The Effect of Smoking Bans on Bars and Restaurants: An Analysis of Changes in Employment

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Abstract

Many communities and several states prohibit smoking in bars or restaurants. Using county-level data on employment from across the US, we find that communities where smoking is banned experience reductions in bar employment compared with counties that allow smoking. Smoking bans have a larger detrimental impact on bars in geographic areas with a high prevalence of smokers. The relative effect on restaurant employment is neutral or mildly positive. The positive effects are concentrated in areas with fewer smokers. We also find that bans have a positive effect on restaurant employment in warmer regions of the country, especially during the cooler winter months, and in the summer in colder regions. This suggests the prevalence of outdoor seating might influence the policy's effect.

KEYWORDS: employment, local government regulation, smoking

I. Introduction

Regulations that restrict smoking in bars and restaurants have sparked heated debates across many communities in the US. Hundreds of cities and counties, as well as a few states, have banned smoking with the majority of these laws being passed in the last five years. Internationally, bans exist on six continents. Therefore, it is important to evaluate the economic impact of such smoking prohibitions. In this paper, we compare changes in industry employment in US counties where smoking was banned with changes in US counties without bans. Changes in employment are a good gauge of the economic impact on bars and restaurants as staffing decisions reflect patronage.

Empirical analysis of the economic impacts of smoking bans is necessary because theoretical predictions are ambiguous. Standard theory does suggest that in efficient markets operating with perfect information and no externalities any regulation that restricts an owner's operating choices would at best have no effect on profitability. Yet, where there exists imperfect information or externalities, the implementation of regulations could lead to even greater profits for firms. The latter may more accurately apply to an environment where smoking is allowed in restaurants and bars.

Interest groups on both sides of the debate appeal to anecdotal evidence of successes or failures frequently, therefore more objective, data-driven analyses are needed. Several existing studies that have gone through an academic peer review process are heavily dependent on surveys of managers' expectations or predictions of economic impacts (e.g., Dunham and Marlow, 2000a and 2000b). A few studies that analyze the actual economic impact of laws on establishments focus on select narrow geographic areas (e.g., Hyland and Cummings, 1999) and the earlier bans.

We add to the understanding of the economic impact of smoking bans by performing the first national difference-in-difference study of the impact of these laws. Given that communities have banned smoking in every region of the country, we use data from the Bureau of Labor Statistics' (BLS) Quarterly Census of Employment and Wages (QCEW). Moreover, since nearly three-quarters of the laws banning smoking have been passed in the last five years, we use recent releases of the QCEW.

Our primary finding is that smoking bans reduce employment in bars. On the other hand, they have a neutral effect or perhaps mildly positive effect on restaurant employment. As the prevalence of smokers in a region increases, negative effects of legislation become more pronounced, especially for bars. In areas with fewer smokers, positive restaurant effects are detected. Finally, we observe whether effects deviate by climate. Although bar effects do not differ by the climate of a region, positive restaurant employment effects appear to occur

where the option of outdoor seating for potential smokers is most attractive—in areas with warmer climates year round (especially during the milder months of October-March) and in areas with cooler climates in the more comfortable months (April-September). Restaurants are negatively affected in the harsher October-March period in colder areas. This suggests that the presence of outdoor seating allows an option for smokers while providing non-smokers with an inside option.

Section II discusses the content, timing, and coverage of smoking bans, theoretical predictions of the economic impacts of smoking bans, and the existing literature. Section III summarizes our data. Section IV outlines our methodology, presents the results, and discusses the findings. Section V concludes.

II. Background

A. Legislation

In the early 1990s, several communities in California passed laws prohibiting smoking within bars and restaurants. The health of patrons and employees was cited as the primary motivation. Aided by a strong national anti-smoking movement, the popularity of smoking bans has grown significantly over the last decade. According to Americans for Nonsmokers' Rights, about 42% of the nation's population lives in communities that have banned smoking in restaurants (about 33% in bars).[1] Early on, these laws were exclusively enacted at the municipal level (either county or city), but as of 2005, nine states (California, Utah, Delaware, Florida, New York, Connecticut, Maine, Idaho, and Massachusetts) had enacted smoking bans in restaurants and/or bars. Although the prevalence of these laws is higher in urban areas, statewide bans have resulted in many rural restaurants and bars being smoke-free.

Table 1 demonstrates how quickly smoking bans have swept the country. The years 2001-2004 represent a period of large growth in legislation and compose the time frame we study. Smoking bans currently exist in every region of the country.[2] With some obvious exceptions, most states have at least one jurisdiction where smoking has been banned by either a city or county government. Clearly, this is a nationwide phenomenon that requires analysis of nationwide data.

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[1] Data were obtained from the Americans for Nonsmokers' Rights website (www.no-smoke.org). Population statistics were derived from the 2000 US census.

[2] See Appendix Table 1 for a complete list of existing legislation that bans smoking in either restaurants or bars and the dates the laws went into effect. The bans are listed by state and include all ordinances passed by January 2005.

Table 1 – Cumulative number of municipalities with smoke-free laws

Year	Restaurant Laws	Bar Laws
1990	1	1
1991	2	2
1992	3	3
1993	9	5
1994	23	12
1995	30	18
1996	29	15
1997	30	16
1998	44	29
1999	56	38
2000	71	48
2001	101	67
2002	128	92
2003	171	125
2004	214	160

Note: The data for this table were collected from Americans for Non-Smokers Rights (www.no-smoke.org). These numbers include all municipalities with ordinances or regulations that do not allow smoking in attached bars or separately ventilated rooms and do not have size exemptions. It does not include municipalities that are covered by state-enforced smoking bans.

B. Theoretical considerations

Smoking bans are typically controversial when proposed at the local level, and the debate is ongoing at the national level with the opponents composed of the tobacco lobby, restaurant and bar associations, and smokers' rights groups, and the proponents composed of the American Heart Association, American Lung Association, American Cancer Society, and a large consortium of antismoking groups. Although health benefits are the primary motivation behind smoking bans, the most contentiously debated point is the economic impact on the restaurants and bars that must comply.

The controversy over the effects on businesses is not resolved by appealing to economic theory as both sides can claim support. The opposition claims that regulations will stifle the restaurant/bar businesses by reducing patronage by smokers. This would reduce the demand for labor. Policy

advocates, on the other hand, claim that smoking regulations do not hurt establishments and may even add to revenue as well as lower costs. For example, if a smoke-free environment induces non-smokers to spend more at restaurants and bars than is lost from a reduction in smoker patronage, bans could increase business, which would in turn increase the demand for labor. Moreover, overall costs could be reduced if restaurants and bars that currently allow smoking face greater expenses due to higher air filtration expenditures or insurance premiums.

At first glance, it appears as if the opposition group is on firmer theoretical ground. If there was the potential for increased business from going smoke-free, restaurants and bars would do so without regulations. Absent legislation, however, many firms might be reluctant to go smoke-free on their own for two reasons. First, they may not have accurate information about the potential changes in revenue that could occur from providing a smoke-free establishment because they are uncertain of the value of clean air at their establishments to non-smokers and the value of a smoker-friendly environment for smokers. No fair arrangements are struck at bars to price second-hand smoke or clean air pre- and post-legislation, so the ultimate effect of legislation on consumer patronage is unknown. Second, even if bar and restaurant owners thought that a smoke-free industry would increase average revenues, they might still be reluctant to ban smoking on their own. Without the guarantee that all competitors would also go smoke-free, firms fear losing the business of smokers to other establishments that still allow smoking. Thus, one can envision situations where potential increases in non-smoker revenues could generate higher profits after a smoking ban, but a firm would not privately ban smoking on its own because it would fear losing profits.

Thus, theory leaves us with no firm guidance as to what to expect following smoking restrictions in the bar and restaurant industry. We do expect that the impact on expected patronage will be reflected in how many workers a bar or restaurant will employ. Labor is a key variable input in the short run. Thus, tracking how employment changes following the passage of laws gives a good estimate of the economic effect of the legislation. Turnover in the bar and restaurant industry is rampant, and the employment relationship is tenuous from both the firm and worker perspective. This turnover, along with the overwhelming overrepresentation of younger workers in these industries, means that eating and drinking establishments and other firms in the same labor market (e.g., hotels and coffee shops) face highly elastic labor supply curves.[3]

We recognize that any discussion of employment effects must consider the potential for wage effects. The highly elastic labor supply curves faced by bars and restaurants, however, suggest there will be little to no wage impacts to go

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[3] The popular press and trade magazines contain many such descriptions of the industry. See Yang (1996) for an example.

along with the employment impacts caused by shifts in labor demand. Moreover, our data do not allow us to conduct a meaningful analysis of wage effects. The main QCEW wage variable is the wage bill of an industry. Given that the wage bill is a function of employment—our main dependent variable of interest—analysis using the wage bill is problematic. The QCEW does contain information on average weekly wages of industry employees, but this information is also derived by dividing the wage bill by employment. Once again, we are not comfortable using this measure. Finally, bar and restaurant employees receive a substantial portion of their compensation through tips and work irregular hours. Available wage data may therefore not be informative. Given these measurement issues in the QCEW and the fact that we are comfortable with the assumption that firms in these entry level industries face labor supply curves that are highly elastic, we limit our attention to employment outcomes in this paper.[4]

Finally, we undertake separate analyses for bars and restaurants because there is no reason to expect similar employment effects in these two industries. Smoking seems to be part of the bar culture and not necessarily part of the restaurant culture, thus rendering negative effects for bars more likely than for restaurants. On the other hand, the unavoidable nature of smoke in bars might make the potential increase in patronage from non-smokers from a ban even greater than in restaurants. In short, smoking bans require a comprehensive empirical analysis of employment effects across the two industries.

C. Past Research

Many estimates of the impact of smoking bans exist, but only a few appear in peer-reviewed outlets. Some of these studies use subjective data, such as owner or manager opinion of the effect of the regulation on revenues (Scollo et al., 2000). Dunham and Marlow (2000b), for example, found that bars are predicted to be more than twice as likely to experience losses as restaurants, a result that suggests potentially large differences in the impact of smoking regulations on these two similar industries.

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[4] There are also two other relevant effects on industry labor supply that are noteworthy but are more long-term and perhaps add to the ambiguity of studying wages rather than employment. First, compensating differential theory suggests that workers should be paid a premium to offset the costs associated with working in a smoke-filled environment (Rosen, 1986). If we again consider the potential of information failure, workers may underestimate the costs of second-hand smoke. Their compensation will be too low, and the presence of a regulation could make them better off. Thus, firm labor costs will fall with a smoking ban as long as workers get some disutility from second-hand smoke. Second, the long-term presence of smoke in a work environment may have a detrimental effect on worker productivity due to illness or other health problems (Musich et al., 2001). Smoking bans therefore may enhance worker output and reduce turnover.

While opinions are informative, it is preferable to find consistently measured variables gauging the economic performance of restaurants before and after laws are passed, such as employment rates or retail sales. We use employment as the measure in this paper because objectively collected, consistently measured data exist for every county in the US through the QCEW. The data are also sufficiently disaggregated to allow for the tracking of quarterly employment for bars and restaurants separately. Bars and restaurants staff their establishments based on consumer demand, and therefore employment data provide an excellent gauge of industry performance.

Many existing studies use retail sales with mixed results. Glantz and Smith (1994, 1997), Huang et al. (1995), Sciacca and Ratliff (1998) and Bartosche and Pope (1999) all find that the regulation had no significant effect on retail sales in restaurants for the communities they study. Hyland et al. (1999) report results that suggest taxable sales increased in eating and drinking establishments in New York City following the implementation of their regulation.

Hyland et al. (2000) examine changes in employment levels in Erie County, New York, finding no statistically significant change in the number or percent of employees following bans relative to other places in their study. Interestingly, they did find slightly higher unemployment rates in restaurants during the winter months, suggesting that climate may play an important role in a law's effect, something we explore in more depth in this paper.

Hyland et al.'s employment study and the studies of retail sales tend to focus on select geographic areas and the earlier wave of bans. Although these studies are quite informative, a national study of all recently passed smoking bans would arguably allow for a more complete analysis of the effect of bans. In our study, we use a national sample that allows for stronger judgments about the general impact of these laws. Moreover, we examine whether the effects differ by the concentration of smokers or by climate though exploiting the geographic variation in both factors in our data.

III. Data

The QCEW is appropriate for this policy analysis because it contains nationwide county-level panel data on employment levels across all North American Industrial Coding System (NAICS) classifications. The classifications are finely disaggregated and easily allow identification of drinking establishments (NAICS code 7224), which we refer to as bars, and full-service restaurants (NAICS code 7221), which exclude limited-service restaurants and cafeterias. Further, we can use population figures from the 2000 US census. We extract quarterly data for every county from 2001 to 2004, the last available quarter at the time the study

was undertaken. Therefore, we will identify effects from laws passed during this time span, which includes the majority of all smoking bans.

Due to changes in the industry coding system used in the QCEW at the start of 2001, there is not a generally reliable way to utilize data from before 2001 in conjunction with the more useful recent years. This is not necessarily a disadvantage if one considers a potential for selection bias in the passage of smoking bans. We identify effects from the passage of laws that came after the first wave of smoking bans. The first wave of smoking bans in the 1990s likely included localities that were better able to absorb the bans on smoking, thus their willingness to pass the legislation. For example, Utah and California, the two states with the lowest prevalence of smokers, passed the first bans. The later bans that we study include counties from every region of the country that are diverse in smoking prevalence, urban-rural status, and socioeconomic factors. Pakko (2005) also makes this argument and further notes that prior studies on smoking bans used data from early bans, which potentially introduces sample selection bias.

We conduct a difference-in-difference analysis, which necessitates the identification of a treatment group of counties with smoking bans. The treatment group includes all jurisdictions that passed an ordinance or regulation during our sample period. Most smoking ban ordinances are similarly worded, and we consider an ordinance as effective if it does not allow smoking in attached bars or separately ventilated rooms and does not have size exemptions. We consider 100% indoor smoking bans, which means laws that do not allow smoking anywhere within the establishment. Information on the timing of laws was obtained from the Americans for Non-Smokers' Rights (www.no-smoke.org).[5]

Although many of the laws were passed at the county level, some were passed at the city level and others at the state level. The state laws certainly render the county bars or restaurants smoke-free, as do the county laws. City laws only render a portion of the county smoke-free, but the city population is a subset of the county population in all cases. Local jurisdictions may pass bans after a statewide ban is in place, but these provide no identifying information for our study as their passage is redundant. Where only part of a county is smoke-free, we use population data from the census to determine the proportion of a county that is subject to a smoking ban. For our basic analysis, however, we consider a county to be smoke-free if at least 75% of the county population is subject to a

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[5] A complete list of states, cities, and counties that had passed smoking bans by January 2005, along with the dates the laws went into effect, appears in the Appendix Table 1. We acknowledge most of these localities had some restrictions on smoking prior to the 100% ban (such as mandated non-smoking sections), and estimates in the rest of the paper must keep this in mind. As long as these restrictions were similar across treatment and control groups, however, this is of minor concern. Moreover, the robustness check in Table 2 that restricts the comparison group to only counties with bans in place prior to the study period will further allay potential concerns on this point.

ban. We drop the counties that are less that 75% but greater than 0% smoke-free from the analysis. This 75% cutoff likely reflects that at least one large city in the county is smoke-free.[6] We test the robustness of this classification in Table 2.

The remainder of U.S. counties, those that did not enact smoking bans during the sample period, composes the control group in all analyses. This group includes all counties that did not have a change in their smoking status from 2001 to 2004 (both those counties with no smoking ban during the entire sample period and those counties with a smoking ban passed prior to the sample period that remained in effect from 2001 to 2004). We analyze a sample that ranges from 1,528 counties for bars to 2,837 counties for restaurants.[7]

IV. Methodology and Results

A. Basic approach and findings

We estimate the effects of smoking bans on the log of employment in treatment counties relative to the control counties. As we have panel data with time-variant policy introduction across counties, a fixed effects model is used. Specifically, we estimate the following regression for the bar and restaurant industries separately:

$$(1) \ Y_{it} = \alpha_i + \tau_t + \beta L_{it} + \varepsilon_{it}.$$

Y_{it} is log employment in county i in quarter t. L_{it} is initially set to one if county i has a smoking ban at time t and zero otherwise.[8] We also set L_{it} equal to the proportion of the population of a county that is smoke-free in some estimations as a robustness check. The fixed effects for county and quarter are captured by α_i and τ_τ, respectively. Standard errors are robust to heteroskedasticity and are corrected to allow for non-independence of observations from the same county through clustering. Without such corrections, the standard errors would likely be understated and significance perhaps overstated (Arellano, 1987; Bertrand et al., 2004). A valid concern with this approach arises if the treatment and control

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[6] In the few cases where multiple cities in a county passed smoking bans at different dates, the counties were excluded from the analysis.

[7] We lose some small counties because we drop those counties for which no bar or restaurant exists according to the data. These exclusions affect the bar sample size more than the restaurant sample size.

[8] Specifically, we start by setting L_{it} equal to one in those counties where at least 75% of the population is subject to a smoking ban. It is set to zero if 0% is subject to a ban. Counties where less that 75% are subject to a ban but greater than 0% are subject to a ban are dropped, but there are few of these.

groups are too dissimilar prior to laws being enacted. We tested the null of equality in quarterly trends in our control and treatment groups prior to smoking ban enactment and found F-statistics of less than one for both bar employment and restaurant employment. Thus, the pre-ban trends in the treatment and control counties in our sample appear similar. Later analyses of lead effects of smoking bans will bolster confidence that policy endogeneity is not driving the results.

Table 2 presents the employment effects using the dummy variable for existence of a smoking ban in the top row of the first two columns. The basic estimates suggest that a smoking ban reduces employment in the bar industry by 4.3% relative to counties not enacting a ban. The effect on restaurants is a small positive effect that is not statistically significant.[9]

The fixed effects specification captures differences across counties that are time-invariant and differences over time that are common to all counties. An obvious concern is that there are other changes that are contemporaneous with the passage of ordinances that may not be picked up by the fixed effects. Notable among these are sales tax rates on cigarettes and alcohol. The latter vary by year and state and could affect industry demand and therefore employment rates. The former also vary and might be correlated with the passage of smoking bans as localities with high cigarette tax rates might be more prone to implement smoking bans. For this reason, we re-estimate the effects of the bar and restaurant ordinances controlling for cigarette and beer taxes. The effect on bars rises slightly to a 4.5% decrease in employment, and the effect on restaurants remains small and insignificant.[10] Finally, we capture any remaining variation in employment or general changes in the economy of a county over time by adding a control to the specification for the log of overall employment in a county in a quarter. This captures differences in sizes of the labor markets over time. The

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[9] A concern may arise with the employment results if there was a change in the number of establishments in the treatment counties following the passage of smoking bans. If bars or restaurants close or choose not to open because of a smoking ban, this could bias the effects of our estimates in the negative direction. We view establishment closures as a potential long term effect of the law and a topic for future research not likely to influence the short run employment estimates in this paper. Nevertheless, we collected data on the number of establishments in a quarter in each county (also available in the QCEW) and found that neither the number of bars nor the number of restaurants decreased after smoking bans in the treatment group relative to the control group in the short run. Thus, there appears to be no negative bias in the employment results presented in the paper.

[10] To keep Table 2 uncluttered, we do not report the coefficient estimates of cigarette and beer taxes on employment in the tables, but note here that cigarette taxes had essentially no effect on either industry's employment. Beer taxes, on the other hand, negatively affect bar employment, although the effects fall short of statistical significance.

Table 2 – Effects of Smoking Bans on Bar and Restaurant Employment

	Bars	*Restaurants*
Existence of a Smoke-Free Law		
Basic effect	-0.043	0.005
	(0.020)	(0.012)
Effect adding controls for cigarette and beer taxes	-0.045	0.007
	(0.019)	(0.011)
Effect adding controls for the log of overall employment	-0.045	0.007
	(0.019)	(0.011)
Sample size	16,248	39,565
Number of counties (clusters)	1,528	2,837
Robustness checks (including all controls)		
Alternative policy variable: proportion of a county in which smoking is banned	-0.046	0.006
	(0.019)	(0.011)
Sample size	16,280	39,970
Number of counties (clusters)	1,533	2,862
Alternative control group: only counties without a ban	-0.045	0.008
	(0.019)	(0.011)
Sample size	15,635	38,249
Number of counties (clusters)	1,481	2,750
Alternative control group: only counties with a ban the entire sample period	-0.039	0.002
	(0.028)	(0.026)
Sample size	1,649	4,778
Number of counties (clusters)	133	307

Note: The dependent variable is log number of employees in a county-quarter. Reported are coefficient estimates from a fixed effects model, with standard errors in parentheses. The standard errors are corrected to allow for non-independence of observations from the same county.

results in the third row suggest that inclusion of this control does not alter the results.[11]

In the remainder of Table 2, we engage in a series of robustness checks. First, we present employment effects for specifications where the policy variable is measured as the proportion of a county that is smoke-free. If there is a state or county law, or all the cities in a county are smoke-free, the proportion is 1. Otherwise it is between 0 and 1. We add back into the sample the counties that have some ban coverage but are less than 75% smoke-free. As noted by the relative sample sizes in the table, this is not many. We report estimates using controls for log of overall employment in a county-quarter as well as cigarette and beer taxes. For bars, the weighted results continue to show a negative effect of smoke-free ordinances. The effect on restaurant employment remains positive but not significant.

We also check to see if our results are robust to changes in the control group. Remember, we identify an effect of smoking bans if the status of a county changes from permitting smoking in bars and restaurants to prohibiting it, comparing this change with control counties that do not change status. The first group of counties not changing status includes places that never had a law in place and did not enact one over the sample period. In estimates reported using all controls and the original dummy policy variable indicating the existence of a ban, the bar effects remain a 4.5% reduction in employment and the restaurant effects inch closer to a 1% increase in employment using the alternative control group. The latter remains statistically insignificant. Another alternative control group contains those counties that enacted a smoking ban prior to the sample period and remained smoke-free throughout our period of study. This reduces the size of the control group considerably as indicated by the sample size, as well as the precision of our estimates. The basic story remains the same, however. Employment in bars falls by about 4% following smoking bans, and there is a small, insignificant positive effect for restaurants.[12]

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[11] We also ran our basic specification with employment outside of the restaurant or bar industry as the dependent variable as a check on whether our basic results are spurious. The estimations showed that following bans there was a statistically insignificant 0.2% increase in employment in industries not affected by a ban. Given the results from Table 2 (0.5% increase in restaurants and 4.3% decrease for bars), this confirms that there is a slight positive but insignificant relative difference in the case of restaurant employment and a large negative relative impact on bar employment. It also provides further assurance that the treatment and control groups in our study appear comparable. The growth in employment in industries not affected by a ban is virtually the same in both groups.

[12] We also investigated an alternative control group that selected counties based on similarities with the treatment group of counties. Specifically, we restricted the sample to only large counties, which did render the average pre-ban employment rates similar across treatment and control groups. The bar results were virtually identical and the restaurant results essentially became zero. Given the similarity to the Table 2 findings and the fact that we always include controls for the log

The results of the analysis on the bar industry in this section are consistent with prior work by Dunham and Marlow (2000b), which suggests that the bar industry is more negatively affected by smoking bans than the restaurant industry. On the other hand, analysis of the restaurant industry is consistent with many of the aforementioned retail sales case studies, which find that smoking bans do not seem to have an effect on business in the restaurant industry as a whole.

B. *Effects of smoking bans over time*

Our identification strategy thus far has uncovered the short run impact of smoking bans on bars and restaurants. Also of interest, however, is whether the effects of bans occur immediately after passage of the law or take place over time. To investigate this question, we replaced our policy variable with an indicator of the quarter a ban went into effect and a series of lagged indicators that allow us to trace out the effects of smoking bans over time. We also added two lead variables to the specification as well. These capture whether there were any meaningful changes in employment in the periods just before the laws went into place. This allows us to see whether bars or restaurants were adjusting employment as the smoking bans grew closer to their effective date. The analysis of leads will also help assess the problem of policy endogeneity as the lack of lead effects would suggest that there are similarities between treatment and control counties.

Table 3 shows that the individual lead variables are not significant in either the bar or restaurant specifications. For the bar analysis, we fail to reject the null of the leads jointly equaling zero in a Wald test, although the same is not true for the restaurants. As a means of further testing this, two more leads were included to the restaurant specification, and under this alternative test, we do fail to reject the null of the leads being jointly equal to zero. This, coupled with the differently signed leads for restaurants, suggests that policy enactment is likely exogenous and not driving our results.

The lagged effects suggest that the negative bar effect occurs immediately and persists over time. We failed to reject the null of equality of the lags, so we do not carry through the lagged effects to later analyses. As for restaurants, the lagged effects are generally uninformative with some hints toward positive effects with longer lags. We suggest that more detailed analysis of the long-lasting effects of smoking bans should be conducted in the future when more smoking bans are enacted and longer panels are available. This would allow for more precise identification of effects at longer lags. It would also allow for an incorporation of establishment data that tests whether smoking bans lead to the

of total employment throughout the paper, we choose to stick with the basic control group using all counties which do not have a change in their ban status (either never a ban or always a ban) during the sample period.

Table 3 - Effects of Smoking Bans on Bar and Restaurant Employment, Leads and Lags

	Bars	*Restaurants*
Lead (2 quarters)	-0.016	-0.009
	(0.018)	(0.014)
Lead (1 quarter)	-0.017	0.015
	(0.020)	(0.014)
Quarter of enactment	-0.037	0.004
	(0.020)	(0.015)
Lag (1 quarter)	-0.021	-0.022
	(0.021)	(0.015)
Lag (2 quarters)	-0.050	0.009
	(0.025)	(0.020)
Lag (3 quarters)	-0.077	0.031
	(0.027)	(0.016)
Lag (4 quarters)	-0.061	0.017
	(0.029)	(0.016)
Lag (5 or more quarters)	-0.051	0.023
	(0.033)	(0.016)
Sample Size	16,248	39,565
Number of counties	1,528	2837

Note: Reported are coefficient estimates from a fixed effects model, with standard errors in parentheses. The standard errors are corrected to allow for non-independence of observations from the same county through clustering. Controls for the log of overall employment, cigarette taxes, and beer taxes have been included.

closure of bars and restaurants or change where bar and restaurant owners choose to locate their establishments.

C. Results by Smoking Prevalence

We now return to our basic specifications that address short-term impacts of bans on bar and restaurant employment but introduce some additional information to the analysis that might help us understand the effects of the smoking bans more deeply. First, Table 4 indicates that the proportion of adults that smoke differs

Table 4 – Smoking Prevalence by State, 2000

High Prevalence	%	Low prevalence	%
Kentucky	30.5	Utah	12.9
Nevada	29.1	California	17.2
Missouri	27.2	Arizona	18.6
Indiana	27.0	Montana	18.9
Ohio	26.3	Hawaii	19.7
North Carolina	26.1	Minnesota	19.8
West Virginia	26.1	Connecticut	20.0
Tennessee	25.7	Massachusetts	20.0
New Hampshire	25.4	Colorado	20.1
Alabama	25.3	Maryland	20.6
Arkansas	25.2	Virginia	20.7
Alaska	25.0	Washington	20.7
South Carolina	24.7	Oregon	20.8
Pennsylvania	24.3	District of Columbia	20.9
Michigan	24.2	New Jersey	21.0
Louisiana	24.1	Kansas	21.1
Wisconsin	24.1	Nebraska	21.4
Maine	23.8	Vermont	21.5
Wyoming	23.8	New York	21.6
Georgia	23.6	South Dakota	22.0
New Mexico	23.6	Texas	22.0
Mississippi	23.5	Illinois	22.3
Rhode Island	23.5	Idaho	22.4
Iowa	23.3	Delaware	23.0
North Dakota	23.3	Florida	23.2
Oklahoma	23.3		

Note: The information in this table was obtained the Center for Disease Control and Prevention. Mean and median prevalence is approximately 23.2%.

remarkably by state.[13] Kentucky has the highest rate of smoking at just below one in three adults. In Utah, only about one in eight adults smoke. It is likely that the number of smokers in a locality will influence how smoking bans affect bars and restaurants. After all, a high prevalence of smokers in an area probably translates into bars being more smoke-filled. Likewise, the tendency of businesses to allow

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[13] This information was obtained from the Center for Disease Control and Prevention.

customers a place to smoke in a restaurant, such as an outdoor patio, is likely greater in high prevalence areas. Moreover, a low prevalence of smokers means that there is a greater likelihood of a net increase in patronage following a smoking ban as well. Thus, the negative impact on bars is expected to be stronger in places with a high prevalence of smokers, and any positive influence on restaurants is more likely in places where fewer people smoke.

We begin in the most straightforward manner possible, classifying counties as high prevalence if they are in a state with greater than the median level smoking prevalence (23.2%) and the remainder as low prevalence. We divide the sample into low and high prevalence counties and repeat our basic estimations for each subsample. Obviously, this approach has some limitations. First, county-level smoking prevalence would be a preferred criterion to divide the sample but no consistently measured county-level smoking prevalence numbers are available. Second, using state-level numbers may be problematic because certain states may differ widely in the smoking prevalence across counties. Texas, California, and Virginia are listed as low prevalence states in Table 3 but many counties within these states have higher than average prevalence of smoking. Third, our division of the sample is a rather blunt instrument forcing us, for example, to classify Florida as a low prevalence state at 23.2% but Iowa, North Dakota, and Oklahoma as high prevalence at 23.3%. We note, however, that while Florida passed a smoking ban and is part of the treatment group, Iowa, North Dakota, and Oklahoma did not, nor did any municipality within these states. Thus, there is a bit of a gap in the smoking prevalence rates in the treatment group around the cutoff prevalence. Understanding the limitations of this analysis, we still think that estimates on these subsamples are informative and, more importantly, serve as a check on our basic difference-in-difference research design. A greater effect in the higher prevalence states is indicative that the earlier estimates reflect non-spurious effects of bans.

The results reported in the top panel of Table 5 suggest that the impact of smoking bans on bar employment is greater in places with a high prevalence of smokers. The point estimates jump to nearly a 12% employment reduction that is significant at the .10 level. In the low prevalence states, the negative effect is smaller (less than 5%) and not significant. The imprecision of the estimates precludes concluding that the differences by smoking prevalence are significant, but they do provide confidence that the basic finding of negative effects on bars reflects a real policy effect.

As for restaurants, the differences in the estimates by prevalence are large enough to judge statistically significant. There is a 2.4% reduction in employment in the high prevalence states that falls short of statistical significance but a positive and significant effect in low prevalence states. These estimates

Table 5 – Effects of Smoking Bans by Smoking Prevalence in the State

	Bars	*Restaurants*
Subsample analysis		
Effect of smoke-free ordinance in	-0.119	-0.024
high prevalence states	(0.064)	(0.018)
Sample size	7,100	18,637
Number of counties	679	1,372
Effect of smoke-free ordinance in	-0.047	0.028
low prevalence states	(0.022)	(0.013)
Sample size	9,148	20,928
Number of counties	849	1,465
Full sample analysis		
Ordinance's effect	-0.071	0.010
	(0.031)	(0.012)
Effect interacted with the de-meaned	-0.018	-0.003
smoking prevalence rate	(0.014)	(0.006)
Sample size	16,248	39,565
Number of counties	1,528	2,837

Note: Reported are coefficient estimates from a fixed effects model, with standard errors in parentheses. The standard errors are corrected to allow for non-independence of observations from the same county. Controls for the log of overall employment and state specific beer and cigarette taxes are included in each specification. High prevalence and low prevalence states were separated by whether they were above or below a smoking rate of 23.2%, which is both the mean and the median smoking prevalence in the year 2000.   Smoking prevalence values used in the interaction were differenced from the mean smoking prevalence (23.21%) prior to being interacted with the policy variable. Therefore, the point estimates of the law by itself illustrate the effect of a smoking ban provided that a location has the mean level of smoking prevalence.

suggest that the neutral effect on restaurant employment reported in Table 2 potentially masks what amounts to a meaningful difference in the effect of smoking bans across different regions of the country. Effects are potentially

negative in places where many people smoke but potentially positive where fewer people smoke. This is sensible because bans in the latter communities might encourage patronage among members of a larger pool of non-smokers.[14]

In the bottom panel, we check the basic results in the top panel using the whole sample rather than the subsamples divided by smoking prevalence. First, we subtract the mean smoking prevalence across counties from the actual smoking prevalence. Then, we interact this de-meaned smoking prevalence with the policy variable (a dummy set to one if a county has a smoking ban) and add this term to our basic specification with controls. By subtracting the mean, we can interpret the coefficient on the uninteracted dummy policy variable as the effect of a smoking ordinance in a county that has the mean smoking prevalence of 23.2%. The interaction effect then allows us to adjust this mean county effect by using how much the county deviates from mean prevalence. More specifically, the effects of smoking bans in a county in a jurisdiction with the mean smoking prevalence is a 7.1% reduction in bar employment and a 1.0% increase in restaurant employment. The effect is only significant for bars, however. The interaction effects, although insignificant, suggest that if a county is in an area with an above mean smoking prevalence, the negative bar effect would be greater and the positive restaurant effect would be lower. Eventually, the restaurant effect turns negative if the mean smoking prevalence becomes high enough.

These estimates allow us to calculate the expected impacts of smoking bans given the smoking prevalence in a locality. For example, assume a county in Indiana with 27% smoking prevalence enacts smoking bans in bars and restaurants. This 27% prevalence is 3.8 percentage points above the mean. This would imply that the effect of a ban on bars would be a 13.94% reduction (-13.94 = -7.1 + (3.8 x -1.8)) in employment, which is highly significant considering the standard error of the estimate for the mean county. The effect on restaurants would be a 0.14% reduction (-0.14 = 1.0 + (3.8 x -0.3)) in employment. This suggests a strong negative effect on bars in areas of high smoking prevalence but non-positive effects on restaurants. We can also look at what a smoking ban would do in a low smoking prevalence area. For example, if a county in Colorado with 20.1% (3.1% below the mean) smoking prevalence enacts smoking bans, bar employment would fall by only 1.52% (-1.52 = -7.1 + (-3.1 x − 1.8)). On the other hand, restaurant employment would increase by 1.93% (1.93 = 1.0 + (-3.1 x − 0.3)). Thus, where smoking prevalence is low, negative effects of smoking bans

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[14] One potential concern with these results is that low smoking prevalence areas may have more voluntary "non-smoking" establishments prior to the ban. This may attenuate effects of bans. We have no dependable way to measure the prevalence of voluntary bans but assume that the positive restaurant effects observed in low prevalence areas are if anything understated.

on bars are mitigated and positive restaurant effects emerge. This story is consistent with the subsample analysis presented in the top panel of Table 5.

D. Results by Climate

The data allow us to explore another potentially important source of cross-regional differences in effects of smoking bans. Specifically, much of the earlier work that found positive or no effects of smoking bans used data from warm weather communities.[15] On the other hand, Hyland at el. (2000), who sampled cold weather Erie County, New York, found slightly higher unemployment rates in restaurants during the winter months, suggesting that climate may play an important role. Thus, in order to truly assess the policy implications of our results, it is important to understand how differences in climate may influence the effects. The most likely way that climate might matter is through the greater potential of outdoor seating in warmer places. This could mitigate negative employment effects in bars and accentuate positive effects in restaurants.

We first divide our sample into counties in warm weather states and counties in cold weather states much like we did with the basic smoking prevalence estimates.[16] Obviously, the division is imperfect. Warm weather states like California and Arizona contain some counties that have warm and cold seasons. Likewise, some counties within these states are too warm in the summer to reasonably warrant outdoor seating, a question we will address below. Our initial aim, however, is to provide the simplest division possible to provide a check on the basic results and to add some additional insight into the potential for heterogeneous effects of smoking bans.

The basic results by climate are presented in Table 6. The effects on bars do not appear to differ by climate. The point estimates suggest a greater negative effect in warm states, but these are too imprecisely estimated to draw any meaningful inference. The restaurant effects, however, are only positive in the warm weather states. Essentially, warm weather restaurants can shift their smoking section outside allowing more options for smokers and non-smokers, but in the colder climates, smoking sections are basically eliminated completely for a greater portion of the year. The more pronounced difference by climate for restaurants is sensible if one considers that the option of outdoor seating is more

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[15] See Glantz and Smith, 1994, 1997; Huang at el., 1995; and Goldstein and Sobel, 1998.
[16] Warm States: AL, AZ, AR, CA, DE, FL, GA, HI, KS, KY, LA, MD, MS, MO, NV, NM, NC, OK, SC, TN, TX, VA, WV. Cold States: AK, CO, CT, DE, ID, IL, IN, IA, ME, MA, MI, MN, MT, NE, NH, NJ, NY, ND, OH, OR, PA, RI, SD, UT, VT, WA, WI, WY.

Table 6 – Effects of Smoking Bans by Climate

	Warm states	Cold states	P-Value for test of difference
Bars			
Effect of smoke-free ordinance	-0.083	-0.023	.4180
	(0.071)	(0.020)	
Sample size	5,579	10,669	
Number of counties	529	999	
Restaurants			
Effect of smoke-free ordinance	0.029	-0.012	.0574
	(0.016)	(0.015)	
Sample size	20,134	19,431	
Number of counties	1,548	1,289	

Note: The dependent variable is the log number of employees. Reported are coefficient estimates from a fixed effects model, with standard errors in parentheses. The standard errors are corrected to allow for non-independence of observations from the same county. Controls for the log of overall employment, cigarette taxes, and beer taxes are included in each specification.

attractive and readily available for restaurants. Moreover, in response to smoking ordinances, many establishments rush to add outdoor seating.[17] Since these often require permits, establishments with pre-existing outside options fare better following smoking bans. Restaurants in warmer climates fit this description.

As a further check on the climate results, we consider that the option to sit outdoors and the comfort in doing so varies across the year in both warm and cold weather areas, but in different ways. In cold weather climates, sitting outdoors in the hotter months is an option. Thus, a test of our outdoor seating hypothesis would be to check whether the large difference in restaurant effects by climate disappear in the hotter months, when outdoor seating in cooler regions is at least as attractive as it is in warmer regions. Therefore, given we have data by quarter, we suspect that the effects by climate should vary much more in the first and fourth quarters of the year (October – March) than in the middle two quarters

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[17] Anecdotal evidence on this abounds. See, for example, Rolland (2006) and Wright (2000).

Table 7 – Effects of Smoking Bans by Climate and Season

	Warm states	*Cold states*	*P-Value for test of column difference*
Bars			
Smoke-free ordinance x dummy for "summer" season (April – September)	-0.082 (0.071)	-0.034 (0.021)	.5156
Smoke-free ordinance x dummy for "winter" season (October – March)	-0.083 (0.071)	-0.011 (0.022)	.3220
Sample size	5,579	10,669	
Number of counties	529	999	
Restaurants			
Smoke-free ordinance x dummy for "summer" season (April – September)	0.025 (0.016)	0.025 (0.021)	.9960
Smoke-free ordinance x dummy for "winter" season (October – March)	0.033 (0.016)	-0.046 (0.019)	.0020
Sample size	20,134	19,431	
Number of counties	1,548	1,289	

Note: Reported are coefficient estimates from a fixed effects model, with standard errors in parentheses. The standard errors are corrected to allow for non-independence of observations from the same county. Controls for the log of overall employment, cigarette taxes, and beer taxes are included in each specification.

(April – September). Since the outdoor seating story more readily applies to restaurants, we are most interested in results by climate and season for eating establishment rather than drinking establishments.

Our test proceeds by continuing to divide the sample by warm and cold weather states. We now interact the smoking ordinance dummy with a dummy for the October-March "winter" season and the April-September "summer"

season. Given that we have quarter and county dummies in the specification, the coefficient on each of these interactions represent the respective policy effects of smoke-free ordinances at different points of the year on restaurant and bar employment in different climates.

We report the results in Table 7. As with Table 6, there are no meaningful differences that emerge for bars. For restaurants, however, the results are strongly supportive of the outdoor seating hypothesis. We see no difference in the positive effects of smoking bans on restaurants in the summer in warm and cold climates (a p-value near one). Thus, colder climates can also benefit from smoking bans in the summer because outdoor seating is possible. In the winter months, however, smoking bans increase employment for restaurants in warm climates but significantly reduce employment in cold climates. In the latter, there is no option to dine outside. In the former, there are certainly options to maintain consistent outdoor seating as the average temperatures in counties in our treatment group are quite attractive in the winter. For example, the average high temperature for both Tempe in February and Tampa in December is 72 degrees Fahrenheit according to the Weather Channel (www.weather.com).

Taken together, the Table 6 and Table 7 estimates suggest that the ability to offer outdoor seating can significantly affect how a smoke-free ordinance will impact the restaurant business. For bars, however, the negative effects of ordinances seem present in all climates and seasons. It is only places with a very low percentage of smokers that can escape the negative effects.

E. Discussion

The employment results presented in this paper address the basic policy question of what the overall economic impacts of smoking bans have been on the restaurant and bar industries. Since employment is a highly variable input in restaurants and bars and varies directly with customer demand, we contend the estimates reflect a negative effect on the bar industry and a neutral to positive effect on the restaurant industry.

The results trigger some additional questions. If it is true that the restaurant industry benefits in some areas of the country from these regulations, or at least is not hurt, why do restaurant associations fight the implementation of these laws so vigorously? As discussed earlier, due to some form of strategic behavior, the incentive to individually ban smoking may not exist even though profits would be higher if a cooperative solution was reached. Of course, even if this is the case, it does not explain why restaurant associations would fight a market-wide smoking regulation. The solution to this paradox may rest in the concept of information failure. If it is true that restaurant owners are not fully aware of the positive cooperative outcome of banning smoking in their

establishments due to uncertainties about consumer valuations of clean air and smoker-friendly environments, then their perception about the impact of smoking regulation would be consistent with their contrarian actions.

It is also worth considering why restaurants and bars are influenced so differently. Although bars and restaurants are similar industries, there are important reasons why smoking may matter differently to both. One might argue that a restaurant is primarily selling food, with drinks secondary, and environment or atmosphere of lesser concern. Clean air is more conducive to enjoying food, especially among non-smokers who may be more likely to come to a restaurant following a ban. Bars, on the other hand, sell environment and atmosphere first, with perhaps drinks second and food third. Given that a smoking ban fundamentally changes the environment of an establishment, the observed negative impact on drinking establishments is not surprising. Moreover, part of the bar environment is the fellow patrons, which in many cases attract customers to a particular drinking establishment. It is therefore possible that a smoking ban alters the environment for non-smokers, leading them to shy away from bars as well following a ban. On a related point, it is easy to envision a situation where a group of friends, half of whom are smokers, all stop frequenting a bar because the change in the environment discourages the smokers from attending.[18] This perhaps explains why the negative impact on bars hits all types of counties, whether warm or cold or whether smoking prevalence is low or high, although the impact is strongest in the latter.[19] The main point is that there are plausible explanations for the different impacts of smoking regulations on these similar industries. Moreover, the differing effects in the bar and restaurant industries may alleviate some mild concerns of selection bias (i.e., counties that can most easily absorb smoking bans being the ones most likely to pass the bans) driving the results.

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[18] Smokers may also be traveling to nearby jurisdictions that still permit smoking. As more bar bans are passed in the future and evidence on establishment openings and closures become available, it would be interesting to explore questions of cross border shopping, as has been done with the purchase of cigarettes (Gruber et al., 2003). One might expect bars to open in jurisdictions that allow smoking near the borders of jurisdictions that do not. Enclaves for potential smokers and drinkers may form.

[19] It should also be noted that estimates of negative effects of smoking bans on bars and relatively neutral effects on restaurants are consistent with the observation that there are far more bans on smoking in restaurants than in bars. This may stem from the simple fact that passing smoking bans on the bar industry is more difficult due to the valid concern of potential adverse economic effects.

V. Conclusion

Over the past 15 years, smoke-free campaigns have moved from their first major victories in California to widespread success resulting in hundreds of bans across the United States. The bulk of these successes have occurred recently, and this paper sought estimates of the effects of these bans on employment in the bar and restaurant industries. The main finding is that smoking bans in general negatively affect bars, but have a neutral to positive effect on restaurants.

We explored the heterogeneity of the impact of laws, looking first at whether the smoking prevalence in a region determines the relative effect of the laws. The negative effect on bars is much stronger in areas where a lot of people smoke, but it remains negative in low prevalence areas as well. There are positive effects on restaurants in localities with fewer smokers, but there are potentially negative effects in localities with many smokers. The differences in the effects by smoking prevalence provide a robustness check on the basic results of the paper but also serve to suggest that characteristics of communities and their population might influence the impact of smoking bans.

The potential differing impacts of bans across regions also motivated our investigation of effects by climate. We did not find strong differences in the results for bars by climate, but the positive effect of bans on restaurant employment was concentrated in warm weather climates. Perhaps non-smokers are attracted to restaurants following bans in all climates, but the offsetting negative effects on smokers is mitigated in warm areas, where there is a more consistent availability of outdoor seating that gives smokers an option. This advantage in warm areas disappears in the summer as restaurants in both cold and warm climates seem to benefit from outdoor seating. In cold climates in the winter, however, bans negatively affect restaurant business.

The negative impact on bar employment and positive impact on restaurants in some areas, however, must be weighed against other potential effects of the legislation. Specifically, more work is needed to quantify the health effects of the legislation. From a policy perspective, this is less important for restaurants as the economic and health effects are in the same direction. For bars, however, the net effect of smoking bans is unresolved. In addition to determining the health benefits, the negative bar effects observed in this paper may indeed signal some positive secondary effects. For example, patrons may be less likely to drink to excess if they cannot smoke in a bar, which may have its own health benefit. Bans may also influence other illegal behaviors correlated with excessive alcohol consumption.

Appendix Table 1 – State and Municipalities with Smoking Bans, by State

Municipality	State	Smoke-Free Restaurants	Smoke-Free Freestanding Bars
Bethel	AK	10/24/98	
Barrow	AK	2/15/02	
Dillingham	AK	12/31/03	
Juneau	AK	1/2/05	
Highfill	AR	7/12/03	
Fayetteville	AR	2/1/04	
Guadalupe	AZ	5/9/02	5/9/02
Tempe	AZ	5/21/02	5/21/02
Coconino County	AZ	2/3/04	
San Luis Obispo	CA	8/3/90	8/3/90
San Diego County	CA	7/28/91	7/28/91
Chico	CA	7/21/92	12/6/96
Berkeley	CA	12/24/92	9/17/96
Shasta County	CA	2/5/93	2/5/93
Visalia	CA	3/3/93	
Davis	CA	3/24/93	3/24/93
Clayton	CA	10/21/93	
San Jose	CA	12/23/93	10/22/98
Fremont	CA	1/1/94	
Monterey County	CA	1/7/94	
Santa Clara County	CA	2/4/94	2/4/94
Livermore	CA	2/10/94	
Pleasanton	CA	3/1/94	
Santa Clara	CA	4/8/94	
Modesto	CA	5/18/94	
Mountain View	CA	7/14/94	
San Juan Bautista	CA	8/19/94	
Union City	CA	11/1/94	
Shafter	CA	11/10/94	
California (State Law)	CA	1/1/95	1/1/1998
Salinas	CA	4/7/95	1/1/98
Millbrae	CA	1/1/98	1/1/98
Laguna Hills	CA	1/1/98	1/1/98
Pittsburg	CA	1/1/98	1/1/98
Alameda County	CA	1/1/98	1/1/98
El Cerrito	CA	1/1/98	1/1/98
Pasadena	CA	7/29/98	
Napa	CA	11/20/98	11/20/98
Newark	CA	8/8/99	8/8/99
Calexico	CA	4/21/00	

Rancho Cucamonga	CA	4/7/01	4/7/01
Santa Barbara	CA	7/26/02	7/26/02
Dublin	CA	11/15/02	11/15/02
Mission Viejo	CA	4/17/03	4/17/03
San Mateo	CA	9/4/03	9/4/03
San Carlos	CA	11/27/03	11/27/03
Laguna Woods	CA	9/12/04	9/12/04
Tiburon	CA		12/4/92
Snowmass Village	CO	5/1/01	5/1/01
Alamosa	CO	11/6/01	
Louisville	CO	10/8/02	10/8/02
Pueblo	CO	1/1/03	1/1/03
Fort Collins	CO	10/1/03	10/1/03
Greeley	CO	12/4/03	12/4/03
Longmont	CO	1/10/04	1/10/04
Breckenridge	CO	6/1/04	6/1/04
Dillon	CO	6/1/04	6/1/04
Frisco	CO	6/1/04	6/1/04
Silverthorn	CO	6/1/04	6/1/04
Summit County	CO	6/1/04	6/1/04
Boulder County	CO	12/19/04	12/19/04
Connecticut (State Law)	CT	10/1/03	4/1/04
Delaware (State Law)	DE	11/27/02	11/27/02
Florida (State Law)	FL	7/1/03	
Effingham County	GA	11/21/02	11/21/02
Loganville	GA	5/10/03	
Valdosta	GA	2/18/04	
Gwinnett County	GA	4/1/04	
Buena Vista	GA	4/2/04	4/2/04
Berkeley Lake	GA	5/1/04	
Douglas	GA	6/26/04	
Tifton	GA	10/9/04	
Peachtree City	GA	10/19/04	
Rockdale County	GA	11/1/04	
Columbia County	GA	1/1/05	
Maui County	HI	1/1/03	
Honolulu	HI	7/1/03	
Idaho (State Law)	ID	7/1/04	
Wilmette	IL	7/1/04	7/1/04
Bloomington	IN	1/1/05	1/1/05
Monroe County	IN	1/1/05	1/1/05
Lawrence	KS	7/1/04	7/1/04
Lexington	KY	10/1/03	10/1/03
Andover	MA	8/13/94	

Amherst	MA	9/1/94	7/1/95
Belmont	MA	1/1/95	1/1/95
Lexington	MA	2/15/95	2/15/95
Arlington	MA	6/15/95	9/15/99
Sharon	MA	6/25/95	6/25/95
Reading	MA	7/1/95	7/1/95
Wakefield	MA	7/1/95	7/1/95
Wayland	MA	1/1/97	
Chilmark	MA	5/7/97	7/1/01
Belchertown	MA	8/1/97	2/1/98
Wellfleet	MA	1/1/98	1/1/98
Cohasset	MA	3/28/98	3/28/98
Fitchburg	MA	7/1/98	
Stoneham	MA	9/30/98	9/30/98
Chatham	MA	1/1/99	1/1/99
Hingham	MA	1/2/99	1/2/99
Orleans	MA	1/2/99	1/2/99
Wellesley	MA	1/27/99	
Holliston	MA	2/1/99	7/15/02
Melrose	MA	3/1/99	3/1/99
Duxbury	MA	4/1/99	4/1/99
Oak Bluffs	MA	7/22/99	7/1/01
Monterey	MA	8/1/99	8/1/99
Scituate	MA	10/1/99	10/1/99
Brewster	MA	11/1/99	11/1/99
Truro	MA	1/1/00	1/1/00
Somerville	MA	1/1/00	10/1/03
Great Barrington	MA	4/1/00	4/1/00
Barnstable	MA	4/3/00	4/3/00
Yarmouth	MA	4/3/00	4/3/00
Marblehead	MA	4/15/00	
Maynard	MA	7/1/00	7/1/00
Leicester	MA	9/11/00	
Wareham	MA	10/15/00	10/15/00
New Braintree	MA	1/5/01	1/5/01
Woburn	MA	3/8/01	
Salem	MA	4/1/01	4/1/01
Barre	MA	4/2/01	4/2/01
Richmond	MA	6/6/01	6/6/01
Tyringham	MA	6/26/01	6/26/01
Bourne	MA	7/1/01	7/1/01
Edgartown	MA	7/1/01	7/1/01
Sandwich	MA	7/1/01	7/1/01
Tisbury	MA	7/1/01	7/1/01

Acushnet	MA	7/1/01	
Whately	MA	7/1/01	
Needham	MA	8/2/01	8/2/01
Nantucket	MA	9/1/01	9/1/01
Plymouth	MA	9/1/01	9/1/01
Southborough	MA	9/1/01	9/1/01
Bellingham	MA	10/29/01	
Dover	MA	11/4/01	11/4/01
Hubbardston	MA	12/1/01	12/1/01
Braintree	MA	1/1/02	1/1/02
Marion	MA	1/1/02	1/1/02
Weymouth	MA	1/2/02	1/2/02
Lincoln	MA	2/21/02	2/21/02
Brimfield	MA	3/1/02	3/1/02
Norton	MA	3/11/02	3/11/02
Westwood	MA	3/28/02	3/28/02
Easthampton	MA	4/1/02	
Walpole	MA	6/3/02	6/3/02
Egremont	MA	7/17/02	7/17/02
Easton	MA	7/19/02	7/19/02
Provincetown	MA	10/1/02	10/1/02
Uxbridge	MA	10/5/02	
Norfolk	MA	1/1/03	1/1/03
Mashpee	MA	4/1/03	4/1/03
Williamstown	MA	4/1/03	4/1/03
Saugus	MA	5/5/03	5/5/03
Boston	MA	5/5/03	5/5/03
Watertown	MA	5/5/03	5/5/03
Westport	MA	5/15/03	5/15/03
Wrentham	MA	7/1/03	7/1/03
Carver	MA	7/1/03	7/1/03
Hancock	MA	7/1/03	7/1/03
Beverly	MA	8/1/03	8/1/03
Peabody	MA	8/19/03	8/19/03
Abington	MA	9/1/03	9/1/03
Bridgewater	MA	9/1/03	9/1/03
Chelsea	MA	9/1/03	9/1/03
Stockbridge	MA	9/15/03	9/15/03
Lee	MA	9/15/03	9/15/03
Lenox	MA	9/15/03	9/15/03
Cambridge	MA	10/1/03	
Concord	MA	10/24/03	10/24/03
Essex	MA	11/3/03	11/3/03
Hopkinton	MA	12/20/03	1/31/02

Dedham	MA	1/1/04	1/1/04
Freetown	MA	1/1/04	1/1/04
Littleton	MA	1/1/04	1/1/04
Middleton	MA	2/1/04	2/1/04
Medfield	MA	6/27/04	6/27/04
Framingham	MA	7/5/04	7/5/04
Massachusetts (State Law)	MA	7/5/04	7/5/04
Lynn	MA	7/5/04	7/5/04
Revere	MA	8/2/04	8/2/04
Newton	MA	9/7/04	9/7/04
Haverhill	MA		9/1/02
Montgomery County	MD	10/9/03	10/9/03
Takoma Park	MD	11/10/03	11/10/03
Rockville	MD	2/1/04	2/1/04
Talbot County	MD	4/3/04	
Maine (State Law)	ME	1/1/04	1/1/04
Maryville	MO	6/9/03	
Metcalfe	MS	9/3/02	9/3/02
Helena	MT	9/1/01	9/1/01
Bozeman	MT	11/1/02	
Lincoln	NE	1/1/05	1/1/05
Dona Ana County	NM	2/8/02	2/8/02
Roswell	NM	8/1/04	8/1/04
Dutchess County	NY	1/1/03	
Nassau County	NY	3/1/03	3/1/03
Suffolk County	NY	3/19/03	3/19/03
New York City	NY	3/30/03	
Westchester County	NY	6/4/03	6/4/03
New York (State Law)	NY	7/24/03	7/24/03
Tompkins County	NY	8/25/03	8/25/03
Columbus	OH	6/28/04	6/28/04
Powell	OH	8/3/04	8/3/04
Worthington	OH	9/7/04	9/7/04
Upper Arlington	OH	9/13/04	9/13/04
Bexley	OH	1/2/05	1/2/05
Grandview Heights	OH	1/2/05	1/2/05
Corvallis	OR	7/1/98	7/1/98
Philomath	OR	12/27/00	12/27/00
Eugene	OR	6/25/01	6/25/01
Alvin	TX	10/21/99	
Rollingwood	TX	6/4/01	6/4/01
El Paso	TX	1/2/02	1/2/02
Robinson	TX	5/9/02	
Round Rock	TX	2/1/03	

Dallas	TX	3/1/03	
Copperas Cove	TX	7/1/04	7/1/04
Woodway	TX	7/14/04	7/14/04
Utah (State Law)	UT	1/1/95	
South Burlington	VT	5/3/04	5/3/04
Winooski	VT	7/19/04	7/19/04
Williston	VT	11/22/04	11/22/04
Burlington	VT		5/1/04
Pierce County	WA	1/3/04	1/3/04
Berkeley County	WV	9/15/01	
Lincoln County	WV	7/1/02	7/1/02
Marshall County	WV	7/1/02	
Tucker County	WV	3/1/03	3/1/03
Marion County	WV	5/1/04	
Braxton County	WV	7/1/04	7/1/04
Webster County	WV	7/15/04	7/15/04
Greenbrier County	WV	7/18/04	

Note: Data were gathered from Americans for Non-smokers Rights (www.no-smoke.org).

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