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W J Bartosch and G C Pope

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TOBACCO SALES/ECONOMIC IMPACT

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See end of article for authors' affiliations

Correspondence to: William J Bartosch, Center for Health Economics Research, 411 Waverley Oaks Road, Suite 330, Waltham, MA 02452, USA; bbartosch@her-cher.org

Objective: To determine if restaurant business declines or improves after the implementation of restrictive restaurant smoking policies.
Design: Analysis used a pre/post-quasi-experimental design that compared town meals tax receipts before and after the imposition of highly restrictive restaurant smoking policies in adopting versus non-adopting communities. The effect of restaurant smoking policies was estimated using a fixed effects regression model, entering a panel of 84 months of data for the 239 towns in the study. A separate model estimated the effect of restaurant smoking policies on establishments that served alcohol.
Main outcome measure: Change in the trend in meals tax revenue (adjusted for population) following the implementation of highly restrictive restaurant smoking policies.
Results: The local adoption of restrictive restaurant smoking policies did not lead to a measurable deviation from the strong positive trend in revenue between 1992 and 1998 that restaurants in Massachusetts experienced. Controlling for other less restrictive restaurant smoking policies did not change this finding. Similar results were found for only those establishments that served alcoholic beverages.
Conclusions: Highly restrictive restaurant smoking policies do not have a significant effect on a community's level of meal receipts, indicating that claims of community wide restaurant business decline under such policies are unwarranted.

Smoke-free restaurant policies can be highly controversial, with some restaurant owners, restaurant lobbying groups, and the tobacco industry voicing concerns over the potential adverse effects of such policies on restaurant business.[1–5] Published research has shown that such policies are not economically harmful.[6–12] However, tobacco and restaurant industry funded studies claim that restaurant jobs are lost and/or restaurant sales decline under such policies.[13–15] While local debates over restaurant smoking have been highly contentious in many Massachusetts communities, survey data indicate most of the state's residents would continue to dine in establishments once they are smoke-free and many would frequent smoke-free establishments more often.[16 17]

For this study, we used 1992–1998 meals tax data collected by the Massachusetts Department of Revenue (DOR) to determine if restaurant business declines after the implementation of restrictive restaurant smoking policies or alternatively if restaurant business improves under such policies. Our analysis builds on previous work that we conducted for the Massachusetts Department of Public Health's Tobacco Control Program (MTCP) analysing meals tax data for cities and towns that adopted such restaurant policies between January 1992 and December 1995.[6] This study expands on our earlier work by analysing three additional years of data, allowing us to consider the economic effect of restaurant policies on a larger group of adopting communities over a longer period of time. We also employ a refined statistical methodology to estimate the impact of smoking restrictions.

For this study we identified Massachusetts communities that enacted highly restrictive restaurant smoking policies between January 1992 and 1998 and those that did not. Most of the adopting towns either completely prohibited smoking in restaurants (including bar sections of restaurants) or required separate, enclosed, and separately ventilated smoking rooms. Twelve additional localities that we categorised as highly restrictive enacted policies that placed severe restrictions on restaurant smoking (including physical segregation of smoking areas or separate ventilation systems) but may not have completely eliminated the presence of secondhand smoke in non-smoking sections. Most notably, we categorised Boston's policy as highly restrictive, even though smoking is allowed in bar areas that are segregated by a 6 foot (2 m) buffer zone. We tested the sensitivity of our findings by conducting a separate analysis that excluded these 12 cities and towns.

METHODS

This analysis used a pre/post-quasi-experimental design that compared town level meals tax receipts before and after the imposition of highly restrictive restaurant smoking policies in adopting and non-adopting communities. "Experimental" communities included cities and towns in Massachusetts that implemented highly restrictive restaurant smoking policies between January 1992 and December 1998. We used data collected by MTCP to identify each city or town's smoking ban status throughout the study period. We supplemented and verified these data using ordinance information tracked by the Americans' for Nonsmokers' Rights and survey data collected by the Massachusetts Association of Health Boards.* We relied on local policy documents and local tobacco control personnel or boards of public health to reconcile inconsistencies or fill in missing data.

Cities and towns without a highly restrictive smoking policy between 1992 and 1998 served as comparison communities. This group included communities that failed to enact any restaurant smoking policy or adopted relatively weak policies (for example, simply designating a percentage of seats as non-smoking). Our statistical analysis controlled for the presence of restaurant policies that were not highly restrictive.

*The Massachusetts Association of Health Boards surveyed communities that did not receive MTCP funding and therefore did not regularly report smoking control policy enactment to the state.

This allowed us to measure any difference in restaurant sales between non-adopting towns and towns that adopted restaurant smoking policies that were not highly restrictive.

Data

This study used taxable meals receipts data collected by the Massachusetts DOR for the period January 1992 through December 1998. These data include all expenditures subject to Massachusetts' 5% tax on meals.† DOR provided us with data for all Massachusetts cities and towns with at least 10 reporting establishments. Data for towns with fewer than 10 restaurants (n = 99) were not provided because of concerns about confidentiality. We adjusted the DOR data for inflation using the consumer price index for Boston consumers with December 1998 serving as the base month.

We also analysed data from alcohol serving establishments separately since these places might be disproportionately affected by smoke-free policies because of the positive correlation between the consumption of alcohol and tobacco. DOR gave us a subset of meals tax data that included only establishments that serve alcohol. These facilities include some restaurants, bar sections of restaurants, and free standing bars. Because the number of towns in Massachusetts with at least 10 alcohol serving restaurants reporting for the 1992–1998 study period is smaller than the number of towns with at least 10 restaurants, this analysis included data from fewer communities (79 *v* 239).

Econometric method

This study estimated the effect of restaurant smoking policies on restaurant business controlling for other factors using a panel data framework in which 84 months of inflation adjusted restaurant sales data were pooled. Our model can be expressed as follows.

$$\ln(\text{PCMEALS})_{i,t} = \alpha_i D_i + \beta_1 \text{TIME}_{i,t} + \beta_2 \text{TIMEH}_{i,t} + \beta_3 \text{TIMEO}_{i,t} + \beta_4 \text{QTR2}_{i,t} + \beta_5 \text{QTR3}_{i,t} + \beta_6 \text{QTR4}_{i,t} + \beta_7 \ln(\text{INCOME})_{i,t} + \beta_8 \text{BORDER}_{i,t} + \epsilon_{i,t}$$

where:

$\text{PCMEALS}_{i,t}$ = taxable meals receipts in town *i* per population in town *i*'s county during month *t* (entered as a natural logarithm)

D_i = town fixed effects entered as a single monthly dummy variable for each town

$\text{TIME}_{i,t}$ = simple monthly count variable

$\text{TIMEH}_{i,t}$ = variable indicating the number of months since town *i* adopted a highly restrictive restaurant smoking policy

$\text{TIMEO}_{i,t}$ = variable indicating the number of months since town *i* adopted a restaurant smoking policy that was not highly restrictive

$\text{QTR2}_{i,t}$ = second quarter

$\text{QTR3}_{i,t}$ = third quarter

$\text{QTR4}_{i,t}$ = fourth quarter

$\text{INCOME}_{i,t}$ = town *i*'s county per capita income

$\text{BORDER}_{i,t}$ = percentage of border towns with a highly restrictive smoking policy surrounding town *i* during month *t*.

We entered fixed effects for each of the 239 towns in our analysis (79 towns for our analysis of alcohol serving places). This is represented by the variable D_i, which is a dummy variable that was entered for each town, *i*. This removed all town specific factors affecting restaurant sales that do not vary over time. Our models estimate changes in restaurant sales over time rather than cross sectional differences at a point in time. By pooling towns in a single model, we assumed that the impact of smoking restrictions is the same across towns. However, we weighted the regressions by the number of restaurants in each town. This assigned greater importance to towns with a larger number of restaurants and corrected for heteroskedasticity in the regression error term, which can arise from differences in the number of establishments across communities. To correct for serial (temporal) correlation of the models' error terms that can bias statistical results, all of the models presented in this study were corrected for first order autocorrelation.

Dependent variables

This study used town level, inflation adjusted, taxable meals receipts per county population (in natural logs) as the dependent variable.‡ County per capita meals were logged so that the impact of smoking restrictions could be interpreted in percentage terms. We ran separate regressions estimating the effect of restaurant smoking policies on alcohol serving restaurants. For these models the dependent variable was the natural logarithm of town level, inflation adjusted taxable meals receipts for only those establishments that served beer, wine, or liquor.

Independent variables

Our independent variables included three time trends. The first variable, TIME, was a simple count of the months in the study (that is, January 1992 = 1 through to December 1998 = 84). This variable was entered to capture any secular trend in restaurant sales. The second time trend (TIMEH) counts the number of months since the implementation of each highly restrictive restaurant policy. To account for other policies, a third time trend counted the number of months since the implementation of each restaurant policy that was not highly restrictive. To test the sensitivity of our findings to the inclusion of this time trend, we ran separate regression models without the TIMEO variable.

Since restaurant sales may be subject to seasonal variations in business, we entered three dummy variables into our models identifying whether a town's monthly meals receipts fell into the second (QTR2), third (QTR3), or fourth quarter (QTR4) of the year. Quarter 1 was the omitted quarterly variable; therefore, the effect of this quarter on restaurant sales is found in the intercept term.

In addition, we included county per capita income (INCOME) in our models. The US Bureau of Economic Analysis calculates annual county per capita income estimates.§ We used these annual data to interpolate monthly changes in per capita income at the county level, and we adjusted the data for inflation using the Boston consumer price index . We entered the monthly per capita income estimates into the models in natural log form.

We created a town level variable measuring the percentage of towns surrounding a community that enacted highly restrictive restaurant policies (BORDER). This variable changes over time as neighbouring communities enacted or repealed highly restrictive policies. It was intended to account for any differences in meals receipts that may be attributed to variation in the adoption behaviour of bordering communities.

†The Massachusetts meals tax is imposed on establishments that serve any food and/or beverages that has been prepared for immediate human consumption and provided by the restaurant or restaurant part of the store. Restaurants include bakeries, grocery stores, food stands, etc, but only food prepared for immediate consumption is subject to the meals tax.

‡Town population by year was unavailable.
§Annual town per capita income was unavailable.

Table 1 Fixed effects regression estimates of the effect of smoke-free restaurant policies on restaurant business, 1992 to 1998

	Model 1	Model 2
	All establishments	Alcohol serving establishments
Dependent variable	ln (per capita MEALS)	ln (per capita ALCMEALS)
Independent variable Time trend (TIME)	0.0031*** (0.0002)	0.0041*** (0.0005)
Highly restrictive restaurant policy post-adoption time trend (TIMEH)	0.0002 (0.0007)	0.0021 (0.0017)
Other restaurant policy post-adoption time trend (TIMEO)	0.0004 (0.0003)	–0.0005 (0.0007)
Second quarter (QTR2)	0.1704*** (0.0039)	0.1403*** (0.0071)
Third quarter (QTR3)	0.2015*** (0.0045)	0.1499 *** (0.0082)
Fourth quarter (QTR4)	0.0902*** (0.0040)	0.0478*** (0.0074)
ln county per capita income (INCOME)	0.1831*** (0.0295)	0.1258 *** (0.0537)
Percent border towns with smoke-free policies (BORDER)	0.0785*** (0.0295)	0.0151 (0.0674)
n	19837	6557
R2 (F) without town dummies R2 (F) with town dummies	0.10 (268.36) 0.92 (983.72)	0.20 (204.22) 0.85 (441.34)

Standard errors of coefficient estimates are in parentheses.
Unit of observation is the town/month.
Regression models were weighted using town level restaurant frequency and corrected for first order autocorrelation.
Income and meals data were adjusted for inflation using the Boston area consumer price index. Base month = December 1998.
*Significant at the <0.1 level; **significant at the <0.05 level; ***significant at the <0.01 level.
Sources: Center for Health Economics Research analysis of the Massachusetts Department of Revenue meals tax database, US Bureau of Economic Analysis county per capita income estimates, and US Bureau of the Census population estimates.

RESULTS

Results of our regression models are shown in table 1. In model 1, we included all towns in Massachusetts with meals data available for the entire January 1992 through December 1998 study period. Since our dependent variable was expressed in logarithmic form, our simple time trend variable (TIME) represents the monthly meals receipts' compounded growth rate. Its coefficient can be interpreted as the monthly percentage change in restaurant sales across all towns in our study, both adopting and non-adopting communities. (Standard errors are shown in parentheses.) Model 1 shows that TIME had a significant relation to restaurant sales. Inflation adjusted per capita meals receipts for all types of restaurants grew an average of 0.31% per month (or 3.67% annually) simply as a function of time after controlling for other factors such as income, seasonal variation, and fixed characteristics of individual towns.

Deviation from the general time trend (TIME) as a result of adopting a highly restrictive restaurant smoking policy was measured by the variable TIMEH. The coefficient for TIMEH was positive but non-significant, indicating that adoption of highly restrictive restaurant policies did not cause a significant deviation from the strong underlying positive time trend in restaurant sales overall controlling for other factors. The variable TIMEH captures any post-adoption changes in county per capita meals that occur over time. An alternative specification would be to use a binary variable to measure any immediate post-adoption effect on restaurant sales. We ran our model using this alternative specification (results not shown) and failed to find a post-adoption change in restaurant sales related to the implementation of restaurant smoking polices.

To account for the presence of other restaurant smoking policies (that is, not highly restrictive), we entered a second post-adoption time trend variable, TIMEO. This variable was also non-significant, demonstrating that implementation of less restrictive smoking policies did not lead to a substantial change in adopting towns' restaurant business relative to the general trend in restaurant sales across the state.

We found that seasonal variation had a significant effect on per capita meals receipts. In model 1, the first quarter of the year (QTR1 = January through March) had 9% less spending than QTR 4 (October through December), 20% less spending than QTR 3 (July through September), and 17% less spending than QTR2 (April through June). County per capita income (INCOME) also had a significant effect on restaurant sales. Model 1 shows that a 1% increase in county per capita income raises per capita meals receipts by 0.18%. Similarly, the percentage of surrounding towns with a highly restrictive restaurant policy (BORDER) was also significant. A town that is completely surrounded by towns with highly restrictive restaurant smoking policies was estimated to have 7.85% higher monthly per capita restaurant sales than a community surrounded by no towns with highly restrictive policies. However, this finding was not robust to limiting the sample to

Table 2 Regression estimates of the annual effect of smoke-free restaurant policies on restaurant business, 1992 to 1998

	Best estimate*	Range of uncertainty†
Model 1: All establishments Time trend (TIME)	3.67%	+3.14% to +4.19%
High restrictive restaurant policy post-adoption time trend (TIMEH)	0.25%	–1.32% to +1.81%
Other restaurant policy post-adoption time trend (TIMEO)	0.53%	–0.20% to +1.26%
Model 2: Alcohol serving establishments Time trend (TIME)	4.95%	+3.63% to +6.27%
High restrictive restaurant policy post-adoption time trend (TIMEH)	2.50%	–1.48% to +6.48%
Other restaurant policy post-adoption time trend (TIMEO)	–0.65%	–2.43% to +1.13%

*Regression coefficient expressed as an annual change.
†95% confidence interval for regression coefficient expressed as an annual change.
The unit of observation for the regressions is town-month.
The study period covered January 1992 through December 1998.
The natural logarithm of the dependent variable was used in all models.
Each model included independent variables controlling for quarterly seasonal variation, income, and the adoption status of bordering towns.
Source: Center for Health Economics Research analysis of the Massachusetts Department of Revenue meals tax data.

alcohol serving establishments or to reclassifying 12 communities (including Boston) into the less restrictive smoking policy category (see below). Therefore, we believe that the bordering towns variable in model 1 is reflecting idiosyncratic, unmeasured factoring affecting non-alcohol serving restaurants business in towns bordering the 12 reclassified communities.

The dependent variable in model 2 was the natural logarithm of monthly per capita meals receipts for alcohol serving establishments. As in our previous model, TIME was positively related to restaurant sales. Model 2 estimated that monthly per capita restaurant sales among alcohol serving facilities grew by 0.4% as a function of time (4.95% annually.) The post-adoption time trend for highly restrictive policies did not vary significantly from the general time trend nor did the post-adoption time trend for other, less restrictive restaurant smoking policies.

Model 2 shows that seasonal variation had a significant effect on the monthly per capita meals receipts of alcohol serving establishments. Restaurant sales in the first quarter of the year were 5% less than in the fourth quarter (QTR4) and 15% less than in the third quarter (QTR3). The model also shows a strong relation between per capita income (INCOME) and restaurant business. A 1% rise in monthly county per capita income is expected to increase monthly per capita meals receipts by 0.12%. Our border variable was not significant in Model 2. The percentage of surrounding communities with a highly restrictive restaurant policy did not effect the level of meals receipts of establishments that serve alcohol within a given town.

Table 2 shows the point estimates and 95% confidence intervals (CI) for our trend variables (TIME, TIMEH, and TIMEO) expressed on an annual basis. Only the simple time trend was significant with a point estimate of 3.67% annually and 95% CI of 3.14% to 4.19% growth in annual meals receipts controlling for other factors included in the regression equation. Among alcohol serving establishments, restaurant sales grew slightly faster, 4.95% annually, with a 95% CI of 3.63% to 6.27%.

In model 1, the point estimates for the post-adoption time trend, TIMEH, was 0.25%, indicating that our best estimate is that per capita meals rose slightly faster after the adoption of a highly restrictive policy. However, the 95% CI for this coefficient ranged from negative 1.32% to 1.81%, indicating that we can not reject the null hypothesis of no effect of smoking policy on sales. We can rule out a large effect—either positive or negative—of smoking restrictions on restaurant sales with a high degree of statistical certainty. In model 2, TIMEH was estimated at 2.50% for alcohol serving restaurants with a range of uncertainty between –1.48% and 6.48%.

In model 1, the point estimates for the post-adoption time trend for other less restrictive restaurant smoking policies (TIMEO) was 0.53% with a 95% CI ranging from –0.20% to 1.26%. Model 2's analysis of alcohol serving establishments had a TIMEO coefficient of –0.65%. This model establishes a 95% CI that implementing less restrictive restaurant smoking policies leads to a deviation from the general trend in restaurant business between –2.43% and 1.13% per year.

We reclassified highly restrictive policies to test the sensitivity of our results to a more restrictive categorisation of policies. For this analysis, we classified the 12 cities and towns that placed relatively severe restrictions on restaurant smoking but may not have completely eliminated the presence of secondhand smoke in non-smoking sections as non-adopters.¶ Results of our sensitivity analysis were generally consistent with our main findings. Across all models, the implementation of highly restrictive restaurant smoking policies did not have a significant effect on restaurant sales. Seasonal variation and income continued to have a measurable influence on the level of per capita meals receipts. Reclassifying restaurant policies did result in a significant (at the 0.10 level) *positive* effect of less restrictive policies on restaurant business. However, this variable (TIMEO) was not significant in our sensitivity analysis of alcohol serving establishments. In addition, the portion of border towns with anti-smoking policies failed to have a significant effect on meals receipts (among all establishments and the subset of alcohol serving places).

¶For this sensitivity analysis, the 12 communities were reclassified as having adopted other less restrictive restaurant smoking policies.

DISCUSSION

Our findings indicate that highly restrictive restaurant smoking policies do not have a significant effect on a community's level of meals receipts. Controlling for other less restrictive restaurant smoking policies (TIMEO) did not change our findings. Similarly, analysing data for only those establishments that served alcoholic beverages or for alternative definitions of highly restrictive policies generated similar results. While restaurants in Massachusetts experienced an overall increase in revenue between 1992 and 1998, the local adoption of restaurant smoking policies did not lead to a measurable deviation from this strong positive trend. Our results are consistent with those of our earlier analysis of meals tax data in Massachusetts.[6] Our null finding is also similar to results of several studies that have been conducted in other regions of the country.[7–12] Also, our results are consistent with survey research that found that most Massachusetts residents would continuing dining in restaurants once they are smoke-free.[16 17]

As with any study, this research has limitations. Firstly, because the study examined aggregate restaurant receipts for each city or town, it estimates community wide impact rather than the effect on any single restaurant. It is conceivable that restaurant smoking policies adversely effect some restaurants (and possibly benefit others) within a given town, but that the effect on individual establishments or classes of restaurants is undetected in the aggregate. Secondly, our study was not a randomised controlled experiment. Communities were not randomly selected and assigned to our highly restrictive policy group. Rather, restaurant policy emerged from local decision making processes. Our study would be subject to "selection" bias if those towns that chose to enact highly restrictive restaurant policies were also those towns that were least likely to experience adverse economic effects. Finally, we did not attempt to verify systematically whether restaurants in adopting communities abided by their town's policy. While towns that enact such policies typically create a system of penalties and regularly conduct compliance checks,** failure to adhere to the policies might bias our study in favour of a null finding.

Despite this study's drawbacks, we find claims of community wide restaurant business decline under highly restrictive smoke-free policies to be unwarranted. One reason for this null finding may be that smokers are not sufficiently inconvenienced by such policies to alter substantially their demand for restaurant meals. Alternatively, non-smokers may increase their demand for restaurant meals, potentially offsetting any reductions in sales among smokers. While it is possible that highly restrictive smoking policies may favour certain classes of restaurants over others, the overall trend in restaurant sales within a given town appears unchanged as a result of such restrictions. While within community analyses may identify differential effects among classes of restaurants, this remains an unexplored area for future research.

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What this paper adds

While numerous studies have found that smoking bans have no effect on restaurant business, policies restricting restaurant smoking continue to generate controversy, particularly from tobacco and restaurant industry influences. In this analysis, researchers update a study of restaurant smoking policies in Massachusetts by adding three additional years of data and use new model specifications to test differing assumptions about hypothesised post-adoption effects. The authors continued to find no effect on restaurant business with the incorporation of new data and with alternative econometric assumptions.

Authors' affiliations

W J Bartosch, G C Pope, Center for Health Economics Research, Waltham, Massachusetts, USA

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**The city of Boston's tobacco programme, for example, employs two staff who regularly visit restaurants and tobacco vendors to ensure that they are complying with the city's environmental tobacco smoke and youth access policies.