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DO WORKPLACE SMOKING BANS REDUCE SMOKING?

William N. Evans
Matthew C. Farrelly
Edward Montgomery

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ABSTRACT

In recent years there has been a heightened public concern over the potentially harmful effects of environmental tobacco smoke (ETS). In response, smoking has been banned on many jobs. Using data from the 1991 and 1993 National Health Interview Survey and smoking supplements to the September 1992 and May 1993 Current Population Survey, we investigate whether these workplace policies reduce smoking prevalence and smoking intensity among workers. Our estimates suggest that workplace bans reduce smoking prevalence by 5 percentage points and average daily consumption among smokers by 10 percent. The impact of the ban is greatest for those with longer work weeks. Although workers with better health habits are more likely to work at establishments with workplace smoking bans, estimates from bivariate probit and two-stage least square equations suggest that these estimates are not subject to an omitted variables bias. The rapid increase in workplace bans can explain all of the recent sharp fall in smoking among workers relative to non-workers.

William N. Evans
Department of Economics
University of Maryland
College Park, MD 20742
and NBER

Matthew C. Farrelly
Department of Economics
University of Maryland
College Park, MD 20742

Edward Montgomery
Department of Economics
University of Maryland
College Park, MD 20742
and NBER

I. Introduction

In recent years there has been a heightened public awareness of the potentially harmful effects of second-hand or environmental tobacco smoke (ETS). This concern has been bolstered by the Surgeon General's 1986 report and the 1986 National Academy of Science/National Research Council's task force report on passive smoke which linked ETS to both higher rates of cancer and heart disease in nonsmokers. After surveying the scientific data on the health effects of passive smoke, the Environmental Protection Agency (EPA) declared ETS a Class A carcinogen, which are "[h]uman carcinogens based on sufficient evidence from epidemiological studies."[1]

In response to the public's growing concern and intolerance to ETS, public and private groups have tried to reduce exposure to passive smoke. State and local governments have passed clean indoor air laws that restrict smoking in a variety of public places such as restaurants, elevators, public meeting rooms, and in the workplace.[2] The public response reached its apex in March of 1994 when the Occupational Health and Safety Administration (OSHA), as part of a larger initiative on indoor air quality, proposed a complete ban on smoking in over 6 million workplaces.[3] In addition to these laws and regulations, firms have increasingly adopted formal policies to restrict or ban smoking in the workplace as a means of reducing exposure to ETS for non-smokers. As we demonstrate below, in 1985 only 25 percent of workers worked in establishments that banned smoking in work areas. By 1993, this

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[1] 51 Federal Register, September 24, 1988.

[2] Twenty-three states had at least moderate restrictions on smoking in public places in 1992, but this number jumped to thirty-three by 1994 (Coalition on Smoking OR Health). Evans and Farrelly (1996) note that while only 14% of private sector employees were covered by state workplace smoking laws in 1985, 43% were covered by 1993.

[3] The proposed indoor law was one of the more controversial regulations ever proposed by OSHA. During the public comment period for this proposal, the Department of Labor received over 110,000 letters, including a number of death threats (*Wall Street Journal*, December 6, 1994, p.B1; *Washington Post*, August 13, 1994, p. B1). This proposed regulation is still pending.

number increased to 70 percent.

Although public smoking restrictions are primarily designed to reduce the exposure of non-smokers to ETS, these policies may also affect the behavior of smokers by reducing their opportunities to smoke. Workplace smoking restrictions are likely to have a greater impact on the prevalence of smoking and/or the level of cigarette consumption than restrictions in restaurants, elevators, and other public areas because workers will be subject to these restrictions for more hours in the day.

If workplace restrictions lead to a change in smoking behavior, firms may have another reason to adopt these policies. A number of authors have demonstrated that smokers impose costs on firms through increased absenteeism (Allen (1981) and U.S. Department of Health and Human Services (1990)) and higher health care costs (Morbidity and Mortality Weekly Report , (1994)). Workplace smoking restrictions may thus reduce the costs associated having employees who are smokers (Warner; 1987 and 1990). Because most private health insurance is provided by employers, and since many larger firms self-insure, policies that discourage smoking may reduce the cost to firms of providing health insurance to workers.[4]

That workplace smoking policies may have had an important effect on smoking habits can be seen by examining the time series patterns of smoking participation rates for workers and nonworkers. In Figure 1, we graph the smoking participation rates for workers and nonworkers, aged 18-65, for the period 1976 through 1993. In Figures 2 and 3, we report results for males and females, respectively. These numbers were generated from the National Health Interview Survey (NHIS) smoking supplements for the years 1976-1980, 1983, 1985, 1987-1993.[5] These data indicate that although smoking

[4] Theoretically if firms are able to reduce smoking worker's wages to reflect the increase in health premiums associated with their behavior, this incentive would not be present. Given the risk spreading nature of firm group health insurance policies as well as the potential unobservability of unhealthy habits, it seems unlikely that employers would be able to adjust wages appropriately.

[5] The data for non-survey years is based on linear interpolations between survey years.

participation rates have fallen for both workers and nonworkers, the decline has been much more pronounced for workers. For instance, smoking rates for males workers declined by 4.8 percentage points more than for male nonworkers over the period 1976-93. For the full sample and the females subsample, smoking participation rates for workers exceeded those of nonworkers in 1976. By 1993, however, workers were less likely to smoke than nonworkers. Between 1985 and 1993, the time period after the Surgeon General's report on passive smoke, smoking participation rates among workers fell 2.6 percentage points (standard error of 0.010) more than the decline for non-workers. These figures are suggestive of the fact that over the past 20 years there has been some external factor that is differentially affecting the smoking habits of workers. One possible explanation for this phenomena is the rise in workplace smoking bans over this time period.[6]

In this paper we will examine whether workplace smoking policies reduce worker's demand for cigarettes. This study advances the literature in a number of important ways. First, by using data from the 1991 and 1993 NHIS we are able to use a much larger and richer cross section of observations on the relationship between firm smoking policies and smoking outcomes. Pooling data across both surveys generates a sample of 18,090 workers. These single-equation results (where workplace smoking bans are assumed to be exogenous) suggest that workplace bans lead to a 5-6 percentage point decline in smoking prevalence and a decrease in average cigarette consumption by smokers of 2.3 cigarettes per day (about 10 percent).

Second, we jointly model both the propensity of a worker to smoke and the match of workers to nonsmoking establishments to control for the nonrandom assignment of workers and firms. The

[6] Although these results are only illustrative, we do not believe the trends are capturing secular changes in the health or health habits of workers relative to nonworkers. Using data from the 1985 and 1993 NHIS, we examined the time-series pattern for two additional measures of health or health habits: whether respondents reported their health status as very good or excellent, and whether respondents always wear their seat belt. The difference in difference estimate (standard error) that measures the change in response between 1993 and 1985 for workers relative to nonworkers are 0.002 (0.011) for very good/excellent health status and 0.004 (0.010) for belt use.

potential endogeneity of the workplace smoking policies requires that we estimate a simultaneous equations model of smoking and workplace ban determination. The results of the bivariate probit and two-stage least squares estimation indicate that workplace smoking bans reduce both the prevalence of smoking and cigarette consumption. Once we control for selection bias, a complete smoking ban in all work areas has a larger impact on the prevalence of smoking. Overall our results suggest that self-selection bias does not dramatically alter the conclusion from the single-equation estimation that smoking bans successfully reduce cigarette consumption on the intensive and extensive margins. The estimated results suggest that the rise in workplace smoking bans can explain all of the recent drop in smoking among workers relative to nonworkers.

The remainder of this paper is structured as follows. In Section II we discuss the results and limitations of previous studies that have examined this question. Section III contains a description of the data source used in our analyses as well as summary statistics that describe the type and frequency of workplace smoking restrictions and the prevalence of smoking. In Section IV we present the results from single-equation models of the effect of smoking restrictions on smoking prevalence and consumption. In Section V, we present evidence suggesting the presence of selection bias in the assignment of workers to firms and results from simultaneous equation models that control for this selectivity. In Section VI we make some concluding remarks and discuss the implications of the estimated effect of smoking bans.

II. Previous Literature

Despite the fact that the vast majority of workers report that they work in an establishment with some form of workplace smoking restriction, there is no clear consensus about whether these policies actually reduce smoking among workers. Several studies find support for a correlation between workplace smoking restrictions or bans and decreased smoking (Baile *et al* (1991), Kinne *et al* (1993), Petersen *et al* (1988), Sorensen *et al* (1991), Stave and Jackson (1991) and Woodruff *et al* (1993)), while others have found no such support (Beiner *et al* (1989) and Gottlieb *et al* (1990)).

The bulk of existing studies examine the change in smoking before and after a workplace smoking ban is adopted on one particular job site. The case method approach of these studies means that their samples are limited, sometimes to just a few hundred individuals. Within these samples, there is an even smaller number of smokers, causing the results to be sensitive to changes in behavior of just a handful of people.[7]

Another cause for concern is that a number of these studies make no attempt to find a control group that will serve as a baseline against which to compare the impact of a workplace smoking policy. Failure to control for the general downward trend in smoking could lead to an overestimate of the effect of these firm policies. Other studies have used similar or neighboring workplaces that have not adopted workplace bans as the control group. In such cases, the populations are assumed to have the same natural rate of smoking cessation. This assumption may be difficult to justify since many of the observed characteristics of the workers and firms vary across the job sites.[8] Since many paired establishments varied along observed dimensions, there is reason to suspect they varied along unobserved dimensions as well.

Most importantly, all of these studies share one common methodological problem. As Woodruff *et al* (1993) noted, there is a potential "self-selection bias (e.g., nonsmokers find work in smoke-free workplaces) [and] the difference in the percentage of smokers by workplace policy is due to other

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[7] One notable exception is the work of Woodruff *et al* (1993) who used a sample of 11,704 workers from the 1990 California Tobacco Survey to examine the impact of workplace smoking policies on smoking participation.

[8] For example, in Stave and Jackson (1991), 400 employees of the Duke University Medical Center, where a restrictive smoking policy was implemented, were compared to 400 employees from the neighboring university campus that had no similar policy in place. The latter population served as the control under the assumption that they have the same observed and unobserved characteristics as the medical center population. This assumption appears to be false as the populations vary along gender (67 percent of the employees at the medical center were female compared to 51 percent of all employees at the university) and other observed dimensions.

factors." If the match of smokers to firms is not random, simple cross-section estimates or paired comparisons across work sites of the impact of smoking bans on smoking prevalence may be biased.

Self-selection bias can be generated through a number of different avenues. First, non-smokers (smokers) may be attracted to firms with (without) workplace smoking bans. In this instance, single-equation estimates would overstate the impact of workplace smoking bans. Second, firms that adopt workplace bans may place a greater emphasis on the health and safety of their employees and, therefore, policies that restrict or ban smoking may simply reflect other programs adopted by the firms. If firms with a workplace smoking ban also offer exercise programs, on-site exercise facilities, and smoking cessation programs, smoking may be lower because of these other programs, not because of the smoking ban. If the firms most likely to adopt smoking bans are also those most likely to adopt these other programs that promote better health, then the single-equation estimate would again overstate the impact of the ban. Finally, it is also plausible that firms with the highest levels of ETS are more likely to ban workplace smoking. In this case, the single-equation model would understate the benefits of the restrictions. Thus, controlling for self-selection of workers and firms would seem to be an important issue in deriving an unbiased estimate of the effect of these workplace restrictions.

III. Data and Descriptive Statistics

The primary source of data for our analysis comes from the NHIS which is designed to provide national estimates of the distribution of illness and the kinds of health services people receive. Each year, the NHIS contains a set of core questions plus special supplements that vary from year to year. Both the Health Promotion and Disease Prevention Supplement to the 1991 NHIS and the Year 2000 Objectives Supplement to the 1993 NHIS contain questions that asked respondents about smoking and other health habits. In addition to determining the current smoking status of respondents, both the 1991 and 1993 NHIS ask current smokers the average number of cigarettes smoked per day. The key dependent variables in our analysis are an indicator that equals 1 if the respondent is a smoker and the number of cigarettes consumed per day.

In both the 1991 and 1993 NHIS, certain employed respondents were also asked about workplace smoking policies. Workers were first asked whether their firm has a formal policy to restrict smoking. Workers responding affirmatively to this question were then asked whether smoking is banned in some or all indoor public areas and in some or all work areas. From the responses to these questions we created three measures of the presence of workplace smoking bans. The first is an indicator variable that equals one if the firm has any type of formal smoking policy and zero otherwise. Indicators for whether the employer bans smoking in all indoor public areas and for whether it bans smoking in all work areas were also created.

It should be noted that the questions concerning workplace smoking policies were only asked of workers who could potentially be subject to a smoking ban, i.e. workers who worked indoors and those who were not self employed. Unfortunately, the definition of indoor work area differed in the two surveys. In the 1993 survey, workers were asked whether they worked primarily indoors or outside. In the 1991 survey, workers were asked to identify their type of work area from a specific list. Some types of indoor workers, such as indoor workers with no fixed work area or workers who listed "other" as their work area, were not asked the workplace smoking policy question. This difference in sampling means that a lower fraction of workers from the 1991 survey were included in the final sample. Of the 25,591 respondents in the 1991 survey who were employed, 3154 were deleted because they were self employed and 12,211 were eliminated based on their answer to the work area question. In the 1993 survey, however, there were 12,392 workers, of which 1,501 were self employed and 2,275 worked outdoors. After deleting observations with missing values for this and other variables, we end up with 9,704 observations from 1991 and 8,386 from 1993 for a final sample of 18,090 observations.[9]

In Table 1, we report descriptive characteristics for our sample of indoor workers and compare them to the characteristics to all workers in the 1991 and 1993 NHIS and to all workers in the 1991 and 1993 Outgoing Rotation Samples from the Current Population Surveys. The all-workers samples from the NHIS and CPS are very similar. The sample of indoor workers is slightly younger and more educated than the full sample. A striking characteristic of the restricted sample is the low fraction of male workers. This is not surprising because workers in some male dominated professions (e.g. construction workers or truck drivers) have been eliminated. By restricting the sample to indoor workers, we increase the fraction of workers in such industries as retail trade and manufacturing relative to the overall work force. The sample selection criteria also increases the fraction of workers in occupations such as administrative support, professional speciality occupations, and executives and administrators.

Table 2 presents summary statistics that describe the type and frequency of workplace restrictions as reported by workers from the two NHIS surveys. In 1991, 76.9 percent of all workers are employed in workplaces that have some type of formal policy to restrict smoking and 58.8 percent are employed in firms that ban smoking in all work areas. By 1993 the fraction of workers whose firm bans smoking in all work areas had jumped to 70.1 percent. There is a clear difference in the frequency of these policies in large and small establishments. For example, 76.8 percent of employees at work sites with fifty or more employees are employed by a firm that bans smoking in all work areas, while only 55.5 percent of employees at smaller work sites face an employer ban. To illustrate the variation in these

[9] It should be noted that in preparing these data for public release, the National Center for Health Statistics (NCHS) recoded the primary sampling units (PSU) codes that identify the location of each respondent to preserve confidentiality. In order to match the respondents with the appropriate state cigarette excise tax we reached a special agreement with the NCHS that allowed us to identify the state of residence for each individual while maintaining the confidentiality of their PSU. For a small number of observations we were unable to identify states of residence for reasons of confidentiality and therefore, these observations were deleted from the sample.

policies by industry and occupation, we report the prevalence of the various policies for some common industries and occupations, collapsing all other industries and occupations into a single category. The reported percentages indicate workers in service industries or professional occupations are far more likely to work in firms that ban or restrict smoking than those in manufacturing industries or administrative occupations.

Although there are many ways to characterize workplace smoking restrictions, we restrict our attention to the effect of work area smoking bans on smoking behavior. We make this restriction for three reasons. First, this policy is most likely to impose the greatest cost on smokers and so it should have the greatest impact on smoking behavior. Second, in analyzing the types of workplace smoking policies adopted by firms, we found that 96 percent all firms banning smoking in public areas also banned smoking in work areas. The converse, however, was not true. Therefore, in our data it would be almost impossible to disentangle a separate effect for public area bans as almost all firms who had them also banned workplace smoking. Finally, the category for the existence of any policy at all is too vague to allow one to be certain what is being measured.

IV. Single-Equation Models

In Table 3 we present mean values for the three dependent variables in our analysis for work sites with and without bans. The first measure of smoking is a variable labeled "current smoker" which is an indicator that equals one if a person indicates they smoked at the time of the survey. The second is a measure of smoking intensity that equals average daily consumption in cigarettes per day. The third is a composite variable that equals daily consumption for all smokers and equals zero for nonsmokers.[10] In both 1991 and 1993 the rate of smoking prevalence was roughly eight percentage points lower at work

[10] Not all smokers report daily consumption so the number of observations for cigarettes per day (smokers only) is less than the number of smokers in the sample.

sites with smoking bans in all work areas compared to work sites without such policies. Similarly, the average number of cigarettes consumed per day by smokers at work sites with smoking bans was lower by 3.8 cigarettes in 1991 and 2.8 in 1993 relative to work sites with no formal smoking policy.

In modeling cigarette demand we employ a "two-part" estimation procedure where in the first stage a probit equation is used to model the decision to smoke. In the second stage, daily cigarette demand among smokers is modeled with a simple linear regression. This two-part model has been used extensively in the health economics field to model the demand for medical care (Duan *et al*, 1982, 1984), smoking (Wasserman *et al*, 1991), and drinking (Manning *et al*, 1995). We also present results from a single-equation where we model average daily consumption of cigarettes for all workers.

The basic model (I) includes as covariates demographic information such as age, age squared, family size, log income, an indicator variable for income missing,[11] three indicator variables for region (Midwest, North, and West), four for education (high school dropout, some college education, college graduate, and postgraduate), three for ethnicity (black, Hispanic, and other race), two for the type of metropolitan area (live in one of the 20 largest metropolitan areas, live in some other metropolitan area), four for marital status (divorced, separated, widowed, and never married), the real cigarette tax (state + federal, in cents),[12] and a year effect. In subsequent specifications we include controls for industry (model II), industry and occupation (III), and finally industry, occupation, and state effects (IV).[13] As noted above, in all of these specifications our policy variable is whether smoking is banned in all work areas. We focus our analysis on the models that combine the 1991 and the 1993 NHIS because the

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[11] For those respondents who did not report income, we set log income to zero and create a dummy variable that equals one when income is missing and zero otherwise.

[12] The data on taxes are published yearly by the Tobacco Institute in their publication *The Tax Burden on Tobacco*.

[13] The NCHS categorizes industries and occupations into 14 and 13 groups respectively. One category in the industry and occupation groups was "unknown." Rather than delete these workers from the analysis, we included an indicator for this group. One of the occupation categories was home work and we deleted these individuals. Therefore, we included 13 industry and 11 occupation indicators.

results of the individual cross-sections varied little across the two years.

The coefficients on the workplace smoking ban variable in the current smoker probit model are reported in the second column of Table 4. The coefficients are normalized probit estimates that measure the "marginal effect" or the change in the probability that an individual smokes given the adoption of a workplace smoking ban.[14] These results indicate that workplace smoking bans yield a substantial decrease in the rate of smoking. The addition of industry, occupation, and state effects reduces the coefficient from a 6.6 percent decline to a 5.5 percent decline. To put this result in perspective, consider the fact that the overall national smoking participation rate also fell by 5 percentage points during the period 1985 to 1992 when most of these workplace restrictions went into effect. Alternatively, consider how much of an increase in cigarette prices would be needed to generate a comparable reduction in smoking prevalence. Most estimates of the elasticity of demand for smoking suggest that half of any demand drop generated by a price hike is attributable to a drop in the smoking prevalence, and half is attributable to a drop in smoking intensity (Lewit, Coate, and Grossman, 1981; Grossman *et al*, 1993; Evans and Farrelly, 1996). If we use a demand elasticity of -0.4 that is consistent with previous work (Viscusi, 1992), the implied smoking participation elasticity with respect to price is -0.2. With the smoking participation rate at about 25 percent for our sample, it would take a doubling of prices or a 400 percent increase in the average tax per pack to induce a 5 percentage point reduction in the smoking.

In row 2, we present the coefficients on the workplace ban variable from OLS regressions where the dependent variable is the number of cigarettes smoked per day for the sample restricted to smokers only. The results for this smokers-only model indicate that workplace restriction also have a statistically and economically significant negative effect on the quantity of cigarettes smoked. The imposition of a workplace ban leads to a reduction in daily consumption of 2.3 cigarettes. For a pack a day smoker

[14] The marginal effect for the j'th variable is calculated as $\beta_j\phi(z)$, where $z=\Phi^{-1}(p)$ and p is the sample mean of the response variable (i.e., indicator variable for smoker), β_j is the probit coefficient, ϕ is the standard normal probability density function, and Φ^{-1} is the inverse of the standard normal cumulative density function.

(mean value), this is about a 10 percent decline in the number of cigarettes smoked per day.

Finally, variations in the demand for cigarettes on the intensive and extensive margin are estimated jointly by setting cigarette consumption for nonsmokers to zero and re-estimating the OLS model. These results are presented in the row 3 in Table 4. These results again suggest that workplace bans have a statistically significant adverse affect on smoking consumption. Overall cigarette demand decreases by 1.6 cigarettes per day per worker as a result of the workplace smoking ban.[15]

To check that these estimates are not a product of having used the NHIS, we attempt to replicated these results using data from special supplements from the September 1992 and May 1993 Current Population Survey (CPS). The CPS is a monthly survey of over 60,000 households designed to provide estimates of important labor market variables. Periodically the CPS has included supplemental questions about smoking. In September 1992 and May 1993 the CPS included a lengthy survey about smoking habits, workplace smoking restrictions, and attitudes towards environmental tobacco smoke. We used these surveys because the questions concerning workplace smoking restrictions were nearly identical to those asked in the NHIS. Workers who work indoors or who are not self-employed were asked whether their firm had any policy to restrict smoking, had any restriction on smoking in public areas, and whether their firm banned smoking in all work areas. By pooling the two CPS surveys, there are about 64,000 observations for workers who work indoors and who are not self employed. The descriptive statistics reported in the CPS are very similar to those found in the two NHIS data sets. The CPS reports that 62 percent of all indoor workers were subject to workplace smoking bans in 1992 and 67 percent in 1993. The 1992 number is roughly the midpoint between the numbers reported for 1991 and 1993 from the NHIS reported in Table 2, and the 1993 number is slightly lower than the estimate for

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[15] The magnitude of this coefficient is simply a linear combination of the previous estimates. The average smoker smokes a pack a day, so a 5.5 percentage point drop in smoking participation and a 2.40 per day cigarette decline for the remaining twenty percent of smokers in establishments with bans would translate into a $(0.055)*20 + (0.20*2.4)=1.6$ per day cigarette decline for the entire sample.

1993 from the NHIS. The smoking participation rates in the CPS data are also nearly identical to numbers reported in the NHIS.

In the lower panel of Table 4, we present results from the pooled CPS data. The covariates have been defined in a similar fashion to the ones used in the NHIS. Although the qualitative nature of the results are insensitive to the choice of data sets, the magnitude of the estimated effects of the workplace ban are somewhat smaller in the CPS data set than in the NHIS. The work area smoking ban is predicted to reduce smoking by 5.7 percentage points in model III from the NHIS sample, whereas the CPS produces only a 3.8 percentage point drop. The work area smoking ban coefficients for the smokers only and all workers smoking intensity models are also smaller in the CPS compared to the NHIS samples. It should be noted that these differences between the estimates are not, however, statistically significant. Overall, these single-equation estimates again suggest a significant reduction in smoking behavior results from the implementation of workplace smoking bans.

If the results in Table 4 indicate a causal impact of work area smoking bans on smoking participation, then we would expect the impact of the bans to be a function of the cost that they impose on workers. For example, the cost of the bans should be related to the amount of time spent in the restricted environment. People who only work 10 hours per week may more easily adjust to the work area smoking ban by shifting the timing of their smoking. On the other hand, the cost of the bans should be greater for workers that work long hours. We test this hypothesis using data from the pooled 1992 and 1993 CPS data set. In these models, we add indicators for the usual hours worked per week and interact the work area smoking ban variable with these work week indicators. Estimates for the current smoker probit and the cigarettes per day (smokers only) models are reported in Table 5.

In the current smoker probit, the coefficients on the smoking ban/hours per week interactions are nearly monotonic in hours worked per week. There is no statistically significant impact of the work area smoking bans for people who work less than 20 hours per week. In contrast, the largest impact of the bans in both models is for those workers who work 50 hours per week or more. In the cigarettes per day OLS, the impact of the bans is monotonic in the hours worked, with the decline in daily consumption largest for workers who work longer hours in establishments with bans

V. Testing for Omitted Variable Bias

The results in the previous section suggesting that smoking is lower in establishments that ban smoking in all work areas does not prove there is a causal relationship between bans and smoking. All of the single-equation models presented in the previous section assumed the match of a worker to a firm with smoking restrictions is exogenous. As we noted in the introduction, this assumption is persistent through out all of the previous evaluations of the impact of workplace smoking bans on smoking. However, if a worker's unobserved propensity to smoke is correlated with the presence of workplace smoking restrictions, then these single-equation estimates will be biased.

A. Is there evidence of a omitted variables bias?

Is there reason to believe that workplace bans are not randomly assigned? If workers have preferences over whether smoking is allowed at the worksite (or firm costs are affected by having workers who smoke), then the theory of equalizing differences implies that workers and jobs with various non-pecuniary characteristics will not be randomly assigned. [16] In equilibrium the number of jobs of a particular type (nonsmoking) will depend on the joint distribution of worker preferences and firms costs, and the assignment of workers and firms will vary systematically with respect to these preferences and costs. The presence of workplace bans will thus be correlated with observed and unobserved (to the econometrician) characteristics of the worker and firm that are correlated with these costs or

[16] See Rosen (1986) for a derivation and review of the theory of equalizing differences.

preferences. The estimates in Tables 3 and 4 clearly show that the presence of a workplace smoking ban is correlated with the observed characteristics of the worker. The raw difference in the smoking rate between workers who work in establishments with and without bans is over 8 percentage points. This difference shrinks to 5.5 percentage points (model IV, Table 4) when we control for observed characteristics of the worker. Since the work area smoking ban indicator is correlated with observed characteristics, it seems possible that it is also correlated with the unobserved characteristics of the worker or firm that may be correlated with smoking behavior of workers.

On the employer side of the market, firms that adopt workplace bans may be those for whom absenteeism or unhealthy workers are the most costly. These costs may lead these firms to adopt a number of policies or programs to improve the health of their workers. To the degree firms with a workplace smoking ban also provide exercise programs, on-site exercise facilities, and/or smoking cessation programs, then smoking may be lower because of these other programs, not because of the smoking ban. If the firms who are most likely to adopt smoking bans are also those most likely to adopt these other programs that promote better health, then the single-equation estimate would overstate the impact of the ban.

We examined whether the work area smoking ban is capturing the benefits of other firm-sponsored health promotion programs. We re-estimated specification III from Table 4 adding three indicators for other programs adopted by the firm: whether the firm has on-site exercise programs or facilities, and whether the worker has employer-provided health insurance.[17] The results from these tests show little evidence of spill-over benefits from the exercise programs and facilities. In none of the specifications were the coefficients for exercise programs and facilities statistically significant. This is also true when we add a covariate for employer-provided private health insurance from the 1991 NHIS.

[17] The health insurance variable is available in the 1991 NHIS.

There is only modest evidence (p-value=0.12) of spill-overs from health insurance provision. Further, the inclusion any of these programs does not alter the work area smoking ban coefficient.

It is also possible that to the degree workers have preferences over whether smoking is allowed on the job, then smokers may be more likely to quit firms who ban smoking or are less likely to apply for jobs at firms with smoking ban. As noted above this non-random turnover behavior will tend to make the OLS estimates overstate the impact of the workplace ban.

To examine this we use data from the 1991 NHIS where workers report their number of months on the current job.[18] If our results were driven *purely* by selection, or the movement of smokers away from firms with bans, then one might expect the effect of smoking bans to be present only for new or low tenure workers. If selection from worker mobility is present but there is some change in smoking behavior, then the effect of workplace smoking bans would decline with tenure. In Table 6, we report estimates of our reduced form smoking prevalence and intensity equations where we interact the presence of a smoking ban with worker tenure. On both the intensive and extensive margins we find that smoking bans have the <u>smallest</u> affect on workers with the least amount of tenure. The reduction in smoking prevalence for high tenure (48 months or more) workers is only slightly smaller than for the whole sample (5.9 vs 6.5 percent) while the reduction in the quantity of cigarettes consumed is the same (2.81 cigarettes per day).

As a second test, we note that if selection occurs because smokers are more likely to leave a firm with a work area ban, we would expect that job tenure for smokers would be lower in smoking establishments than in work place bans. We find no evidence of this systematic difference in job

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[18] From the wording of the question, it is not clear whether people are reporting tenure for their current job at the firm, or tenure at the firm.

tenure.[19] These results combined with the absence of a decline in the size of the reported coefficients with tenure, again strongly suggests that selection from differential turnover of smokers is not driving our results.

Single-equation estimates may also be biased if the presence of a smoking ban alters the type of workers attracted to a firm. Firms with a smoking ban in place may attract a "healthier" type of worker than those that do not. If "healthier" workers have lower smoking rates than workers with worst health habits, then the OLS estimates of the effect of smoking bans will be overstated. Smoking rates can be lower among "healthier" workers either because they are more likely to quit smoking or because they are less likely to start smoking independent of the presence of the smoking ban. We performed two indirect tests of this notion that firms with smoking bans may have attracted healthier workers.

Most people who smoke start smoking at a young age. Using data from the smoking supplement to the May 1993 CPS, we calculate that roughly 90 percent of all smokers start smoking by age 20. Since the fraction of the population that will ever smoke is already determined by the time a worker is 21, working in a firm that bans smoking after the worker is 21 should have no affect on whether she *ever* smoked. Consequently, in an adult working population, the work area bans should reduce smoking primarily by inducing workers to quit smoking and not lowering take up rates. If selection serves to allow firms with bans to attract "healthier" workers (those with lower innate smoking take up rates), bans would be correlated with the fraction of the adult population who ever smoked.

Secondly, since "healthy" workers are likely to engage in a range of other healthy habits or practices, evidence for selection would exist if the presence of a workplace smoking ban is highly

[19] An alternate construction of this test can be had using a difference in difference analysis of average tenure on the current job by smoking status and type of workplace smoking policy. To control for worker differences across establishment we scale the difference in tenure between ban and no ban establishments for smokers (5.0 months), by using the difference in tenure for non-smokers in these establishments (6.8 months). The difference in difference estimate suggests that smokers do indeed have a lower tenure at establishments with work area smoking bans, but the magnitude is small (1.8 months) and is not statistically significant (standard error is 4.7). Again, there appears to be little evidence to support the conclusion that selection occurs through differential turnover behavior of smokers at firms with workplace bans.

correlated with other health habits that they are not designed to alter. To examine these possibilities, we used data from NHIS[20]on 6 different health measures including whether the worker never smoked, always adds salt to their food, whether they always wear their seat belt, whether they own a smoke detector, whether the worker is in excellent or very good health, and whether the workers is 20 percent or more overweight.[21] The marginal effects of the work area smoking ban from the health habit probits using specification III from Table 4 are listed in Table 7.

The coefficient on the work area smoking ban is statistically significant in the health habit probit models measuring ever smoked, salt use, seat belt use, and use of a smoke detector. While it could be argued that salt use and *not smoking* are complimentary activities, so that a reduction in smoking may improve these outcomes, it is hard to reconcile how work area smoking bans increase seat belt use. This pattern is, however, consistent with the hypothesis that healthier workers migrate to firms with work area smoking bans. Because smokers are also likely to engage in unhealthy habits, this correlation would explain the statistically significant workplace smoking restriction coefficients in the other equations.[22]

In total, these results suggest that the workplace smoking ban is not measuring some other health program adopted by the firm and the smoking ban is not encouraging smokers to quit their jobs and seek employment in establishments that allow smoking. The results in Table 7 suggest that workers with better health habits may be attracted to firms with workplace smoking bans. If these healthier workers

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[20] Unfortunately, we were not able to replicate these results using the 1992 CPS data because it does not include questions about other health habits or information about firm size.

[21] These habits were chosen because there is the possibility that some habits could be altered indirectly by the smoking ban. If health habits are complimentary, then a reduction in smoking could alter other habits as well. For example, smoker may find it difficult to exercise because of reduced aerobic capacity. A worker who has recently quit smoking may now be able to exercise more often.

[22] If this type of selection is occurring, then workplace smoking bans may not reduce aggregate smoking, but simply redistribute smokers across firms. However, if firms wish to reduce the health costs of smoking or attract healthier workers, then these policies would have achieved their goal.

also have lower smoking rates, then the results presented in Table 4 may be overstated.[23] In the next section, we construct a model to test whether these results are indeed biased by sample selection.

B. A Bivariate Probit Model

If the match of workers to smoke-free firms is not exogenous, then to accurately evaluate these programs, we must model this non-random match of workers and firms. In this section we outline two simultaneous equation models that allow for the possibility that the firm policy and the propensity to smoker are correlated. For the smoking prevalence models, the exercise is complicated by the fact that the two variables of interest, the workplace policy and smoking participation, are both discrete. The appropriate simultaneous equation model in this context is a bivariate probit. Our second simultaneous equation systems describes the number of cigarettes consumed per day (for all workers) as a function of the workplace smoking ban. In this case the dependent variable is continuous so a relevant procedure is a two-stage least squares model.

The decision to smoke can be described by the latent variable model:

$$(1)\ Y_i^* = X_i\beta + W_i\delta + \epsilon_{1i}$$

where Y_i^* is the net benefit an individual receives from smoking, X_i is a vector characteristics of the worker that proxy for his/her tastes, income, and other determinants of the demand for smoking, W_i is a workplace smoking ban dummy variable which affects the cost of being a smoker, and ϵ_{1i} is a normally distributed random error, with mean zero and unit variance. Let $Y_i = 1$ if a worker smokes and let $Y_i = 0$

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[23] It is also possible that firms with the highest fraction of smokers and hence the worst ETS problems may also institute restrictions on workplace smoking. In this instance, the single-equation models may understate the causal impact of the programs. It is however, hard to reconcile this hypothesis with the observed correlations between the workplace bans and other health habits.

otherwise. An individual will smoke if the expected net benefits to smoking are positive or the probability than an individual will be a smoker is:

$$(2)\ \text{Prob}[Y_i = 1] = \text{Prob}[X_i\beta + W_i\delta + \epsilon_{1i}>0] = \Phi[X_i\beta + W_i\delta]$$

where $\Phi(\bullet)$ is the standard normal cdf. A worker will work in a firm with a workplace smoking ban if the net benefits are positive. The net benefits , W_i^* are modeled as being determined by the following the linear equation:

$$(3)\ W_i^* = Z_i\eta + \xi_i$$

where Z_i is a vector of covariates that capture worker tastes for a smoke-free workplace and other attributes of the firm which affect the benefits of choosing that work site, and ξ_i is a random error. To allow for the possibility that unobserved characteristics of a worker's decision to smoke and their preference for the type of firm are correlated, we assume that ϵ_{1i} and ξ_i are distributed according to a bivariate normal, with $E[\xi_i]=E[\epsilon_{1i}]=0$, $Var[\xi_i] = Var[\epsilon_{1i}] = 1$ and $cov\ [\xi_i\ \epsilon_{1i}] =\rho$. Given that both the decision to smoke and the decision to adopt a workplace smoking ban are dichotomous, the likelihood function is a bivariate normal probit.

To provide a basis of comparison for our bivariate probit results, we also estimate a standard two-stage least-square (2SLS) model. The 2SLS model is useful as we can use it to preform tests of over identifying restrictions on our instruments. Although this model ignores the discrete nature of both Y and W, Angrist [1991] showed in his Monte Carlo study that ignoring the fact that the dependent variable is dichotomous, and estimating (1) with instrumental variables, the IV estimate of δ is very close to the estimated "average treatment effect" calculated in a bivariate probit model. The average treatment effect is the average difference between the probability that a worker smokes if he or she works in a firm with a workplace bans and the probability they smoke if they did not. Thus, if n is the sample size and β and δ are the maximum likelihood estimates of the parameters in equation (2), then the average treatment effect equals $(1/n)\Sigma_i [\Phi(X_i \beta + \delta) - \Phi(X_i \beta)]$. We use the "delta" method to calculate the variance of the average treatment effects.

C. Instruments for Work Area Smoking Bans

To identify the bivariate probit and 2SLS models requires that at least one covariate in Z_i must be excluded from X_i. Since Z_i includes both worker and firm characteristics measuring the attractiveness of a job, while X_i includes variables that affect a workers demand for cigarettes, firm characteristics are a potential source of valid instruments. We tried two variables as instruments for work area smoking bans. The first is an indicator variable that is equal to one if the worker is at a worksite with fifty or more employees, and zero otherwise. Based on the descriptive statistics from Table 3, establishments with fifty or more employees are more likely to adopt workplace smoking bans than smaller firms. In Table 8, we report the first-stage probit where we model the probability a firm has adopted a work area ban as a function of the establishment size indicator and the other covariates listed in Model III, Table 4. The marginal effect for the establishment size indicator suggests that job sites with more than 50 employees have a 22 percentage point higher probability of adopting a work area smoking ban.

These results are consistent with the work of Kenkel and Supina (1992) who show that establishment size is correlated with a variety of employer-sponsored health promotions. Clearly, the presence of smoking restrictions is correlated with firm size. This is not altogether a surprising result. A number of other studies have noted that larger establishments are more likely to institute smoking restrictions.[24] There are at least two reasons why large firms are more likely to ban smoking. First, in

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[24] These studies are reviewed in the 1986 Surgeon General's report on *The Health Consequences of Involuntary Smoking.*

smaller firms, differences in preferences concerning ETS can be dealt with on a case by case basis *à la* Coase. In small firms, property rights to the workplace air can potentially be decided by parties with minimal transactions costs. In bigger establishments, formal rules are more often employed to handle these types of situations. Second, a number of authors have noted that firms have adopted work place smoking bans for fear of possible liability for illnesses caused by second hand smoke (Fielding 1982; Walsh 1984). If big firms that allow smoking are considered by potential plaintiffs to have deep pockets, and they are more likely to be sued by employees because of their size, then larger firms may be most likely to ban smoking in the workplace.

A potential problem with the use of firm size as an instrument is that since Brown and Medoff (1989) and others have shown that compensation (and hence fringe benefits like health care) vary with firm size, the correlation between firm size and workplace bans could simply be capturing the incentives that firms which provide insurance have to improve the health of their workers. Survey data, however, suggests that the economic costs of smoking are a minor reason why firms adopt work place smoking bans. In surveys of companies with workplace smoking restrictions, Peterson and Massengill (1986) found only 9 percent of establishments cited a reduction in insurance cost as a reason for their smoking restrictions. A similar study by the Human Resources Policy Group (1985) found that only 3 percent of firms with workplace smoking restrictions listed a reduction in costs as their motivation for adopting smoking restriction. To test this hypothesis directly, we used data from the 1991 NHIS and re-estimated the first stage work area ban probit models replacing the establishment size indicator with one for whether the firm provides insurance. As seen in Table 8, the marginal effect of the insurance indicator is substantially smaller than firms size (only .05) and is less significant (t-statistic of 3). When both variables are added to the model, the marginal effect for private insurance is now statistically insignificant while both the significance and coefficient on firms size are unaltered. Thus, the firm size variable is picking up effects that are orthogonal to the health insurance considerations which may encourage firms to adopt workplace bans.

A second instrument we utilize is a measure of the supply of jobs in an industry within a state that have work area smoking bans. As we noted above, there are persistent patterns across industries and states in the adoption of the work area smoking bans. Using data from the 64,000 indoor workers from the smoking supplements to the September 1992 and May 1993 CPS, we calculate state by industry means of the fraction of workers with work area smoking bans. We then merge these means into the NHIS data sets using the state codes supplied by the NCHS.

The state/industry supply of non-smoking jobs is a good predictor of whether an individual is subject to a work area smoking ban. In the second column of Table 8, we report the marginal effect for this coefficient from a basic probit model. The marginal effect is 0.406 (with a t-statistic over 9) indicating that a one percentage point increase in the fraction of jobs in an industry within a state that are subject to work area smoking bans increases the probability of working in a non-smoking job by 0.4 percentage points. When both firm size and the state/industry means of the work ban are added to the model, both coefficients are similar to the estimates in columns (1) and (2) respectively.

For our instruments to be valid, they must be uncorrelated with a worker's unobserved propensity to smoke. Although there is no reason to suspect that smokers are less attracted to large firms than small firms per se, large and small firms differ in the observed characteristics of their worker. Firm size would not be a valid instrument if it is picking up unobserved worker characteristics that affect the demand for cigarettes and are correlated with firm size. For instance, a number of authors have demonstrated that workers in large firms receive higher wages than workers in smaller firms (Brown and Medoff , 1989). Part of this pay premia is due to the fact that large firms attract workers with better observed skills, such as education. Evans and Montgomery (1994) have demonstrated that measures of human capital investment, such as education, are correlated with measures of health habits, such as smoking. They postulate that the correlation is a signal of interpersonal differences in the discount rates.

If large firms attract people with higher levels of human capital, and these people have lower discount rates, then these workers may also have lower rates of smoking prevalence. Thus, to the degree large firms attract workers with "better" observed or unobserved skills, there may be a correlation between firm size and smoking.

Unfortunately, we cannot explicitly test whether smokers are more or less attracted to larger establishments. We do not see large differences in the means of observed worker characteristics in our data.[25] For example, workers in establishments with 50 or more employees have only 0.15 more years more education than workers in smaller firms. Another indirect way to examine this issue is to see to what degree other health habits are correlated with firms size. If large firms attract or hire healthier workers (nonsmokers) then they should also have fewer workers with other "bad" habits. In the first column of Table 9, we present results from the health habit probit equations using the establishment size indicator instead of the workplace bans variable. This model does not include the work area smoking ban indicator. None of the habits that were correlated with the presence of workplace bans are correlated with firms size. Further, the only variable that is statistically significant (20 percent or more overweight) is uncorrelated with whether the firm had workplace restrictions. Although the lack of correlation between establishment size and the other health habits does not prove that the unobserved propensity to smoke is not correlated with establishment size, the results are consistent with this hypothesis.

One might also be concerned that because of economies of scale, large firms may be able to invest in policies (other than workplace bans) that promote the health of their workers. Kenkle and Supina (1992) for example demonstrate that larger firms were more likely to adopt any of the nine employer initiated health programs they considered. This result is also found in the NHIS data. Large firms are more likely have on-site exercise facilities and exercise programs, and they are more likely to

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[25] This may be due in part to the fact that our establishment size dummy equals ones for relatively small firms (firms that are much smaller that the "large" firms considered by Brown and Medoff).,

provide health insurance. The results in Table 8 demonstrate that these health policies have little impact on smoking and that their inclusion in the smoking equations does not alter the coefficient on workplace restrictions. Thus, the effects of bans is orthogonal to whatever factors may make large or small firms more or less likely to adopt these other health policies.

In the case of the state/industry mean of non-smoking jobs, we suspect that this should have little impact on whether a worker smokes, conditional on the type of smoking rules at their current firm. As with the firm size indicator, we anticipate that once we control for industry effects, workers with different health habits should not be attracted to a particular establishment given the mean level of non-smoking jobs in the state/industry cell. Unfortunately, there are mixed results for this hypothesis. In the final column of Table 9, we add the state/industry supply of non-smoking jobs to the basic health habit probit models. The coefficient on the never smoked indicator is small and insignificant. However, the state/industry percent of jobs that are non-smoking is statistically significant in the salt, self-reported health status, and smoke detector equations. Importantly, these coefficients become statistically insignificant, however, once we add state effects to the model.

D. Bivariate probit results

The results of the bivariate probit models are reported in Table 10. In the first line of the table, we report results from a standard probit model where the work area ban variable is treated as exogenous. To facilitate comparison between bivariate probit and 2SLS models, we report the average treatment effect which was defined above. In model (2), we present bivariate probit estimates that utilize the single establishment size indicator as the instrument for the workplace smoking ban. Using firm size as an instrument, the results show that a smoking ban reduces the prevalence of smoking by 7.7 percentage points compared to the 5.5 percentage point reduction in the single-equation models. Further, in this model, the coefficient on ρ is imprecisely estimated.[26] In total, these results suggest that the single-equation estimates presented in Table 4 are not seriously affected by omitted variable bias.

As an alternative to the bivariate probit model, we estimate the model by 2SLS. In row (3), we report the basic 2SLS results using the establishment size indicator as an instrument and find that the average treatment effect in line (2) from the bivariate probit model and the 2SLS coefficient are very similar. The coefficient (t-statistic) on the smoking ban in the smoking prevalence equation indicate a reduction in the prevalence of smoking by 6.3 percentage points (1.80).

Because firm size, occupation, and industry are so strongly correlated with the presence of a work area smoking ban, we generated an additional class of instruments by interacting firm size with the industry and occupation indicator variables. This strategy has the advantage of allowing us to construct specification tests such as the test of over identifying restrictions (Newey, 1984). The additional instruments have tremendous explanatory power. In a first-stage probit model where we model the probability of observing a work area smoking ban as a function of the covariates corresponding to model III in Table 4, the establishment size indicator, plus interactions of establishment size with industry and occupation effects, we reject the null that these interactions are jointly zero with a p value of 3.6*E-11. The 2SLS results for models with these alternative instruments are presented in line (4) of Table 10. In this model, the 2SLS estimate of the work area smoking ban coefficient is similar to the results in line (3), but the t-statistic is 10 percent larger in absolute value. The p-value of the test of over identifying restrictions indicates that we cannot reject the null hypothesis that the model is properly specified.

Recently, a number of authors have expressed concern that 2SLS models with substantially more instruments than endogenous regressors are biased towards the OLS estimates, especially when many of the instruments are weakly correlated with the endogenous covariate of interest (Bound, Jaeger, and

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[26] The coefficient (t-statistic) on ρ is 0.041 (0.68).

Baker (1995), Staiger and Stock (1994), Angrist and Krueger (1995), Angrist, Imbens, and Krueger (1995)). This is not likely to be a problem here as the additional instruments generated by the interactions are so strongly correlated with the presence of workplace bans. Nonetheless, limited information maximum likelihood estimation (LIML) does not suffer the same types of bias as 2SLS when many weak instruments are used. The LIML results in specification (5) demonstrate that our results are not subject to this criticism.

In model (6) we utilize our alternative instrument, the state/industry mean of work area smoking ban jobs. In this model, the 2SLS estimate of the work area smoking ban parameter is slightly smaller but it is imprecisely estimated. As the model is exactly identified, we cannot construct a test of over identifying restrictions. We can however, estimate a model that uses both firm size and the state/industry supply of non-smoking jobs of instruments. These results are presented in model (7). Here the 2SLS estimates are similar to the OLS values, the coefficient is statistically significant, and the p-value on the test of over identifying restrictions is an incredibly high value of 0.93.

In the next 5 lines of the table, we report results for these specifications using as the outcome of interest cigarettes per day for all smokers and setting daily consumption for non-smokers equal to zero. Comparing results in models (3) and (1a), we find that the 2SLS estimates are somewhat smaller than the OLS estimates. The results from specification (3) where the firm size indicator is used as an instrument are replicated in model (4) where we add industry and establishment size, plus occupation and establishment size interactions. In this model, the p-value on the test of over identifying restrictions is large. The basic results do not change when we re-estimate the model with LIML, or when we use the state/industry mean of work area bans with the establishment size indicator as instruments.

Both the bivariate probit and two-stage least squares estimates suggest that once we control for self-selection bias, that workplace smoking bans lead to a statistically significant decrease in both the prevalence of smoking and cigarette demand. A complete ban on smoking in all work areas leads to a decrease in the rate of smoking prevalence by roughly 7 percentage points and a decline in cigarette demand by all workers of 1.5 cigarettes per day. The results of the simultaneous equation models also suggest that self-selection bias is not a serious concern and that the correlation between other health habits and workplace smoking bans is spurious. The system estimates indicate that for smoking participation, the single-equation estimates are not subject to an omitted variables bias.

As a further check on whether the firm size instrument is performing as it is designed, we also instrument for the workplace smoking bans with firm size in models of worker's decision to never smoke, use salt and wear seat belts. If the correlation between these health habit and workplace bans that we found in Table 7 is spurious, then the instrumental variable coefficients on the work area ban variable should decline in absolute value. These results are reported as model (2) in the lower half of Table 10. In these models, the estimated ρ take on the expected signs, but they are not precisely estimated.[27] The bivariate probit models from these models show that once we control for self-selection bias the effect of the smoking ban on these other health measures is eliminated. In the single-equation version of the never smoked model, the coefficient on the work area smoking ban was positive, large and statistically significant. In the bivariate probit models, the coefficient is reduced by over 50 percent and the t-statistic is about .5. Similarly, there is a negative and statistically significant correlation between workplace smoking bans and salt use in the single-equation models, while it is positive and insignificant in the bivariate probit model. The average treatment effect (t-statistic) for the smoking ban on salt use is just 0.8 (0.35) percentage points in the bivariate probit. The results for the seat belt use equation show similar patterns. The average treatment effect (t-statistic) for the smoking ban in the bivariate probit is reduced by 75 percent relative to the single-equation estimate (from 4.3 to 1.3 percentage points) and is no longer statistically significant (t-statistic 0.46). These results suggest that our instrument has removed

[27] The MLE estimate (t-statistic) of ρ in the never smoked, salt, and belt use models are 0.042 (0.76), -0.078 (-1.10), and 0.031 (0.49) respectively.

the spurious selection induced correlation between workplace bans and health habits.

In Table 10, we also report results for 2SLS models where we use never smoked, salt use, and belt use indicators as the dependent variables of interest. Again, if our instruments are working properly, we should see the coefficients on the work area ban decline in absolute value. We find this pattern for each instrument set used. In some cases, we can reject the null hypothesis that the instruments should be excluded from the structural equation of interest. The fact that our instrument eliminates the effect of bans on these health habits but not on smoking suggests that the correlation between the workplace smoking bans and these other health habits is spurious rather than a signal of omitted variables bias. This could occur if firms that adopt workplace smoking bans are also more likely to adopt other program that promote health and these programs only alter the health habit in question -- diet programs reduce salt, smoking bans reduce smoking, general health education increases belt use. In this instance, other health habits such as belt and salt use would be correlated with workplace smoking bans, because smoking bans signal that other health program were also adopted but the correlation between the ban and non-smoking health habits is spurious.

E. Alternative ways to control for omitted variables bias

The results in the previous two sections suggest that although there was reason to be concerned about an omitted variable bias, firm size provides a valid instrument to remove this bias. These 2SLS results suggest that the basic conclusions from the single-equation results are not sensitive to the fact that workers with better health habits maybe sorting into firms most likely to ban smoking on the job. In Table 11 we present the results from several other tests to support the conclusions of the 2SLS results that this sorting does not bias our results.

In the first row of Table 11, we reproduce the basic probit results from Table 4 for the 1991/1993 NHIS sample. In the second row, we add indicators for other proxies of health status (belt use, salt use, and self-reported health status).[28] If there is an omitted variables bias in the basic single-equation estimates, then including these health habit indicators should greatly reduce the impact of the work area smoking ban. As the results in model (2) of Table 11 indicate, including these measures has a small and insignificant effect on the coefficient on work area smoking ban, reducing it from 5.7 to 5.3 percentage points.

A second test is to restrict the sample by "health type" and see if the results of a work place ban still exist within groups. If sorting bias exists, then among unhealthy workers the effects of the ban should be small or zero, while under our behavioral story it will be larger than in the combined sample. To do this test we need an indicator for health type that is not influenced by the presence of the work place ban. Since smoking behavior is strongly correlated with other health habits, it seems plausible that individuals that *never* smoked would have different unobserved levels of the determinants of heath status (be healthier) than current or former smokers. Further, since almost all smokers start smoking before age 20, and most workers are over age 20, work area bans should not reduce the fraction of people who ever smoked.

In lines (3) of Table 11, we report the marginal effect on smoking prevalence when we restrict the NHIS sample to individuals that ever smoked, while in lines (4)-(5) analogous results are reported using the CPS data. In both cases we find that smoking bans reduce smoking amongst ever smokers. Further, for both the NHIS and CPS results the point estimate on the effect of the ban in the restricted sample is higher than for the full sample. Since the degree of sorting within ever smokers should have

[28]Because current smokers can never be classified as having never smoked, we cannot add the never smoked indicator to this model. To demonstrate this point, consider a simple probit model where $Y_i=1$ if a person currently smokes, and let $X_i=1$ if a worker never smoked be the only covariate. By construction, the probability that $Y_i=1$ is $\Phi(\alpha+\beta X_i)$. If there are N observations and if N_{yx} is the number of observation for a particular (y,x) pair, the log likelihood function is $L=N_{11}\ln[\Phi(\alpha+\beta)] + N_{10}\ln[\Phi(\alpha)] + N_{01}\ln[1-\Phi(\alpha+\beta)] + N_{00}\ln[1-\Phi(\alpha)]$. Without loss of generality, we can let $P_a=\Phi(\alpha+\beta)$ and $P_b=\Phi(\alpha)$, write the log likelihood function as $L=N_{11}\ln[P_a] + N_{10}\ln[P_b] + N_{01}\ln[1-P_a] + N_{00}\ln[1-P_b]$, and maximize the log likelihood function by choosing P_a and P_b. Because $N_{10}=0$ (given the way the variables are defined), the first-order conditions are $\partial L/\partial P_a = N_{11}/P_a - N_{01}/(1-P_a)$ and $\partial L/\partial P_b = -N_{00}/(1-P_b)$. For any value of P_b the gradient $\partial L/\partial P_b$ can never equal zero, and therefore, unique estimates of P_a and P_b, and hence, α and β cannot be identified.

been smaller than for the full sample, this is inconsistent with the hypothesis that our results are driven by sorting. It is consistent with a change in behavior resulting from the ban and suggests that our full sample single-equation estimates may even understate the impact on behavior of the groups whose smoking habits the bans could possibly affect (ever smokers). Interestingly, the marginal effect in the NHIS restricted sample (7.3 percent) is almost identical to the bivariate probit results in Table 10 (7. 4 percent). Finally, we can further restrict sorting possibilities by limiting the sample to non-movers (more than four years tenure) as well ever smokers. Again, as seen in line (6) of Table 11, smoking bans have a large and statistically significant affect on smoking prevalence and the point estimate is still slightly larger than the full sample single-equation results. These tests, when combined with 2SLS estimates, reinforces the conclusion that the presences of sorting or selection bias does not change the qualitative nature of the single-equation results on the effect of workplace smoking bans.

VI. Conclusions and Implications

In this paper we addressed several shortcomings in the existing work on the impact of workplace smoking policies on smoking. First, we use a large nationally representative data set of over 18,000 workers. More notably, all previous studies failed to control for the possibility the match of workers to firms may be a non-random process. We illustrate the potential for self-selection bias by demonstrating in single-equation models that restrictions on smoking in the workplace not only reduce smoking, but also appear to affect other health habits. These results suggested at least two possible sources of self-selection bias. The first is that non-smokers (smokers) may be attracted to firms with (without) workplace smoking bans. Second, firms that adopt such policies may adopt other programs to promote better health and that workplace smoking bans may reflect the effects of these other programs.

After establishing that single-equation estimates of workplace smoking bans reduce the prevalence of smoking by 5 percentage points and decrease cigarette demand by all workers by 1.6

cigarettes per day, we estimated simultaneous equation models that control for the potential for self-selection. To identify both the bivariate probit model of smoking prevalence and the two-stage least squares model of cigarette demand, we used the size of the worksite and the supply of non smoking jobs in an industry within a state as our instruments. The results of our simultaneous equation models show that smoking bans decrease the prevalence of smoking and decrease cigarette consumption. These results also indicate that once we controlled for potential biases, that the correlation between other health measures and smoking bans disappears. Thus, although workplaces with fifty or more employees are much more likely to adopt smoking bans, there is no spurious correlation between firm size and smoking.

We began this paper by noting that over the past 15 years, the smoking participation rate for workers has fallen faster than that for nonworkers. We suggested that one cause for the larger reduction in smoking among workers may have been the introduction of workplace smoking bans. The results from the previous sections suggest that the large scale adoption of workplace smoking bans may indeed be the cause for the differences in the times series for the two groups.

Using the NHIS data from figure 1, we calculate that between 1985 and 1993, the smoking participation rates for workers fell 2.6 percentage point more than the decline for nonworkers. Some of this decline could potentially be attributed to a change in the characteristics of workers relative to nonworkers. To capture this, we estimated a probit equation with data from the 1985 and 1993 NHIS where we model the probability an individual smokes. In this model, we included as covariates a quadratic term in age, log income, family size, plus indicators measuring race, sex, education, marital status, region of the country, and whether income was missing. To calculate the amount of the change in smoking rates of workers relative to nonworkers that is due to factors other than changes in demographic characteristics, we included three year/employment interactions: workers in 1993, nonworkers in 1993, and nonworkers in 1985, where the reference category being workers in 1985. The marginal effects for these three indicators are -0.048, -0.024, 0.007, and therefore the unexplained change (standard error) in smoking for workers relative to nonworkers between 1985 and 1993 is [-0.048 - (-0.024-0.007)]= -0.017 (0.010).

How much of this decline can be attributed to work area smoking bans? To answer this question, we need the change in exposure to work area smoking bans between 1993 and 1985. Unfortunately, there are no worker-based surveys from 1985 that have the required information. Instead, we use a 1985 survey of firms and measures of firm size to estimate the exposure of workers to work area smoking bans in that year.

In 1985, the Department of Health and Human Services sponsored the Worksite Health Promotion Survey. This was a survey of 1328 establishments designed to determine the types of health promotion activities sponsored by employers.[29] Establishments were asked a number of questions about what types of health programs provided by the firm, including whether they had any written policy restricting smoking in the work place. For the 372 firms that answered yes to this question, respondents recorded up to a 120 character description of the firm's policy. From these short descriptions, we classified firms as having work area smoking bans or not.[30] Using the sample weights and measures of establishment size, we calculate that about 38 percent of all workers worked in firms with a policy that restricts smoking, but only 25 percent worked in firms that banned smoking in work areas. The results from Table 2 indicate that by 1993, 70 percent of indoor workers worked in establishments with work area smoking bans, and also from he 1993 NHIS survey, we know that indoor and non-self employed workers represent two thirds of all workers. If work area smoking bans reduce smoking participation by 5.7 percentage points (Model III, Table 4), then these numbers suggest that between 1985 and 1993, bans should have reduced smoking participation rates among workers by (.70-.25)*(0.67)*(-5.7) = -1.7

[29]We wish to thank Don Kenkle for providing us with a copy of this data set.

[30]We classified a firm as having a work area smoking ban if: a) smoking was banned indoors, b) smoking was banned except for a designated area such as a lounge or the cafeteria.

percentage points. Therefore, all of the unexplained drop in smoking among workers can be explained by the rise in workplace smoking bans.

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Figure 1: Smoking Participation Rates
Workers and Nonworkers

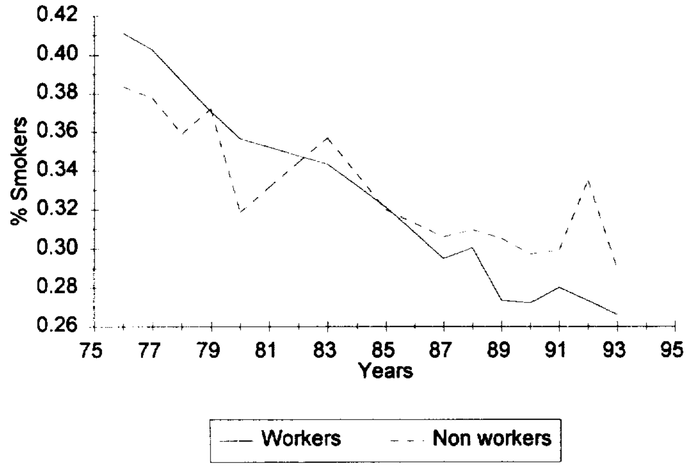

Figure 2: Smoking Participation Rates
Workers and Nonworkers: Males

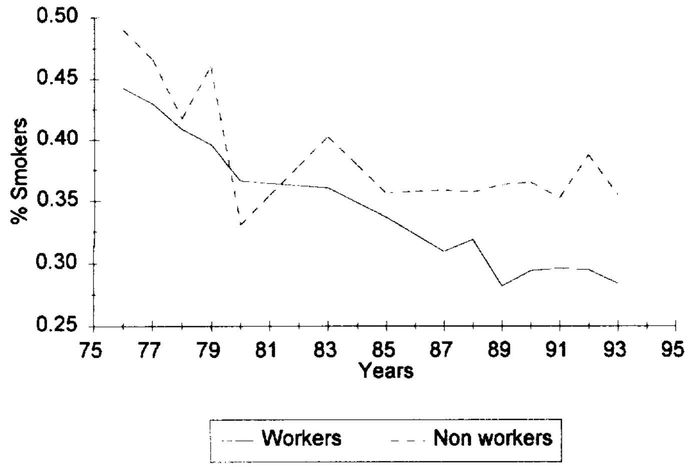

Figure 3: Smoking Participation Rates
Workers and Nonworkers: Females

Table 1
Sample Characteristics
1991 and 1993 NHIS

Variable	1991 and 1993 NHIS: Non self-employed and indoor workers	1991 and 1993 NHIS: All Workers	1991 and 1993 CPS/OR Sample
Age	37.8	38.8	38.3
Years of education	13.8	13.3	13.1
% Male	44.1%	55.0%	54.4%
% Black	10.5%	10.5%	10.2%
% Hispanic	6.7%	7.7%	7.7%
% in industry:			
Professional and related services	27.9%	23.0%	23.2%
Manufacturing	20.3%	18.3%	17.2%
Retail trade	13.3%	15.7%	16.0%
Finance, insurance, real estate	12.1%	6.7%	6.8%
Construction	2.6%	6.0%	6.1%
Other	23.8%	30.3%	30.7%
% in occupation:			
Administrative support	25.5%	15.1%	15.8%
Professional specialty occupations	18.0%	14.6%	14.2%
Executive, administrative, etc.	19.2%	14.6%	13.0%
Sales occupations	9.4%	11.3%	11.7%
Precision production, etc.	5.4%	10.9%	11.4%
Service occupations	7.4%	10.5%	10.8%
Other	15.1%	23.0%	23.1%
Number of observations	18,090	37,982	397,726

Sample weights used in all calculations.

Table 2
Type and Frequency of Workplace Smoking Restrictions
1991 and 1993 NHIS,
by Type of Workplace

Percent Answering Yes

	Any workplace smoking restriction		Indoor public area smoking ban		Work area smoking ban	
Type of workplace	1991	1993	1991	1993	1991	1993
Indoor workers						
all establishments	76.9	80.1	37.7	51.3	58.8	70.1
< 50 employees	57.3	62.1	36.1	44.5	44.1	55.5
50 + employees	84.9	87.5	38.4	54.1	65.0	76.8
Major Industries						
Manufacturing	81.7	83.1	31.2	43.6	57.5	70.4
Finance, Insurance, and Real Estate	80.1	82.8	45.4	63.3	66.3	76.3
Professional and Related Services	79.9	88.0	51.1	72.1	68.2	82.8
All Other Industries	70.9	73.5	30.9	42.9	51.2	63.3
Major Occupation						
Executive, Administrative and Managerial	77.1	82.6	37.8	58.2	57.2	71.3
Professional Specialty	84.4	89.2	44.5	72.0	65.7	83.8
Administrative Support	74.8	81.1	39.4	56.2	60.4	72.8
All Other Occupations	74.9	76.9	32.6	42.9	55.1	66.5
Government Workplaces	89.6	94.8	45.6	74.6	73.3	88.2

Table 3
Measures of Smoking,
By Workplace Smoking Policy
1991 and 1993 NHIS

	1991	1993	Pooled
Current smoker			
Work area smoking ban	20.2%	22.7%	21.9%
No ban	28.3%	32.0%	30.5%
Cigarettes per day (smokers only)			
Work area smoking ban	17.4	17.8	17.7
No ban	21.2	20.6	20.8
Cigarettes per day (all workers)			
Work area smoking ban	2.9	3.3	3.2
No ban	5.2	5.9	5.6

Table 4
Normalized Probit and OLS Estimates,
Coefficient on Work Area Smoking Ban ,
Current Smoker and Cigarettes per day Equations,
(t-statistics in parentheses)

A. 1991 and 1993 NHIS

Dependent Variable	Sample Mean	No. of Obs.	Model I	Model II	Model III	Model IV
Current smoker	0.242	18,090	-0.066	-0.059	-0.057	-0.055
			(-9.42)	(-8.25)	(-7.93)	(-7.69)
Cigarettes per day (smokers only)	19.0	3,678	-2.51	-2.53	-2.48	-2.40
			(-6.17)	(-7.69)	(-7.50)	(-7.20)
Cigarettes per day (all workers)	3.9	17,209	-1.86	-1.72	-1.71	-1.67
			(-13.0)	(-12.1)	(-11.8)	(-11.5)

B: 1992 and 1993 CPS

Dependent Variable	Sample Mean	No. of Obs.	Model I	Model II	Model III	Model IV
Current smoker	0.247	63,997	-0.046	-0.039	-0.039	-0.037
			(-12.4)	(-10.4)	(-9.85)	(-9.71)
Cigarettes per day (smokers only)	19.4	15,819	-2.11	-1.97	-1.90	-1.85
			(-12.8)	(-11.7)	(-11.2)	(-10.9)
Cigarettes per day (all workers)	3.9	63,997	-1.30	-1.17	-1.13	-1.11
			(-18.0)	(-15.9)	(-15.2)	(-15.0)

Coefficients in the smoking prevalence model are normalized probit coefficients that represent the change in the probability of smoking given a change in the workplace policy. Normalized probit estimates are calculated for the j'th variable as $\beta_j\phi(z)$, where $z=\Phi^{-1}(p)$, p is the sample mean of the response variable, and β_j is the probit coefficient for the variable.

Variables in model in addition to the type of smoking policy shown above:

I: Age, age2, family size, state cigarette tax, log income, an indicator variable for income missing, three indicator variables for region, four indicator variables for education, three indicator variables for ethnicity, two indicator variables for type of msa, four indicator variables for marital status, and a year effect.

II: Model I plus major industry effects

III: Model II plus major occupation effects

IV: Model III plus state effects.

Table 5
Normalized Probit and OLS Estimates,
Coefficient on Work Area Smoking Ban,
Smoking Prevalence and Cigarettes per Day Equations
1992-1993 CPS
(t-statistics in parenthesis)

Independent Variables	Current Smoker	Cigarettes per day (smokers only) OLS
Work area smoking ban x worked <20 hours per week	-0.016 (-1.12)	-0.74 (-1.05)
Work area smoking ban x worked 20-29 hours per week	-0.024 (-2.52)	-1.08 (-2.03)
Work area smoking ban x worked 30-39 hours per week	-0.038 (-5.54)	-1.73 (-5.62)
Work area smoking ban x worked 40-49 hours per week	-0.038 (-6.57)	-2.14 (-8.50)
Work area smoking ban x worked ≥50 hours per week	-0.055 (-5.69)	-2.37 (-5.57)

Other covariates include those listed in Model III, Table 4, plus 4 dummy variables for hours worked per week. See footnotes in Table 4 for calculation of the marginal effect in the current cmoker probit model. The fraction of the sample for each of the hours groups are: ≤20 hours per week (6.7%), 20-29 hours per week (10.0%), 30-39 hours per week (28.0%), 40-49 hours per week (41.0%), ≥50 hours per week (14.3%).

Table 6
Normalized Probit Estimates,
Current Smoker Model,
1991 NHIS
(t-statistics in parenthesis)

	Current smoker		Cigarettes per day (smokers only)	
Independent Variable	Model (1)	Model (2)	Model (1)	Model (2)
Work area smoking ban	-0.065 (-6.81)		-2.81 (6.08)	
≤ 6 months of tenure x work area smoking ban		-0.034 (-1.20)		-1.56 (-1.16)
6-12 months of tenure x work area smoking ban		-0.078 (-2.70)		-2.17 (-1.63)
12-24 months of tenure x work area smoking ban		-0.051 (-2.01)		-3.31 (-2.72)
25-48 months of tenuse x work area smoking ban		-0.110 (-4.68)		-3.87 (-3.42)
> 48 months tenure x work area smoking ban		-0.059 (-4.28)		-2.81 (-4.21)

The fraction of the sample that has≤6, 6-12, 13-24, 25-48, and >48 months of tenure are 11.4 percent, 10.5 percent, 13.3 percent, 16.3 percent, and 48.4 percent respectively.

Table 7
Normalized Probit Estimates,
Other Health Habit Models
1991 and 1993 NHIS
(t-statistics in parentheses)

Dependent variable (1=yes, 0=no)	Data set	Sample mean	Work area smoking ban
Never Smoked cigarettes?	1991/3 NHIS	0.539	0.049 (5.68)
Always add salt to food?	1991/3 NHIS	0.128	-0.014 (-2.53)
Always wear seat belt?	1991/3 NHIS	0.793	0.040 (5.91)
Excellent or very good health?	1991/3 NHIS	0.733	-0.004 (-0.48)
20 % or more overweight?	1991 NHIS	0.262	-0.001 (-0.13)
Smoke detector in home?	1993 NHIS	0.981	0.026 (3.66)

Other covariates include those listed for Model III in Table 4. See footnotes from Table 4 for the calculation of the marginal effects.

Table 8
Normalized Probit Coefficients
Work Area Smoking Ban Models
1991 and 1993 NHIS
(t-statistics in parentheses)

Independent variable	1991/3 NHIS	1991/3 NHIS	1991/3 NHIS	1991 NHIS	1991 NHIS	1991 NHIS
Establishment has 50 or more employees	0.221 (25.9)		0.223 (26.0)	0.225 (18.6)		0.224 (18.2)
State/industry mean of work area smoking ban		0.406 (7.98)	0.394 (7.63)			
Has private insurance					0.055 (3.06)	0.019 (1.00)

Other covariates include those listed for Model III in Table 4. See footnotes from Table 4 for the calculation of the marginal effects.

Table 9
Normalized Probit Estimates,
Other Health Habit Models
1991 and 1993 NHIS
(t-statistics in parentheses)

Dependent variable (1=yes, 0=no)	Data set	Model (1) 50 or more employees at establishment	Model (2) State/industry mean of work area smoking ban
Never Smoked cigarettes?	1991/3 NHIS	-0.004 (-0.42)	0.012 (0.22)
Always add salt to food?	1991/3 NHIS	0.004 (0.067)	0.061 (1.70)
Always wear seat belt?	1991/3 NHIS	0.008 (1.04)	-0.011 (-0.26)
Excellent or very good health?	1991/3 NHIS	-0.012 (-1.45)	0.112 (2.32)
20 % or more overweight?	1991 NHIS	0.032 (3.84)	-0.018 (-0.38)
Smoke detector in home?	1993 NHIS	-0.001 (-0.29)	-0.102 (-2.20)

The work area smoking ban variable is NOT included in these models. Other covariates include those listed for Model III in Table 4. See footnotes from Table 4 for the calculation of the marginal effects.

Table 10
Bivariate Probit, 2SLS, and LIML Estimates,
Health Habit Models
1991 and 1993 NHIS
(t-statistics in parentheses)

Dependent Variable	Model	Est. Method	Instruments	Coefficient/ Average Treatment Effect on Work Area Smoking Ban	p-value, test of overid. restrictions (d.o.f)
Current smoker	(1)	Probit	---------	-0.054 (-7.71)	
	(2)	Bivariate Probit	Size 50	-0.074 (-2.39)	
	(3)	2SLS	Size 50	-0.063 (-1.82)	
	(4)	2SLS	Size 50 x major industry and major occupation effects	-0.065 (-2.03)	0.15 (24)
	(5)	LIML	Size 50 x major industry and major occupation effects	-0.065 (-2.00)	
	(6)	2SLS	State/industry mean of work area smoking ban	-0.053 (-0.48)	
	(7)	2SLS	State/industry mean of work area smoking ban, Size 50	-0.062 (-1.87)	0.93 (1)
Cigarettes per day, all workers	(1a)	OLS	---------	-1.71 (-11.8)	
	(3)	2SLS	Size 50	-1.62 (-2.24)	
	(4)	2SLS	Size 50 x major industry and major occupation effects	-1.61 (-2.39)	0.58 (24)
	(5)	LIML	Size 50 x major industry and major occupation effects	-1.60 (-2.35)	
	(7)	2SLS	State/industry mean of work area smoking ban, Size 50	-1.88 (-2.74)	0.93 (1)
Never smoked	(1)	Probit	---------	0.045 (5.68)	
	(2)	Bivariate Probit	Size 50	0.020 (0.59)	
	(2)	2SLS	Size 50	-0.017 (-0.43)	
	(4)	2SLS	Size 50 x major industry and major occupation effects	-0.023 (-0.63)	0.02 (24)

Table 10
(continued)

Dependent Variable	Model	Est. Method	Instruments	Coefficient/ Average Treatment Effect on Work Area Smoking Ban	p-value, test of overid. restrictions (d.o.f)
Always add salt to food	(1)	Probit	---------	-0.014 (-2.55)	
	(2)	Bivarate Probit	Size 50	0.011 (0.50)	
	(3)	2SLS	Size 50	0.014 (0.51)	
	(4)	2SLS	Size 50 x major industry and major occupation effects	0.015 (0.59)	0.18 (24)
Always wear seat belt	(1)	Probit	---------	0.038 (5.79)	
	(2)	Bivariate Probit		0.024 (0.80)	
	(3)	2SLS	Size 50	0.016 (0.48)	
	(4)	2SLS	Size 50 x major industry and major occupation effects	0.026 (0.85)	0.04 (24)

Other covariates include those listed in Model III, Table 4.

Table 11
Normalized Probit Coefficients,
Work Area Smoking Ban Coefficient,
Current Smoker Equations.
1991 and 1993 NHIS,
September 1992 and May 1993 CPS

Model	Data Set	Sample	Additional covariates	Marginal effect, Work area smoking ban
(1)	1991/3 NHIS	full sample		-0.057 (-7.93)
(2)	1991/3 NHIS	full sample	indicators for salt use, belt use, and excellent/very good health status	-0.053 (-7.23)
(3)	1991/3 NHIS	ever smoked		-0.073 (-5.84)
(4)	1992/3 CPS	full sample		-0.039 (-9.85)
(5)	1992/3 CPS	ever smoked		-0.046 (-5.84)
(6)	1991 NHIS	ever smoked, ≥ 4 years of tenure		-0.058 (-2.66)

Other covariates include those listed in Model III, Table 4. See the footnotes in Table 4 for the calculation of the marginal effects.