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Do Workplace Smoking Bans Reduce Smoking?

By WILLIAM N. EVANS, MATTHEW C. FARRELLY, AND EDWARD MONTGOMERY*

In recent years workplace smoking policies have become increasingly prevalent and restrictive. Using data from two large-scale national surveys, we investigate whether these policies reduce smoking. Our estimates suggest that workplace bans reduce smoking prevalence by 5 percentage points and daily consumption among smokers by 10 percent. Although workers with better health habits are more likely to work at firms with smoking bans, estimates from systems of equations indicate that these results are not subject to an omitted variables bias. The rapid increase in bans can explain all of the recent drop in smoking among workers relative to nonworkers. (JEL J28, I18)

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 1986 Surgeon General's report and the 1986 National Academy of Science/National Research Council's task force report on passive smoke, which linked ETS to higher rates of cancer and heart disease in nonsmokers. After surveying the scientific data on the health effects of passive smoke, the U.S. Environmental Protection Agency (1992) declared ETS a Class A carcinogen.

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.¹ Simulta-

neously, many firms began voluntarily adopting workplace smoking restrictions. The impact of these factors on exposure to workplace smoking bans has been dramatic. As we demonstrate below, only 25 percent of workers in 1985 worked in establishments that banned smoking in work areas. By 1993, this number increased to 70 percent. The public response to smoking in the workplace reached its apex in March 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 six million workplaces.² A strict public area smoking ban was also a component of the global tobacco settlement reached between state attorney generals and tobacco manufacturers in July of 1997. This policy would have restricted indoor smoking in public facilities (i.e., any building regularly entered by ten or more individuals at least one day per week) to ventilated areas. The final tobacco agreement signed by state attorney generals in December of 1998 however did not contain these provisions.

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¹ Twenty-three states had at least moderate restrictions on smoking in public places in 1992, but this number jumped to 33 by 1994 (Coalition on Smoking OR Health).

Farrelly and Evans (1996) note that while only 14 percent of private-sector employees were covered by state laws restricting workplace smoking in 1985, 43 percent were covered by 1993.

² The proposed indoor air quality initiative was one of the most 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.

In addition to reducing exposure to ETS, smoking restrictions may affect smoking behavior by reducing opportunities to smoke. Of all the various policies, workplace smoking restrictions are likely to have the greatest impact on both ETS and smoking habits because of the number of hours that workers are subject to these restrictions. The potential importance of workplace smoking policies' effect on smoking habits can be seen by examining the time-series patterns of smoking prevalence for workers and nonworkers. In Figure 1, we graph the prevalence of smoking for workers and nonworkers, aged 18–65, for the period 1978 through 1993. These numbers were generated from the National Health Interview Survey (NHIS) supplements with smoking questions for the years 1978–1980, 1983, 1985, 1987–1993.³ These data indicate that between 1978 and 1985, there was no systematic difference between smoking rates among workers and nonworkers.

Between 1986 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 nonworkers.⁴ This change does not appear to simply reflect changes in demographic characteristics or relative healthiness of workers compared to nonworkers.⁵ Nor is the change due to changing

³ The numbers for nonsurvey years are based on linear interpolations between survey years.

⁴ The large spike in smoking rates for nonworkers in 1992 is an artifact of that year's survey. Respondents to the NHIS are surveyed throughout the year. In 1992, the supplements that contained smoking questions (the Cancer Control and Cancer Epidemiology Supplements) were pulled from the field at the beginning of the third quarter. Subsequently, smoking data are not available for the final half of the year. Interestingly, smoking rates among nonworkers show an incredible seasonal pattern. Using data from the 1991 NHIS, quarterly smoking rates for nonworkers are 0.3316, 0.3042, 0.2837, and 0.2796. Quarterly rates for workers show no such pattern: 0.2845, 0.2789, 0.2835, 0.2729.

⁵ For example, 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 is 0.002 (0.011) for very good/excellent health status and 0.004 (0.010) for belt use.

characteristics of workers and nonworkers. We estimate that only 0.9 percentage points of the drop can be explained by changing demographics, leaving a 1.7-percentage-point drop unexplained.⁶ These figures suggest that there has been some external factor that has been differentially affecting the smoking habits of workers since 1985. One possible explanation for this phenomenon is that the rise in workplace smoking bans over this time period has reduced workers' demand for cigarettes.

The existing literature that has examined this issue has produced conflicting results. Further, these studies have generally been limited to a small number of firms and workers and suffer from other methodological problems that are discussed below. This paper advances the literature in a number of important ways. First we use a nationally representative data set to examine the impact of workplace smoking restrictions on smoking behavior. We use data from the 1991 and 1993 NHIS that provide a much larger and richer cross section of observations on the relationship between firm smoking policies and smoking outcomes than in previous studies. Pooling data across both surveys generates a sample of 18,090 workers. Single-equation results (where workplace smoking bans are assumed to be exogenous) suggest that workplace bans lead to a 5.7-percentage-point decline in smoking prevalence and a decrease in average cigarette consumption by smokers of 2.3 cigarettes per day (about 10 percent). These basic results are replicated using data from over 97,000 indoor workers who responded to the smoking supplements from the September

⁶ We estimated a probit model with data from the 1985 and 1993 NHIS where we modeled the probability an individual smokes. In this model, we included as covariates a quadratic term in age, log income, and 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, with the reference category being workers in 1985. The marginal effects for these three indicators are -0.048, -0.024, and 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).

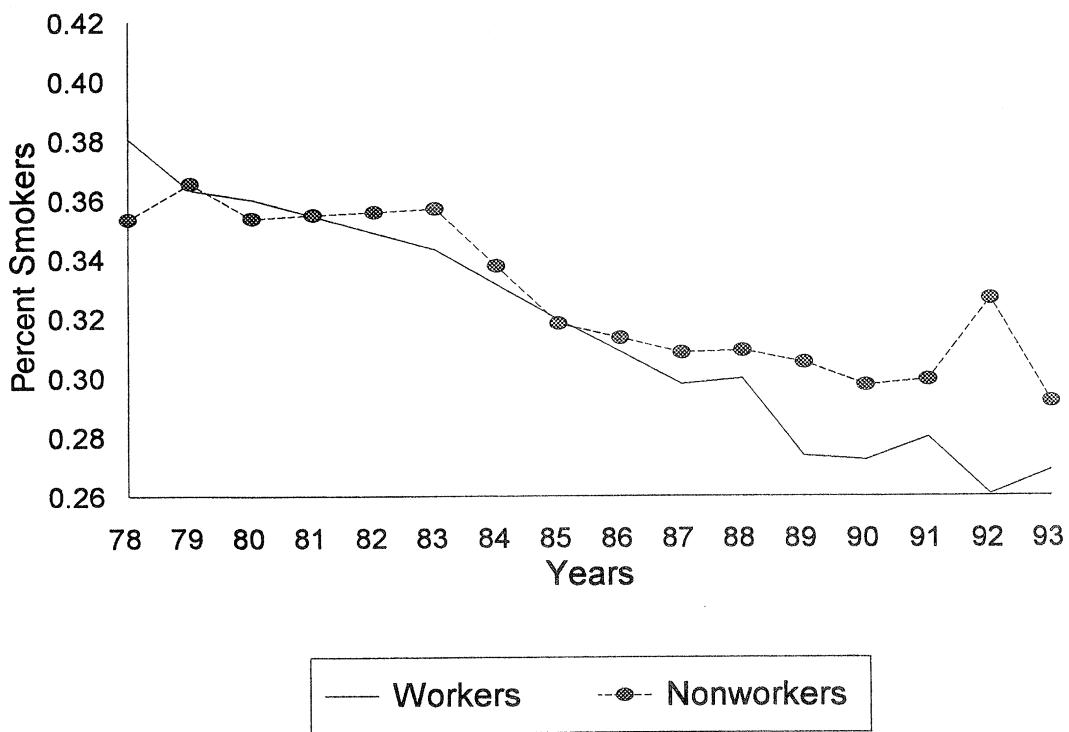


FIGURE 1. SMOKING PARTICIPATION RATES
WORKERS AND NONWORKERS

1992, January 1993, and May 1993 Current Population Survey (CPS).

Second, we address the issue that these single-equation estimates may be subject to an omitted variables bias. Using three different approaches we find that the basic results can be interpreted as having a causal interpretation. First, even when we control for a number of variables that may signal the underlying healthiness of the individual or firm, the estimated impact does not change. Second, we show that the impact of workplace smoking bans is greatest for workers with the longest work weeks. Since the cost of the ban to smokers is proportional to the hours exposed to the restrictions, this result is consistent with a causal interpretation. Third, we use two-stage least squares (2SLS) to control for unobserved differences between workers and nonworkers. We use a measure of establishment size as our instrument for the workplace smoking policy. Larger firms are more likely to ban smoking in work areas, but they are not more likely to attract healthier

workers. Once we control for omitted variables bias, we find that a complete smoking ban in all work areas has a slightly larger impact on the prevalence of smoking than models in which we treat bans as exogenous. Overall, the results suggest that omitted variables bias does not dramatically alter the conclusion from the single-equation estimation. Finally, we show in the conclusion that the estimated results suggest that the rise in workplace smoking bans can explain all of the unexplained drop in smoking among workers relative to nonworkers since 1985.

Taken in total, these estimates suggest that workplace smoking bans are an effective way to reduce smoking among adults. We should note, however, that these estimates provide but one piece of evidence needed to evaluate the net benefits of work area smoking bans. We have not analyzed any costs associated with these plans (e.g., such as higher worker turnover, lost work due to increased smoking breaks, etc.), nor have we addressed the more difficult ques-

tion as to whether or not the federal government should adopt the proposed ban on workplace smoking.

The remainder of this paper is structured as follows. In Section I, we discuss the results and limitations of previous studies that have examined this question. Section II contains a description of the data sources 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 III, we present the basic results from single-equation models of the impacts of smoking restrictions on smoking prevalence and consumption. In Section IV, we use a variety of techniques to decipher whether the single-equation estimates represent a causal impact or whether they simply represent an omitted variables bias. In Section V, we make some concluding remarks and discuss the implications of our results.

I. Previous Literature

As the number of workplaces subject to smoking restrictions has grown, so too has the number of studies that have examined the impact of these policies on cigarette use. Several studies find support for a correlation between workplace smoking restrictions or bans and decreased smoking (Lyle R. Petersen et al., 1988; Frances A. Stillman et al., 1990; Walter F. Baile et al., 1991; Glorian Sorensen et al., 1991; Gregg M. Stave and George W. Jackson, 1991; Susan Kinne et al., 1993; Tracey J. Woodruff et al., 1993; Daniel L. Longo et al., 1996), while others do not (Lois Biener et al., 1989; Nell H. Gottlieb et al., 1990). The fact that there are conflicting results is not surprising given the variety of samples, statistical methods, and types of policies and firms investigated by these authors. The bulk of these studies are limited in their ability to determine whether smoking bans reduce consumption because as Longo et al. (1996 p. 1253) point out, most of these studies have "... generally lacked control groups and investigated restrictions in only one location over relatively short periods ... and the majority of workplace smoking studies examine hospital employees." One of the more detailed efforts is that of Longo et al. (1996) who surveyed hospital workers before and after the implementa-

tion of workplace bans and compared the changes in smoking quit rates among employees with changes in rates for workers in establishments that did not ban workplace smoking. They found that five years after the adoption of the workplace ban, the smoking quit rate was 50.6 percent compared to only a 37.7 percent for workers in establishments without bans.

Even the most sophisticated of these studies share one common methodological problem. As Woodruff et al. (1993 p. 1491) noted, there is a potential for "self-selection bias (e.g., non smokers find work in smoke-free workplaces)." Differences in the prevalence of smoking across workplaces may be due to factors other than smoking policies. If the match of smokers to firms is not random, the estimated impact of bans on smoking prevalence may be biased.

This bias can be generated through a number of different avenues. First, nonsmokers (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. In this case, the single-equation estimates would again overstate the impact of the ban. Third, firms with a high fraction of nonsmokers may have successfully promoted the adoption of workplace smoking bans. 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 may underestimate the benefits of the restrictions.

Longitudinal studies that analyze smoking prevalence in establishments before and after the imposition of bans mitigate some, but not all, of these problems. For example, longitudinal comparisons of smoking behavior in firms with and without bans are not subject to the criticism that firms with a lower-than-average prevalence of smoking may be more likely to ban smoking. In these studies, the results

are driven by comparisons of within-work site changes over time in the prevalence of smoking rather than cross-sectional differences. This type of analysis helps control for fixed unobserved heterogeneity across firms or establishments. However, the results of these longitudinal studies could still be suspect if bans change the types of workers that are attracted to a firm or encourage smokers to leave a firm in higher numbers than nonsmokers. Consider the following example. Suppose a workplace smoking ban is instituted in a firm where 25 percent of the workers smoke. Suppose this change has no impact on smoking among those workers that remain with the firm, but the policy reduces the fraction of new hires that are smokers. If 25 percent of all jobs turn over in the establishment in a given year (a number consistent with results from the manufacturing sector [Steven J. Davis and John C. Haltiwanger, 1992]), the fraction of quitters that smoke is also 25 percent, but because of the policy, the fraction of new hires that smoke is only 15 percent, then the smoking rate will fall to 22.5 after one year. In this case, a longitudinal analysis will suggest that bans reduced smoking but, in fact, the ban just changed the composition of workers.

II. Data and Descriptive Statistics

The primary 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 about smoking and other health habits. From these questions, we construct an indicator for whether a person is a current smoker and measures of average daily cigarette consumption.

Both the 1991 and 1993 NHIS asked workers detailed questions about workplace smoking policies. First, workers were asked to describe their type of work area (e.g., enclosed office, open area, outside). Next, 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. These workers were then asked whether their firm had a formal policy to restrict smoking and whether smoking is banned in some or all indoor public areas and in some or all work areas.

It should be noted that the initial questions about work areas 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 had the effect of lowering the fraction of workers from the 1991 survey that were included in the final sample. Of the 25,591 respondents in the 1991 survey who were employed, 3,154 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 whom 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.

In Table 1, we report and compare descriptive characteristics for three samples of workers: the pooled 1991 and 1993 NHIS sample of indoor workers; all workers in the 1991 and 1993 NHIS sample; and all workers in the 1991 and 1993 Outgoing Rotation Samples from the Current Population Surveys (CPS/OR). 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 all-worker samples. While at first blush the low fraction of male workers in the restricted sample may be striking, this results from the fact that 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 workforce. The sample selection criteria also increases the fraction of workers in occupations such as administrative support, professional specialty occupations, and executives and administrators.

Table 2 presents summary statistics that de-

TABLE 1—SAMPLE CHARACTERISTICS
1991 AND 1993 NHIS

Variable	1991 and 1993 NHIS		
	Nonself-employed and indoor workers	All workers	1991 and 1993 CPS/OR sample
Mean age	37.8	38.8	38.3
Mean years of education	13.8	13.3	13.1
Percent male	44.1	55.0	54.4
Percent black	10.5	10.5	10.2
Percent Hispanic	6.7	7.7	7.7
Percent 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
Percent 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

Note: Sample weights used in all calculations.

scribe the type and frequency of workplace restrictions as reported by workers from the two NHIS surveys. The results for the combined 1991/1993 data set show that over 80 percent of workers are subject to some type of workplace policy that restricts smoking, while 67 percent are subject to a work area smoking ban. In 1991, 79.1 percent of all workers are employed in workplaces that have some type of formal policy to restrict smoking and 61.7 percent are employed in firms that ban smoking in all work areas. By 1993, the fraction of workers subject to work area smoking bans jumped to 73.4 percent. There is also a clear difference in the frequency of these policies in large and small establishments. For example, in the combined 1991/1993 sample, 73.0 percent of employees at establishments with 50 or more employees are employed by firms that ban smoking in all work areas, while only 50.3 percent of employees at smaller work sites face such a ban.

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 so it should have the greatest impact on smoking behavior. Second, public area bans appear to be nearly a proper subset of work area smoking bans. We found that 96 percent of all firms banning smoking in public areas also banned smoking in work areas. The converse, however, was not true. Finally, the category for the existence of any policy at all is too vague to allow one to be certain what is being measured.

III. Basic Results

In our work, we use three measures of smoking. The first is a simple indicator that equals 1 if a worker is a current smoker. 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 zero for nonsmokers.⁷ Sample

⁷ 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.

TABLE 2—TYPE AND FREQUENCY OF WORKPLACE SMOKING RESTRICTIONS
1991 AND 1993 NHIS, BY TYPE OF WORKPLACE
PERCENT ANSWERING YES

Type of workplace	Any workplace smoking restriction	Indoor public area smoking ban	Work area smoking ban
All workers	80.7	46.7	67.3
By year:			
1991	79.1	39.3	61.7
1993	82.5	55.4	73.4
By establishment size:			
<50 employees	60.2	40.7	50.3
50+ employees	87.6	48.8	73.0

means for these three variable are presented in column (2) of the top panel of Table 3. In columns (3) and (4), we present mean values of these three variables for workers who are and are not exposed to work area smoking bans, while in column (5), we report the differences in means. These statistics show that workers are 8 percentage points less likely to smoke in firms with a smoking ban than those in firms with no ban. The results for cigarettes consumed per day shows a similar pattern, with a drop in the average number of cigarettes smoked per day of a little more than three cigarettes for smokers and 2.3 cigarettes for the entire sample.

In the final column of the table, we estimate multivariate models that control for an extensive set of covariates. We estimate models for the current smoker variable by a probit model and the cigarettes per day equations by OLS. The covariates we add to these models are age and its square, family size, log income, an indicator variable for income missing,⁸ 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 white or 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),⁹ two-digit industry and occupation effects, and a year effect.

The coefficients on the workplace smoking ban variable in the probit model are normalized estimates that measure the "marginal effect" or the change in the probability that an individual smokes given the adoption of a workplace smoking ban.¹⁰ These results indicate that workplace smoking bans reduce smoking participation rates by 5.7 percentage points. To put this result in perspective, consider the fact that the overall national smoking participation rate fell by 5 percentage points from 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

⁸ For those respondents who did not report income, we set log income to zero and created a dummy variable that equals one when income is missing and zero otherwise.

⁹ It should be noted that in preparing these data for public release, the National Center for Health Statistics (NCHS) renumbered 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 the individual's PSU. For a small number of observations, we were unable to identify states of residence for reasons of confidentiality, so these observations were deleted from the sample. The data on taxes are published yearly by the Tobacco Institute (1995) in its publication *The Tax Burden on Tobacco: Historical Compilation 1994*.

¹⁰ The marginal effect for the j th variable is calculated as $\hat{\beta}_j \phi(z)$, where $z = \Phi^{-1}(p)$ and p is the sample mean of the response variable (i.e., indicator variable for smoker), $\hat{\beta}_j$ 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.

TABLE 3—IMPACT OF WORKPLACE SMOKING BANS ON SMOKING
DIFFERENCE IN MEANS AND MULTIVARIATE ANALYSIS
(STANDARD ERRORS IN PARENTHESES)

A. 1991 and 1993 NHIS						
Dependent variable	Observations (1)	Means				
		Full sample (2)	With work area smoking ban (3)	Without work area smoking ban (4)	Difference in means (3) – (4) = (5)	Normalized probit or OLS estimate (6)
Current smoker	18,090	0.242	0.215	0.297	–0.082 (0.007)	–0.057 (0.007)
Cigarettes per day (smokers only)	3,679	19.0	17.6	20.9	–3.3 (0.33)	–2.5 (0.33)
Cigarettes per day (all workers)	17,209	3.9	3.1	5.4	–2.3 (0.14)	–1.7 (0.14)

B. September 1992, January 1993, and May 1993 CPS						
Dependent variable	Observations (1)	Means				
		Full sample (2)	With work area smoking ban (3)	Without work area smoking ban (4)	Difference in means (3) – (4) = (5)	Normalized probit or OLS estimate (6)
Current smoker	97,882	0.249	0.219	0.313	–0.094 (0.003)	–0.048 (0.003)
Cigarettes per day (smokers only)	19,956	19.2	18.0	20.9	–2.9 (0.14)	–2.0 (0.14)
Cigarettes per day (all workers)	93,367	4.1	3.3	5.8	–2.5 (0.06)	–1.4 (0.06)

Notes: 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: age, age squared, 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 metropolitan area, four indicator variables for marital status, year effects, major industry effects, and major occupation effects.

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 (Eugene M. Lewit et al., 1981; Michael Grossman et al., 1993; Evans and Farrelly, 1998). If we use a demand elasticity of –0.4, which is consistent with previous studies [see W. Kip Viscusi (1992) for a review], 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 rate.

In row 2 of column (6) of Table 3, we present the coefficient on the workplace ban variable from OLS regressions where the dependent

variable is cigarettes/day for smokers only. The results for this model indicate that workplace restrictions reduce smoking intensity by two and a half cigarettes per day, which is slightly more than 10 percent of daily consumption.

Because workplace bans have such a large impact on the probability of smoking, estimates from the cigarettes-per-day equation for smokers must be interpreted with caution. If low-consuming smokers are the first to quit when bans are imposed, then the composition of the remaining smokers may be different than before the ban. To capture changes in the demand for cigarettes on the intensive and extensive margins, we estimated a model of daily consumption setting cigarette consumption for non-smokers to zero. These results, presented in row 3 of Table 3, suggest that workplace bans have a statistically significant negative impact on

smoking consumption. Overall cigarette demand decreases by 1.7 cigarettes per day per worker as a result of the workplace smoking ban.¹¹ Given the problems of interpreting the results from the smoking intensity equation for smokers only, we focus primarily on the current smoker and cigarettes/day for all workers equations for the remainder of the paper.

To check that these estimates are not a product of having used the NHIS, we replicated the basic results using data from special smoking supplements to the 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, January 1993, and May 1993, the CPS included a lengthy survey about smoking habits, workplace smoking restrictions, and attitudes toward 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 worked indoors or who were 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 three CPS surveys, there are 97,882 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 (Table 2). The CPS indicates that the fraction of workers subject to work area smoking bans increased from 64.2 percent in September 1992 to 66.9 percent in January 1993 and to 69.7 percent in May 1993. The number for January is roughly the midpoint between the numbers reported for the 1991 and 1993 NHIS data reported in Table 2, while the number from the May CPS is slightly lower than the estimates from the 1993 NHIS. As the means in column

(2) of Table 3 indicate, the smoking participation rates in the CPS data are nearly identical to numbers reported in the NHIS.

In column (6) in the lower panel of Table 3, we present multivariate results from the pooled CPS data. The covariates have been defined in a similar fashion to these from 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 15 to 20 percent smaller in the CPS data set than in the NHIS. In the CPS data, smoking participation is 4.8 percentage points lower among workers exposed to work area smoking bans. The coefficients on the work area smoking ban variables in the smoking intensity models from the CPS samples are also similar to the results from the NHIS. Overall, these single-equation estimates again suggest that smoking is much lower in establishments with workplace smoking bans.

IV. Correlation or Causation?

The results in the previous section suggest, but do not prove, that there is a causal relationship between workplace smoking bans and reduced smoking. Although the single-equation models seem to imply that smoking bans significantly reduce smoking, all of these models assumed the match of a worker to a firm with smoking restrictions is exogenous. As we noted above, nearly all previous studies on this topic make this assumption. 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 subject to an omitted variables bias.

Is there reason to be concerned that such types of biases exist? 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 5.7 percentage points when we control for a long list of covariates. Since the work area smoking ban indicator is correlated with observed characteristics, it seems possible that it is also correlated with the unobserved propensity for a worker to smoke.

To see if omitted variable bias is a particular concern in this context we replace our current smoking indicator with other health habits re-

¹¹ The magnitude of this coefficient is simply a linear combination of the previous estimates. The average smoker smokes 20 cigarettes (one pack) a day, so a 5.7-percentage-point drop in smoking participation and a 2.40-per-day cigarette decline for the remaining 21.5 percent of smokers in establishments with bans would translate into a $(0.057)*20 + (0.215*2.5) = 1.68$ -cigarette per-day decline for the entire sample.

ported in the NHIS that should not directly be altered by workplace smoking bans such as whether respondent: never smoked cigarettes;¹² always add salt to their food; always wear a seat belt; report excellent or very good health; are 20 percent overweight; and have a smoke detector in their home.¹³ Since all outcomes are discrete, we estimate probit models that include the same set of covariates used in Table 3. The probit marginal effects, (standard errors) [sample means] for each variable are as follows: never smoked cigarettes, 0.049 (0.009) [0.539]; always add salt to food, -0.014 (0.006) [0.128]; always wear seat belt, 0.040 (0.007) [0.793]; in excellent or very good health, -0.004 (0.008) [0.733]; 20 percent or more overweight, -0.001 (0.008) [0.262], smoke detector in home, 0.026 (0.007) [0.981]. These results suggest that workers in establishments with smoking bans are less likely to have ever smoked, less likely to salt their food, more likely to wear their seat belt, and are more likely to have a smoke detector in their home. Taken in total, these estimates suggest the results in Table 3 are possibly subject to an omitted variables bias because it is hard to argue that bans should have a causal impact on these other variables.

In the next sections, we take a more systematic look at this potential problem and ask whether the results in Table 3 signal causation or correlation. To address the issue of an omitted variables bias, we use three basic procedures. First, we control for more observed characteristics; in particular, we control for other health habits and firm characteristics that are consistent with the four sources of omitted variables bias outlined above. Second, we attempt to generate results that are consistent with a causal interpretation. Last, we use standard instrumental variables procedures.

A. Controlling for Additional Covariates

The results in the previous section suggest that workers with above-average health characteristics can be found in firms with workplace smoking bans. If there is an omitted variables bias in the basic single-equation estimates, then including these health-habit indicators as covariates should greatly reduce the impact of the work area smoking ban. The results are, however, virtually unchanged when we perform this exercise. The results in Table 3 do not appear to be capturing the benefit of some other workplace health-promotion program. When we estimated models where we include indicators for whether the establishment has workplace exercise programs and facilities, the coefficient on workplace ban is nearly identical to the estimate in Table 3. By restricting our attention to only the 1991 NHIS data, we can also include an indicator for whether the firm provides health insurance. In none of the specifications are the coefficients for exercise programs and facilities or employer-provided private health insurance statistically significant. Finally, we estimated a model using the outgoing rotation from the CPS where we added an indicator for whether the job is covered by a union contract. Again the marginal effect of the workplace smoking bans is identical with and without a union status indicator. In summary, the effect of the workplace smoking ban did not change in any of these alternative specifications.

B. Results Consistent with a Causal Impact

In this section we provide some direct evidence that smoking bans lead to a drop in smoking by considering models in which the results are consistent with a causal interpretation. If there is a causal link between bans and smoking, 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 with long work weeks. We test this hypothesis using

¹² Since nearly 90 percent of ever-smokers begin smoking by age 20 (our calculations based on smoking history information from the pooled CPS data sets) and over 95 percent of workers in the NHIS data are aged 21 or over, workplace smoking bans should not be correlated with whether a person ever smoked a cigarette.

¹³ All of these health indicators are taken from the pooled 1991/93 NHIS except the overweight and smoke detector questions which were taken from the 1991 and 1993 surveys, respectively.

data from the pooled 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 (all workers) model are reported in Table 4.

In both models, the coefficients on the smoking ban/hours per-week interactions are nearly monotonic in hours worked per week. The largest impacts of the smoking bans are for those workers who work 50 hours per week or more. In contrast, the impact of smoking bans on smoking participation and cigarettes per day (all workers) is much smaller for those working less than 20 hours per week. Test statistics reported in the final row of the table indicate that we can reject the hypothesis that the estimates are the same across the ban/hours interactions.

If smokers have preferences over whether smoking is allowed on the job, they may be more likely to quit firms that ban smoking or are less likely to apply for jobs at firms with a smoking ban. To examine this hypothesis we use data from the 1991 NHIS where workers report the number of months on their current job.¹⁴ If our results were driven *purely* by selection, or by the movement of smokers away from firms with bans, 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 as smokers eventually migrate to jobs without bans. We estimated both smoking prevalence and intensity equations where we interact the smoking ban variable with worker tenure. In each model, we also include a full set of indicators for job tenure. On both the intensive and extensive margins we find that smoking bans have the *smallest* effect on workers with the

least amount of tenure. The marginal effects (standard errors) on the smoking ban/tenure interactions in the current smoking probit model are: ban \times ≤ 6 months of tenure, -0.031 (0.027); ban \times 7–12 months, -0.077 (0.029); ban \times 13–24 months, -0.050 (0.025); ban \times 25–48 months, -0.105 (0.023); ban \times > 48 months, -0.058 (0.013). The OLS estimates on the interactions in the all-workers cigarettes/day equation are: ban \times ≤ 6 months, -0.84 (0.57); ban \times 7–12 months, -1.71 (0.58); ban \times 13–24 months, -1.71 (0.52); ban \times 25–48 months, -2.68 (0.46); ban \times > 48 months, -1.73 (0.27). In both models, we cannot reject the null hypothesis that the interaction terms are all equal.

As a final test, we note above 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 establishments that allow smoking than in firms with bans. We find no evidence of this systematic difference in job tenure.¹⁵ These results again strongly suggest that selection from differential turnover of smokers is not driving our results.

C. 2SLS Estimates

A third way to deal with possible omitted variables bias is to allow for the nonrandom match of workers and firms. In this section, we present estimates of two-stage least-squares models in which we treat the workplace bans as a potentially endogenous variable.¹⁶ To obtain

¹⁴ In the 1991 NHIS, respondents were asked the question: "You told me that you were employed during the past two weeks. How long have you worked at your main job?" The next two questions asked the respondents about the size of their current establishment and how many hours they worked per week at their main job. This sequence of questions leads us to believe that respondents answered the job tenure question for a particular firm rather than for a particular industry or occupation.

¹⁵ An alternate construction of this test is to use 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 establishments, we scale the difference in tenure between ban and no-ban establishments for smokers (5.0 months), by using the difference in tenure for nonsmokers 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.

¹⁶ In the current smoker equations, the outcome of interest and potentially endogenous variable are both discrete. An appropriate model in this context is a bivariate probit. In

TABLE 4—NORMALIZED PROBIT AND OLS ESTIMATES, CURRENT SMOKER AND CIGARETTES-PER-DAY EQUATIONS
 POOLED CPS SAMPLES
 (STANDARD ERRORS IN PARENTHESES)

Independent variables	Current smoker	Cigarettes per day (all workers)
Work area smoking ban \times worked <20 hours per week	-0.029 (0.010)	-0.93 (0.194)
Work area smoking ban \times worked 20–29 hours per week	-0.037 (0.010)	-0.65 (0.206)
Work area smoking ban \times worked 30–39 hours per week	-0.045 (0.006)	-1.25 (0.129)
Work area smoking ban \times worked 40–49 hours per week	-0.049 (0.005)	-1.56 (0.093)
Work area smoking ban \times worked ≥ 50 hours per week	-0.067 (0.008)	-2.13 (0.154)
χ^2/f -statistic	10.3	11.52

Notes: Other covariates include those listed in Table 3, plus four dummy variables for hours worked per week. See notes in Table 3 for calculation of the marginal effect in the current smoker probit model. The fraction of the sample for each of the hours groups are: <20 hours per week (9.7 percent), 20–29 hours per week (8.7 percent), 30–39 hours per week (22.8 percent), 40–49 hours per week (43.9 percent), ≥ 50 hours per week (14.8 percent). The null hypothesis for the test in the final row table is that coefficients on the hours/ban interactions are equal to each other. The 95th percentiles of the $\chi^2(4)$ and $F(4, \infty)$ distributions are 9.49 and 1.94, respectively.

consistent estimates of the workplace smoking ban impact in the 2SLS model, we must identify some variable that alters smoking rates only through the presence of workplace smoking bans. The choice of an instrument is complicated by the fact that the instrument must generate a very large change in workplace smoking bans if we are to have any hope of detecting a statistically significant 2SLS estimate. Using a procedure outlined in Evans and Jeanne S. Ringel (1999), we calculate that given the size of the NHIS sample, the instrument must generate at least a 20-percentage-point change in the probability of observing a work area smoking ban to generate a statistically significant 2SLS coefficient the magnitude of the single-equation estimates in Table 3.¹⁷

practice, however, estimates from 2SLS models generate results similar to the “average treatment effect” in a bivariate probit model (Joshua D. Angrist, 1991). As we demonstrate below, this is also the case here.

¹⁷ To see this consider the bivariate regression model, $y_i = \alpha + \beta x_i + \varepsilon_i$ where y_i is the current smoking indicator and x_i is an indicator for workplace smoking bans. Let z_i denote a binary instrument. The IV estimate of β in this equation is

$$\begin{aligned}\beta_{IV} &= [(\bar{y}|z_i = 1) - (\bar{y}|z_i = 0)]/[(\bar{x}|z_i = 1) - (\bar{x}|z_i = 0)] \\ &= [(\bar{y}_1 - \bar{y}_0)/(\bar{x}_1 - \bar{x}_0)] = \delta_1/\delta_2\end{aligned}$$

A variable that we believe meets these demanding requirements is a measure of establishment size. The NHIS asked workers whether or not

where $(\bar{y}|z_i = 1) = \bar{y}_1$ is the mean of y for those observations with $z_i = 1$ and other terms are similarly defined. This is the Wald estimate that was reintroduced into evaluation research by Angrist (1990). The numerator in the Wald estimate is calculated from a regression of y on z , and the statistical significance of this coefficient functionally determines the statistical significance of the 2SLS estimate. Suppose there are n observations for both where $z = 1$ and $z = 0$. Since y is discrete, $y_1 = \bar{y} - 0.5\delta_1$ and $\bar{y}_0 = \bar{y} + 0.5\delta_1$ and $\text{var}(\delta_1) = (\bar{y}_1(1 - \bar{y}_1) + \bar{y}_0(1 - \bar{y}_0))/n$. The absolute value of the t -statistic for δ_1 can then be written as $|t(\delta_1)| = |\delta_1|n^{0.5}/(2(\bar{y}(1 - \bar{y}) - 0.25\delta_1^2))^{0.5}$. In the NHIS data, there are roughly 18,000 observations so let $n = 9,000$ and set \bar{y} (the sample mean of smoking) to 0.24. Solving the equation above for δ_1 , these numbers indicate that to generate a $|t(\delta_1)| > 1.96$ requires an estimate of $|\delta_1| > 0.0125$. By construction, $\delta_1 = \delta_2\beta$, and assume that β equals the single-equation estimate of -0.057 from Table 3. Then the estimate for δ_2 must equal $0.219 = (-0.0125/-0.057)$. Using notation similar to that introduced above, $|t(\delta_2)| = |\delta_2|n^{0.5}/(2(\bar{x}(1 - \bar{x}) - 0.25\delta_2^2))^{0.5}$, assuming the sample mean of x is 0.7, the estimate of $\delta_2 = 0.219$ will have a t -statistic of 32.6. To put these results differently, to identify a statistically significant reduced-form relationship between y and z of a size comparable to the single-equation estimates, a discrete instrument for x must change the presence of workplace smoking bans by a minimum of 22 percentage points in absolute value. Even though our sample is relatively large, we still need an incredibly powerful instrument to have any hope of detecting a statistically 2SLS coefficient on the workplace smoking ban variable.

their work site has 50 or more employees. In this sample, 74.4 percent of indoor workers work for establishments with 50 or more workers. Based on the descriptive statistics from Table 2, larger establishments are 22.7 percentage points more likely to adopt workplace smoking bans than are smaller firms. This suggests that firm size may generate enough of a change in the presence of workplace smoking bans to produce a statistically significant 2SLS coefficient.

In Table 5, we report the marginal effect from a probit model in which we model the probability that a firm has adopted a work area ban as a function of an establishment size indicator and the other covariates listed in Table 3. The indicator is defined as the variable "50 + employees" which equals 1 if the establishment has 50 or more workers and zero otherwise. The marginal effect for the establishment size indicator suggests that job sites with 50 or more employees have a 22.1-percentage-point higher probability of adopting a work area smoking ban. Since we will ultimately be using 2SLS, the appropriate comparison should be the coefficient on the establishment size indicator in a linear probability model. In column (2), we see that the linear probability estimates are nearly identical to the probit model. These estimates, and our variance calculations, suggest that if the 2SLS coefficient approaches the magnitude of the OLS coefficient, then the 2SLS value will have at best marginal statistical significance.

The results in Table 5 are consistent with a number of other studies that have also noted that larger establishments are more likely to institute smoking restrictions.¹⁸ There are at least three reasons why large firms are more likely to ban smoking. First, in 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 transaction costs. In bigger establishments, formal rules are more often used to handle these types of situations. This hypothesis is bolstered by survey data that suggest that one of the major

reasons for adding workplace smoking bans was to reduce workers' complaints about secondhand smoke (U.S. Department of Health and Human Services, 1986b p. 580).

Second, a number of authors have noted that firms have adopted workplace smoking bans for fear of possible liability for illnesses caused by secondhand smoke (Jacob Sullum, 1998). 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 more likely to ban smoking in the workplace.

Third, since smoking bans are unpopular with smokers, large firms may be more likely to adopt work area smoking bans because they can set up segregated smoking areas somewhere else in the firm. This theory would predict that large establishments that ban smoking in work areas are also more likely to allow smoking in public areas. Evidence in support of this hypothesis is easy to generate. In a sample of establishments that have banned smoking in work areas, we ran a probit model where the outcome of interest is whether smoking is banned in public areas. The marginal effect (standard error) for "50 + employees" in that model is $-0.159 (0.013)$ —indicating that large firms which ban smoking are also more likely to allow smoking in some public area than smaller firms that impose work area bans. This hypothesis may generate a negative covariance between firm size and workplace smoking if allowing some smoking in public areas mitigates the benefits of the workplace ban. However, this does not appear to be the case. In a smoking participation probit model identical to the one we estimate in Table 3, we replace the work area smoking ban dummy with two variables—one for establishments that have both public and work area smoking bans and another one for just work area bans. The marginal effects (standard errors) on these two coefficients are $-0.059 (0.008)$ and $-0.050 (0.009)$, respectively. Testing the equivalence of these two marginal effects, we obtain a chi-square statistic of about 1 with 1 degree of freedom. We cannot reject the null hypothesis that allowing smoking in some public areas does not mitigate the impact of work area smoking bans.

A potential problem with the use of firm size

¹⁸ These studies are reviewed in the 1986 Surgeon General's report on *The Health Consequences of Involuntary Smoking* (U.S. Department of Health and Human Services, 1986b).

TABLE 5—FIRST-STAGE OLS AND NORMALIZED PROBIT ESTIMATES
 WORK AREA SMOKING BAN EQUATIONS
 1991 AND 1993 NHIS
 (STANDARD ERRORS IN PARENTHESES)

Independent variable	Probit (1)	OLS (2)	OLS (3)	OLS (4)
50+ employees	0.221 (0.009)	0.215 (0.008)		0.195 (0.008)
Establishment has exercise programs			0.053 (0.009)	0.030 (0.009)
Establishment has exercise facilities			0.094 (0.010)	0.060 (0.009)

Note: Other covariates include those in Table 3.

as an instrument is that since Charles Brown and James 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 incentive firms which provide insurance have to improve the health of their workers. Survey data, however, suggest that the economic costs of smoking are a minor reason why firms adopt workplace smoking bans. In surveys of companies with smoking restrictions, Donald J. Petersen and Douglas P. 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 restrictions. To test this hypothesis directly, we used data from the 1991 NHIS and reestimated the first-stage work area ban regression, adding an indicator for whether the firm provided health insurance. In this model, the coefficient on firm size was nearly identical to the value reported in Table 5, and the coefficient on the health-insurance indicator was small and statistically insignificant. Thus, the establishment size variable is picking up effects that are orthogonal to health-insurance considerations.

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. Donald Kenkel and Dylan Supina (1992), for example, demonstrate that larger establishments were more

likely to adopt any of the nine employer-initiated health programs the authors considered. This result is also found in the NHIS data. Large establishments are more likely to have on-site exercise facilities and exercise programs, and they are more likely to provide health insurance. Results presented above 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 presence of bans is orthogonal to whatever factors may make large or small establishments more or less likely to adopt these other health policies.

For our instrument to be valid, it 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 establishments *per se*, workers in large and small establishments differ in observed characteristics. Establishment size would not be a valid instrument if it is picking up *unobserved* worker characteristics that affect the demand for cigarettes and that are correlated with establishment size.¹⁹ Unfortunately, we cannot explicitly

¹⁹ For instance, as we noted above, workers in large firms receive higher wages than workers in smaller firms. Part of this pay premium is due to the fact that large establishments attract workers with better observed skills, such as education. Evans and Edward 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 establishments 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 establish-

test whether smokers are more or less attracted to larger establishments. We should note that we do not see large differences in the means of observed worker characteristics in our data.²⁰ For example, workers in establishments with 50 or more employees have only 0.15 more years education than workers in smaller establishments. We also note that in Table 2, the difference in the prevalence of workplace smoking bans by establishment size is 22.7 percentage points. Once we control for the myriad of covariates, this difference only falls slightly to 21.5 percentage points.

An indirect way to examine this issue is to see if other health habits are correlated with establishment size. If large establishments attract or hire healthier workers (nonsmokers), then they should also have fewer workers with other "bad" habits. In probit models when we include the establishment size indicator instead of the workplace smoking ban variable in the never smoke, salt, seat belt, health status, overweight, and smoke detector models, the marginal effects (standard errors) on establishment size in these models are -0.004 (0.009), 0.003 (0.006), 0.005 (0.008), -0.012 (0.008), 0.032 (0.008), and -0.001 (0.007), respectively. None of the habits that were correlated with the presence of workplace bans are correlated with establishment size. Further, the only variable that is statistically significant (20 percent or more overweight) is uncorrelated with whether the establishment had workplace smoking 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.

The 2SLS estimates for the current smoker and cigarettes-per-day (all workers) models are reported in Table 6. In the first two columns of the table we report the instrument used and any

ments attract workers with "better" observed or unobserved skills, there may be a correlation between establishment size and smoking.

²⁰ This may be due in part to the fact that our establishment size dummy equals one for relatively small establishments (establishments that are much smaller than the "large" establishments considered by Brown and Medoff, 1989).

other exogenous covariates used in the analysis. In the next two columns, we report the 2SLS coefficient on the work area smoking ban variable as well as, when appropriate, the *p*-value and degrees of freedom for the test of overidentifying restrictions. In the final two columns, we report analogous results for the models where the outcome of interest is cigarettes per day for all workers.

To provide some basis of comparison, in the first row of Table 6 we report OLS results of the workplace smoking ban in a linear probability model for current smoker and the results for the parameter from the cigarettes-per-day equation from Table 3. In row (2), we present estimates using establishment size as the only instrument for the workplace smoking ban. In this case, the results indicate that work area smoking bans reduce smoking participation by 6.3 percentage points (standard error is 3.5 percentage points) and decrease daily cigarette consumption by a statistically significant 1.62 cigarettes. This second result is only about 6 percent smaller than the OLS estimates from Table 3. Here, we find no evidence that our single-equations estimates are subject to an omitted variables bias.

Because establishment 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 establishment size with the industry and occupation indicator variables. Results using these additional instruments are reported in row (3) of Table 6. The additional instruments have tremendous explanatory power. In a first-stage linear probability regression where we include the establishment size indicator plus interactions of establishment size with industry and occupation effects, we reject the null that the additional 24 interactions are jointly zero with a *p*-value of 5.6E-16. In both the current smoker and cigarettes-per-day models, the 2SLS estimates of the work area smoking ban coefficients are nearly identical to the previous estimates, but the *t*-statistics increase in size slightly. The *p*-values of the tests of overidentifying restrictions (Whitney K. Newey, 1985) indicate that we cannot reject the null hypothesis that we can exclude the instrument from the equation of interest.

In row (4), we can lessen any bias in the basic 2SLS models from correlation between the es-

TABLE 6—OLS AND 2SLS ESTIMATES OF CURRENT SMOKER AND CIGARETTES-PER-DAY EQUATIONS
1991 AND 1993 NHIS
(STANDARD ERRORS IN PARENTHESES)

Instruments	Other covariates	Current smoker		Cigarettes per day (all workers)	
		Work area smoking ban	p-value, test of overidentifying restrictions (degrees of freedom)	Work area smoking ban	p-value, test of overidentifying restrictions (degrees of freedom)
(1)		−0.054 (0.007)		−1.70 (0.14)	
(2) 50+ employees		−0.063 (0.035)		−1.62 (0.72)	
(3) 50+ employees × major industry and major occupation effects		−0.065 (0.033)	0.15 (24)	−1.61 (0.67)	0.58 (24)
(4) 50+ employees × major industry and major occupation effects	50+ employees	−0.076 (0.083)	0.12 (23)	−1.52 (1.80)	0.52 (23)
(5) Exercise programs, exercise facilities		−0.086 (0.060)	0.35 (1)	−2.80 (1.26)	0.78 (1)
(6) Exercise programs, exercise facilities, 50+ employees		−0.067 (0.033)	0.61 (2)	−1.94 (0.69)	0.69 (2)
(7) 50+ employees	Salt, seat belt, and health status questions	−0.067 (0.034)		−1.70 (0.72)	
(8) 50+ employees × major industry and major occupation effects	Salt, seat belt, and health status questions	−0.067 (0.032)	0.18 (24)	−1.61 (0.67)	0.66 (24)

Notes: The results in row (1) are OLS estimates. Other covariates include those listed in Table 3.

tablishment size indicator and the unobserved propensity to smoke by including establishment size as a covariate and using the size*industry and size*occupations as the instruments for the model. As expected, the precision of the estimates falls, but the parameter estimates for this model are virtually the same as the estimates in row (3). More importantly, the coefficient (standard error) on firm size in the current smoker and cigarettes-per-day equations are 0.0028 (0.019) and −0.021 (0.422), respectively, indicating that establishment size appears to be uncorrelated with smoking, except through the impact of establishment size on the presence of bans.

As an alternative instrument, we use indicators for whether the firm provides any on-site exercise programs or facilities. These programs will be correlated with the presence of workplace smoking bans if “healthy” firms institute programs to promote worker health. The instrument will be uncorrelated with the unobserved

determinants of smoking if: (1) there are no spillovers from one program to the next (i.e., having an exercise program does not encourage you to quit smoking), and (2) the programs do not attract healthy workers who may be less likely to smoke. That these programs do not attract “healthier” workers is verified when we add these variables to the never smoked, salt use, belt use, health status, overweight, and smoke detector linear probability equations for the workplace ban indicator. The *p*-values on the *F*-test that these two coefficients are zero are 0.26, 0.98, <1E-5, 0.10, 0.20, and 0.73, respectively. Only in the belt use model is there any statistically significant correlation between exercise program and facilities and the health habit.

The first-stage relationship between these instruments and workplace bans is given in the final two columns of Table 5. Clearly these variables are correlated with workplace bans, even when we add establishment size as a

covariate. In row (5) of Table 6, we report 2SLS estimates using indicators for exercise programs and facilities as instruments for workplace smoking bans. Here, the results are nearly identical to the results in row (2), but the estimates are statistically insignificant. In the next row, we add the establishment size indicator as an instrument and generate a statistically significant estimate that is very close to the OLS value for both the current smoker and cigarettes/day equations. In all four models, we obtain high *p*-values for the test of overidentifying restrictions.

In the last two rows of the table, we add covariates that measure the health of the workers (whether they always add salt, always use seat belts, and report excellent or very good health). We estimate models for the exactly identified case where established size indicator is the only instrument, plus we estimate models where the establishment size is interacted with the industry and occupation indicators as instruments. In each of these specifications, for both the current smoker and cigarettes-per-day equations, the 2SLS estimates again are very similar to the OLS estimates, and in the cases where the model is overidentified, we cannot reject the null hypothesis that the instruments can be excluded from the equation of interest.

We also estimated simultaneous equation systems in which we explicitly model the discrete nature of the current smoker and workplace smoking ban variables. The appropriate model in this context is a bivariate probit. In this case, the bivariate probit has an identical structure to that used by Evans and Robert M. Schwab (1995). From the bivariate probit estimates of the current smoker equation, we can calculate the average treatment effect, which is the average difference between the probability that a worker smokes if he or she works in a firm with a workplace ban and the probability a worker smokes if he or she did not. In the current smoker model with establishment size as the instrument, the results show that a smoking ban reduces the prevalence of smoking by 7.7 percentage points (standard error is 3.2 percentage points). Further, in the bivariate probit model, the correlation coefficient (standard error) between the errors in current smoker and workplace smoking ban equations is 0.041 (0.060), which is statistically insignificant.

These results are very similar to the 2SLS estimates of the workplace smoking ban effect.

As a final check on whether the establishment size instrument is performing as it is designed, we instrument for workplace smoking bans with establishment size in models of workers' decisions to never smoke, use salt, and wear seat belts. If the correlation between these health habits and workplace bans that we found above is spurious, then the instrumental variable coefficients on the work area ban variable should move toward zero. This is exactly the case. The 2SLS estimate (standard error) of the workplace smoking ban coefficients in the never smoke, salt use, and belt use equations are -0.017 (0.040), 0.014 (0.028), and 0.016 (0.033), respectively. These estimates show that once we control for omitted variables bias, the effect of the smoking ban on these other health measures is eliminated.

V. Conclusions and Implications

Using data from two large nationally representative samples, we find that smoking participation rates are 4 to 6 percentage points lower in establishments that ban smoking in work areas. As with other studies that have examined this issue, there is reason to be concerned that this estimate is subject to an omitted variables bias. As we show above, the most likely reason for concern is that workers with good health habits (one of them being not smoking) are generally attracted to "healthy" establishments that have workplace smoking bans as one of their characteristics. In this case, the "healthiness" of the worker or the establishment is an omitted variable in a single-equation model. In the end, however, most of the evidence we present is consistent with the hypothesis that simple cross-sectional estimates presented in Table 3 reflect the causal impact of bans on smoking. For example, once we control for other health habits of the worker or other health programs provided by the firm, the basic results are unchanged. Likewise, if bans have a causal impact on smoking, the impact on smoking should be proportional to the costs they impose on workers. Consistent with this hypothesis, we find that workplace smoking bans have the largest impact on workers who have longer work weeks and the smallest impact on part-time workers.

Last, we find that estimates from simulta-

neous-equation models are quite close to the single-equation estimates, indicating again that the basic results described above are not subject to an omitted variables bias. In these models, the primary instrument for the presence of workplace bans was an indicator for the size of an establishment. There appears to be no correlation between the health habits of workers and establishment size indicating that the unobserved determinants of smoking, given the observed characteristics of the workers, are uncorrelated with our measure of establishment size.

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 large 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 points more than the decline for nonworkers and that 1.7 percentage points of this decline cannot be attributed to changing observed characteristics. How much of this unexplained decline can be attributed to work area smoking bans? To answer this question, we need to calculate the change in exposure to work area smoking bans between 1985 and 1993. Unfortunately, there are no worker-based surveys from 1985 that have the required information. Instead, we use data from the 1985 National Survey of Worksite Health Promotion Programs (U.S. Department of Health and Human Services, 1986a), which is a survey of 1,328 establishments designed to determine the types of health-promotion activities sponsored by employers.²¹ Establishments were asked a number of questions about what types of health programs were provided by the firm, including whether they had any written policy restricting smoking in the workplace. 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.²² 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 the 1993 NHIS survey, we know that indoor and nonself-employed workers represent two-thirds of all workers. If work area smoking bans reduce smoking participation by 5.7 percentage points, then these numbers suggest that between 1985 and 1993, bans should have reduced smoking participation rates among workers by $(0.70 - 0.25)(0.67)(-5.7) = -1.7$ 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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²¹ We wish to thank Don Kenkel for providing us with a copy of this data set.

²² We classified a firm as having a work area smoking ban if: (1) smoking was banned indoors, and (2) smoking was banned except for a designated area such as a lounge or a cafeteria.

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