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ABSTRACT

The one-third rise in the teen smoking rate in the 1990s has led to considerable interest in understanding the determinants of the youth smoking decision. We explore four aspects of this decision. First, we consider the demographic correlates of smoking participation, and find that smoking participation is not simply concentrated among the most disadvantaged youth; indeed, increasingly over time youth smoking is taking place among white, suburban youth with college educated parents and good grades. Second, we show that neither changes in demographic characteristics nor changes in attitudes towards smoking can explain the striking increase in smoking rates in the 1990s. Third, we document that price is a powerful determinant of smoking for high school seniors; using state fixed effects models on data for the 1991-1997 period we estimate an elasticity of smoking participation of -0.67, which suggest that the drop in cigarette prices in the early 1990s can explain 26% of the subsequent upwards smoking trend for seniors. But price is not important for younger teens, although we do find some evidence that restrictions on access to cigarette purchases can lower the quantity that younger teens smoke. Finally, we document that there is an important intertemporal correlation in the decision to smoke. In particular, we find that there is a significant correlation across cohorts in teen smoking and later smoking of adults, and that the taxes that teens face on cigarettes have a significant negative effect on their smoking later in life. These findings suggest that between 25 and 50% of the rise in youth smoking in the 1990s will persist into adulthood for this cohort; rough calculations suggest that the long run cost to the U.S. will be at least 1.6 million years of life lost from this youth smoking increase.

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One of the most striking trends in the behavior of youth in the U.S. during the 1990s has been the increased incidence of smoking. After steadily declining over the previous 15 years, youth smoking began to rise precipitously in 1992. By 1997, smoking by teenagers in the U.S. had risen by one-third from its 1991 trough. This trend is particularly striking in light of the continuing steady decline in adult smoking in the U.S. Indeed, today we are in the alarming position of having a youth smoking rate that is roughly fifty 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 youth 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 20s and beyond. For example, among high school seniors who smoke, 56% say that they won't be smoking 5 years later, but only 31% of them have in fact quit five years hence. Moreover, among those who smoke more than 1 pack/day, the smoking rate five years later among those who stated that they would not be smoking (74%) is actually higher than the smoking rate among those who stated that they would be smoking (72%) (Department of Health and Human Services, 1994).

If youth smoking leads to adult smoking, particularly in a manner that is underappreciated by the youth 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 U.S., and

smokers on average live 6.5 (males) to 5.7 (females) fewer years, relative to never smokers (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 nor 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 by both clearly exogenous background characteristics, as well as by potentially endogenous attitudinal variables. And we assess how these relationships 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 youth; indeed, increasingly over time youth smoking is taking place among white, suburban youth 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 towards smoking, can explain the precipitous recent rise in youth smoking in the 1990s. In short, we find that neither plays an important role in explaining this rise; background characteristics can explain at most 10% of the rise.

The third is an understanding of the role that public policy can play in deterring youth smoking. We provide a comprehensive analysis of the impact 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.

The findings of this analysis are clear: the most important policy determinant of youth smoking, particularly among older teens, is prices. 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 robust effect of other public policies targeted to reducing youth smoking, although there is some suggestion that restrictions on youth purchase of cigarettes reduce the quantity of cigarettes smoked. And we find that black youth and those with less educated parents are much more responsive to cigarette price than are white teens and those with more educated parents, suggesting a strong correlation between price sensitivity and socioeconomic status.

The final part of this paper then builds upon 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 U.S. We do so in two ways. First, we pursue a cohort analysis, examining what the historical record tells us about the implications of higher youth smoking for adult smoking of those same cohorts as they age. Second, we use data from 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 and adult smoking, with each percentage point of additional smoking by youths translating into only 0.25 to 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 (due to significant recent price increases), the adult smoking rate for this cohort will rise by 8 to 16%, and at least 1.6 million total life years to this cohort will be lost.

The paper proceeds as follows. Part I provides background on trends in youth smoking and on previous work in this area. Part II presents our cross-sectional analysis of demographic determinants of smoking decisions, and what this analysis implies for explaining time trends. Part III explores the role of price and other public policies. Part IV then turns to the intertemporal implications of youth smoking. Part V concludes.

Part I: Background

Youth Smoking: Where It's Been, Where It's Going, and Why We Should Care

The time series trends in youth smoking are depicted in Figures 1 and 2. Figure 1 shows the trend since the 1970s for the three available surveys of seniors: the Monitoring the Future (MTF) data, which has surveyed seniors since 1976 but 8th and 10th graders only since 1991; the National Health Interview Survey (NHIS); and the National Survey of Household Drug Abuse (NHSDA). The latter two surveys are household surveys, for which we use data on older teens.[1] There is considerable uncertainty over the relative value of in-school vs. 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

[1]In particular, for the NHIS, we use 18-19 year olds, and for the NHSDA we use 17-19 year olds through 1991, and 18-20 year olds for 1996-97.

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 focused on the trend for the 1990s for all high school youths, using data from the MTF and the Youth Risk Behavior Survey (YRBS), which collected data for 1991, 1993, 1995, and 1997 on a large sample of 9-12th 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 1 shows tabulations from the 1992 and 1995 National Health Interview Surveys on the age of initiation of smoking by current or former adult smokers. This table reveals that 42% of current or former adult smokers started before their 16th birthday, and 75% started before their 19th birthday. Conversely, of those smoking a pack a day as high school seniors in the Monitoring the Future survey, 87% are smoking five years later. Even among those smoking 1-5 cigarettes per day, 70% are smoking 5 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 causality of this relationship between youth and adult smoking, which

is to assess whether exogenous shifts in the smoking of youth affect smoking of those same individuals later in life. This is the exercise that we will take up in Part IV.

Second, however, there may also have been a structural shift in the nature of youth smoking. Those youths who are newly taking up smoking today may be different from those in the past, and in particular more resolved to quit, and therefore this intertemporal correlation may be broken. We do find some evidence for this view 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.

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 the Surgeon General's Report of 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 below, as they draw on the same data that we will use.

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

There is also a sizeable 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 impacts 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 Lewit, Coate and Grossman (1981) and Lewit and Coate (1982), although the result is disputed by Wasserman et al. (1991).

This literature has been strongly criticized, however, by DeCicca et al. (1998) and Evans and Huang (1998), who point out that in cross-sectional data it is impossible to disentangle price and policy impacts 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 relationship. These two papers take different approaches to solving this problem. DeCicca et al. 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 impact. Evans and Huang, on the other hand, use repeated cross-sections of youth and include state fixed effects to control for fixed state tastes towards smoking, and they find a significant participation price elasticity of -0.5 over the 1985-1992 period (using repeated cross-sections of the restricted MTF data discussed below).

Both of these approaches have weaknesses. The DeCicca et al. 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 et al. Moreover, neither paper focuses on the period of most interest, the 1990s.

There is a smaller literature which has studied the impact of other anti-smoking policies on youth smoking. DeCicca et al. (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) impacts of the former, but fairly strong negative impacts 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 lowers 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 towards smoking. Two interesting case studies of communities that implemented tough youth access restrictions found mixed results, with Jason et al. (1991) finding substantial (50%) declines in youth smoking in Woodridge, Illinois, while Rigotti et al. (1997) finding very limited impacts on sales to youth and 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 one age group of teens. In fact, as we document at length below, there is considerable heterogeneity among the teen population in their responsiveness to policy variables.

Part II: Determinants of Youth Smoking

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 attitudinal variables which makes it an excellent source for trying to comprehensively model the determinants of youth smoking. These public use data do have two drawbacks, however. First, they are only for high school seniors, so that we cannot explore how these background characteristics affect smoking decisions by younger teens. Second, they do not contain very detailed locational information, so that we cannot in these data explore the role of state level policies. We address both of these issues in our policy analysis below. We use data from the MTF surveys over the 1976-1997 period.

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 compositional impacts on the conditional number of cigarettes smoked.

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 Table 2A, for participation, and 2B, for 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 non-whites) 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 to smoke, but this appears to be reversed by the inclusion of variables such as work status and family structure.
- Those in are suburbs (the omitted locational 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 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-squared 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 on 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 parental education. On the other hand, youth smoking is much more likely among those with worse academic performance, who miss more school, and who don't 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 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.

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 which might play an important role. We do this assessment by taking our cross-sectional models and using the estimated relationships to predict smoking in each year, given the values of the Xs 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 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 does rise very slightly in the 1990s, by 0.7 percentage points, but this is less than 10% 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 4 shows the same exercise for smoking intensity. Here, the model predicts somewhat better the changes over time, but this is perhaps unsurprising since the changes are much more modest.

Changes in the Estimated Relationships

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

We investigate this by examining the changes in the coefficients in our cross-sectional model over time. Table 3 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 (1986 coefficient minus 1976 coefficient), and a t-statistic on that difference; the next column does the same for 1986 to 1997; and the final column does the same for the entire 1976 to 1997 period.

The results show substantial stability in the characteristics of smokers over this twenty-one year period; only 11 of the 35 coefficients change significantly over this time period. But there is a striking and consistent story from the coefficients that do change: smoking is rising

much more among "advantaged" youth, particularly in the 1990s. There is a significant rise in smoking among whites relative to non-whites, among those students who don't work relative to those students who do work, among those students who live in suburban settings relative to town or urban settings, among those whose mothers are college educated relative to lower education groups, 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 youth 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 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%) and low (bottom 25%) 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 towards more smoking by advantaged students over the past 10-20 years, it is swamped in the past decade by other factors that are affecting all students.

Adding Attitudinal Variables

The MTF data have a variety of other attitudinal 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 attitudinal variables which may be relevant for smoking decisions:

- do you disapprove of those who smoke 1 or more packs per day?
- how often do you see anti-smoking ads?
- does smoking make a guy/girl your age look cool, insecure, independent, conforming, mature, or trying to look mature & sophisticated?
- do you prefer to date a non-smoker?
- has the harm from smoking been exaggerated?
- does smoking reflect bad judgement?
- 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 for students in your school of smoking?

The full set of these variables are only available since 1989, so for this exercise we focus on the period from 1989-1997. Over this period, somewhat surprisingly, there do not appear to be any clear trends in attitudinal variables. There is a slight rise in the share of teens exposed to counter-advertising in 1996-97, although this variable has no impact 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 6 shows, the model with attitudinal variables actually predicts declining smoking in 1996 and 1997, when smoking continues to rise. So changing attitudes, at least as reported to the MTF, do not appear to explain the rise in smoking in the 1990s either.

The Role of Other Substances

One interesting question is what role other risky behaviors, and in particular use of other substances, is playing in this time series pattern. We explore this issue in Figure 7 which shows, along with smoking participation, use of alcohol, marijuana, and other drugs; in each case, participation is defined as any use over the past 30 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 three possibilities. The least likely is that there exogenous shifts in the use of these other substances that led to increased smoking; although there is no statistically convincing evidence, most work on pathways of substance use suggest that smoking is a pathway to use of other substances, and 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% or more of the trend in smoking by seniors over this time period. And there are a number of studies which suggest that smoking is complementary with 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 of these strong trends, particularly for marijuana. A third alternative is therefore that there were other shifts in tastes that led to more use of all of these substances. One piece of suggestive evidence for this view is the lack of correspondence between the time trends in use of cigarettes and these other substances in the 1980s: use of other substances fell much more than did smoking. These trends cast some doubt

on either of the two alternatives above, in favor of a story based on a taste shift towards all substance use in the 1990s.

Part III: 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 8 documents, 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 1992-1994 period when prices were falling. Moreover, recently released data for 1998 reveals that there was both a sharp increase in prices (by 10%) and the first decline in youth smoking of the decade, as smoking among seniors fell by 1.4 percentage points (3.8%). So a natural question 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.

Data

The public use data from the MTF are not suitable for this exercise because they do not

contain information on state of residence, not are they 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 criteria, and all three are used.[2] The first, and best, source for our purposes is a restricted use sample of the MTF data which includes information on smoking behavior, race, age, sex, and state of residence, for 8th, 10th, and 12th graders, from 1991 through 1997. We focus on 1991 as the starting point for the analysis for three reasons: first, this is the last year before teen smoking began to rise; second, this is the year in which the 8th and 10th grade MTF data become available; and third, this is the year that 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 9–12th graders. The MTF and YRBS data are comparable, in that they provide nationally-representative, in-school surveys of youth. As noted earlier with reference to Figure 2, they suggest different levels of smoking among teens, but similar trends. The sample sizes of these surveys are also comparable. But the MTF data has the strong advantage of being a more complete survey over this sample period; the survey includes data on 35 states in every year from 1991 to 1997, and a total of 277 state/year pairs over this time period. On the other hand,

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[2]A fourth data set that could have been used here is the National Survey of Household Drug Abuse data, but this was excluded for two reasons. First, the quality of these data before the mid 1990s is quite suspect, due to the use of in-home surveys without computer assistance that could suffer from bias due to parental observation; 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 NSHDA data available with state identifiers; only selected researchers can access these data.

the YRBS is a survey that is phasing into coverage of the entire nation, with only 10 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 59 tax changes to be studied in the MTF data, there are only 14 in the YRBS data. Thus, the results that we obtain in the MTF 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 U.S., 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 onwards, providing even more variation than the MTF (73 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 19th birthday. Due to the enormous size of the Natality data (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*year*age cells, and analyze cell mean rates of smoking and conditional smoking intensity, where the regressions are weighted by cell size.

The means for all three data sets are presented in Table 4. We consider two measures of smoking: participation, defined as any smoking over the past month, and conditional intensity.[5]

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[3]All of 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 in any year and for New York for 1991-93; smoking data for New York City, but not the remainder of the state, is available beginning in 1994.

[5]In the MTF and YRBS data, conditional intensity is asked in intervals, and we use the midpoint of each interval for intensity. In the natality data, the intensity question is continuous.

The latter measure has the difficulty as a dependent variable that if there are policy impacts 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 compositional effect.

As noted above, smoking rates are somewhat higher in the YRBS than in the MTF data sets; for seniors over this time period, participation rates in the MTF are 31% while they are 36% in the YRBS. Smoking rates are much lower for teen mothers; for 17-18 year old teen mothers, the smoking rate is only 18%. However, smoking intensity is higher in the MTF survey, averaging 7.2 cigarettes per day for seniors as compared to 6.1 cigarettes per day in the YRBS. Intensity is even higher for teen mothers, averaging over 10 cigarettes per day for 17-18 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 are very similar in the YRBS and MTF surveys, and intensity is actually somewhat higher for males.

Smoking is less frequent, and smoking intensity is 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 towards 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 the Tobacco Institute (1998), and a monthly series of information on taxes can be constructed from information 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 anti-smoking 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 which restrict smoking from certain public areas. We constructed a comprehensive data base of such laws using information from the state legislative records, Coalition on Smoking OR Health (various years) , and the Centers for Disease Control web site **http://www2.cdc.gov/nccdphp/osh/state/** . While there are a panoply of such laws, we categorize them into five categories: private workplace restrictions; public (e.g. state and local government) workplace restrictions; restaurants; schools; and other (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 youth is not. As reviewed in Jacobson and Wasserman (1997), states have therefore endeavored along various dimensions to implement barriers to youth access to cigarette purchase. Categorizing these state efforts is difficult, as there are a large number of different regulatory tools, and states enforce them with differential 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 considered a wide variety of state laws in this area and formed an index to

capture their overall "bite" in limiting youth access. They consider nine categories of state regulation, and provide a score within each, which is aggregated into a total index. Their index is available for 1993-1996; 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 Ammendments proposed by the Food and Drug Administration.

We describe in more detail how this index was created in the Appendix. Appendix Table A1 also shows means for the MTF 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 worksites, schools, and other sites were more common.

The other frequently discussed public policy intervention 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]

Empirical Strategy

For all three data sets, we pursue a similar estimation strategy, considering the impact of

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

prices and public policies on smoking in the following regression framework:

$$(17) \quad SMOK_{ijt} = \alpha + \beta PRICE_{jt} + \delta ACCESS_{jt} + \gamma CLNAIR_{jt} + \eta X_{ijt} + \lambda S_j + \nu T_t + \epsilon_{ijt}$$

where i indexes individuals; j indexes states; and 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
T is a set of year dummies

By including a complete set of state fixed effects, this regression surmounts any problems with fixed differences across states in both their level of prices and the propensity to smoke, e.g. due 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% of the variation in prices within states over time are driven by tax changes (Gruber and Koszegi, 1999), there remains 20% 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% 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. It is difficult with this short panel 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 we really only have variation in our key variables 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.

Results - MTF Data

We begin the analysis by focusing on the MTF data, which as discussed above is the highest quality source of nationally representative data. We also start with 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 5. The most important finding is that there is a negative and statistically significant impact of prices on smoking participation. The implied elasticity at the sample mean is -0.67. The impact 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 compositional bias to the estimates.

We obtain much less convincing evidence for the role of other policies, however. There is a negative impact 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 impact of restrictions in government worksites on youth; it is perhaps more plausible that restrictions on other sites such as public transportation might matter.

Does this significant price impact 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 14 cents. At our estimated coefficient on participation, this explains 26% of the 8 percentage point rise in smoking for seniors over this time period. Thus, price is playing an important role, but not the dominant one.

The next four columns of Table 5 investigate the impact of prices and policies on younger smokers (8^{th} and 10^{th} graders). Interestingly, there is little impact of price on the smoking of younger teens. The coefficients on both participation and intensity are insignificant for 8-10th graders, and as a result for the full sample of 8-12th 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 8-10th graders and for seniors. On the other hand, even though younger smokers are less price sensitive, their estimated price sensitivity is still non-trivial; it is similar, for example, to the elasticity of smoking participation estimated for adults by Evans et al. (1999).

The impacts of other policies on the smoking of 8-10th graders is more interesting than for seniors. We now estimate a highly significant impact of youth access restrictions on the conditional quantity of cigarettes consumed by younger teens, which is not subject to selection

bias due to the insignificant impacts 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%. This is interesting because it is indeed possible that access restrictions, by raising the hassle costs of obtaining cigarettes, do not deter youths from smoking at all, but rather limit the extent to which they do smoke. We also again obtain negative impacts of government worksite restrictions on smoking intensity, and negative impacts 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 (due to data restrictions in these MTF data), but they have the expected pattern: older children within each grade smoke more. Non-white youths are much less likely to smoke, and there is a positive effect of being male among seniors, but a negative effect among 8th and 10th graders, so that for the full sample the effect is insignificant. As showed above, the positive impact of being male for the seniors becomes negative when other covariates available in the public use data are included.

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 impacts of public policy on smoking. In this spirit, Tables 6 and 7 replicate the results for the MTF data in the YRBS and Natality data, once again for older teens (seniors in the YRBS; 17-18 year olds in the Natality data), younger teens (9-11th graders in YRBS; 13-16 year olds in

Natality data), and overall.

The most strikingly consistent finding across all three data sets is the negative impact 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 of the smoking decision to information about the hazards for newborns. This smaller elasticity is not due to the gender composition of the sample. In both the MTF and 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 impact of prices on young smokers. In the YRBS data, the elasticity of participation is wrong-signed, and the elasticity of conditional intensity is insignificant, for 9-11th 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, such as Becker and Murphy (1988), and which is as a result less sensitive to economic factors such as price. But by the time these youths have become seniors they have completed their experimentation phase, and smoking follows expected relationships with price and other economic factors. This type of story is consistent with the fact

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

Alternatively, younger teens may be pursuing smoking as a mode of acceptance into a peer group, and by the time they are 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 impact 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 impact 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 which is significant for either age group in all three data sets, or even in both the data sets representing the full teen population (MTF and YRBS). The most robust finding appears to be for the impact of youth access restrictions on the quantity of cigarettes smoked, which is negative for both younger teens and 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 impacts of clean air regulations for restaurants (which are significant for participation by 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 impact of sex, and an enormous negative impact 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 our findings above using MTF microdata, there is little impact of parental education on smoking, at least for seniors; there is some evidence that having more educated parents leads to less smoking for 8th and 10th graders.[7] There are very few covariates in the Natality data, but they do confirm that smoking rises with age and is much higher for whites.[8] One interesting difference between the Natality and YRBS data sets is that smoking among Hispanics is much lower in the Natality data, but is only marginally lower in the YRBS. Unfortunately, due to the restricted nature of the MTF data, we can't bring that evidence to bear on racial distinctions; the data only report whether the youth is white or non-white in those data.

In summary, there are four conclusions from these basic results. First, there is a sizeable and significant negative impact of price on smoking by 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 impact of price on younger teens, so that in aggregate the price impacts 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 which restrict youth access to tobacco products reduce the intensity of youth smoking, but not

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[7]Parental 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 parents who are either high school dropouts or for whom education is missing.

[8]Note that the covariates here are cell means, e.g. percent white in the age/year/state cell.

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 only have data on students, and not 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 change much 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 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/capita themselves are generally insignificant. Thus, it appears that correlations between aggregate consumption and both tax setting and youth smoking cannot explain our findings.

Heterogeneity

The analysis thus far has considered youth smoking as a simple aggregate, and has not explored the heterogeneity in policy impacts across different groups of youth. But there are considerable differences across youth in their underlying propensity to smoke. Most noticeable is racial differences, and the YRBS suggests some differences by parental education as well, at least for younger teens. In this section we explore the heterogeneity in the price responsiveness of youth smokers. In particular, we assess whether socioeconomically disadvantaged youth 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 8. For the MTF sample, we can only compare white and non-white 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 non-whites in these other data sets, they are similar but more muted than the results for whites and blacks.

The results for the MTF and YRBS data for seniors are striking: there is much higher price responsiveness among blacks than among whites. In the MTF, the price elasticity of participation for white seniors is only -0.35, and is insignificant, and there is a positive coefficient on conditional intensity. But for black 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 impact 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 incomes. A number of articles have found for adult smokers price elasticities that fall with income (e.g. Evans et al. 1999). If the same is true for teens, then the lower incomes of black seniors may explain their increased responsiveness. Unfortunately, none of these data sets contain information on income. But the YRBS data do have an excellent proxy for permanent income: parental education.

In the final panel of Table 8 we therefore present results which 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 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 relationship 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 in Table 8 suggest two important conclusions. First, for seniors, there is a strong cross-elasticity between price and incomes. Lower income groups, either racially or by parental 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 non-economic factors. Not only are younger teens not price sensitive, there is no pattern of increased relative sensitivity with income, as proxied by either race or parental education.

Part IV: 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

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[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, among white teen mothers median income was $3000, while it was $4300 among black teen mothers.

adult smoking rate in the future? If shifts in youth smoking imply long run increases in adult smoking, then we are headed towards a substantial reversal in the downward trend in smoking in the U.S.

There are two extreme possibilities for thinking about the linkage between youth and adult smoking. At one extreme, which we label the "public health view", all that matters for adult smoking is 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 smoking as a youth simply delays the period of experimentation until after the teen years. Under this view, reducing teen smoking has little impact on long run adult smoking.

Some casual evidence on this issue is provided by Table 9. This table presents cross-tabulations of the odds of smoking at older ages against the age of initiation, using data from the 1992 and 1995 NHIS surveys. We find that, as the public health view would suggest, initiation at 12-14 is worse than in later teen years in terms of subsequent likelihood of smoking. On the other hand, initiation at age 21-25 appears to have similar implications for later smoking participation, if not intensity, as does initiation at ages 12-14. This is not simply an artifact of the feature of the data that at any age past 25, ages 21-25 are closer than ages 12-14. As the age group used for this table ages, the relative relationships persist; indeed, for those age 38 and over, the odds of smoking participation are higher for those who started at ages 21-25 than for those who started at ages 12-14.

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 20 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 smoking by youth will translate into increased adult smoking. The first, and most direct, is to examine whether shifting patterns of smoking across cohorts of youth are reflected in the smoking rates of those cohorts as adults. This approach will yield an estimate of the relationship 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 youth. 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 to exogenously induce some groups of teens to smoke and others to not smoke. But it has the disadvantage that one can only indirectly infer the implications of our findings for the long run implications of youth smoking. To the extent that this indirect inference yields similar results to the first method, then these provide two reinforcing approaches to estimating this important intertemporal correlation.

The only paper of which we are aware which attempts to carry out an exercise of this nature is Glied (1999), who examines the impact of cigarette taxes in the state where you were 14

on later smoking, using the National Longitudinal Survey of Youth (NLSY), and who finds no effect of youth taxes. But her relatively small sample does not permit the inclusion of fixed effects for either the state of birth or residence; as well, the standard errors on her estimates are too large to rule out relatively sizeable impacts of youth taxes on later smoking.

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 seniors from the MTF data, matched to the smoking of this same cohort 10 years later, as 27-28 year olds, from the Behavioral Risk Factor Surveillance System (BRFSS) data, which provide a annual survey of smoking rates for a large representative sample of the U.S. population. We match data from 1976-1987 on smoking of seniors to data from 1986 to 1997 on the smoking of this same cohort as 27-28 year olds. We focus on young adults because this maximizes the number of cohort comparisons 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 10. We first show a simple bivariate regression of smoking of 27-28 year olds on smoking of this cohort as seniors 10 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 have lower smoking both as teens and as adults. We attempt to control for this in two ways in Table 10. 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 12 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 42-43 year olds, as a proxy for trends in adult smoking that should not be determined by the smoking of 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 National Health Interview Survey (NHIS), which in several years (1978, 1979, 1980, 1987, and 1988) asked current and former smokers the age at which they initiated 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 the cohort were seniors (age 17-18), as well as when they were 27-28 and 42-43 as well.[10] We can then draw comparisons between the smoking rate of 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 60 and below, to minimize any bias to this exercise through differential mortality of smokers. As a result, the earliest cohort is those who were 60 in 1978, or high school seniors in 1936 and 42-43 year olds (our "control" group) in 1960. Data are available on adult smoking from these lookback surveys through 1988, and then from cross-sectional NHIS datasets through 1995. Thus, we can model the smoking of 27-28 year olds on their smoking

[10]For most years, we can backcast from more than one NHIS survey (e.g. for the cohort that were 17 year olds in 1940, we can use those who were 55 in 1978, 56 in 1979, and 57 in 1980); in those cases, we average the smoking rates for each cohort that we obtain from the various NHIS surveys, 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 that this backcasting exercise is not possible in those years.

rates as 17-18 year olds, and include 42-43 year olds as a control, from 1960 through 1995 (36 observations).

The results of doing so are shown in the next panel of Table 10. Once again, when one simply examines the correspondence between smoking of these cohorts as youths and young adults, the correlation is quite strong, with a coefficient here well in excess of one. But, once again, when we control for time trends, the coefficient falls, and here it falls further when we control as well for the smoking of 42-43 year olds (to capture general trends in taste for smoking). The final column reveals an intertemporal correlation of 0.5, with a time trend of -0.3 percentage points per year over this period, and a very significant positive effect of the smoking of older adults.

Thus, the findings from this first exercise suggest that higher smoking rates among youths translates in a significant way to the smoking rates of adults. The final estimates are strikingly similar across these data sets, suggesting an intertemporal correlation of 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.

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 is not definitively prove that there is an important intertemporal correlation, since there may be underlying time series trends that cannot be captured by my controls. We therefore attempt the second approach

laid out above, examining the impact of taxes on youth 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 current 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 non-movers, due to changes in state tax policy over time.

We do this using the Natality data used earlier, which in addition to its other strengths also has data on the state of birth of these mothers, to assess the impact 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 14-17 years old. Of course, state of birth is not the ideal measure for this exercise, since some individuals will move between birth and the teen years. However, in the 1990 census, 74% of 13-17 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 only moved from the state of birth to the state of residence (no other moves), and assign her a weighted average of the tax rates in the two states when she were a teen. These weights come from tabulations from the National Longitudinal Survey of Youth (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 state of residence to reflect, given current age, the odds of moving before age 14.[11] In practice, this correction has little impact on the estimates; for the most reliable estimates, we will use non-movers only, to mitigate

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

this measurement error concern. We focus on women age 24 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 8 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 a 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, high school graduates, 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 impact 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 Table 11), women who give birth smoke roughly 1.95 cigarettes per day. This consists of a participation rate of 15.8%, and conditional cigarette consumption among those who do smoke of 12.85. These smoking rates can be compared to the full population of 24-45 year old females over this period, where smoking rates were 26%, and conditional cigarettes per day were 17;[12] women giving birth smoking less than the typical adult, but smoking is still distressingly common in this population.

Table 11 shows the results that include both the contemporaneous tax and the teen tax,

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[12]Authors' tabulations from the 1989-1997 Behavioral Risk Factor Surveilance System data.

along with the control variables described above. For total cigarettes smoked, we find an elasticity of -0.47.[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), although it is somewhat lower than Gruber and Koszegi's estimate through 1996 using aggregate data (an elasticity of -0.6). 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 -0.24,which is over 40% 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 of Table 7 we use only the sample of non-movers, 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 impact of both current taxes and taxes as a teen are somewhat larger, and the impact of youth taxes is somewhat larger, so that the overall elasticity with respect to youth taxes is -0.22, and the participation elasticity is -0.078.

These findings clearly provide evidence for the addictive nature of smoking: if you

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

exogenously shift women to non-smoking status early in life with higher taxes, they will smoke less later in life as a result. But their 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 youth during the period 1957 to 1985, and our estimates pertain to the 1990s, but these youth elasticities nevertheless provide a sensible benchmark. In the MTF data, over all youth (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% 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 impact of youth smoking. Youth smoking is clearly an important determinant of adult smoking, with an intertemporal correlation of -0.25 to -0.5, and our second piece of evidence suggests in particular that the taxes that youth face clearly have an important impact 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.

Part V: Conclusions

The 1990s is a decade which has produced a very mixed track record with respect to risky behaviors among youths. While teen births and crime rates are steeply down (Levine, 1999; Levitt and Lochner, 1999), 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 impacts 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 have four findings of interest. First, smoking participation is not simply concentrated among the most disadvantaged youth; indeed, increasingly over time youth smoking is taking place among white, suburban youth with college educated parents and good grades. Second, we show that neither changes in demographic characteristics nor changes in attitudes towards 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% 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 relationship 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 teen and 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 the access of youth to cigarettes reduce the quantity of cigarettes smoked by those youth, but this finding is not nearly as robust as the price relationship. There is no consistent evidence that restrictions on smoking in public places lowers smoking.

Finally, the results imply that this rise in youth smoking will have important implications for the long run stock of smokers in the U.S. Evidence from two different approaches, examining the intertemporal correlation across cohorts and modeling the impact of youth taxes on adult smoking, suggest that between 25 and 50% of the rise in youth smoking will persist into adulthood. Over this period, smoking rose by 8 percentage points for seniors in the MTF. This implies a long run rise in the adult smoking rate of 2 to 4 percentage points. Compared to the current adult smoking rate of 25%, this is a rise of 8 to 16 percent, a non-trivial 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 which 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 not cause a permanent upward shift in adult smoking, but perhaps a "bulge" in smoking rates across cohorts. The prices of cigarettes rose by roughly 30% over the course of 1999, as a result of 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% 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 to 4 percentage point rise in smoking for this seven year cohort, along with a somewhat reduced increase for the 1998 cohort of seniors (as prices began to rise), implies 477,000 to 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 60 onwards) can substantially reduce their mortality risk. Based on the NHIS data for 1987/88 on age of initiation and age of quitting, of those who started smoking as youths and are still smoking at age 35, 45% will quit by age 60. So a conservative estimate is that 263,00 to 525,000 additional persons will have their lives shortened due to increased smoking.

As noted above, smoking throughout one's life shortens life expectancies 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 to 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% rate from age 69 (typical life expectancy for smokers) to age 19, this is a foregone value of life years in today's dollars of $36 billion to $73 billion. Once again, this is a vast oversimplification, as both quitting technologies and the mortality impacts of smoking are evolving rapidly over time. But it suggests the importance that 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 them to considering cigarette taxes

as the most effective means of reducing youth smoking. Of course, with youths only smoking about 2% of cigarette packs, taxes are a very blunt instrument 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 (1999), Evans, Ringel, and Stech (1999) and Gruber and Koszegi (1999) provide further discussions of these factors. But the results here suggest that consideration of optimal cigarette tax policy must include the very strong effect that taxes have on smoking by teens.

Appendix: Youth Access Index

Our Youth Access Index (YAI) is based upon 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 below 18; a score of 3 if the minimum age is 18 but there is no requirement of sign posting and/or there is no specific penalty for failure to post a sign; a score of 4 if the minimum age is 18 with specific sign posting requirements and penalties for failure to post; and a score of 5 if there is a minimum age above age 18 and there are posting/penalty provisions. These points are then summed across categories to get a total access index score. Then, states points are reduced by two points (to a minimum of zero) 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. These were Advertising, Licensing, and Restrictions on Minors. Points are awarded for advertising restrictions on a scale between one and four. A state earns one point for minimal limitations (no advertising on school buses, etc.) and four points 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 four points while states mandating only wholesale licenses received one point. The Restrictions on Minors category encompasses laws relating to underage purchase, possession, and use of tobacco. Those states outlawing these actions, but implementing no penalties for violating the laws received between zero and one point. The highest possible score, four points, is given to states outlawing purchase, possession, and use and implementing graduated penalties.

The YAI also allows for more point levels under each category than 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 3.5 points instead of 4. This affected twenty-one states overall. Similar half point steps were added to each 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.

We also in several instances 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, upon consultation with NCI representatives, it was revealed that Connecticut received two points for the vending machine category in 1996. The justification for this score was that a 1996 law added new restrictions. However, certain sections were not effective until after the time period of the NCI study. Since the law had already passed, though, NCI awarded points to reflect it in their study. For this project, however, this point assignment was inappropriate. Credit for laws was only awarded after the law came into effect. Therefore, the YAI contains several modifications to the factual basis of the NCI index.

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Table 1: Percent of Ever Smokers who Initiated at Each Age

Age	% of Ever Smokers Starting at Age	Cumulative Percent
6	0.47%	0.47%
7	0.79%	1.26%
8	1.05%	2.31%
9	1.24%	3.55%
10	2.67%	6.22%
11	1.85%	8.08%
12	6.50%	14.57%
13	6.63%	21.21%
14	8.81%	30.01%
15	11.52%	41.53%
16	13.53%	55.06%
17	8.73%	63.79%
18	11.09%	74.88%
19	4.47%	79.35%
20	5.48%	84.82%
21	3.21%	88.03%
22	1.76%	89.79%
23	1.05%	90.83%
24	0.85%	91.68%
25	1.47%	93.16%
26	0.39%	93.54%
27	0.42%	93.97%
28	0.40%	94.37%
29	0.19%	94.56%
30	0.98%	95.53%

SOURCE: National Health Interview Survey Smoking Supplements, 1992 and 1995

Table 2A
1997 MTF Public Use Regression Results: Participation

covariate	coeff (SE)	covariate	coeff (SE)
Smsa	-.0276 (.0102)	Age<18	.0126 (.00746)
# of Sibs	-.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)	Seperated/divorced omitted = unmarried	.045 (.0414)
Sex (1=male)	-.0122 (.00769)	Coll prep prog omitted = general high school program	-.0233 (.0091)
Grew up in rural omitted = "grew up mostly in suburbs"	-.0357 (.0139)	Voc prep prog omitted = general high school program	.0269 (.014)
Grew up in a town omitted = grew up mostly in suburbs	-.0178 (.0116)	No plan do coll omitted = plan to attend college	.0741 (.0142)
Grew up in city omitted = grew up mostly in suburbs	-.0307 (.0109)	Prob won't coll omitted = plan to attend college	.0418 (.0139)
Father ed < hs omitted = high school education	-.0082 (.0133)	Prob will coll omitted = plan to attend college	.0175 (.0101)
Mother ed < hs omitted = high school education	-.0318 (.0136)	Mom worked omitted = mom never worked w/growing up	.00753 (.0101)
Fath some college omitted = high school education	.0076 (.0117)	Works 1/0	-.0177 (.0124)
Moth some college omitted = high school education	.0232 (.011)	Job hrs/week	.00224 (.000559)
Fath college omitted = high school education	.0248 (.0108)	$/wk from job	.000458 (.000109)
Moth college omitted = high school education	.0373 (.0103)	$/wk oth sources	.000665 (.000131)
Northeast region omitted = midwest region	.0134 (.0108)	Religion important	-.00545 (.00451)
South region omitted = midwest region	-.0263 (.0099)	Attend rel. services reg	.000242 (.00439)
West region omitted = midwest region	-.0546 (.0127)	Days Missed School	.0161 (.001)
Adjusted R-squared	.1125	N	15525

Notes: Regressions estimated using 1997 cross-section of MTF data, by linear probability model; mean of dependent variable is 0.353. Standard errors in parentheses.

Table 2B
1997 MTF Public Use Regression Results: Conditional Intensity

covariate	coeff (SE)	covariate	coeff (SE)
Smsa	.143 (.303)	Age<18	.318 (.234)
# of Sibs	-.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)	Seperated/divorced Omitted = unmarried	5.29 (1.19)
Sex (1=male)	.867 (.244)	Coll prep prog Omitted = general high school program	-1.02 (.285)
Grew up in rural omitted = "grew up mostly in suburbs"	-.394 (.418)	Voc prep prog Omitted = general high school program	.915 (.409)
Grew up in a town omitted = grew up mostly in suburbs	-.761 (.359)	No plan do coll Omitted = plan to attend college	1.77 (.419)
Grew up in city omitted = grew up mostly in suburbs	-.994 (.341)	Prob won't coll Omitted = plan to attend college	.776 (.416)
Father ed < hs omitted = high school education	.569 (.417)	Prob will coll omitted = plan to attend college	-.279 (.321)
Mother ed < hs omitted = high school education	.059 (.441)	Mom worked omitted = mom never worked w/growing up	-.0113 (.321)
Fath some college omitted = high school education	-.312 (.367)	Works 1/0	-2.2 (.419)
Moth some college omitted = high school education	.25 (.342)	Job hrs/week	.0837 (.0177)
Fath college omitted = high school education	-.188 (.334)	$/wk from job	.0135 (.00347)
Moth college omitted = high school education	-.042 (.321)	$/wk oth sources	.0209 (.00394)
Northeast region omitted = midwest region	.205 (.322)	Religion important	.2 (.142)
South region omitted = midwest region	-1.06 (.312)	Attend rel. services reg	-.419 (.138)
West region omitted = midwest region	-2.48 (.405)	Sch days missed	.187 (.028)
Adjusted R-squared	.139	N	5450

Notes: Regressions estimated using 1997 cross-section of MTF data, by linear probability model; mean of dependent variable is 7.329. Standard errors in parentheses.

Table 3: Smoking Participation Coefficient Stability, MTF Public Use Data						
Covariate	(2)	(3)	(4)	(5)	(6)	(7)
	1976	1986	1997	1986-1976	1997-1986	1997-1976
Smsa	-.041 (.01)	-.034 (.01)	-.028 (.01)	.007 (.014)	.006 (.014)	.013 (.014)
Age*	.017 (.009)	-.003 (.008)	.013 (.007)	-.02 (.012)	*.016 (.011)	*-.004 (.011)
Black	.025 (.013)	-.2 (.012)	-.31 (.012)	-.226 (.018)	-.1 (.017)	-.326 (.018)
Other Non-White	-.011 (.013)	-.075 (.011)	-.134 (.01)	-.065 (.017)	-.059 (.015)	-.124 (.016)
Works 1/0	.009 (.012)	.001 (.012)	-.018 (.012)	-.008 (.017)	-.019 (.017)	-.027 (.017)
Job hours per week	.003 (.0004)	.002 (.001)	.002 (.001)	-.0009 (.0007)	.0004 (.0008)	-.0004 (.0007)
Job inc/week (/100)	.04 (.001)	.03 (.01)	.05 (.01)	-.01 (.01)	.02 (.02)	0.00 (.02)
Oth inc/week (/100)	0.00 (.01)	.04 (.02)	.07 (.01)	.04 (.02)	.02 (.02)	.07 (.02)
Male	-.063 (.008)	-.067 (.007)	-.012 (.008)	-.004 (.011)	.055 (.011)	.051 (.011)
Grew up rural	-.028 (.013)	-.039 (.013)	-.036 (.014)	-.011 (.019)	.003 (.019)	-.008 (.019)
Grew up town	.001 (.011)	-.035 (.011)	-.018 (.012)	-.036 (.016)	.017 (.016)	-.02 (.016)
Grew up urban	.024 (.011)	-.012 (.01)	-.031 (.011)	-.035 (.015)	-.019 (.015)	-.055 (.015)
Dad's ed<h. school	.01 (.011)	-.019 (.011)	-.008 (.013)	-.029 (.016)	.011 (.017)	-.018 (.017)
Mom's ed<h. school	-.023 (.011)	.001 (.011)	-.032 (.014)	.023 (.016)	-.033 (.018)	-.009 (.018)
Dad's ed some coll	-.007 (.014)	.003 (.011)	.008 (.012)	009 (.018)	.005 (.016)	.014 (.018)
Mom's ed some coll	.007 (.013)	.002 (.01)	.023 (.011)	-.004 (.016)	.021 (.015)	.017 (.017)
Dad college-ed	.021 (.012)	.013 (.01)	.025 (.011)	-.009 (.016)	.012 (.015)	.003 (.016)
Mom college-ed	-.02 (.013)	.009 (.01)	.037 (.01)	.028 (.016)	.029 (.014)	.057 (.016)
GPA	-.282 (.02)	-.203 (.018)	-.265 (.02)	.08 (.027)	-.063 (.027)	.017 (.028)
Northeast	.022 (.01)	.009 (.01)	.014 (.011)	-.013 (.014)	.005 (.015)	-.008 (.015)
South	-.007 (.01)	-.029 (.009)	-.026 (.01)	-.022 (.014)	.003 (.014)	-.019 (.014)
West	-.11 (.011)	-.106 (.011)	-.055 (.013)	.004 (.015)	.051 (.017)	.056 (.017)
Mom ever worked	.007 (.008)	.016 (.008)	.008 (.01)	.005 (.012)	-.008 (.013)	-.004 (.013)
Parents<2	-.001 (.01)	.022 (.008)	.021 (.009)	.022 (.013)	-.001 (.012)	.022 (.013)
Married	.039 (.025)	-.095 (.025)	-.037 (.027)	-.134 (.036)	.058 (.037)	-.077 (.037)
Engaged	.054 (.015)	.053 (.015)	.069 (.016)	-.001 (.021)	.016 (.022)	.015 (.022)
Seperated/divorced	.066 (.058)	.004 (.042)	.045 (.041)	-.062 (.071)	.042 (.059)	-.022 (.071)
Coll prep program	-.038 (.01)	-.061 (.009)	-.023 (.009)	-.023 (.013)	.038 (.013)	.015 (.013)
Voc'l program	.032 (.011)	.023 (.012)	.027 (.014)	-.009 (.016)	.004 (.018)	-.005 (.018)
No plan attend coll	.093 (.012)	.073 (.012)	.074 (.014)	-.02 (.017)	.001 (.019)	-.019 (.019)
Prob won't go coll	.044 (.012)	.042 (.012)	.042 (.014)	-.003 (.017)	0 (.018)	-.003 (.019)
Prob will go coll	.025 (.011)	.004 (.01)	.017 (.01)	-.021 (.014)	.013 (.014)	-.008 (.015)
Days missed school	.019 (.001)	.016 (.001)	.016 (.001)	-.003 (.001)	0 (.001)	-.003 (.001)
Relig services reg'ly	-.035 (.004)	-.029 (.004)	0 (.004)	.006 (.006)	.029 (.006)	.036 (.006)
Religion important	-.029 (.005)	-.014 (.004)	-.005 (.005)	.016 (.006)	.008 (.006)	.024 (.006)
Adjusted R-squared	.123	.112	.113	.127	.117	.119
N	16078	15310	15525	31388	30835	31603
Joint F-test of stability interacts:				probability >F = 0	probability >F = 0	probability >F = 0

* MTF data provides only age<>=18 in 1997; year of birth in 1976 and 1986. We construct a <>18 measure in the 1976 and 1986 data, but the mean and distribution differ substantially from 1997. Accordingly, the stability tests for age in columns (6) and (7) are probably inaccurate.

Table 4: Means of MTF, YRBS, and Natality Price Regression Samples

	Any Smoking	Cigs/Day when smoke	Real Price ($1982)	Real Excise Tax ($1982)
Monitoring the Future Data - 1991-97				
12th Grade (N=91,567)	0.309 (0.462)	7.21 (8.87)	1.39 (0.17)	0.21 (0.10)
8-10th grade (N=213,527)	0.217 (0.412)	5.42 (8.38)	1.38 (0.17)	0.21 (0.10)
8-12th Grade (N=336,665)	0.246 (0.431)	6.13 (8.63)	1.39 (0.17)	0.21 (0.10)
Youth Risk Behavior Survey Data - 1991,1993,1995,1997				
12th Grade (N=14,346)	0.358 (0.479)	6.06 (6.13)	1.28 (0.15)	0.21 (0.10)
9-11th Grade (N=38,932)	0.315 (0.464)	5.15 (5.70)	1.28 (0.15)	0.21 (0.10)
9-12th Grade (N=53,278)	0.326 (0.469)	5.42 (5.85)	1.28 (0.15)	0.21 (0.10)
Natality Data - 1991-1997				
17-18 Years Old (N = 666)	0.180 (0.075)	10.23 (1.31)	1.23 (0.14)	0.19 (0.10)
13-16 Years Old (N =1319)	0.127 (0.071)	9.21 (1.70)	1.22 (0.14)	0.19 (0.10)
13-18 Years Old (N = 1985)	0.164 (0.078)	9.93 (1.51)	1.22 (0.14)	0.19 (0.10)

Notes: Author's 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 text, with means weighted by cell count. Cigs/day when smoke is cigarettes per day smoked on days when smoking. Standard deviations in parentheses.

Table 5: Impact of Price and Regulations on Youth Smoking in MTF Data

	12th Graders		8th & 10th Graders		8th - 12th Graders	
	Partic.	Cigs/Day	Partic.	Cigs/Day	Partic.	Cigs/Day
Price	-0.148	-0.310	-0.033	-0.013	-0.055	-0.129
	(0.078)	(2.388)	(0.035)	(1.243)	(0.034)	(1.132)
	[-0.666]	[-0.059]	[-0.21]	[-0.003]	[-0.311]	[-0.029]
Access Index/100	0.084	-3.48	0.033	-5.520	0.066	-5.22
	(0.106)	(2.76)	(0.060)	(1.640)	(0.056)	(1.49)
Private Work	-0.041	0.462	-0.006	1.464	-0.021	1.045
	(0.028)	(0.589)	(0.017)	(0.489)	(0.017)	(0.348)
Gov't Work	0.022	-1.128	-0.019	-0.813	-0.001	-0.834
	(0.026)	(0.517)	(0.015)	(0.394)	(0.013)	(0.251)
Rest.	0.032	2.166	0.012	0.868	0.016	1.318
	(0.030)	(0.783)	(0.017)	(0.615)	(0.015)	(0.483)
Schools	0.050	0.931	0.044	0.788	0.040	0.645
	(0.030)	(0.915)	(0.018)	(0.553)	(0.015)	(0.392)
Other	-0.080	-2.791	-0.032	-1.424	-0.038	-1.617
	(0.041)	(1.234)	(0.020)	(0.775)	(0.019)	(0.621)
Male	0.016	1.235	-0.009	0.926	-0.001	1.041
	(0.004)	(0.115)	(0.003)	(0.085)	(0.003)	(0.069)
Non-White	-0.153	-1.908	-0.076	-0.436	-0.1	-0.962
	(0.007)	(0.171)	(0.004)	(0.119)	(0.005)	(0.115)
Grade 8			-0.088	-2.746	-0.185	-3.815
			(0.018)	(0.576)	(0.014)	(0.538)
Grade 10					-0.098	-1.055
					(0.019)	(0.632)
Grade 8, Age<=13			-0.027	0.217	0.03	0.181
			(0.018)	(0.573)	(0.014)	(0.519)
Grade 8, Age>=14			0.026	1.829	0.083	1.8
			(0.018)	(0.572)	(0.014)	(0.515)
Grade 10, Age<=15			-0.037	-1.211	0.019	-1.334
			(0.003)	(0.104)	(0.019)	(0.625)
Grade 10, Age>=16					0.055	-0.119
					(0.019)	(0.627)
Grade 12, Age <=17	-0.013	-0.546			-0.012	-0.512
	(0.003)	(0.113)			(0.003)	(0.112)
Number of Obs	106539	32868	230126	49927	336665	82795

Notes: Standard errors (corrected for state/year clustering) in parentheses; standard errors in square brackets. All regressions also include full set of state and year fixed effects.

Table 6: Impact of Price and Regulations on Youth Smoking in YRBS Data

	12th Graders		9th - 11th Graders		9th - 12th Graders	
	Partic.	Cigs/Day	Partic.	Cigs/Day	Partic.	Cigs/Day
Price	-0.429	-7.462	0.103	-0.912	-0.032	-2.228
	(0.200)	(3.461)	(0.134)	(1.847)	(0.103)	(1.937)
	[-1.534]	[-1.576]	[0.419]	[-0.227]	[-0.126]	[-0.526]
Access Index/100	-0.060	0.461	-0.098	-0.804	-0.098	-0.316
	(0.169)	(3.985)	(0.092)	(2.223)	(0.088)	(2.308)
Private Work	0.006	-2.723	0.064	1.146	0.051	0.905
	(0.047)	(1.742)	(0.046)	(0.552)	(0.042)	(0.476)
Gov't Work	-0.075	-0.168	-0.088	-1.800	-0.087	-1.971
	(0.032)	(1.459)	(0.037)	(0.473)	(0.034)	(0.396)
Rest.	-0.162	-1.435	-0.006	0.383	-0.050	-0.447
	(0.028)	(1.466)	(0.025)	(0.627)	(0.025)	(0.783)
Schools	0.006	-0.578	0.008	0.517	0.008	0.578
	(0.060)	(1.161)	(0.045)	(0.567)	(0.034)	(0.533)
Other	0.012	6.164	-0.015	0.775	0.002	1.842
	(0.065)	(2.260)	(0.054)	(0.813)	(0.048)	(0.950)
Male	0.002	1.283	-0.001	0.846	0.000	0.984
	(0.014)	(0.268)	(0.010)	(0.164)	(0.008)	(0.146)
White	0.048	0.585	0.046	-0.148	0.044	0.071
	(0.028)	(0.634)	(0.014)	(0.506)	(0.012)	(0.382)
Black	-0.206	-3.16	-0.128	-2.69	-0.149	-2.73
	(0.033)	(0.621)	(0.018)	(0.541)	(0.015)	(0.417)
Hispanic	-0.013	-2.10	0.003	-2.09	-0.002	-2.02
	(0.030)	(0.627)	(0.016)	(0.448)	(0.014)	(0.354)
Grade 10			-0.055	0.261	-0.055	0.266
			(0.010)	(0.366)	(0.011)	(0.363)
Grade 11			-0.076	0.289	-0.075	0.379
			(0.017)	(0.455)	(0.017)	(0.452)
Grade 12					-0.092	0.103
					(0.022)	(0.551)
Age 15	-0.052	-12.28	0.079	1.04	0.079	1.00
	(0.211)	(6.70)	(0.013)	(0.369)	(0.013)	(0.370)
Age 16	-0.044	-14.86	0.146	1.39	0.146	1.36
	(0.161)	(4.93)	(0.016)	(0.443)	(0.016)	(0.444)

Age 17	-0.106	-15.50	0.191	2.28	0.191	2.13
	(0.145)	(4.62)	(0.020)	(0.542)	(0.020)	(0.544)
Age 18	-0.086	-14.95	0.244	2.71	0.218	2.53
	(0.144)	(4.66)	(0.035)	(0.772)	(0.023)	(0.618)
Dad Educ	0.014	-0.349	-0.007	-0.384	-0.002	-0.362
HS Grad	(0.022)	(0.431)	(0.015)	(0.252)	(0.012)	(0.215)
Dad Educ	0.008	-1.07	-0.035	-0.488	-0.025	-0.660
Some Coll	(0.027)	(0.549)	(0.018)	(0.410)	(0.014)	(0.332)
Dad Educ	-0.004	-1.26	-0.055	-0.905	-0.042	-0.971
Coll Grad	(0.024)	(0.493)	(0.019)	(0.305)	(0.015)	(0.232)
Mom Ed	-0.058	-0.701	-0.014	-0.640	-0.027	-0.686
HS Grad	(0.024)	(0.403)	(0.017)	(0.416)	(0.015)	(0.339)
Mom Ed	-0.031	-0.382	-0.030	-0.935	-0.030	-0.751
Some Coll	(0.030)	(0.508)	(0.013)	(0.429)	(0.015)	(0.326)
Mom Ed	-0.044	-0.785	-0.038	-0.908	-0.041	-0.891
Coll Grad	(0.027)	(0.446)	(0.017)	(0.423)	(0.015)	(0.338)
Number Obs	14346	4429	38932	11368	53278	15797

Notes: Standard errors (corrected for state/year clustering) in parentheses; standard errors in square brackets. All regressions also include full set of state and year fixed effects.

Table 7: Impact of Price and Regulations on Youth Smoking in Natality Data

	17-18 Year Olds		13-16 Year Olds		13-18 Year Olds	
	Partic.	Cigs/Day	Partic.	Cigs/Day	Partic.	Cigs/Day
Price	-0.055 (0.018) [-0.376]	-1.209 (0.527) [-0.145]	-0.025 (0.018) [-0.240]	-0.436 (0.638) [-0.058]	-0.047 (0.016) [-0.353]	-1.003 (0.440) [-0.124]
Access Index/100	0.023 (0.026)	-1.771 (0.718)	-0.006 (0.024)	-1.010 (1.106)	0.013 (0.023)	-1.485 (0.651)
Private Work	0.009 (0.007)	0.037 (0.329)	0.013 (0.007)	0.555 (0.466)	0.011 (0.005)	0.134 (0.254)
Gov't Work	0.000 (0.003)	0.014 (0.109)	-0.005 (0.004)	0.180 (0.160)	-0.002 (0.003)	0.021 (0.089)
Rest.	-0.012 (0.004)	-0.493 (0.233)	0.002 (0.005)	-0.972 (0.355)	-0.007 (0.004)	-0.597 (0.193)
Schools	0.002 (0.004)	-0.195 (0.175)	0.006 (0.005)	-0.613 (0.220)	0.004 (0.004)	-0.316 (0.148)
Other	-0.003 (0.005)	0.231 (0.238)	-0.011 (0.006)	0.299 (0.327)	-0.006 (0.004)	0.300 (0.205)
% White	0.179 (0.093)	2.16 (2.67)	0.131 (0.056)	-0.839 (2.59)	0.171 (0.046)	-0.016 (1.61)
% Black	-0.263 (0.096)	2.09 (2.80)	-0.139 (0.052)	-2.40 (2.39)	-0.203 (0.044)	-2.42 (1.49)
% Hispanic	-0.255 (0.044)	-1.90 (1.64)	-0.238 (0.028)	0.34 (1.13)	-0.238 (0.025)	-1.15 (0.896)
Age 14			0.017 (0.004)	-0.158 (0.366)	0.009 (0.004)	-0.187 (0.363)
Age 15			0.024 (0.005)	0.317 (0.354)	0.009 (0.005)	0.213 (0.349)
Age 16			0.033 (0.007)	0.734 (0.375)	0.011 (0.006)	0.557 (0.354)
Age 17					0.014 (0.007)	0.912 (0.368)
Age 18	0.003 (0.002)	0.502 (0.055)			0.019 (0.007)	1.362 (0.376)
Number Obs	1319	1189	666	666	1985	1855

Notes: Standard errors (corrected for state/year clustering) in parentheses; standard errors in square brackets. All regressions also include full set of state and year fixed effects.

Table 8: Price Coefficient Heterogeneity by Race

	Older Teens		Younger Teens		All Teens	
	Partic.	Cigs/Day	Partic.	Cigs/Day	Partic.	Cigs/Day
			MTF Data			
Whites	-0.091	0.721	-0.054	-1.611	-0.057	-0.848
	(0.010)	(2.637)	(0.047)	(1.214)	(0.041)	(1.225)
	[-0.350]	[0.130]	[-0.300]	[-0.393]	[-0.277]	[-0.181]
Non-Whites	-0.323	-7.690	0.025	4.962	-0.039	2.417
	(0.163)	(3.749)	(0.050)	(2.843)	(0.045)	(2.395)
	[-2.324]	[-2.03]	[0.226]	[1.488]	[-0.327]	[0.691]
			YRBS Data			
Whites	-0.198	-13.70	0.083	0.470	0.026	-3.563
	(0.271)	(4.554)	(0.177)	(2.326)	(0.123)	(2.344)
	[-0.628]	[-2.662]	[0.303]	[0.106]	[0.092]	[-0.775]
Blacks	-1.187	-22.78	-0.132	12.48	-0.369	11.24
	(0.485)	(20.50)	(0.372)	(12.44)	(0.351)	(10.81)
	[-9.259]	[-8.248]	[-0.874]	[4.958]	[-2.530]	[4.393]
			Natality Data			
Whites	-0.079	-0.934	-0.060	0.307	-0.079	-0.639
	(0.023)	(0.556)	(0.023)	(0.682)	(0.021)	(0.453)
	[-0.412]	[-0.109]	[-0.385]	[0.040]	[-0.433]	[-0.076]
Blacks	0.026	-3.357	0.033	-2.809	0.028	-3.256
	(0.017)	(1.286)	(0.019)	(2.113)	(0.015)	(1.144)
	[0.534]	[-0.539]	[1.115]	[-0.494]	[0.671]	[-0.539]
			YRBS Data - Parental Education			
Mom&Dad HS Drop or Grad	-1.266	-2.036	0.79	10.806	0.207	4.464
	(0.583)	(8.497)	(0.369)	(5.889)	(0.206)	(5.4)
	[-4.387]	[-0.401]	[2.721]	[2.514]	[0.715]	[0.103]
Mom&Dad Some or Grad Coll	-0.067	-10.068	0.228	-1.432	0.157	-3.285
	(0.238)	(4.159)	(0.231)	(3.144)	(0.188)	(2.353)
	[-0.236]	[-2.393]	[0.956]	[-0.390]	[0.645]	[-0.874]

Notes: Coefficient on price from regressions in MTF (first panel), YRBS (second and fourth panels), and Natality data (third panel). Regressions include all of the controls shown in Tables 2, 3, and 4, including full set of state and year fixed effects. Standard errors (corrected for state/year clustering) in parentheses; price elasticity in square brackets.

Table 9: Age of Initiation vs. Current Smoking				
	Start age 12-14	Start age 15-17	Start age 18-20	Start age 21-25
		Those Age 28+		
Smoke Now?	0.52	0.49	0.46	0.52
Smoke Every day?	0.45	0.41	0.36	0.42
Cigarettes Per Day	19.6	18.1	15.8	16.7
		Those Age 33+		
Smoke Now?	0.49	0.46	0.43	0.49
Smoke Every day?	0.42	0.38	0.35	0.40
Cigarettes Per Day	20.4	18.6	16.3	16.8
		Those Age 38+		
Smoke Now?	0.45	0.43	0.40	0.46
Smoke Every day?	0.40	0.36	0.33	0.38
Cigarettes Per Day	21.2	19.1	16.8	17.4

Notes: Authors tabulations of the 1992 and 1995 National Health Interview Survey. Each cell shows either the proportion smoking or the cigarettes per day, for the group that is labeled in the top row, at the age denoted for each panel.

Table 10: 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, 1986-1997)				
MTF Senior Smoking Rate in year t-10	0.648 (0.088)	0.397 (0.157)	0.781 (0.273)	0.500 (0.298)
Time Trend		-0.0030 (0.0016)		-0.0029 (0.0017)
42-43 Year Old Smoking Rate			-0.202 (0.389)	-0.148 (0.354)
NHIS 27-28 Year Olds (N=36, 1950-1995)				
NHIS 17-18 Smoking Rate in year t-10	1.817 (0.096)	0.833 (0.119)	0.544 (0.186)	0.506 (0.135)
Time Trend		-0.0048 (0.0005)		-0.0033 (0.0006)
42-43 Year Old Smoking Rate			0.811 (0.112)	0.402 (0.110)

Notes: Standard errors in parentheses. Regressions in first panel match data from BRFSS on 27-28 year olds and 42-43 year old controls for 1986-1997 to corresponding MTF data from 10 years earlier; second panel matches backcast data from NHIS for 27-28 and 42-43 year olds to data from ten 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.

Table 11: The Effect of Current and Teen Taxes on Smoking of Pregnant Women

	All			Non-Movers Only		
	Cigarettes Smoked	Particip-ation	Cigs/Day if Smoke	Cigarettes Smoked	Particip-ation	Cigs/Day if Smoke
Current Tax	-0.810 (0.033) [-0.455]	-0.080 (0.002) [-0.552]	0.536 (0.131) [0.046]	-0.970 (0.064) [-0.513]	-0.086 (0.004) [-0.559]	0.327 (0.186) [0.028]
Teen Tax	-0.480 (0.061) [-0.188]	-0.013 (0.004) [-0.061]	-0.678 (0.242) [-0.040]	-0.598 (0.111) [-0.221]	-0.017 (0.007) [-0.078]	-0.500 (0.324) [-0.030]
% HS Dropout	5.73 (0.038)	0.390 (0.002)	4.66 (0.134)	3.98 (0.228)	0.256 (0.017)	7.26 (0.677)
% HS Grad	2.93 (0.018)	0.214 (0.001)	1.90 (0.079)	3.59 (0.136)	0.255 (0.010)	3.94 (0.409)
% Some College	1.56 (0.021)	0.121 (0.001)	0.797 (0.094)	2.84 (0.166)	0.260 (0.012)	0.902 (0.512)
% White	0.838 (0.045)	0.022 (0.003)	2.58 (0.189)	2.31 (0.347)	0.102 (0.026)	6.19 (1.14)
% Black	-0.047 (0.049)	0.015 (0.003)	-1.87 (0.205)	2.72 (0.361)	0.210 (0.027)	1.47 (1.18)
% Hispanic	-2.71 (0.042)	-0.178 (0.003)	-3.17 (0.181)	-3.20 (0.258)	-0.218 (0.019)	-6.11 (0.844)
Number of Obs.	337690	338167	182379	9941	9947	8945
Mean of Depend. Variable	1.95	0.158	12.85	2.07	0.167	12.86

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

Appendix Table 1: Means of Regulatory Variables in MTF

	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	0.44	0.44	0.44
	(0.50)	(0.50)	(0.50)
Clean Air: Government Work	0.73	0.71	0.72
	(0.44)	(0.45)	(0.45)
Clean Air: Restaurants	0.64	0.61	0.62
	(0.48)	(0.49)	(0.49)
Clean Air: Schools	0.90	0.85	0.87
	(0.30)	(0.36)	(0.34)
Clean Air: Other	0.93	0.91	0.92
	(0.25)	(0.29)	(0.28)
Number of Obs	106,539	230,126	336,665

Notes: From authors' tabulations of 1991-1997 MTF restricted sample data desribed in text. Standard deviations in parentheses.

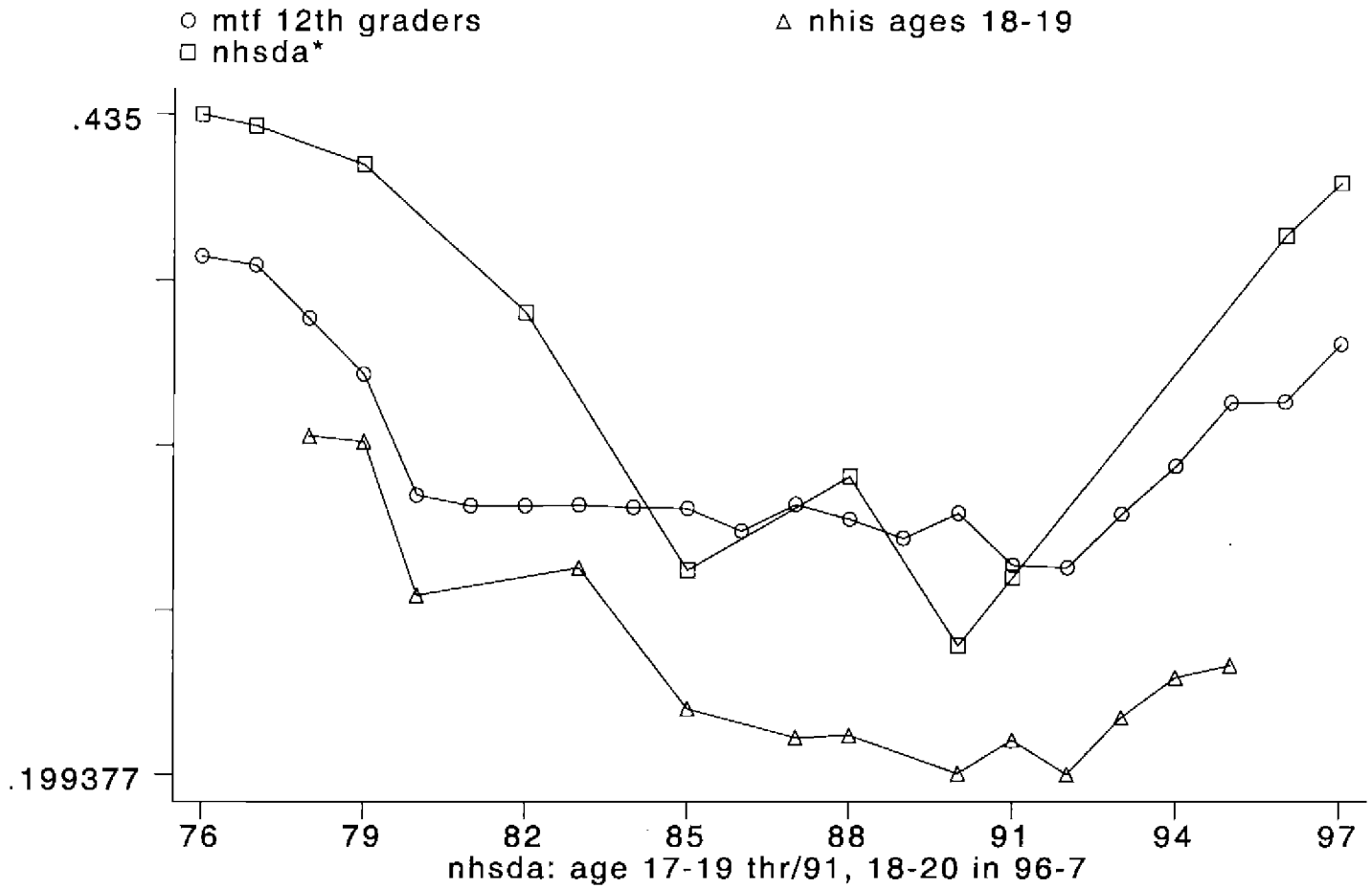

Figure 1. Smoking Participation: Older Youth Time Series

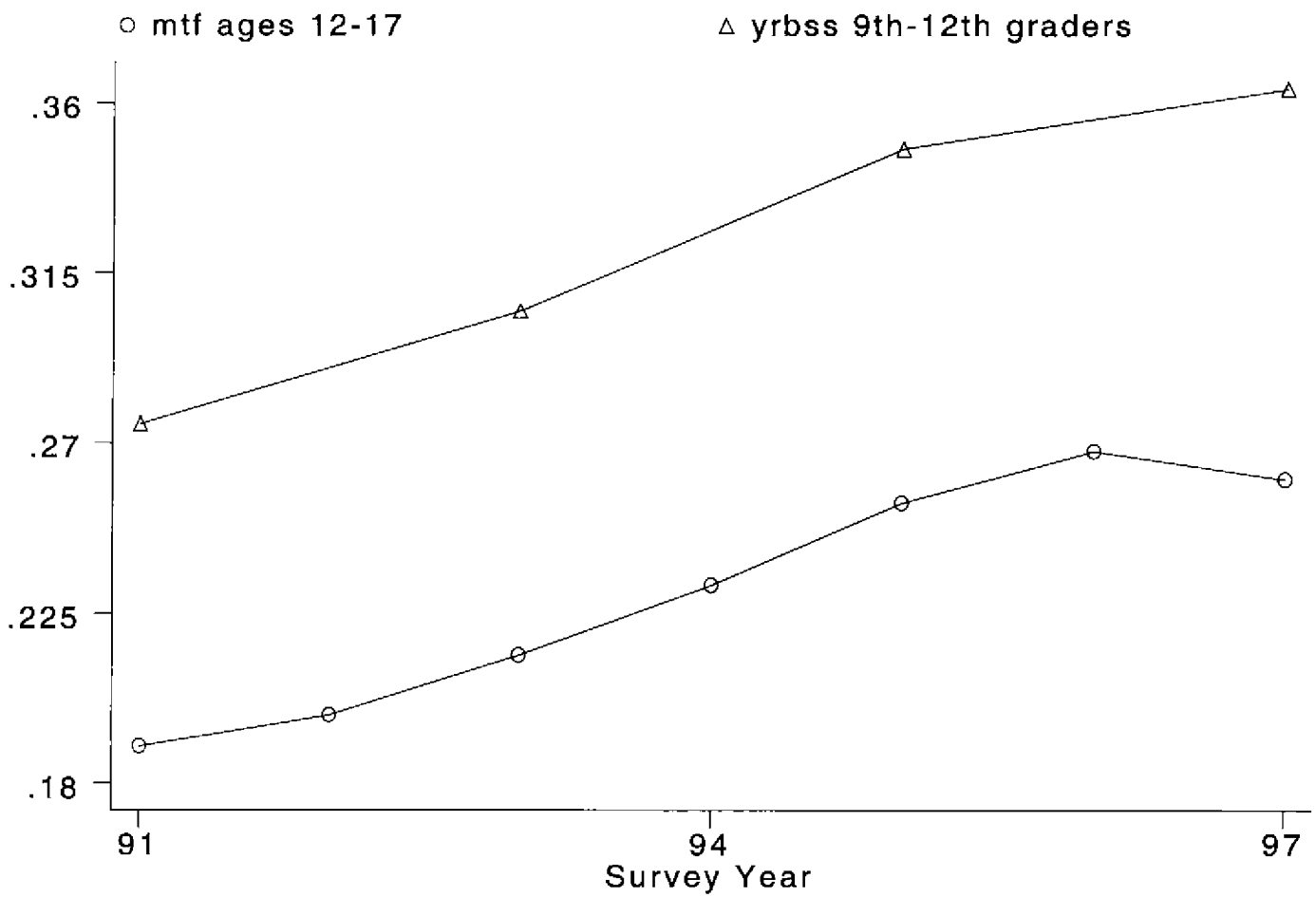

Figure 2. Youth Smoking Participation in 90s

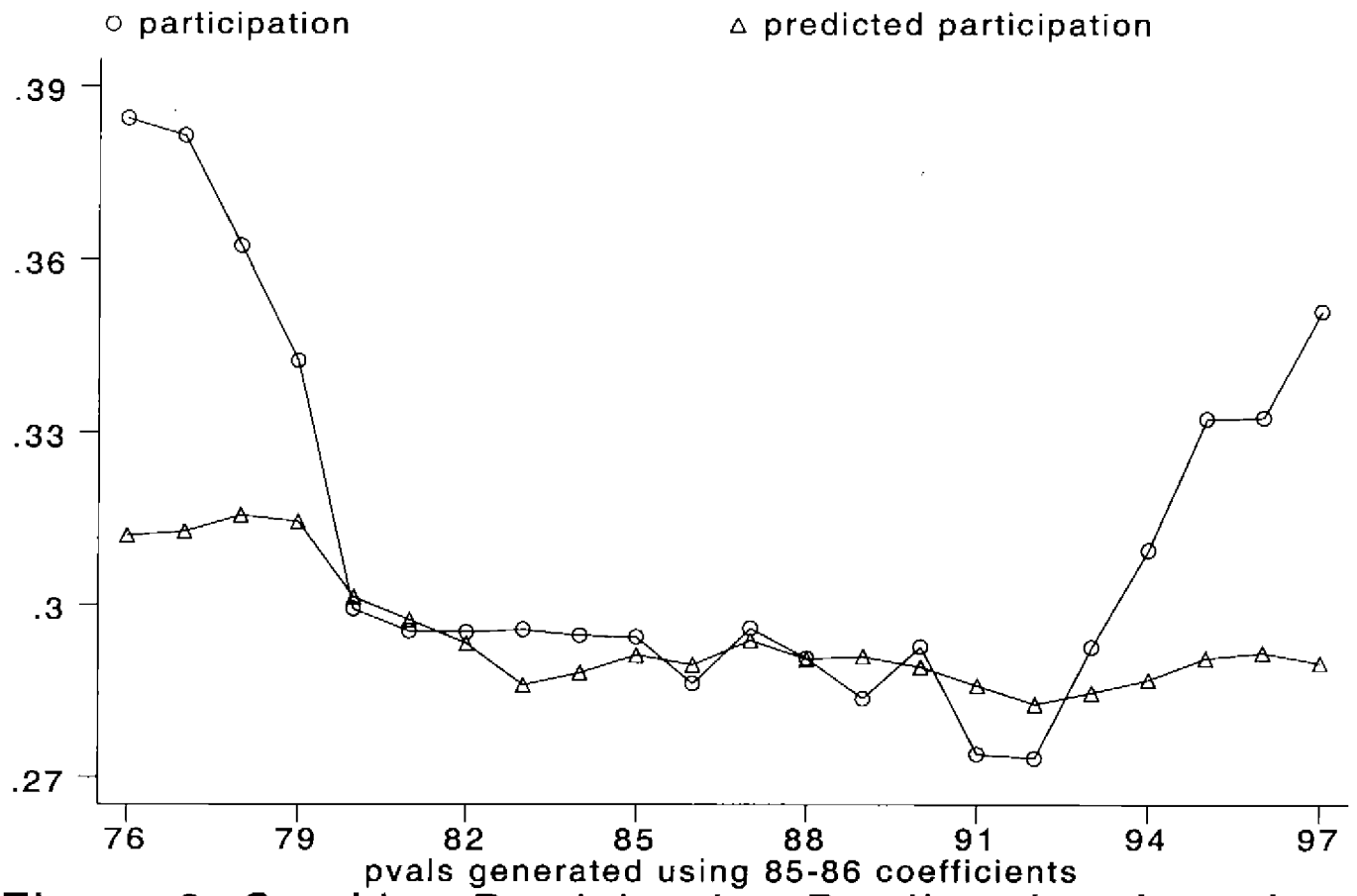

Figure 3. Smoking Participation Predicted v. Actual

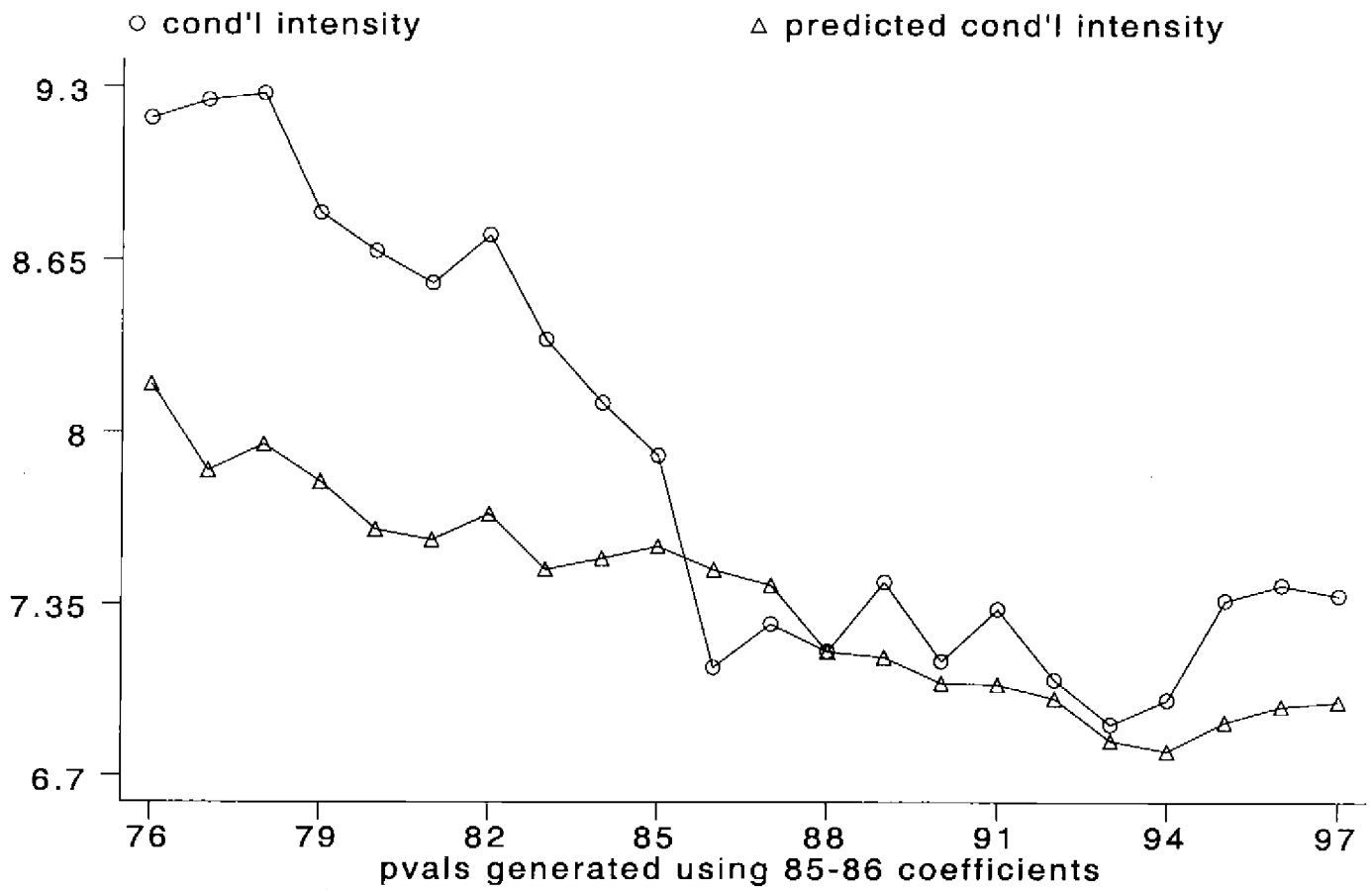

Figure 4. Smoking Intensity Predicted v. Actual

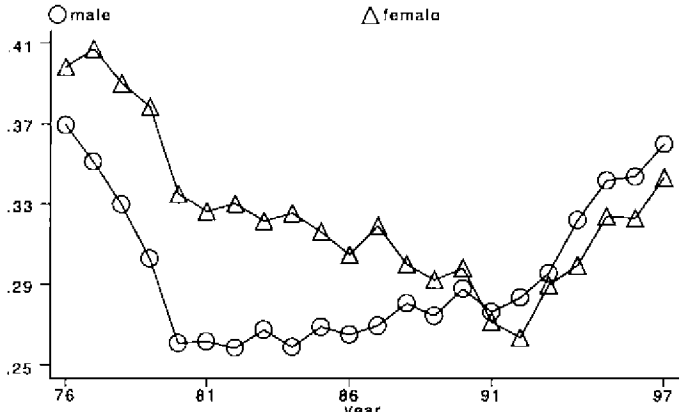

Figure 5a. Smoking Participation by Gender

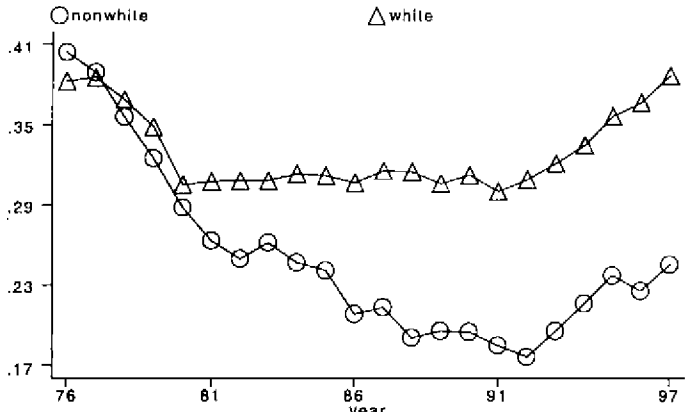

Figure 5b. Smoking Participation by Race

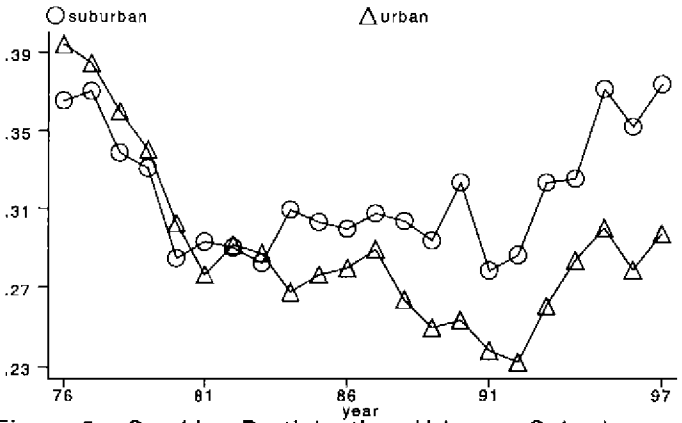

Figure 5c. Smoking Participation: Urban v. Suburban

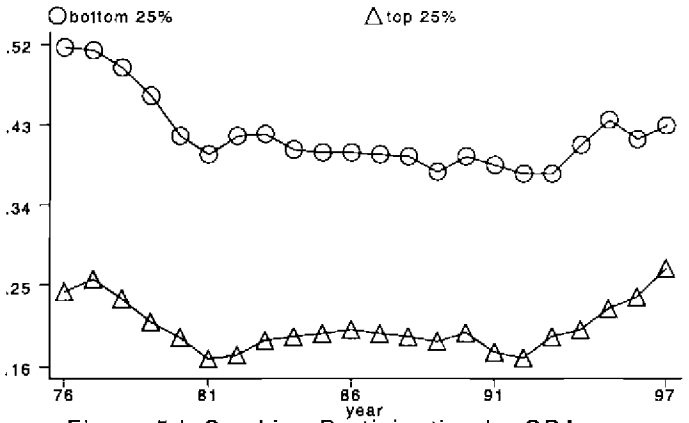

Figure 5d. Smoking Participation by GPA

Figure 5: Smoking Participation by Sub-Group

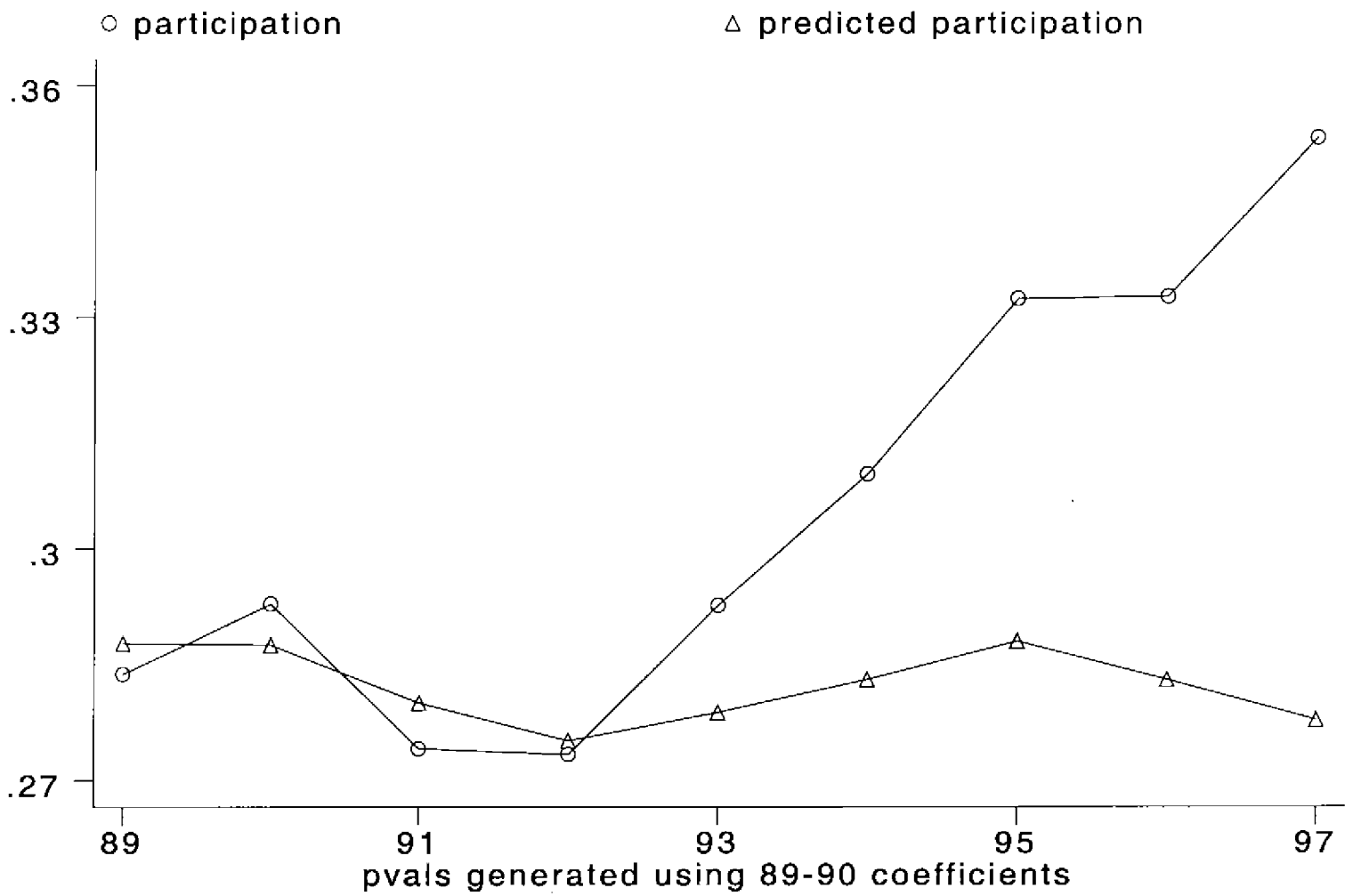

Figure 6. Predicted vs. Actual Participation w/ Attitude Vars

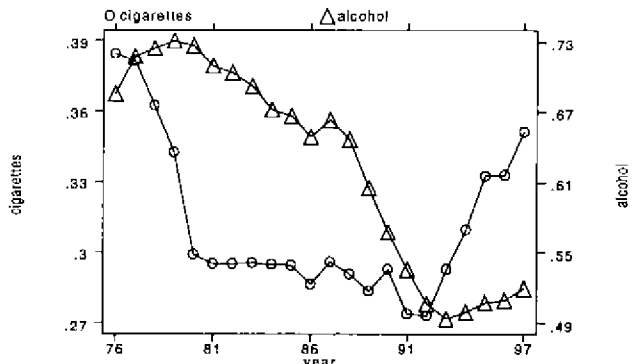

Figure 7a. Alcohol v. Cigarettes

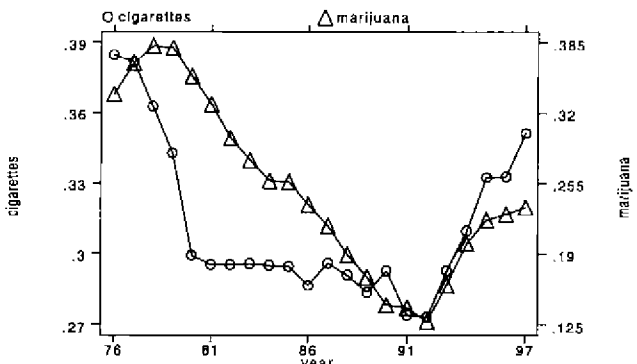

Figure 7b. Marijuana v. Cigarettes

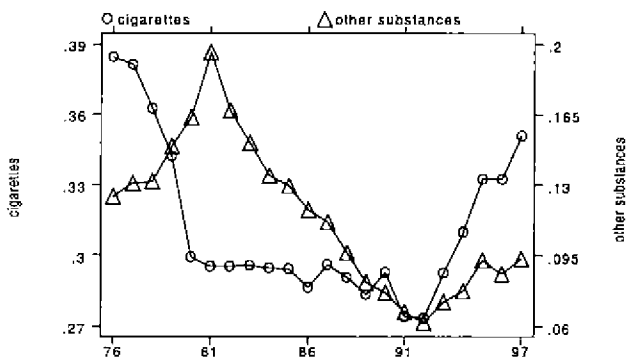

Figure 7c. Other Drugs v. Cigarettes

Figure 7. Other Substance Participation v. Smoking

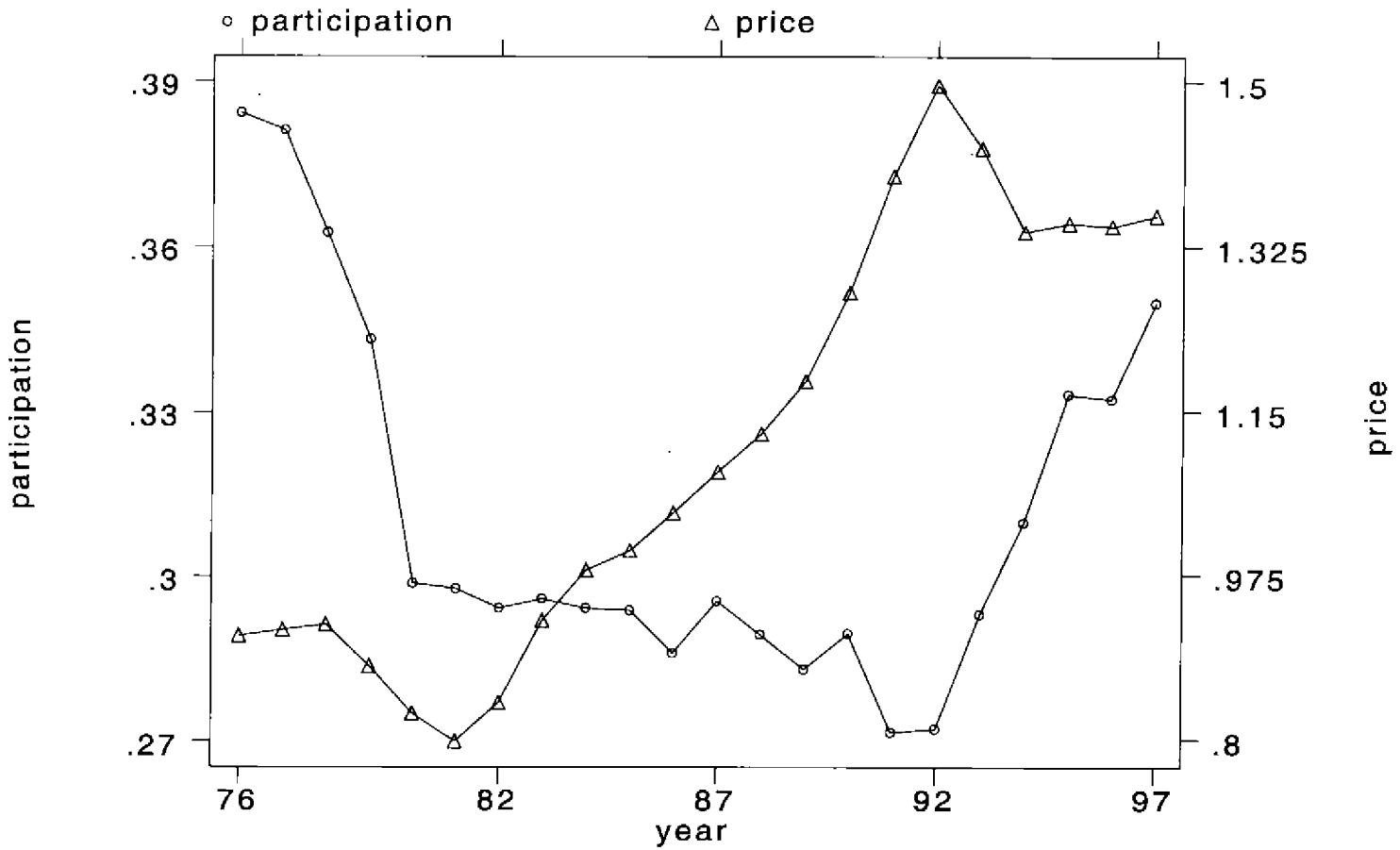

Figure 8: Smoking Participation v. Cigarette Price