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MORE BAD NEWS FOR SMOKERS?

THE EFFECTS OF CIGARETTE SMOKING ON WAGES

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Using National Longitudinal Survey of Youth data, the authors examine the effect of smoking on wages. Their analysis controls for differences in individual characteristics that may be correlated with both smoking and wages, including unobservable person-specific characteristics that are constant over time, and unobservable characteristics that are constant within a family. Estimates from alternative specifications indicate that smoking reduced wages by roughly 4–8%. Empirical tests of three potential explanations for this finding yield no conclusive results.

Since the release of the 1964 Surgeon General's report asserting that smoking causes cancer and other serious diseases, evidence of the adverse health effects of smoking, both on smokers and on others who are exposed to cigarette smoke, has continued to mount. Over time, mild public intolerance of smoking has developed into fairly widespread hostility. More recent public policy in this area has been designed to protect nonsmokers from second-hand smoke. Policies enacted at the federal, state, and local levels of government include the federal ban on smoking on domestic airline flights and state and municipal regulations banning or restricting smoking in government buildings, private workplaces, bars, and restaurants. Moreover, many employers have instituted their own smoking policies banning smoking in buildings or restricting it to designated areas. The Oklahoma City fire department even maintains a "nonsmokers only" employment policy that has been upheld by a federal court. In this atmosphere, discriminatory employment practices against smokers may have emerged, reducing smokers' wages and employment prospects.

Discrimination is not the only reason one may expect smokers to perform less well than nonsmokers in the labor market, however. Smoking may reduce net worker productivity by interfering with workers' ability to carry out manual tasks, for in-

All programs used to create the data sets and estimate the models reported in this paper are available upon request. Please contact Phillip Levine, Department of Economics, Wellesley College, Wellesley, MA 02181.

stance. Smokers may also be more costly than nonsmokers as employees, due to increased absenteeism, higher health and fire insurance premia, higher maintenance costs, and negative effects on morale. In addition, the health problems associated with smoking may lead smokers to prefer jobs that include employer-provided health insurance at the cost of a lower wage. Finally, smoking may indicate a high "rate of time preference" (a high value placed on present as opposed to future consumption), which would be associated with fewer investments in human capital and, hence, lower wages.

Our goal in this paper is to provide empirical evidence regarding the effects of smoking on wages. Toward that end, the primary task is to disentangle the effects of smoking on wages from the effects of observed and unobserved personal characteristics that may be correlated with both smoking behavior and labor market outcomes. Using data from the National Longitudinal Survey of Youth (NLSY), we employ three techniques directed at addressing this issue. Initially, we estimate a standard, cross-sectional, human capital earnings function including a dummy variable indicating whether an individual smokes along with an extensive set of personal and family background measures, including an individual's score on an aptitude/achievement test. In addition, we take advantage of the household structure of the NLSY, comparing the difference in wages between siblings who differ in their smoking behavior. This approach controls for unobservable characteristics that are constant within a family. Finally, we use the panel nature of the NLSY data to estimate changes in wages as a function of changes in smoking behavior over time, therefore controlling for any person-specific characteristics that are constant over time.

Background and Literature Review

There are four principal sets of reasons we might expect smoking to have an adverse effect on wages, as mentioned above. If employers, co-workers, or customers dis-

like smokers, discrimination against smokers leading to lower wages could result. Recent publicity about the effects of second-hand smoke makes it likely that both co-workers and customers may object to working with smokers, causing some employers to discriminate against them.

Employers may also find that employees who smoke are less productive or more costly (or both) than those who do not, and may, therefore, offer them lower wages. Smokers' productivity would be lower if the act of smoking itself draws time away from work; if smokers are less physically fit and therefore less able to perform certain manual tasks; if smokers have higher absentee rates due to illness; or if smokers impose some other costs on firms, such as higher fire insurance or cleaning costs. Kristein (1983), drawing together evidence from a number of studies, estimated the productivity costs of smoking to be between $80 and $160 per smoker per year, measured in 1980 dollars. Absenteeism by smokers, he argued, imposed an additional $40 to $80 in costs per smoker per year. These findings are echoed by Bertera (1991), who argued that smokers miss an average of one additional work day per year due to illness, controlling for other factors such as education and age.

Health is always an important consideration in discussions of the consequences of smoking and may affect smokers' labor market outcomes. The major health effects of smoking generally appear late in life, so one might be concerned that smoking would not have an impact on the current health and labor market outcomes of the young adults sampled in the NLSY. A number of studies, however, indicate that smoking does indeed have negative effects on the health of younger people. Two studies of the fitness of military personnel in the United States and United Kingdom (Conway and Cronan 1992; Hoad and Clay 1992) found that smoking was associated with lower physical endurance and less improvement following physical training, even among young and relatively fit individuals. A study of basic trainees in the military (Blake, Abell, and Stanley 1988)

found that smokers were 46% more likely than nonsmokers to have experienced upper respiratory infections during the 13-week training period. Andreski and Breslau (1993) found that nicotine dependence in young smokers was associated with poor physical and psychological health. Therefore, although we may expect the cumulative effects of smoking to be experienced late in life, medical evidence indicates that current smoking by younger people does have adverse current health consequences.

The health consequences of smoking may be relevant for current earnings insofar as they increase the cost of health insurance provision. Smokers will use more medical care than nonsmokers, both in the treatment of respiratory problems in youth and in the treatment of serious diseases later in life. Hodgson (1992) estimated that the present discounted value of excess medical expenditures over the lifetime of current smokers at $6,239 per smoker. Kristein (1983) estimated the health insurance cost per smoker per year at $204 in 1980 dollars. These higher health care costs would translate into lower wages if employers provide health insurance benefits and the principle of compensating differentials applies. Employers, recognizing that smokers are more costly to insure, would be willing to employ them only if their total cost to the firm were no higher than the cost of nonsmokers. The higher health insurance costs, then, would need to be offset by lower wages paid to smokers. Smokers, aware of the potential health consequences of their behavior, would be attracted to jobs that provide insurance, and would be willing to accept lower wages in compensation. This compensating differentials argument would produce a negative wage consequence for smoking among employers who provide health insurance.

Finally, an individual's decision to smoke may be correlated with other preferences and behaviors that reduce wages. Since the pleasures associated with smoking occur today, while the adverse health consequences are largely concentrated in the future, the decision to smoke may reflect a high rate of time preference, as argued by

Becker and Murphy (1988) and Becker, Grossman, and Murphy (1994). Individuals with higher rates of time preference are less likely to invest in human capital, which would result in lower wages. Smokers do appear to have lower levels of educational attainment than nonsmokers (Evans and Montgomery 1994). They may also choose to invest less in on-the-job training, which would result in flatter earnings profiles. We assess the empirical validity of these potential causes of the wage effects of smoking later in the paper.

To our knowledge, the only study examining the relationship between smoking and wages was conducted by Leigh and Berger (1989). That study examined the relationship between smoking and being overweight on earnings using data from the 1973 Quality of Employment Survey. No statistically significant effects of either smoking status or being overweight were found. The research we conduct has the advantage of using a larger and more comprehensive database that can more precisely estimate the effect of smoking on wages. As described in the following section, it also allows us to estimate models reducing or eliminating some of the potential biases that are inherent in the previous research.

Methodology

The challenge in this research is to separate the effects of smoking status from the effects of other personal characteristics that are correlated with both smoking status and labor market outcomes. To that end, we apply several methodologies similar to those that have been used in related literatures on the labor market effects of other personal behaviors, such as alcohol use.[1]

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[1]The approaches employed in this work are quite similar to those used by Kenkel and Ribar (1994) in their examination of the effect of alcohol consumption on labor market outcomes. Alternative techniques have been employed in studies of the effects of drug and alcohol use on wages, including two-stage least squares techniques (Kaestner 1991) and Heckman (1979)–style corrections (see Register and Williams 1992; Gill and Michaels 1992; Berger and

One approach is simply to include a dummy variable for smoking behavior in a standard human capital earnings regression (Willis 1986) augmented by a full range of personal and family background characteristics.[2] This regression takes the form

$$(1) \quad \ln W_i = \alpha + X_i \beta_1 + F_i \beta_2 + S_i \beta_3 + e_i,$$

where W_i represents wages, X_i is a vector of observed individual characteristics, F_i is a vector of family background characteristics, and S_i is an indicator variable that takes the value of unity if the individual is a smoker and zero otherwise. The coefficient, β_3, can then be interpreted as the wage consequences of smoking.

This approach presents problems if there are unobserved characteristics of individuals that are correlated with smoking and with wages even after holding constant other observable characteristics. In that event, the estimate of β_3 will be biased. For example, if people with poor judgment are likely to choose to smoke, and no measure of judgment is included in the regression, the estimate of β_3 will be biased downward, as the negative wage consequences of having poor judgment would be attributed to the act of smoking instead.

A simple mechanism for dealing with this difficulty is to include as regressors measures that may be correlated with relevant unobservables. One potential measure is the individual's score on an aptitude/achievement test, such as the Armed Forces Qualifying Test (AFQT); AFQT scores are available in the NLSY. This

approach has been used by Blackburn and Neumark (1993) using the NLSY data examining the returns to education. To the extent that test scores capture otherwise unobservable productivity-related characteristics, they can reduce the downward bias on the smoking coefficient.

It is also possible, of course, that smokers are different from nonsmokers in ways that are not correlated with test scores or other observable characteristics. One approach to account for the effects of this unobservable heterogeneity is to exploit the household structure of the NLSY by looking at differences in wages and smoking behavior across siblings. This technique is similar to that used by Averett and Korenman (1993) in their discussion of the effect of weight on earnings and Ashenfelter and Zimmerman (forthcoming) in their estimation of the returns to education. To the extent that unobserved characteristics are correlated across siblings, this technique will produce unbiased estimates of the effects of smoking. These estimates can be generated either in single cross-sections or by "stacking" multiple years of data. An advantage of the latter approach over using a single year is that the additional data improve the precision of all coefficient estimates, although standard errors need to be adjusted to correct for repeated observations on each sibling pair.

More formally, equation (1) can be modified to represent the wages of sibling s in time period t,

$$(2) \quad \ln W_{st} = \alpha + X_s \beta_1 + F_s \beta_2 + S_{st} \beta_3 + \delta_s + e_{st},$$

where δ_s represents a family-specific component of an individual's wage that is time-invariant (a "family fixed effect"). Taking the difference between the wages of an older sibling, o, and a younger sibling, y, yields

$$(3) \quad \ln W_{ot} - \ln W_{yt} = \alpha + (X_{ot} - X_{yt}) \beta_1 + (S_{ot} - S_{yt}) \beta_3 + (e_{ot} - e_{yt}).$$

Because δ_s is constant across siblings, differences in wages between siblings are not a function of this component of wage determination. Therefore, the coefficient, β_3,

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Leigh 1988). These techniques present difficulties in the present context because of the inability to identify suitable variables that are correlated with smoking behavior that are also unrelated to wages, as described below.

[2]Another approach would be to estimate separate wage regressions for smokers and nonsmokers and then conduct a wage decomposition to determine what fraction of the wage differential could be accounted for by differences in the coefficients and by differences in characteristics. The model we estimate restricts all the coefficients except the intercept to be equal between the two groups. An F-test of this restriction cannot reject equality of the coefficients.

measures the effects of smoking behavior on wages or employment after controlling for differences in unobservable family background characteristics.

An alternative approach is to apply panel data techniques to examine the relationship between changes in wages and changes in smoking behavior over time. A modification of equation (1) may be specified to represent an individual's wage at time t,

$$(4)\ \ln\ W_{it} = \alpha + X_{it}\ \beta_1 + F_i\ \beta_2 + S_{it}\ \beta_3 + \gamma_i + e_{it},$$

where γ_i represents an individual-specific component of an individual's wage that is time-invariant (an "individual fixed effect").[3] If information is available for two periods, then differencing these data across periods yields

$$(5)\ \ \ \ \ \ \ \ \Delta\ln\ W_i = \Delta X_i\ \beta_1 + \Delta S_i\ \beta_3 + \Delta e_i.$$

This model will provide unbiased estimates of β_3 even in the presence of individual fixed effects.

Neither of these models necessarily eliminates all of the potential bias created by unobservable heterogeneity. First, differences in smoking status between siblings may be correlated with differences among individuals within a family. Second, changes in an individual's smoking behavior over time may be correlated with other changes in an individual's life (marriage, for instance). A combination of the two methods, however, can eliminate an additional element of the potential bias. This approach treats differences in wage growth between siblings as a function of differences in changes in smoking status between siblings, and would eliminate both individual- and family-specific components of wage determination.[4] More formally, consider an individual's wage being modeled as

$$(6)\ \ln\ W_{it} = \alpha + X_{it}\ \beta_1 + F_i\ \beta_2 + S_{it}\ \beta_3 + \gamma_{it} + e_{it},$$

where all the notation is the same as equation (4), except γ_{it} represents unobservable characteristics of an individual that vary over time. If the change in this component of wages is equal between siblings ($\Delta\gamma_o = \Delta\gamma_y$), then one can estimate the model

$$(7)\ \ \ \ \ \ (\Delta\ln\ W_o - \Delta\ln\ W_y) = (\Delta X_o - \Delta X_y)\beta_1$$
$$+ (\Delta S_o - \Delta S_y)\beta_3 + (\Delta e_o - \Delta e_y)$$

and obtain unbiased estimates of β_3 even in the presence of time-varying unobservable heterogeneity as long as it is constant across siblings.

A final method that could be employed to deal with selectivity problems would be to estimate an instrumental variables model using an instrument that is correlated with an individual's smoking behavior but uncorrelated with his or her wages. We explored several alternative sets of instruments based on characteristics of the respondent's state of residence, including cigarette prices, excise tax rates, and the share of smokers in the population over age 16.[5] These variables are very weak instruments because their correlation with a respondent's smoking status is quite low in the NLSY data.[6] A second alternative was the use of state fixed effects as instruments that could pick up more general cross-state differences in the environment regarding smokers and smoking. First stage regres-

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[3]This model subsumes a specification based on family fixed effects, such as the sibling differences model previously presented. It is advantageous to estimate both models rather than an individual fixed effects model alone because each exploits a different source of variation in the data. Consistent results across both specifications would, therefore, strengthen the findings.

[4]An example of the benefit of this approach is that all children in some families may be "late bloomers."

Siblings from such a family may experience large relative wage gains as they mature and also be more likely to quit smoking. This hypothetical case is appropriate given the composition of the NLSY data, since respondents in 1984, the first of the two years in which smoking questions were asked, were only 19-26 years old.

[5]Similar results were obtained using state of current residence and state of residence at age 14.

[6]Mullahy and Sindelar (1995) experienced similar problems using state tax data as instruments in estimating the employment consequences of problem drinking. Their IV estimates generated a tenfold increase in standard errors relative to their OLS estimates.

sion results indicate that these variables provide "weakly correlated instruments" that, as has been pointed out by Staiger and Stock (1994), yield second stage parameter estimates that are biased toward the OLS estimates. We therefore do not report results of an instrumental variables model.

An additional issue that may affect the interpretation of results obtained from the methods employed here is the possibility of a structural relationship between smoking and wages. In particular, there is some evidence indicating that the demand for cigarettes is related to an individual's income (see Becker, Grossman, and Murphy 1994). Statistical techniques designed to reduce the problem of omitted variable bias, presented above, will not purge the bias caused by this sort of endogeneity from parameter estimates. Unfortunately, instrumental variables strategies that could address this issue have proven inadequate, as just described. The bias that such a relationship will cause, however, will reduce the estimated impact of smoking on wages. If cigarettes are a normal good, then higher wages should lead to more smoking.[7] To the extent that a negative relationship is observed between smoking and wages, this finding would underestimate the true negative effect of smoking.

Data

Our analysis uses data from the National Longitudinal Survey of Youth (NLSY). The NLSY is an ongoing study of 12,686 men and women who were between the ages of 14 and 21 when they were first interviewed in 1979.[8] These data supply a wealth of information for each respondent, including labor market behavior, demographic characteristics, and family background char-

acteristics. By 1992, these individuals ranged in age from 27 to 34, and most of them had entered the labor force. Analysis of the effects of smoking on hourly wages will focus on full-time, full-year workers.[9] We concentrate on these workers to avoid entangling issues of labor force participation and employment in our discussion of the wage effects of smoking.[10]

Most important for the purposes of this project are the smoking questions that were asked of respondents in the 1984 and 1992 surveys.[11] The two surveys differ somewhat

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[7]In studies estimating the demand for cigarettes, researchers have found income elasticities that are positive (Becker et al. 1994; Lewit and Coate 1982) or not significantly different from zero (Keeler et al. 1988; Wasserman et al. 1991).

[8]The NLSY oversamples blacks, Hispanics, and, through 1991, poor whites. Sampling weights are employed in estimation so that results can be interpreted as nationally representative.

[9]Hourly wages are computed as annual wages divided by annual hours worked. Full-time, full-year workers are defined as those who have worked at least 50 weeks and 1,750 hours in a calendar year. Results are robust to small changes in this definition (that is, 1,500 or 2,000 hours). Other sample restrictions were made because of missing demographic and family background characteristics. For instance, in 1991, sample attrition and responses to the smoking questions reduce the sample to roughly 8,500. Missing demographic data eliminates another 1,000 observations or so, of which about one-third did not have an AFQT score. Another 1,000 respondents were missing data on family background characteristics; over two-thirds of these were missing data on parents' education. (Increasing the available sample by omitting these variables from the model had no noticeable affect on parameter estimates.) The full-time, full-year sample restriction lowered the sample size to the final level of 3,473.

[10]We have also explored the relationship between smoking and employment in an earlier version of this paper (Levine et al. 1995), and the results of that investigation are in additional tables that are available from the authors upon request. In that analysis we mainly found that smoking was negatively related to employment in OLS models, but this finding was not robust to alternative model specifications that attempt to control for unobservable heterogeneity.

[11]The 1992 survey also includes retrospective data on smoking that could allow us to implement an approach similar to that implied by equation (5). These data can be used to identify the effect of smoking on wages by comparing wages before and after quitting smoking for those workers who quit during the panel. Unfortunately, these data appear to be measured with a substantial degree of error. For example, estimates of the model represented by equation (1) can be obtained using reported smoking behavior in 1984 or by using the retrospective smoking history data for that year. Estimates using the retrospective data provided much lower estimated effects of smoking on wages, indicating the presence of measurement error bias. For this reason, we do not report our attempts to estimate models using these data.

in the format of the questions asked, and because of those differences they may address somewhat different forms of smoking behavior. In 1992, respondents were asked whether they currently smoked daily.[12] Respondents in 1984 were asked the average number of cigarettes smoked per day and are defined as daily smokers if they averaged more than one cigarette per day. This definition of a daily smoker may not match that implied by the 1992 survey if some respondents "binge" on weekends, smoking large numbers of cigarettes a few days a month. In particular, the difference may lead to some false transitions in smoking status between the two survey dates. Using data from the 1991 National Health Interview Survey (NHIS), we have estimated that the frequency of "bingeing" (defined here as not smoking daily, but smoking more than 30 cigarettes per month) is about 4% for individuals between the ages of 26 and 33, the same age as NLSY respondents in that year.[13]

Another step involved in determining smoking status is identifying smokers who have quit smoking temporarily. Smokers often attempt to quit unsuccessfully. For instance, among a nationally representative sample of smokers obtained from the 1991 NHIS, we have estimated that over 40% of current smokers comparable in age to NLSY respondents attempted to quit

smoking in the previous year. In the 1992 NLSY survey, respondents who reported having quit smoking were asked when they quit. At least four times as many respondents reported quitting in the past year than in any preceding year, indicating that most such attempts to quit do not succeed. Given the general lack of evidence regarding smoking's effect on labor market outcomes, we believe this initial exploration should focus on the effects of permanent smoking behavior.[14] A thorough investigation of the effects of temporarily quitting smoking is beyond the scope of this paper and is left for future research.[15]

To create measures of smoking status, we exploit the longitudinal nature of the NLSY data and the retrospective smoking questions asked in the 1992 survey. A measure of smoking status in 1991 starts from the respondent's smoking status in 1992. Respondents who reported quitting within the previous year are recoded as smokers in 1991.[16] Retrospective data from 1992 are also used to reconsider the smoking status of individuals who reported in 1984 that

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[12]Although the survey instrument allows individuals to identify themselves as occasional smokers, preliminary analysis indicated that occasional smoking exerts no significant influence on wages, and we therefore treat occasional smokers as nonsmokers. In addition, current daily smokers are asked the number of cigarettes they smoke per day. We find that wages are unaffected by smoking intensity once we control for whether an individual smokes daily.

[13]The bias introduced by this discrepancy is probably small in all models with the exception of models of wage changes over time. Here, the smoking effect is identified by those individuals who changed their smoking behavior between 1984 and 1991. A significant fraction of these changers may not have altered their smoking behavior, but are coded differently simply due to the change in definition. Estimates of these models, therefore, are likely to be downward-biased.

[14]Most of the potential explanations provided above regarding the relationship between smoking and labor market outcomes would indicate that these outcomes should be unaffected by a short-term abstention from smoking. Even if quitting for a short time is rewarded, the benefits may occur with a lag. A comparison of the wages of smokers and nonsmokers, including those who have quit temporarily, would then be biased toward zero.

[15]The longitudinal nature of the NLSY data allows us to identify quits in 1984 as temporary or permanent. Therefore, we have conducted a preliminary examination into the sensitivity of our results to the types of behavior we use to define smoking status. In cross-sectional models, both definitions led to very similar parameter estimates, as should be expected given the relatively small number of individuals for whom the definition of smoking behavior changes. In the wage change equations, however, the estimated wage penalty was larger when smoking status was defined by more permanent behavior. This finding may be explained by false transitions in long-term smoking status over time if temporary quitters in 1984 are defined as nonsmokers. This would bias estimates of the wage penalty toward zero.

[16]We also recoded the considerably smaller number of respondents who reported starting smoking within the previous year as nonsmokers in 1991.

Table 1. Mean Characteristics in 1984 and 1991 Cross-Section Data, by Smoking Status.

	1984 Data		1991 Data	
Characteristic	Smokers	Nonsmokers	Smokers	Nonsmokers
Personal Characteristics				
Log Hourly Wage	2.08**	2.19	2.27**	2.44
Years of Education	12.5**	13.3	12.7**	14.1
Years of Work Experience	5.0	5.0	10.8	10.8
AFQT Score	72.0**	77.8	69.8**	77.2
% Urban Residence	81.5	81.1	79.6	80.0
% Residence in South	34.6	33.5	36.5	34.2
Number of Children	0.38**	0.27	1.03**	0.89
% Married	33.9	35.0	53.7**	63.4
% Female	47.8**	43.8	38.8*	42.2
% Nonwhite	10.1	11.5	12.6	13.8
Family Background Variables[a]				
% Urban Residence at Age 14	79.1	77.0	77.7	75.8
% South Residence at Age 14	30.8	31.0	31.9	31.7
Mother's Education	11.7**	12.0	11.6**	12.0
Father's Education	11.9**	12.3	11.8**	12.4
Number of Siblings	3.2	3.0	3.2	3.0
% Reside with Both Parents	85.5	88.0	85.1	86.5
% Reside with Mother Only	11.1	9.7	10.7	10.7
% Mother Worked at Age 14	56.6	53.0	54.7	56.4
% Father Worked at Age 14	97.3	96.5	96.2	96.2
Sample Size	1,033	1,784	917	2,311

[a]Means for parent's characteristics are presented for those parents present in the household and, for the labor market variables, those in the labor market. In the analysis to follow, these variables are interacted with a dummy variable indicating if the parent is present in the household and, for the work force variables, whether the parent worked.

*Mean value for smokers is significantly different from the mean value for nonsmokers at the 10% level; **at the 5% level.

they had not smoked within the previous 30 days, but had smoked within the past year. If these respondents reported smoking daily in 1992 or reported that they quit smoking daily any time after 1986, they are reclassified as having been smokers in 1984.[17]

Descriptive Statistics

Descriptive statistics for these data, weighted to provide nationally representative estimates for this cohort, are presented

in Tables 1–3. The data in Table 1 show that 37% of the sample smoked in 1984 and 29% did so in 1991. The 1991 statistic is comparable with other nationwide estimates of smoking frequency for this age group, but the 1984 statistic is a few percentage points higher than national estimates (National Center for Health Statistics 1995). The discrepancy in 1984 is consistent with the definitional differences in smoking status, as described earlier. Individuals in the 1984 NLSY who smoked more than one cigarette a day, on average, but did not smoke daily were defined as smokers. Based on our estimates from the 1991 NHIS, the difference in smoking rates between the two survey years is roughly equally attributable to differences in definitions of smoking and an actual reduction in smoking.

Results in Table 1 also indicate that there

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[17]Given the potential for recall bias in the exact year in which quitting occurred, we experimented with alternatives to this 1986 cutoff, including only using smoking status in 1992 to recode these observations. Results of this analysis were robust to all of these alternatives.

Table 2. Sample Characteristics of Sibling Data
in 1984 and 1991, by Difference in Smoking Status.[a]

	1984			1991		
Explanatory Variable	*Younger Sibling Smokes*	*No Difference in Smoking Status*	*Older Sibling Smokes*	*Younger Sibling Smokes*	*No Difference in Smoking Status*	*Older Sibling Smokes*
Difference in Log Hourly Wage	0.303**	0.117	0.178	0.175**	0.031	–0.092**
Difference in Years of Education	0.68	0.48	–0.18**	0.94**	0.03	–0.80**
Difference in Years of Work Experience	1.08	1.26	1.58	1.60	1.73	1.07**
Difference in AFQT	7.91	6.25	1.29**	9.20**	3.73	–1.43**
% Difference in Marital Status	16.1	15.0	27.2	16.3**	0.9	12.1**
Difference in Number of Children	0.01	0.06	0.24*	0.12	0.29	0.43
% Difference in Southern Residence	–2.0	0.1	–4.8	–2.1	–0.1	4.1**
% Difference in Urban Residence	0.03	2.9	–4.8*	3.7**	–4.7	–0.6
Difference in Gender[b]	10.2	3.3	–13.5**	21.3**	–1.1	–8.9
Sample Size	74	263	77	115	498	113

[a]Differences represent characteristics of older sibling minus characteristics of younger sibling.
[b]Indicates the difference in the proportion female between the two siblings.
*Mean sibling difference is significantly different from the mean for sibling pairs with the same smoking status at the 10% level; **at the 5% level.

is a large wage differential associated with smoking in both years, with smokers earning 11% less than nonsmokers, on average, in 1984, and 17% less in 1991. Smokers and nonsmokers also differ in several observable characteristics that may explain this differential.[18] Most important, smokers have substantially less education, on average, than do nonsmokers. This education gap widened from eight-tenths of a year in 1984 to nearly a year and a half in 1991. Much of the change in this gap is attributable to the

fact that many respondents had not finished their schooling in 1994, and nonsmokers were more likely to continue their education than were smokers.[19] Many of the other characteristics listed in the table are consistent with the difference in earnings between smokers and nonsmokers: smokers have lower AFQT scores, less educated parents, and a higher likelihood of living in the South than do nonsmokers.

Summary statistics for the sibling comparisons are presented in Table 2. The data for this exercise are ordered so that characteristics of younger siblings are differenced from those of older siblings. Because differences in wages based on smoking status depend on whether the older sibling or younger sibling smokes, the statistics are further conditioned upon which sibling

[18]There are several alternative explanations for the change in the wage differential over time. First, this difference may be due to the growing rate of return to "skill" (Levy and Murnane 1992). To the extent that smoking is correlated with these observable and unobservable components of a worker's ability, the penalty associated with smoking will appear to be greater in 1991 than in 1984. Another explanation is that more highly skilled smokers in 1984 were more likely to quit over time, so that those workers still smoking in 1991 may be less skilled. These possibilities highlight the need to estimate models that control for differences in observable and unobservable characteristics.

[19]To explore this issue in more detail, we restricted the 1984 and 1991 samples to those respondents who were age 22 or over in 1984 and were likely to have completed their schooling. In this subsample, the educational attainment gap grew from about one year to about 1.2 years over the period.

Table 3. Mean Changes in Characteristics by Change in Smoking Behavior, 1991–1984.

Explanatory Variable	Continuous Smokers	Continuous Nonsmokers	Quitters	Starters
Change in Log Hourly Wage	0.263	0.280	0.347*	0.201
Change in Years of Education	0.184**	0.272	0.276	0.211
Change in Years of Work Experience	7.11*	7.17	7.29**	7.19
Change in Southern Residence	−0.008	−0.001	0.006	−0.016
Change in Urban Residence	−0.006	0.001	0.005	0.005
Change in Number of Children	0.665*	0.750	0.744	0.628
Change in Marital Status[a]	0.245**	0.311	0.371	0.338
Sample Size	388	964	156	68

[a]This variable represents the difference in a "married" dummy variable between 1991 and 1984.
*Change is significantly different from the change for continuous nonsmokers at the 10% level; **at the 5% level.

smokes. The case in which both siblings have the same smoking status is included as a reference.

In both years, we observe that older siblings earn more than younger siblings among those pairs with the same smoking status, as expected. In 1991, we also see that smoking by the younger sibling appears to increase the size of the wage gap, while older sibling smoking reduces it. In the data for 1984, however, although smoking by the younger sibling increases the wage gap by about 19% (from 11.7% in sibling pairs with the same smoking status to 30.3% here), older sibling smoking appears to make the wage gap larger as well, by 6.1%. This pattern may be attributable to the small sample sizes in the 1984 data. Overall, the wage difference associated with smoking in these data is negative.

Similar comparisons of observable characteristics reported in Table 2, particularly years of education, show that differences between siblings in smoking behavior are correlated with the observed differences in these characteristics. These results largely parallel those reported in Table 1.

Average changes in characteristics of individuals between 1984 and 1991 are reported in Table 3. Statistics are reported separately depending on the workers' smoking status in the two years. The results indicate that wages increased by 26–28% for workers whose smoking status was the same in both years. In contrast, workers who quit smoking over the period experienced about a 35% wage increase. Wages of workers who started smoking between the two years increased by 20%. Trends in years of education across groups between the two years are consistent with these wage trends, but these differences are so small that they could not plausibly explain the differences in wage growth. Quitters were somewhat more likely to have gotten married over the period, but again, differences are quite small compared to the differential wage growth experienced by this group. No other obvious patterns are present in any of the data.

Wage Results

The results from the OLS regression in equation (1) allow us to separate out the effects of smoking from the effects of the differences in human capital and other observable characteristics, including the AFQT score. These results are reported in Table 4. Columns 1 and 4 replicate the raw wage differentials presented in Table 1. The inclusion of education in the regression, reported in columns 2 and 5, substantially reduced the measured impact of smoking, to 6.2% in 1984 and 8.0% in 1991. The addition of a full range of personal and family background characteristics, including AFQT scores, generated small addi-

Table 4. OLS Estimates of the Effect of Smoking Behavior on Wages
for Full-Time, Full-Year Workers by Demographic Group, 1984 and 1991 NLSY Data.
(Standard Errors, Corrected for Heteroscedasticity, in Parentheses)

Explanatory Variable	1984 Data			1991 Data		
	(1)	(2)	(3)	(4)	(5)	(6)
Smoke Daily	−0.114	−0.062	−0.042	−0.176	−0.080	−0.069
	(0.020)	(0.019)	(0.018)	(0.021)	(0.021)	(0.019)
Years of Education		0.060	0.045		0.070	0.045
		(0.005)	(0.006)		(0.004)	(0.005)
Years of Work Experience			0.052			0.029
			(0.005)			(0.003)
AFQT Score			0.004			0.005
			(0.001)			(0.001)
Urban Residence			0.118			0.029
			(0.021)			(0.021)
Residence in South			0.009			−0.044
			(0.038)			(0.031)
Number of Children			0.002			−0.014
			(0.013)			(0.008)
Married			0.073			0.087
			(0.019)			(0.018)
Female			−0.170			−0.182
			(0.017)			(0.017)
Nonwhite			0.031			0.021
			(0.025)			(0.022)
Family Background Variables[a]			x			x

[a]Family Background Variables are those listed in Table 1.

tional reductions in the measured effect of smoking, as reported in the final estimates in columns 3 and 6.[20] Clearly, much of the effect of being a smoker is generated by differences in the educational decisions of smokers and nonsmokers rather than the smoking behavior itself. Nevertheless, after controlling for a wide array of observable characteristics, smoking is estimated to lower a worker's wage by about 4–7% in these models (columns 3 and 6).

Although the inclusion of individual characteristics in the OLS regression substantially reduces the measured effect of smoking, estimates of the differential may still be

biased if unobservable characteristics are correlated with both smoking and wages. Table 5 presents the results of addressing this bias by employing sibling data to estimate equation (3). Three versions of the sibling regression were estimated: each cross-section separately and the two years pooled. Estimates are slightly larger than those obtained from the OLS regressions reported in Table 3, all clustered around an 8% wage reduction associated with smoking. The similarity between these estimates and the OLS estimates suggests that there is no evidence that the OLS estimates were biased by the presence of unobservable correlates of smoking.

Estimates of equation (5) using panel data are presented in the first column of Table 6. The point estimate of the effect of smoking presented in column 1, indicating a 6% reduction in wages associated with

[20]Inclusion of nonlinear experience terms, occupation controls, and state fixed effects has a very small effect on these estimates, so these results are not reported.

Table 5. Effects of Sibling Differences in Smoking Behavior
on Differences in Wages, Alternative Data Sets.

Explanatory Variable	1984 Data (4)	1991 Data (5)	Pooled Data (6)
Difference in Smoking Behavior	−0.080 (0.042)	−0.081 (0.043)	−0.079 (0.033)
Difference in Years of Education	0.006 (0.016)	0.047 (0.011)	0.036 (0.010)
Difference in Years of Work Exp.	0.059 (0.013)	0.032 (0.007)	0.037 (0.006)
Difference in AFQT Score	0.005 (0.002)	0.004 (0.002)	0.004 (0.001)
Difference in Urban Residence	0.285 (0.093)	0.145 (0.058)	0.179 (0.052)
Difference in Southern Residence	0.065 (0.136)	−0.041 (0.083)	−0.030 (0.070)
Difference in Number of Children	−0.068 (0.035)	0.001 (0.018)	−0.012 (0.016)
Difference in Marital Status	0.187 (0.045)	0.081 (0.034)	0.116 (0.028)
Difference in Gender[a]	−0.195 (0.042)	−0.140 (0.036)	−0.158 (0.029)

[a]Equals unity if older sibling is female and younger sibling is male, zero if both siblings are the same gender, and −1 if older sibling is male and younger sibling is female.

smoking, is similar in magnitude to the OLS estimates reported in Table 4.[21] This finding is further evidence that the OLS estimates are not generated primarily by differences in the unobservable characteristics of the two groups.

The second column of Table 6 presents estimates of equation (7), in which the effect of smoking is identified in a model of sibling differences in wage changes. Estimates from this model indicate that an individual who quit smoking earned a 25% larger wage increase between 1984 and 1991 than a sibling whose smoking behavior was unaltered. This effect is considerably larger than any estimated previously. The stan-

dard error of 12% associated with this estimate, however, is also quite large. This imprecision is due to the small sample size resulting from the requirement that both siblings must have worked full-time, full-year in 1984 and 1991 to be included in this analysis. While the parameter estimate is significantly different from zero, a 95% confidence interval would include most of the range of point estimates obtained from the model specifications previously reported in this paper. Because of the imprecision, this result is insufficient to refine our estimate of the precise effect of smoking on wages, but it further highlights the inability of unobservable heterogeneity to explain the findings obtained from OLS estimates.

Differential Effects of Smoking on Wages

Estimates of the effect of smoking on wages reported in the preceding section are obtained from all full-time, full-year

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[21]We have also estimated models that indicate wage changes of continuous smokers were no different from wage changes of continuous nonsmokers. These findings suggest that smoking affects the intercept of a worker's age-earnings profile, but not the slope.

workers available in the NLSY data. It is quite possible, however, that the wage penalty associated with smoking differs in different segments of the labor market. Access to employer-provided health insurance or the degree of discrimination against smokers, for instance, may differ across demographic groups or workers in different types of jobs. In this section of the paper, we split the sample into different subgroups and explore the relationship between smoking and wages separately among each group. The groups we consider are men versus women, more-educated (defined as those with schooling beyond a high school degree) versus less-educated, blue-collar versus white-collar, and full-time, full-year versus part-year.[22] Models analogous to those reported in Tables 4–6 are then estimated.[23]

Results of this exercise are reported in Table 7. The first row of this table summarizes the findings based on all full-time, full-year workers reported in Tables 4–6, and indicates that the wages of smokers are roughly 4–8% less than those of nonsmokers. In the remainder of the table, we report the effect of smoking differentiated by type of worker. We conclude from these results that the wage penalty associated with smoking is fairly robust across sectors and statistical specifications among workers who work full-time, full-year.

OLS estimates seem to indicate that men's wages are more affected by smoking than are women's, although the point estimates are significantly different from each

Table 6. Effect of Changes in Smoking Behavior on Changes in Wages, 1991–1984.[a] (Standard Errors in Parentheses)

Explanatory Variable	Difference over Time (1)	Difference over Time Between Siblings (2)
Change in Smoking Status	−0.063	−0.251
	(0.034)	(0.118)
Change in Years of Education	0.058	0.044
	(0.017)	(0.054)
Change in Years of Work Experience	0.070	0.053
	(0.023)	(0.047)
Change in Southern Residence	−0.069	−0.212
	(0.075)	(0.103)
Change in Urban Residence	0.038	0.023
	(0.034)	(0.097)
Change in Number of Children	−0.007	−0.046
	(0.013)	(0.034)
Change in Marital Status[b]	0.083	0.014
	(0.021)	(0.062)
Constant	−0.259	−0.040
	(0.165)	(0.054)

[a]Sample consists of full-time, full-year workers in both 1984 and 1991.

[b]This variable represents the difference in a "married" dummy variable between 1991 and 1984.

other only in the 1984 cross-section.[24] In sibling-difference models and models of wage changes over time, point estimates indicate a larger effect for women than for men, although imprecision in the parameter estimates makes it difficult to distinguish differences in the effect by gender. We conclude from this that there is no robust gender differential in the effect of smoking on wages. As for differences by level of education and type of occupation, point estimates in the two groups are simi-

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[22]Part-year workers are those who worked fewer than 1,750 hours in the year. We also estimated models splitting this group into two groups depending on whether or not the worker exceeded 1,000 hours per year. Imprecision in the estimated parameters due to small sample sizes makes it difficult to draw firm conclusions in these models.

[23]Models of sibling differences are estimated among those sibling pairs in which each sibling is in the same labor force subgroup (that is, among pairs of sisters or brothers). Similarly, models of differences over time are estimated for those individuals who do not change labor force subgroups over time (that is, who worked part-time in both periods).

[24]This finding is obtained from a regression model estimated on the full sample of full-time, full-year workers that includes an interaction term between every variable and the female dummy variable. The significance test is based on the t-statistic of the interaction between smoking status and a female dummy variable.

Table 7. Effect of Smoking on Different Labor Force Subgroups.
(Standard Errors in Parentheses)[a]

Effect of Smoking on:	1984 Cross-Section[b] (1)	1991 Cross-Section[b] (2)	Data Differenced Between Siblings, Pooled 1984 and 1991[c] (3)	Data Differenced Between 1984 and 1991[d] (4)
All Full-Time, Full-Year Workers	−0.042 (0.018)	−0.069 (0.019)	−0.079 (0.033)	−0.063 (0.034)
Men	−0.090 (0.026)	−0.087 (0.025)	−0.045 (0.047)	−0.037 (0.060)
Women	0.019 (0.022)	−0.044 (0.030)	−0.101 (0.048)	−0.113 (0.056)
Less-Educated (12 years of education or less)	−0.039 (0.021)	−0.062 (0.023)	−0.087 (0.046)	−0.117 (0.045)
More-Educated (more than 12 years of education)	−0.058 (0.031)	−0.071 (0.033)	−0.046 (0.052)	−0.102 (0.065)
Blue-Collar	−0.094 (0.029)	−0.063 (0.028)	−0.113 (0.063)	−0.118 (0.075)
White-Collar	−0.004 (0.021)	−0.073 (0.026)	−0.072 (0.039)	−0.138 (0.049)
All Part-Time Workers	−0.027 (0.046)	0.016 (0.029)	0.039 (0.115)	0.078 (0.079)

[a]Each row in this table represents the coefficient on smoking status obtained from separate regressions for the relevant labor force subgroup. Samples are restricted to full-time, full-year workers except where noted.

[b]Models include all personal and family background characteristics listed in Table 1 and are analogous to columns 3 and 6 in Table 4.

[c]Models include all personal characteristics listed in Table 1 that may differ between siblings, and are analogous to column 6 in Table 5.

[d]Models include all personal characteristics listed in Table 1 that may differ over time, and are analogous to column 1 in Table 6.

lar in virtually every specification, indicating that there is no apparent difference in the wage penalty associated with smoking by broad labor market sector. Finally, we find no evidence that smoking affects the wages of workers working less than full-time, full-year in each model specification.

Examining the Potential Causes of the Wage Penalty to Smoking

The evidence presented in this paper indicates that smokers earn less than non-smokers even after we control for differences in observable and unobservable characteristics between the two groups. Earlier in the paper we presented several alternative hypotheses about the possible causes

for such a wage penalty to smoking. In this section we report an analysis of the NLSY data examining whether some of these hypotheses are supported empirically.

The first theory to be considered is whether the health consequences of smoking make smokers more costly to employ through higher health insurance costs, leading firms that offer health insurance to pay smokers less. This hypothesis suggests that the wage penalty to smoking should be concentrated among smokers employed by firms that provide health insurance to their workers. To test this hypothesis, we include a dummy variable representing employer-provided health insurance and an interaction between this variable and smoking status.

Table 8. Alternative Model Specifications Examining Potential
Explanations for the Relationship Between Smoking and Wages.
(Standard Errors in Parentheses)

Explanatory Variable	1984 Cross-Section[a] (1)	1991 Cross-Section[a] (2)	Data Differenced Between Siblings, Pooled 1984 and 1991[b] (3)	Data Differenced Between 1984 and 1991[c] (4)
Smoke Daily	−0.033 (0.043)	−0.014 (0.056)	−0.056 (0.071)	−0.014 (0.060)
Employer-Provided Health Insurance	0.221 (0.029)	0.296 (0.034)	0.183 (0.045)	0.209 (0.038)
Employer-Provided Health Insurance *Smoke Daily	−0.012 (0.046)	−0.057 (0.058)	−0.026 (0.077)	−0.054 (0.057)
Smoke Daily	−0.040 (0.017)	−0.070 (0.019)	−0.079 (0.033)	−0.070 (0.032)
Health-Related Work Limitations	−0.073 (0.052)	0.033 (0.047)	0.103 (0.061)	0.054 (0.072)
Smoke Daily	−0.108 (0.048)	−0.179 (0.070)	−0.130 (0.064)	−0.074 (0.046)
Years of Experience*Smoke Daily	0.013 (0.009)	0.010 (0.006)	0.007 (0.007)	0.001 (0.004)

[a]Models include all personal and family background characteristics listed in Table 1 and are analogous to columns 3 and 6 in Table 4.

[b]Models include all personal characteristics listed in Table 1 that may differ between siblings, and are analogous to column 6 in Table 5.

[c]Models include all personal characteristics listed in Table 1 that may differ over time, and are analogous to column 1 in Table 6.

Results of this analysis are reported in the top panel of Table 8. We estimate cross-sectional models using 1984 and 1991 data, models of sibling differences, and models of wage changes over time. We find that workers who receive health insurance through their employer earn roughly 20–30% more than workers who do not. This result contradicts the hypothesis that health insurance benefits constitute a compensating differential, but is consistent with the fact that "good jobs" offer both higher wages and better benefits. Point estimates on the interaction term are uniformly negative, as predicted, but never significantly different from zero. Although reasonably large standard errors make it difficult to reject the null hypothesis that the coefficient is zero, point estimates are small in the 1984 cross-section and the sibling differenced

models as well. Therefore, these estimates provide little support for the compensating differential explanation.

We also consider theories indicating that smokers earn less because poor health reduces their productivity. We test this hypothesis by including a dummy variable indicating whether an individual reports a health-related work limitation. If the estimated wage penalty associated with smoking arises because smoking is a proxy for poor health, then including this measure of health status would reduce the estimated effect of smoking on wages.

Tests of this proposition are reported in the middle panel of Table 8. We find that health-related work limitations are not a significant determinant of wage rates, and excluding these measures imposes no bias on estimates of the effect of

smoking on wages.[25] This finding may be attributable to the relatively few workers in the sample who report such limitations and the fact that such limitations may be more important in determining employment than wages. Unfortunately, no more precise measures of health status are universally available in the NLSY.

The final test we conduct examines whether smoking is an indicator of an individual's rate of time preference. If smokers have higher rates of time preference, then they should be less likely to invest in on-the-job training and should receive a lower rate of return to work experience. To test this hypothesis, we include an interaction term between years of work experience and smoking status and examine whether estimated values of this interaction are negative. Results of this exercise, reported in the bottom panel of Table 8, provide no support for this hypothesis. Point estimates on the interaction term are positive in all cases, though never significantly different from zero.

We thus find no support for several potential explanations for the wage penalty associated with smoking. The tests we have conducted are somewhat crude, however, and it is possible that one or more of these explanations would be supported by additional testing using alternative data sources and methodologies. The NLSY offers no such alternatives that we can identify.

We have not considered some other hypotheses that are perhaps more difficult to test, including reduced productivity and discrimination against smokers. Again, evaluating the usefulness of these theories in explaining the wage penalty associated with smoking will require additional research.

Conclusions

This analysis has shown that smoking has a deleterious effect on smokers' wages. The

large wage differential observed in simple comparisons of the wages of smokers and nonsmokers, however, strongly overstates the causal effect of smoking. Smokers clearly differ from nonsmokers in a variety of ways, particularly with respect to their levels of education, and controlling for these differences in OLS regression models substantially reduced the measured effect of smoking. Using additional econometric tools designed to control for unobservable characteristics that might influence smoking and wages produced virtually no change in the magnitude of this effect. We find that workers who smoked earned 4–8% less than nonsmokers (as summarized in Table 7, row 1) after we control for differences between the groups.

Although we have argued that the statistical specifications employed in this analysis address the problem of unobservable heterogeneity, potential problems with each individual method remain. Including AFQT scores may not control for all relevant unobservable characteristics. Changes in an individual's smoking behavior over time may be correlated with other changes in an individual's life that themselves are correlated with changes in wages. Differences in smoking status between siblings hold family background characteristics constant, but not differences between siblings within a family. Nevertheless, the fact that we obtained roughly consistent findings from each method, all with unrelated weaknesses, strongly supports the hypothesis that smoking lowers wages.

We have also discussed four hypotheses that may explain the wage gap between smokers and nonsmokers and conducted crude empirical tests of a subset of these theories. No support for any of these potential explanations is observed. This finding may reflect limitations of the tests we employ, so more refined tests may need to be developed. Equally possible, an explanation we did not test, such as discrimination against smokers, may be the chief cause of the observed wage differential.

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[25]Not surprisingly given these findings, Probit estimates indicate that smoking does not significantly affect the likelihood of having a health-related work limitation.

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