI contains several modifications to the factual basis of the NCI index.

Table 2A.1 Means of Regulatory Variables in the MTF Survey

	12th Graders	8th & 10th Graders	8th–12th Graders
Access index	11.91	11.69	11.76
	(5.29)	(5.46)	(5.41)
Clean air:			
Private workplace	.44	.44	.44
	(.50)	(.50)	(.50)
Government workplace	.73	.71	.72
	(.44)	(.45)	(.45)
Restaurants	.64	.61	.62
	(.48)	(.49)	(.49)
Schools	.90	.85	.87
	(.30)	(.36)	(.34)
Other	.93	.91	.92
	(.25)	(.29)	(.28)
No. of observations	106,539	230,126	336,665

Note: From authors' tabulations of 1991–97 MTF restricted sample data described in the text. Standard deviations are given in parentheses.

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