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Exploring heterogeneity in the impact of smoking bans among early and late adopters

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HIGHLIGHTS

- We estimate the effect of smoking bans on revenue for early and late adopters.
- We exploit the staggered nature of bans with difference-in-differences.
- We are the first study to use U.S. establishment level data.
- We find similar adjustments between early and late adopters.
- Overall smoking bans do not adversely affect sales.

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ABSTRACT

This paper exploits variation in the timing of smoking bans in bars and restaurants, and examines whether sample selection drove the null results of earlier economic impact studies. An untested hypothesis posits that early adopters could better absorb the shock of bans, but among worse selected late adopters, bans would adversely impact bar and restaurant sales. We are the first U.S. study to use administrative tax records from roughly 28,000 establishments. We find similar adjustment trajectories between late and early adopters. Overall bans do not produce a significant adverse economic impact.

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1. Introduction

A recent report from the Centers for Disease Control and Prevention finds that as of December 2015, only half of the US population is protected by comprehensive smoking bans in workplaces, restaurants, and bars (Tynan et al., 2016). Most of the gains in protections were made between 2000 and 2010, where the share of the protected US population increased from 2.7% to 47.8%. Despite growing evidence from the US and Europe that smoking bans do not adversely affect the profitability of bars and restaurants (IARC, 2009; Pieroni et al., 2013), between 2010 and 2015 the share of the US population protected by comprehensive bans increased by only 1.8 percentage points.[1]

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[1] Other studies find that smoking bans may affect profitability among bars and restaurants. Adda et al. (2007, 2012), using a phone survey of pubs in Scotland and Northern England find that sales in the short- and medium-term fell by 10%, and 29% respectively, and profit margins were equally adversely impacted after Scotland adopted a smoking ban.

Table 1
The effect of smoking bans on alcohol.

	Establishment—Alcohol tax (1)	Liquor store sales—Restaurant ban (2)	Liquor store sales—Bar ban (3)
Ban	−0.022***	−0.149	−0.281
	(0.007)	(0.161)	(0.188)
Ban ∗ Late	0.003	0.0749	0.212
	(0.010)	(0.148)	(0.200)
Cities	672	229	229
Establishments	27,785	–	–
Obs.	1,382,290	5,618	5,618
$H_0 : \beta_{Ban} = \beta_{Ban*Late} = 0$ *P*-value	0	0.547	0.122
Mean of Outcome Variable (Thousands of $)	**4.59**	**4,567.27**	**4,567.27**

Notes: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ and clustered at the city. Establishment level results include establishment fixed-effects, establishment specific linear time trends, month-by-year fixed-effects. Liquor store results utilize city aggregate data reported on a quarterly basis. Aggregate results include city fixed-effects, city specific linear and quadratic time trends, quarter-by-year fixed-effects, nonlinear pre-implementation trends, and log population and log city level sales.

Concerns of tax revenue loss among local governments, during the Great Recession, may explain the slowing progress. At the same time, a paucity of evidence regarding the adjustment trajectories of early and late adopters may delay further policy action. An untested hypothesis in the literature posits that the null results in numerous studies estimating the impact of smoke-free ordinances arose from positive selection bias. Early adopters could better weather any potential losses in profitability caused by bans. The first wave of smoking bans included localities, which were unrepresentative of later waves of adopters (i.e. Utah and California). If bans were extended to all bars and restaurants, later adopters, which presumably exhibit less positive selectivity, could fare worse.

We are the first to test this hypothesis, and the first to utilize establishment level data from the US to track the adjustment of late and early adopters.[2] In doing so, we provide new evidence that adjustment paths look similar among localities that adopted bans, regardless of the timing of policy adoption.

2. Data and methods

We obtain the universe of monthly data on alcohol tax remitted by establishments authorized to sell mixed beverages in the State of Texas from 2002 through 2011. Data on smoking bans come from American's for Non-Smokers' Rights, and include cities that adopt comprehensive bans in bars or restaurants. The first city to adopt a smoking ban in Texas was Rollingwood, a small city in the Austin-Round Rock metro area. Several large cities followed suit, including Dallas, Austin, and Corpus Christi. Most, but not all, banned smoking in both restaurants and bars. Several cities only adopted bans for restaurants. For example, Fort Worth, the second largest city in the Dallas-Fort Worth metro area, only bans smoking in restaurants. We only have two cities in our data that ban smoking only in bars but not restaurants, Alpine and Kerrville.

The impetus for local smoking bans in Texas did not follow a noticeable pattern. Cities adopted smoking bans at various times under different economic conditions. For example, El Paso, a city with a population around 650,000, was one of the first to adopt a smoking ban in bars and restaurants in January 2002. A city of about the same population, Fort Worth, on the other hand, did not ban smoking in restaurants until January 2008 and still allows smoking in bars. Elected officials and voters adopted smoking bans in small and large cities with varied demographic compositions. The city of Harlingen with a population of 75,000 on the southern tip of the state, adopted a smoking ban in March 2005. Austin, the state's capital with a population over 900,000, was the next city to adopt such a ban in September 2005.

We estimate a difference-in-differences (DiD) model and exploit plausibly exogenous variation in the timing of smoking ban implementation to identify the causal impact of smoking bans on the outcome.

$$Y_{it} = \beta_0 + \beta_1 Ban_{ct} + \beta_2 Ban_{ct} * Late_c + \beta_3 X_{it} + e_i + \tau + e_i trend + \varepsilon_{it} \quad (1)$$

Our outcome variable (Y_{it}) is an inflation-adjusted measure of log alcohol tax revenue for individual establishments. The ban (Ban_{ct}) variable takes the value of 1 if the city, where the establishment operates, implemented a smoking ban during a given month–year and zero otherwise. The parameter β_2 captures the differential effect for cities that adopt after 2007, which represent the sample of late adopters.[3] The fully specified model includes establishment fixed-effects, year-by-month fixed-effects, and establishment specific linear time trends.

We considered the first ban adopted (bar or restaurant). Most cities in our sample institute both a restaurant and bar ban at the same time, many cities only ban smoking in restaurants, and some cities institute bans in bars only.[4] Four cities adopt restaurant bans before bar bans—Dallas, Corpus Christi, Victoria, and Brownsville. For these cities, we consider only the first of the two bans adopted. We restrict the sample to exclude these cities and re-estimate results. The effects are similar to the sample that includes them.[5]

Alcohol taxes in Texas have not changed during the study period. Changes to cigarette taxes were perfectly collinear with other controls. We clustered errors at the city level.

3. Results

Table 1 summarizes results on the alcohol tax remitted by establishments selling alcohol on premises. Establishments remit

2 Though, we are not the first to use establishment level data. Pieroni et al. (2013) use data from approximately 6,500 establishments from Italy, France, and Spain.

3 Several robustness checks using different years of implementation for late adopters (after 2006 and 2008) produce qualitatively similar results.

4 Forty-two of the forty-six cities that institute both bar and restaurant bans do so at the same time. Fifteen cities institute bans in only restaurants, and Kerrville and Alpine are the only cities to institute bans only in bars.

5 We considered two other approaches. The first alternative considered was to include two variables; one for bans in bars and one for restaurant bans. Unfortunately, because the majority of cities institute bans in both restaurants and bars at the same time, it is not possible to separate the effect of a restaurant ban from that of a bar ban due to collinearity. The second alternative was to include three variables for cities with (1) both bans, (2) only restaurants bans, and (3) only bar bans. This approach is also problematic because interpretation of the results of the smaller cells (i.e., the group with only bar adopters Alpine and Kerrville) may not be representative of the rest of Texas cities.

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