THE OVER TIME IMPACTS OF SMOKE FREE AIR ORDINANCES IN TEXAS

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ABSTRACT

We examine an untested hypothesis that posits that null results in early studies examining the economic impacts of smoking bans were driven by sample selection. Early adopters could better absorb the shock of bans, but among worse selected late adopters bans would adversely impact bars and restaurants. We exploit variation in the timing of ban institution among Texas municipalities and track their impact over time. We find similar adjustments trajectories between late and early adopters, but late adopters appear unaffected by bans in the long-term. Consistent with earlier studies, bans do not significantly affect bar and restaurant sales or establishment level alcohol tax expenditures.

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I.   BACKGROUND

Currently, 77.6% of the US population is protected by smoke free legislation in bars and restaurants, but the state of Texas lags behind with roughly half of the population of the state being protected by these same provisions[1]. The lack of comprehensive state legislation in Texas, has led to a patchwork of local ordinances with only 80[2] out of 1,209 Texas municipal governments having adopted smoke free ordinances in bars and restaurant as of 2016. A growing literature documents that smoking bans are linked to significant and large reduction in second hand smoke (SHS) exposure (IARC 2009.)

The literature that estimates the impact of smoking bans on the profitability of bars and restaurants across the US find that such bans do not adversely affect restaurants and bars (Eriksen and Chaloupka 2007; IARC 2009; Scollo et al. 2003.) Studies from the state of Texas suggest similar results (CDC 1995; Hayslett and Huang 2000; CDC 2002; Loomis, Shafer, and van Hasslet 2013; MGT of America, 2006.) However, several studies find that smoking bans may adversely affect bar employment and alcohol sales among bars and restaurants (Adams and Cotti 2007; Clower and Weinstein 2004.)

One of the more carefully conducted studies in this literature is a 2008 study by Fleck and Hanssen, who investigate the impact of smoke free legislation on restaurant sales in California. The authors highlight two important threats to identifying the causal impact of smoking bans in the current literature. First, the authors find that that treating municipality, county, and state level bans as homogenous is incorrect, because municipalities that adopt such policies are likely different from municipalities that have bans forced upon them by a state

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[1] See American Nonsmokers' Rights Foundation at: http://www.no-smoke.org/pdf/100ordlist.pdf
[2] See: http://www.no-smoke.org/pdf/100ordlist.pdf

mandate. For example municipal smoking bans may be adopted during periods of robust bar and restaurant sales[3]. In contrast, state level bans are more likely to be exogenous at the municipality level, since they affect a large set of municipalities with varying levels of restaurant and bar sales. Second, failure to control for trends in the outcome prior to policy adoption may lead to spurious results. The authors show that negative trends in restaurant sales prior to the implementation of the statewide ban in California could account for a share of the negative impact that was erroneously attributed to the ban.

Meyer (1995) details additional threats to identification in differences-in-differences estimation, even when there is variation in the timing of policy adoption. First, the treatment effect may be biased by other shocks that take place concurrently with policy adoption. Secondly, secular trends in outcomes, which are a function of time, could confound estimates of the policy intervention, a point also raised in Fleck and Hansen (2008). Third, the treatment group may be much different than the control group, producing results that are not generalizable to the extended set of localities. Fourth, significance in results may be overstated because of how researchers handle standard errors in the presence of serial correlation. And finally, there may be interactions of treatment within different policy settings and over time. Additional critiques in the literature also highlight the importance of careful treatment of policy interventions in panel data (Bertrand, Duflo and Mullainathan 2004; Besley and Case 2000; Fleck and Hanssen 2008.)

In this paper we investigate the impact of smoke free ordinances on restaurant and bar sales in Texas. We are able to address concerns over heterogeneity in bans, because we only consider bans taking place at the municipality level. Our paper departs from other analyses, in three ways. First, our study estimates the impact of smoke free bans by measuring the impact of

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[3] For example restaurant owners in cities which exhibit robust sales may be less likely to lobby against such bans (Fleck and Hanssen 2008).

such legislation on alcohol sales tax expenditures for establishments which became smoke free from a municipality level ordinance. To our knowledge only one prior study conducts such analyses at the establishment level (Lopez et. al, 2011), and most previous studies uses aggregate city or county level data to measure the impact of the legislation. We consider this analysis an improvement to previous studies, because we are able to control for establishment level unobservables that could drive our results. Since our analysis is at the establishment level, we are able to provide more precise estimates of the effect of smoking bans. In addition, because our establishment level analysis estimates the impact of smoke free air ordinances on the tax revenue generated by alcohol sales, we are able to track the impact of such legislation on alcohol use, a variable that is believed to be most affected by the legislation[4].

Second, with our data we are able to capture impacts of such policies up to several months after implementation. With the exception of Adams and Cotti (2007), who estimate the short and intermediate-term effects of bans on employment, no previous studies estimate the impact on restaurant and bar revenue over time. Finally, we address an untested hypothesis in the literature which posits that the null results in numerous studies estimating the impact of smoke free ordinances were due to selection bias. This hypothesis suggests that early adopters of smoke free policies could better weather any potential losses in profitability linked to the policy, as such, if smoke free policies were extended to all bars and restaurants later adopters would likely fare worse and experience larges losses in profitability (Adams and Cotti, 2007.) Nonetheless, some evidence suggests that policy adoption may shift preferences among restaurant and bar

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[4] Between 80 and 95 percent of alcoholics smoke cigarettes, a rate that is three times higher than among the population as a whole. And 70 percent of alcoholics are *more than* a pack a day smokers compared with 10 percent of the general population. See: National Institute on Alcohol Abuse and Alcoholism, No. 39, January 1998 at http://pubs.niaaa.nih.gov/publications/aa39.htm.

patrons; with currently, a majority of Americans supporting smoking bans in public places[5]. The selection and shift in preferences arguments, work in opposite directions, thus the impact of smoking bans on late adopters is ambiguous a priori.

Like previous studies, we exploit variation in timing that smoke free ordinances took effect to identify the impact of smoking bans. However, we do not assume the timing of adoption is exogenous, but employ a methodology that addresses the threats to identification outlined by Meyer (1995) and Fleck and Hansen (2008). We test the exogeneity of the timing of adoption by tracking changes in sales and tax expenditures immediately prior to policy implementation. We find evidence supportive of policy exogeneity for restaurant sales. We also find evidence that early adopters of smoking bans adopted during periods where bar sales are robust, but this effect is not present among late adopters. We subject the data to different robustness checks to make sure that our effects are not spurious and do not depend on model specification or choice of control group. When using aggregate data, we find that smoke free laws do not reduce bar and restaurant sales. Our analysis at the establishment level suggests losses in mixed beverage tax expenditures are small. We find that smoking bans reduce alcohol tax expenditure by 1.1% with the effect being significant only at the 10% level. Among municipalities that adopt smoking bans late in the study period, we largely find neutral effects to such policies on tax expenditures. Our contribution to the literature is that we conduct the analysis at the establishment level and are able to control for establishment unobservable characteristics that may affect the outcome of

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[5] Majority support for smoking bans in all public places is a recent occurrence. The first year that Gallup provides evidence that 59% of Americans support smoking bans in all public places is 2011. The trend has not changed since, with a majority of Americans supporting such bans in 2015. See: "For First Time, Majority in US Supports Public Smoking Ban" http://www.gallup.com/poll/148514/first-time-majority-supports-public-smoking-ban.aspx and "Ban on Smoking in Public Retrains Majority Support" http://www.gallup.com/poll/184397/ban-smoking-public-retains-majority-support.aspx.

interest. Finally, to our knowledge we are the first study to estimate the ordinances' effects many quarters after implementation.

II. DATA AND EMPIRICAL STRATEGY

We obtain quarterly municipality-level restaurant and bar sales data from the State of Texas beginning with the 1st quarter of 2002 through the 4th quarter of 2011. The establishment level data includes monthly data on mixed beverage tax paid by all establishments authorized to sell mixed beverages on premises for the period January 2002 through December 2011.

Table 1A provides municipalities in Texas that have adopted a smoking ban in bars or restaurants along with effective implementation dates. The smoking ban data comes from Americans for Nonsmoker's Rights Foundation. The first municipality 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. These were among the first adopters of such policies, and most – but not all - banned smoking in both restaurant 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, during our study period, that ban smoking only in bars but not restaurants, Alpine and Kerrville. One nice characteristic of the Texas data is that it covers cities of small, medium, and large sizes. Thus our results are not representative of solely large municipalities, but cover a diverse group of plausible adjustments to the policy.

Table 1B presents summary statistics of the data utilized in our analysis. The average municipality records roughly $2.8 million in bar sales and $9.5 million in restaurant sales each quarter. Adopting cities have higher bar and restaurant sales and tend to be more populous, on average. Bars are located in municipalities with more access to alcohol.

We also present summary statistics from the establishment level data. Our establishment level data is at the establishment-month level and present the average alcohol tax expenditures incurred by each establishment. On average each establishment paid $4,593 in alcohol tax per month to the State of Texas. Unlike our aggregate data, our establishment level data come from cities that are larger in population. This result is not surprising; most establishments selling alcohol are likely located in municipalities where alcohol is accessible. This is corroborated by the higher scores on the index for availability of alcohol. The group of late adopters is comprised of municipalities with smaller populations.

We estimate a difference-in-differences model below:

$$Y_{it} = \beta_0 + \beta_1 SFO_{it} + \beta_2 X_{it} + c_i/e_i + \tau + \varepsilon$$

Our outcome variables(Y_{it}) are inflation adjusted log restaurant sales, log bars sales, or log mixed beverage tax revenue.[6,7] For the aggregate data the smoke free ordinance (SFO_{it}), takes the value of 1 if a municipality has implemented a smoke free ban during a given quarter-year and zero otherwise. For the establishment level tax data where tax revenues are reported on a monthly basis, we redefine the variable to be equal to 1 if in a given month-year the establishment was smoke free and zero otherwise. Some municipalities institute such policies throughout a given quarter/month the smoke free variable takes decimal values equal to the percentage of the quarter/month that the policy was in effect. For example if a policy was introduced during the 15th day of February[8] our variable takes the value of 0.5 for the first quarter and 1 for the second quarter and subsequent quarters thereafter, a value of 0 was in effect prior to policy adoption.

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[6] All sales and tax data are expressed in 2012 dollars. Box-Cox tests were conducted to determine if outcome or control variables needed to be logged or used in raw scale.
[7] The subscript i, denotes either municipality in the aggregate data, or establishment in the establishment level data.
[8] for non-leap years.

For the establishment level data the variable would take the value of 0.5 for the month February and 1 for each month thereafter.

Our control group includes all municipalities that have no bans in bars and restaurants and municipalities that have only instituted non-comprehensive policies. Thus the interpretation of our results measures the difference in sales or taxes between municipalities that instituted smoke free policies comprehensively and municipalities with either non-comprehensive bans or no bans.

In order to capture differences in economic activity between municipalities and within municipalities over time we include log sales for general merchandise in each municipality for each quarter-year or month-year. To account for changes in demand that arise due to changes in population we include yearly log municipality population as a control in our estimation. To capture the degree of availability of alcohol, we include an index which takes the value of 0 through 7, with 0 being dry jurisdictions, and 7 being jurisdictions where alcohol is easily accessible[9].

We include municipality fixed effects/establishment fixed effects (c_i/ e_i) to address differences in sales/taxes expenditure that are municipality/establishment specific. We account for quarter-by-year/ month-by-year[10] fixed effects, (τ), to address secular changes in restaurant and bars sales and alcohol tax expenditures over time. We also address some of the threats to identification highlighted by Meyer (1995). First, to account for secular trends in outcomes, which are a function of time, and could confound the estimates of policy impact, we include

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[9] We were also concerned that smoke free ordinances may be instituted concurrently with more permissive alcohol sales ordinances. To address this concern we include any changes over time in the alcohol index. The index construction is discussed in detail in Appendix A.

[10] We have a total of 40 quarter-by-year fixed effects, of which 39 are utilized in the analysis. For the establishment level data we have data from January 2002 through September 2012, which produces a total of 129 month-by-year dummies, where the 129th dummy is excluded.

municipality specific linear and quadratic time trends and establishment specific linear time trends. The methodology allows each municipality and each establishment to have different trends in the outcome, reducing the likelihood that these trends are attributed to the policy change.

Even though we exploit the variation in timing of adoption of smoke free ordinances to measure the impact of smoke free ordinances on restaurant and bar sales, the choice of adoption may be endogenous. If municipalities choose smoke free ordinances when they experience robust restaurant and bar sales or if they believe that the expected impact on bars and restaurants to be trivial, then events immediately prior to implementation may be driving our results in the difference-in-differences specification. To address concerns over endogeneity, we include a series of dummy variables that capture differences in preexisting trends in restaurant and bar sales and alcohol tax expenditures between our treatment and control groups.

Another concern with difference-in-difference estimates is that the treatment group may be much different than the control group, producing results that are not generalizable to the extended set of municipalities. To address this concern, in addition to including our several layers of controls for unobservable effects, we estimate our results across different samples in order to uncover any spurious effects in results. First we estimate our results using our full sample, then limit the analysis only to municipalities that at some point adopt such policies, and finally compare municipalities that adopt in later periods with the extended set of municipalities. We also think that differences in treatment and control groups may be less of a concern in our data. Because we utilize the variation in timing of adoption, many of the early adopters have controls in similar municipalities that adopt later in the study period.

We would like to control for changes in cigarette taxes, alcohol taxes and prices, and other factors that could affect our outcome or could confound the effect of smoke free legislation. Alcohol taxes in Texas have not changed during the period of the study. The mixed beverage tax remained constant at 14% of the sales generated throughout the entire study period, and beer and wine taxes were last changed in the 1990s. While there were several changes to the cigarette tax during the study period these changes are perfectly collinear with other controls from estimation. All standard errors are clustered at the municipality level to allow for correlation in errors across units. In our aggregate estimates the standard errors are block bootstrapped to remove any concerns over spurious results.

III.   RESULTS

A.   Aggregate Results

The baseline results for restaurant sales are presented in Table 2. Even though we exploit the variation in timing of adoption of smoke free ordinances in our analysis, the choice of adoption may be endogenous. More robust restaurant and bar sales can lead municipalities to adopt such bans. Unfortunately the difference-in-differences estimator assumes these differences in group trends to be zero, even though differences in sales before policy implementation between treatment and control groups could contaminate the impact of smoking bans. To address concerns over endogeneity, we include a series of dummy variables that capture any differences in preexisting trends in restaurant and bar sales between our treatment and control groups. More particularly, we include dummy variables for every quarter before implementation, up to 8 quarters prior[11]. If these pre-implementation trends are positive and significant they would

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[11] We conducted this analysis by including fewer than 8 and 12 dummies prior to implementation and our results did not change.

imply that cities adopt policies during periods of time when sales are robust, and would suggest that policy adoption is in fact endogenous.

Columns 1-3 summarize results for the whole sample and include different levels of controls (city fixed effects, quarter-by-year fixed effects, and city specific linear and quadratic trends). Looking at column 1, which does not include linear or quadratic trends, the pre-implementation dummies are all close to zero and not statistically significant, suggesting that, differences in pre-trends between adopting and non-adopting municipalities do not drive our results for restaurants. On average the effect of smoking bans on restaurant sales is negative, but insignificant (column 1). Controlling for city specific linear and quadratic time trends, makes the result switch signs, and become positive but still insignificant. Our all sample results suggest that smoking bans did not affect restaurant sales.

To probe the validity of our results in the baseline specification and make sure they are not subject to choice of control group, we estimate the impact of smoking bans only among municipalities that adopted such ordinances. These municipalities are likely more similar to one another in unobservables, which we are concerned could be driving our results. Thus, treatment municipalities that adopt early in study periods are compared to other municipalities, which will at some point in the future adopt such policies, but have not done so just yet. The results are summarized in table 2, columns 4-6. The pre-implementation dummies suggest that cities may be adopting policies during times when restaurant sales are robust, but the effect of the ban is zero. Inclusion of trends in subsequent columns does not alter our results in a meaningful way.

Table 2 (columns 7-9) summarizes results among late adopters. We define late adopters (treatment) to be municipalities that instituted smoke free ordinances beginning in 2008 and

later[12]. The control group includes all other Texas municipalities that lack comprehensive bans. We conduct an additional robustness check to test whether there is a differential impact of the policy among late adopters. The results that do not contain city specific time trends, suggest a reduction in revenue of approximately 10% (column 7). But inclusion of time trends suggests that the effect of such policies on late adopters is zero (columns 8 and 9).

The other controls have the expected signs. Higher levels of economic activity are associated with higher restaurant sales. A higher population is associated with higher restaurant sales, but increased alcohol availability is not linked to increased restaurant sales – in the aggregate.

Table 3 summarizes results for bar sales. We summarize results for the entire sample (1-3), only adopters (columns 4-6) and late adopters (columns 7-9). In our entire sample results, it appears that counties that adopt a smoking ban experience higher relative bar sales two quarters before ban implementation. If we did control for pre-trends we would be wrongly attributing the drop in sales to the policy. The effect of the policy moves from negative and insignificant to positive and insignificant as we include more controls. When comparing among only cities that adopt, we find no strong pre-implementation trends, and the effect of the policy is once again close to zero. Among late adopters (columns 7-9) we find some evidence of relatively higher sales up to 3 quarters before implementation and the effects of smoking bans are not different from zero.

Overall our aggregate results suggest that there is some selection into smoking bans and that municipalities are more likely to adopt such policies during times when restaurant and bar

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[12] Several robustness checks using different years of implementation for late adopters (2007 and 2009) produce qualitatively similar results.

sales are higher. But overall, the effect of smoking bans, conditional on observables and unobservables is zero.

We conduct an additional robustness check for our results by tracking what happens to liquor store sales once smoking bans go into effect. Liquor stores are a substitute to drinking in bars and restaurants. If smokers flee bars and restaurants due to bans, then demand for liquor store alcohol purchases should increase. In appendix B, we show that, liquor store sales do not change after smoking bans take effect; further strengthening our previous results.

B.   *The Impact of Smoke Free Ordinances Over Time*

Up to this point our analysis has measured the average impact of smoking bans. However, the average effect of the policy can disguise plausible adjustment to the policy. For example, the null effects could arise if losses in revenue in the first quarter of implementation are followed by gains in revenue in subsequent quarters. Alternatively, it could be that case that the policy generates no effects at all. The average effect produced in both cases would be identical, but the policy implications may be different. If losses in revenue in the short-term are followed by increase in revenue in the intermediate- or long-term, then this information may reduce uncertainty regarding policy impacts to businesses which may wish to ban smoking on premises. To capture the impact of smoke free ordinances over time we replace our smoke free variables with a dummy for the quarter of adoption and a series of dummies variables for up to three years after policy implementation. The last category, 14th quarter also includes impacts in subsequent quarters after the 14th. If smoking bans exert any impacts over time, then these post implementation dummies will be statistically different from zero. The results are summarized in table 4 and include pre-implementation dummies from tables 2 and 3, and the full set of controls including city specific linear and quadratic time trends.

The results over time, suggest that smoke free air ordinances have no effect on restaurant and bars sales over time. This effect is consistent throughout all different samples. Most importantly these results suggest that adjustment trajectories among late and early implementers of such policies are very similar.

C. Establishment Level Results

Our establishment level analysis extends the aggregate analysis but looks at the real alcohol tax paid by individual establishments. These establishments can be either bars or restaurants which are licensed to sell mixed beverages for on premise consumption. Both bars and restaurant can have a mixed beverage permit. We have information on roughly 28,000 establishments in the period 2002-2011. Our control variable is coded 1 for municipalities that have a comprehensive ban in bars, restaurants, and both bars and restaurants. Most municipalities in our sample institute both a restaurant and bar ban at the same time, many municipalities only ban smoking in restaurants, and some municipalities institute bans in bars only. We have four municipalities that adopt restaurant bans before bar bans - Dallas, Corpus Christi, Victoria, and Brownsville. For these municipalities we consider only the first of the two policies adopted. We ran results by excluding these municipalities and the effects are similar to the sample that includes them.

We considered two other approaches before we settled on the current one. An alternative to including only one ban variable would be to include two variables; one for bans in bars and one for restaurant bans. One concern with this approach is that in places that institute bans in both restaurants and bars at the same time, it would not be possible to separate the effect of a restaurant ban from that of a bar ban. Alternatively, we could have included variables for municipalities with both bans, restaurants bans only, and bar bans only. However, we felt this

approach made some of the smaller cells (i.e. the group with only bar adopters) non-representative of the population, and plausible extension to the rest of Texas municipalities. We estimated results using these other approaches and the results were similar.

We first run difference-in-differences models of establishment level log mixed beverage tax expenditures on the ban variable while controlling for sales and population at the municipality level, establishment fixed effects and month-by-year fixed effects. We then use several levels of locality fixed effects. More particularly, we include establishment linear and quadratic time trends. The establishment and establishment time trends allow us to control for establishment level unobservables and secular time trends at the establishment level that could affect the sale of mixed beverages and as a result mixed beverage tax expenditures. It should be noted, that the mixed beverage tax rate has not changed in the study period and it is 14 percent of all taxable sales[13].

Columns 1-3 report results for the full sample, and as we move from columns 1 through 3 we add additional levels of controls for unobservables. Establishments in municipalities that institute bans on smoking appear to experience a reduction in alcohol expenditures of 2.7%. The results among only adopters (columns 4-6) and late adopters (columns 7-9) largely mirror the impact among the full sample. Among late adopters, municipalities that adopt smoking bans experience an average 2.2% reduction in alcohol tax expenditures. The other controls have the expected signs; higher overall economic activity is linked to higher alcohol tax expenditure, while the effect of log population is not statistically different from zero.

As the previous section suggests there may be reason to believe that these policies are more likely to be adopted during time of high economic activity. One concern when policies are

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[13] The mixed beverage tax changed in Texas starting January 1st, 2014, which is outside of the study period.

adopted during times of high expenditures could be that maybe alcohol tax expenditures would have fallen even absent policy intervention. Unfortunately, the difference-in-differences estimator does not account for this type of mean reversion (Autor, Donohue, and Schwab 2006). To address this concern we utilize a lagged dependent variables model, which allows for this type of mean reversion in outcomes. Results are summarized in table 6. The results suggest that the effect of such policies is about half of what we find in our baseline estimate that of table 5, which do not account for mean reversion. Among the full sample of municipalities, we find that smoking bans reduce tax expenditure by 1.5%. The results for late adopters appear to be smaller and suggest a reduction of approximately 1.3%. Since we are interested on the magnitude, and not just simply the sign of smoking bans on the outcome, for the remainder of the analysis we present both results that account for mean reversion and results that do not.

In the aggregate analysis we discovered that municipalities adopt smoking bans during times when restaurant and bar sales were high. To account for some of the dynamics before implementation we re-estimate results but include dummies to account for pre-implementation trends. The results are summarized in table 7A. First, we notice that two quarters before ban implementation alcohol tax expenditures are rising but fall the period before implementation and continue to do so after implementation. In fact, simply accounting for pre-implementation trends reduces the average impact and significance of the ban effect. The results among late adopters now suggest little evidence of a policy effect, even though the policy continues to matter in the full sample results with a reduction in alcohol tax expenditure of 2.5%. Table 7B presents the lagged dependent variable models. Once we account for mean reversion and also pre-implementation trends, the effects get smaller, all less than 1.5%, and in either insignificant, or only significant at the 10% level. Thus the results that for both pre-implementation trends and

mean reversion would suggest no significant impact of smoking bans on alcohol tax expenditures.

D. The Impact of Smoking Bans Over Time at the Establishment Level

To estimate the impact of smoke free ordinances over time, we replace our ban variable with a series of dummies tracking the quarter of implementation and several quarters post implementation. We include dummies for up to three years after ban implementation, beginning with the first quarter through the 13th quarter of implementation. The dummy for 13th quarter after implementation includes any subsequent periods after that (i.e. for more than three years). All specifications include different levels of fixed effects as the results in tables 5 and 6 as well as pre-implementation trends for the 4 quarters prior to policy adoption.

Results are summarized in Tables 8A and 8B. We interpret the results from the specification which includes establishment specific linear and quadratic time trends, as these specifications likely pick up several unobservable effects that may matter in determining the outcome. Overall the results suggest that the effect of the policy is immediate and it persists over time for the full sample and the sample of adopting municipalities. However, the effects for late adopters suggest a different pattern of adjustment. Among late adopters group, periods of negative effects are followed by periods of no effect.

Table 8B accounts for mean reversion and includes the lagged dependent variable. We find that the pattern of coefficients resembles that of table 8A, but the results are much smaller. One significant difference is the lack of impact after the 6th quarter post implementation for late adopters. Among late adopters the policy experts an effect up the 6th quarter, but it fails to produce additional impacts after that.

IV.   DISCUSSION

In this paper we use both aggregate and establishment level data to measure the impact of smoke free ordinances on restaurant and bar sales and mixed beverage tax expenditures among Texas municipalities and establishments. We utilize the fact that different municipalities adopt smoking bans at different points in time, but do not assume that such bans are exogenous. Instead we test the exogeneity of policy adoption by tracking trends in the outcome right before policy implementation. We find that municipalities that adopt smoking bans exhibit higher than average sales prior to policy adoption, suggesting that studies that do not account for policy selection likely produce biased estimates of the true impact.

Our aggregate analyses suggest no adverse impact on sales due to the institution of comprehensive smoking bans in bars and restaurants. We then turn our attention to alcohol tax expenditures at the establishment level, and conduct analyses among a sample of 28,000 establishments. Our establishment level analyses account for establishment fixed-effects and time trends, thus reducing concerns over omitted variable bias and because of the large sample generate very precise estimates of the impact of smoking bans. Even though alcohol tax expenditures are expected to be affected disproportionally from smoking bans (Adams and Cotti, 2007, 2008[14]), we find that reductions in mixed beverage tax expenditures from smoking bans are small. More particularly, when we account for threats to identification in difference-in-differences estimation we find the effects of smoking bans have no significant effects on alcohol tax expenditures based on a two-tailed test at conventional significance levels of 5%. Our results are only significant at the 10% significance level, a finding that is surprising given the large sample size. Taken together our aggregate and establishment level results imply smoking bans do not adversely affect restaurants and bars.

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[14] Adams and Cotti (2008) find that alcohol related fatal accidents increase following bans of smoking in bars as smokers drive longer distances to bars that allow smoking indoors.

We also test the hypothesis that early adopters were better able to adjust to the effects of smoking bans. We find no support for this hypothesis. On the contrary, we find that late adopters were able to adjust to changes in policy better, with no long term impacts due to policy adoption. One implication of our over-time analysis is that the many municipalities that are currently considering joining the group of adopters will not experience negative impacts due to adoption. To our knowledge our analysis is the first to look at the long term impacts of such policies on bar and restaurant sales, and it is only the second study that estimates results at the establishment level.

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Adopting Municipalities

Municipality	Restaurant Ban	Bar Ban	Municipality	Restaurant Ban	Bar Ban
Rollingwood	6/4/2001	6/4/2001	Fort Worth	1/1/2008	
El Paso	1/2/2002	1/2/2002	Sweeny	1/1/2008	
Round Rock	2/1/2003		Benbrook	2/1/2008	2/1/2008
Dallas	3/1/2003	4/10/2009	Alton	4/1/2008	4/1/2008
Copperas Cove	11/2/2004	11/2/2004	Tyler	6/1/2008	6/1/2008
Woodway	7/14/2004	7/14/2004	Brownsville	7/1/2008	2/2/2013
Austin	9/1/2005	9/1/2005	McKinney	9/4/2008	9/4/2008
Harlingen	3/23/2005	3/23/2005	Granbury	4/15/2008	4/15/2008
Corpus Christi	9/10/2005	6/15/2009	Nacogdoches	6/27/2008	6/27/2008
Victoria	11/25/2005	6/21/2006	Robinson	11/11/2008	11/11/2008
Vernon	7/1/2006	7/1/2006	Flower Mound	1/1/2009	1/1/2009
Beaumont	8/1/2006	8/1/2006	College Station	2/1/2009	2/1/2009
Laredo	7/3/2006	7/3/2006	Rowlett	5/1/2009	5/1/2009
Portland	10/3/2006	N/A#	Killeen	6/1/2009	
Socorro	12/8/2006	12/8/2006	El Lago	1/19/2009	1/19/2009
Spring Valley	11/14/2006	11/14/2006	Mesquite	6/14/2009	6/14/2009
Baytown	11/20/2006	11/20/2006	Horseshoe Bay	8/25/2009	8/25/2009
Arlington	1/1/2007		Conroe	3/1/2010	
Angleton	3/1/2007		Missouri City	10/1/2010	10/1/2010
Rockwall	5/1/2007		Panorama Village	1/26/2010	1/26/2010
Plano	6/1/2007	6/1/2007	San Angelo	12/2/2010	12/2/2010
Southlake	6/1/2007	6/1/2007	University Park	2/16/2010	2/16/2010
Marshall	8/1/2007	8/1/2007	Ennis	6/21/2010	6/21/2010
Houston	9/1/2007	9/1/2007	Eagle Pass	9/28/2010	9/28/2010
Pasadena	9/1/2007		Highland Village	6/1/2011	N/A#
Abilene	1/3/2007	1/3/2007	Rosenberg	4/5/2011	
Boerne	3/27/2007		San Antonio	8/19/2011	8/19/2011
Brenham	7/18/2007		Frisco	11/17/2011	11/17/2011
Prosper	9/20/2007		Lewisville	10/1/2012	10/1/2012
Pearland	11/16/2007	11/16/2007	Humble	2/23/2012	
Sugar Land	12/31/2007		Kerrville		6/20/2008
			Alpine		7/12/2010

Notes: N/A# implies cities do not have freestanding bars. Data comes from American Nonsmokers' Rights Foundation, available at http://www.no-smoke.org/pdf/EffectivePopulationList.pdf.

TABLE 1B

Summary Statistics of Analysis Variables

	All	Adopters	Late
	Bar Data		
City Bar Sales	2,827.14	10,790.06	1,976.44
(Thousands of $)	(12,173.30)	(23,910.60)	(8,973.44)
City Sales	1,325,723.71	5,443,457.92	875,015.59
(Thousands of $)	(6,329,412.67)	(15,004,074.32)	(3,203,364.84)
Population	75.01	266.36	55.57
(Thousands)	(216.86)	(475.89)	(147.85)
Index	5.12	4.88	5.10
	(1.56)	(1.77)	(1.58)
N	7,597	1,207	7,050
	Restaurant Data		
City Restaurant Sales	9,447.62	49,767.80	5,285.49
(Thousands of $)	(44,806.93)	(117,425.64)	(19,040.98)
City Sales	674,970.17	3,625,168.55	346,073.60
(Thousands of $)	(4,361,216.04)	(11,774,732.14)	(1,426,339.46)
Population	40.19	195.85	25.28
(Thousands)	(151.65)	(386.85)	(79.66)
Index	4.43	4.91	4.36
	(2.02)	(1.83)	(2.03)
N	16,352	2,053	15,250
	Establishment Data		
Establishment Alcohol	4,593.46	5,375.59	3,796.35
Tax ($)	(7,512.58)	(8,284.90)	(6,278.52)
City Sales	17,458,370.29	27,411,801.37	4,103,333.96
(Thousands of $)	(28,158,108.72)	(31,710,755.85)	(7,863,292.83)
Population	658.75	1,017.88	249.91
(Thousands)	(759.69)	(759.72)	(418.33)
Index	5.55	6.06	5.04
	(1.40)	(0.88)	(1.61)
N	1,380,486	861,357	776,072

Notes: Standard deviation in parentheses.

TABLE 2
OLS Regression of Log Restaurant Sales

	All Restaurants			Restaurants - Adopters			Restaurants - Late		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
8 Quarters Before	-0.005	-0.019	-0.021	0.033	-0.014	-0.009	-0.042	-0.048*	-0.035
	(0.040)	(0.020)	(0.017)	(0.035)	(0.018)	(0.015)	(0.062)	(0.028)	(0.026)
7 Quarters Before	0.000	-0.019	-0.018	0.037	-0.018	-0.008	-0.047	-0.042	-0.025
	(0.040)	(0.019)	(0.018)	(0.035)	(0.018)	(0.017)	(0.063)	(0.031)	(0.030)
6 Quarters Before	0.019	-0.001	0.001	0.065*	0.002	0.016	-0.017	-0.011	0.006
	(0.040)	(0.022)	(0.019)	(0.034)	(0.020)	(0.016)	(0.062)	(0.038)	(0.031)
5 Quarters Before	0.036	0.008	0.013	0.089**	0.014	0.030*	0.001	-0.001	0.026
	(0.041)	(0.024)	(0.019)	(0.036)	(0.022)	(0.018)	(0.060)	(0.037)	(0.025)
4 Quarters Before	0.021	-0.008	-0.002	0.084**	0.005	0.022	-0.053	-0.043	-0.013
	(0.044)	(0.026)	(0.023)	(0.041)	(0.024)	(0.023)	(0.064)	(0.038)	(0.033)
3 Quarters Before	0.038	0.012	0.021	0.106**	0.027	0.048*	-0.054	-0.036	-0.007
	(0.042)	(0.026)	(0.026)	(0.041)	(0.025)	(0.027)	(0.060)	(0.040)	(0.035)
2 Quarters Before	0.033	0.009	0.021	0.114***	0.029	0.053*	-0.049	-0.028	0.004
	(0.041)	(0.025)	(0.025)	(0.041)	(0.027)	(0.029)	(0.057)	(0.038)	(0.032)
1 Quarter Before	0.019	-0.011	0.007	0.101**	0.006	0.037	-0.040	-0.021	0.025
	(0.042)	(0.031)	(0.027)	(0.042)	(0.030)	(0.028)	(0.056)	(0.045)	(0.039)
Restaurant Ban	-0.054	-0.035	0.006	0.083	-0.011	0.043	-0.110**	-0.020	0.053
	(0.048)	(0.033)	(0.033)	(0.070)	(0.036)	(0.036)	(0.056)	(0.053)	(0.048)
Log-Sales	0.097***	0.058***	0.045***	0.087	0.032	0.042	0.101***	0.061***	0.047***
	(0.021)	(0.016)	(0.013)	(0.055)	(0.027)	(0.033)	(0.022)	(0.017)	(0.014)
Log-Population	0.300**	0.110	0.083	0.222	0.190	0.178	0.292**	0.108	0.082
	(0.142)	(0.087)	(0.079)	(0.296)	(0.306)	(0.249)	(0.145)	(0.088)	(0.080)

Index	0.008	0.004	0.003	-0.001	0.001	0.000	0.008	0.005	0.003
	(0.006)	(0.003)	(0.003)	(0.009)	(0.006)	(0.005)	(0.006)	(0.003)	(0.003)
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Specific Linear Trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
City Specific Quadratic Trend	No	No	Yes	No	No	Yes	No	No	Yes
Number of Cities	586	586	586	56	56	56	556	556	556
N	16,352	16,352	16,352	2,053	2,053	2,053	15,250	15,250	15,250

Notes: Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered at the municipality level.

TABLE 3
OLS Regression of Log Bar Sales

	All Bars			Bars - Adopters			Bars - Late		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
8 Quarters Before	-0.078	-0.001	-0.003	-0.088	-0.022	-0.035	0.084	0.183**	0.158***
	(0.168)	(0.128)	(0.130)	(0.146)	(0.137)	(0.132)	(0.160)	(0.076)	(0.056)
7 Quarters Before	0.187**	0.257*	0.270*	0.151	0.226	0.230	0.221**	0.316	0.304
	(0.094)	(0.138)	(0.144)	(0.117)	(0.147)	(0.149)	(0.110)	(0.193)	(0.200)
6 Quarters Before	0.228**	0.298**	0.285*	0.223	0.260*	0.241	0.249**	0.355*	0.288
	(0.104)	(0.149)	(0.166)	(0.134)	(0.149)	(0.153)	(0.116)	(0.200)	(0.229)
5 Quarters Before	0.094	0.186*	0.222**	0.089	0.171	0.190	0.044	0.186*	0.189*
	(0.116)	(0.108)	(0.108)	(0.115)	(0.125)	(0.121)	(0.147)	(0.106)	(0.100)
4 Quarters Before	0.099	0.201	0.246	0.102	0.168	0.189	0.090	0.246	0.254
	(0.111)	(0.164)	(0.174)	(0.157)	(0.168)	(0.172)	(0.119)	(0.225)	(0.231)
3 Quarters Before	0.161	0.252	0.284	0.108	0.188	0.193	0.201*	0.352	0.324
	(0.115)	(0.169)	(0.182)	(0.148)	(0.168)	(0.169)	(0.117)	(0.225)	(0.241)
2 Quarters Before	0.212*	0.290*	0.315	0.173	0.202	0.199	0.229	0.365	0.316
	(0.127)	(0.174)	(0.197)	(0.160)	(0.162)	(0.166)	(0.142)	(0.234)	(0.261)
1 Quarter Before	0.081	0.139	0.224	0.058	0.094	0.137	0.033	0.140	0.182
	(0.115)	(0.140)	(0.151)	(0.145)	(0.144)	(0.137)	(0.138)	(0.191)	(0.187)
Bar Ban	-0.159	-0.076	0.067	-0.150	-0.164	-0.080	-0.376	-0.137	-0.047
	(0.151)	(0.145)	(0.159)	(0.149)	(0.136)	(0.146)	(0.238)	(0.220)	(0.220)
Log-Sales	0.003	0.069	0.042	0.198	-0.002	-0.035	-0.008	0.061	0.031
	(0.087)	(0.067)	(0.059)	(0.255)	(0.156)	(0.146)	(0.091)	(0.071)	(0.062)

Log-Population	0.786	0.324	-0.420	0.782	-0.144	-1.762	0.912	0.371	-0.401
	(0.570)	(0.435)	(0.520)	(0.907)	(1.158)	(1.701)	(0.618)	(0.446)	(0.534)
Index	0.089**	0.048**	0.042**	0.007	0.072	0.080	0.097**	0.053**	0.046**
	(0.038)	(0.024)	(0.021)	(0.034)	(0.078)	(0.074)	(0.039)	(0.025)	(0.022)
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Specific Linear Trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
City Specific Quadratic Trend	No	No	Yes	No	No	Yes	No	No	Yes
Number of Cities	279	279	279	33	33	33	265	265	265
N	7,597	7,597	7,597	1,207	1,207	1,207	7,050	7,050	7,050

Notes: Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered at the municipality level.

TABLE 4
The Impact of Smoke Free Air Ordinances Over Time

	All Restaurants	All Bars	Restaurants Adopters	Bars Adopters	Restaurants Late	Bars Late
Quarter of Adoption	-0.014	0.213	0.014	-0.032	0.047	0.238
	(0.041)	(0.205)	(0.058)	(0.225)	(0.074)	(0.306)
2nd Quarter of Adoption	-0.030	0.196	0.002	-0.064	0.031	0.191
	(0.044)	(0.207)	(0.063)	(0.240)	(0.078)	(0.321)
3rd Quarter of Adoption	-0.040	0.135	-0.007	-0.154	0.048	-0.023
	(0.048)	(0.210)	(0.068)	(0.243)	(0.084)	(0.314)
4th Quarter of Adoption	-0.011	0.137	0.016	-0.138	0.136	-0.051
	(0.053)	(0.245)	(0.072)	(0.284)	(0.100)	(0.416)
5th Quarter of Adoption	-0.017	0.093	0.010	-0.221	0.102	-0.104
	(0.055)	(0.248)	(0.077)	(0.292)	(0.103)	(0.441)
6th Quarter of Adoption	-0.023	0.077	-0.001	-0.264	0.095	-0.152
	(0.055)	(0.263)	(0.081)	(0.308)	(0.102)	(0.434)
7th Quarter of Adoption	-0.023	0.165	-0.006	-0.188	0.101	-0.015
	(0.059)	(0.258)	(0.086)	(0.315)	(0.112)	(0.417)
8th Quarter of Adoption	-0.024	0.180	-0.012	-0.203	0.163	-0.051
	(0.065)	(0.282)	(0.090)	(0.359)	(0.132)	(0.493)
9th Quarter of Adoption	-0.052	0.222	-0.042	-0.188	0.091	0.151
	(0.065)	(0.321)	(0.096)	(0.390)	(0.137)	(0.620)
10th Quarter of Adoption	-0.031	0.239	-0.024	-0.164	0.103	0.070
	(0.066)	(0.331)	(0.100)	(0.415)	(0.138)	(0.645)
11th Quarter of Adoption	-0.068	0.267	-0.062	-0.164	0.106	0.052
	(0.071)	(0.293)	(0.102)	(0.374)	(0.150)	(0.527)
12th Quarter of Adoption	-0.070	0.185	-0.068	-0.254	0.124	-0.154
	(0.076)	(0.318)	(0.108)	(0.406)	(0.171)	(0.693)
13th Quarter of Adoption	-0.090	0.160	-0.085	-0.283	0.022	-0.185
	(0.076)	(0.330)	(0.115)	(0.426)	(0.183)	(0.720)
14 or More Quarters After Adoption	-0.087	0.169	-0.081	-0.316	0.082	-0.007
	(0.080)	(0.330)	(0.124)	(0.442)	(0.186)	(0.730)

Pre-Implementation Trends	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
City-Specific Linear and Quadratic Time Trends	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of Cities	586	279	56	33	556	265
N	16,352	7,597	2,053	1,207	15,250	7,050

Notes: Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered at the municipality level.

TABLE 5
Estimates of Smoke Free Ordinances on Log Mixed Beverage Tax

	All Establishments			Only Adopters			Late Adopters		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Ban	-0.047****	-0.025****	-0.027****	-0.025***	-0.023****	-0.023***	-0.037***	-0.021****	-0.022**
	(0.011)	(0.006)	(0.007)	(0.008)	(0.006)	(0.007)	(0.012)	(0.006)	(0.010)
Log Sales	0.059****	0.062****	0.065****	0.036***	0.039***	0.038***	0.068****	0.070***	0.076***
	(0.014)	(0.017)	(0.019)	(0.013)	(0.013)	(0.013)	(0.020)	(0.023)	(0.026)
Log Population	0.058	0.058*	0.01	0.105	0.07	0.017	-0.01	0.04	0.005
	(0.044)	(0.033)	(0.028)	(0.066)	(0.047)	(0.039)	(0.047)	(0.045)	(0.042)
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear Trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Quadratic Trend	No	No	Yes	No	No	Yes	No	No	Yes
Number of Cities	670	670	670	61	61	61	639	639	639
Number of Establishments	27,768	27,768	27,768	17,115	17,115	17,115	15,779	15,779	15,779
Observations	1,380,486	1,380,486	1,380,486	861,357	861,357	861,357	776,072	776,072	776,072

Notes: Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered at the municipality level. Linear and quadratic trends are establishment specific.

TABLE 6

Estimates of Smoke Free Ordinances on Log Mixed Beverage Tax

	All Establishments			Only Adopters			Late Adopters		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
Ban	-0.020***	-0.013****	-0.015****	-0.010**	-0.013****	-0.015***	-0.017***	-0.014***	-0.013**
	(0.005)	(0.003)	(0.004)	(0.005)	(0.004)	(0.005)	(0.006)	(0.004)	(0.006)
Lagged Dep. Variable	0.578****	0.431****	0.371****	0.579****	0.424****	0.360****	0.568****	0.432****	0.376****
	(0.006)	(0.005)	-0.005	(0.008)	(0.006)	-0.005	(0.007)	(0.007)	-0.008
Log Sales	0.038****	0.049****	0.053****	0.034**	0.041**	0.040**	0.035****	0.045***	0.053***
	(0.009)	(0.014)	-0.015	(0.013)	(0.017)	-0.017	(0.011)	(0.015)	-0.018
Log Population	0.008	0.025	0.015	0.031	0.028	0.018	-0.024	0.018	0.013
	(0.021)	(0.020)	(0.018)	(0.031)	(0.028)	(0.027)	(0.024)	(0.027)	(0.026)
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear Trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Quadratic Trend	No	No	Yes	No	No	Yes	No	No	Yes
Number of Cities	670	670	670	61	61	61	639	639	639
Number of Establishments	27,768	27,768	27,768	17,115	17,115	17,115	15,779	15,779	15,779
Observations	1,346,629	1,346,629	1,346,629	840,509	840,509	840,509	756,839	756,839	756,839

Notes: Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered at the municipality level. Linear and quadratic trends are establishment specific.

TABLE 7A

Estimates of Smoke Free Ordinances on Log Mixed Beverage Tax

	All Establishments			Only Adopters			Late Adopters		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
4 Quarters Before Ban	-0.003	-0.002	-0.004	0.007	-0.003	-0.003	-0.007	0.008	0.001
	(0.010)	(0.007)	(0.008)	(0.009)	(0.008)	(0.008)	(0.010)	(0.008)	(0.009)
3 Quarters Before Ban	0.005	0.006	0.003	0.013	0.001	0	-0.011	0.005	-0.003
	(0.009)	(0.013)	(0.012)	(0.010)	(0.009)	(0.009)	(0.012)	(0.009)	(0.006)
2 Quarters Before Ban	0.017	0.018***	0.015**	0.030**	0.016***	0.015**	0.006	0.020**	0.011
	(0.012)	(0.006)	(0.007)	(0.013)	(0.005)	(0.007)	(0.013)	(0.009)	(0.011)
1 Quarter Before Ban	-0.007	-0.002	-0.005	0.011	0	-0.001	-0.015	-0.001	-0.011
	(0.011)	(0.005)	(0.006)	(0.009)	(0.007)	(0.008)	(0.013)	(0.010)	(0.012)
Ban	-0.045***	-0.020****	-0.025**	-0.016	-0.020***	-0.020*	-0.037**	-0.017*	-0.024
	(0.015)	(0.006)	(0.010)	(0.013)	(0.006)	(0.011)	(0.015)	(0.009)	(0.016)
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear Trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Quadratic Trend	No	No	Yes	No	No	Yes	No	No	Yes
Number of Cities	670	670	670	61	61	61	639	639	639
Number of Establishments	27,768	27,768	27,768	17,115	17,115	17,115	15,779	15,779	15,779
Observations	1,380,486	1,380,486	1,380,486	861,357	861,357	861,357	776,072	776,072	776,072

Notes: Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered at the municipality level. Linear and quadratic trends are establishment specific.

Estimates of Smoke Free Ordinances on Log Mixed Beverage Tax

	All Establishments			Only Adopters			Late Adopters		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
4 Quarters Before Ban	-0.003	0	-0.001	0	-0.003	-0.002	-0.006	0.003	-0.001
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)
3 Quarters Before Ban	0.002	0.006	0.006	0.005	0.001	0.003	-0.006	0.004	0
	(0.004)	(0.007)	(0.007)	(0.005)	(0.005)	(0.005)	(0.007)	(0.007)	(0.006)
2 Quarters Before Ban	0.01	0.015^{****}	0.015^{***}	0.015^{**}	0.013^{****}	0.014^{***}	0.006	0.015^{**}	0.009
	(0.007)	(0.004)	(0.005)	(0.007)	(0.004)	(0.005)	(0.007)	(0.006)	(0.008)
1 Quarter Before Ban	-0.005	0	0	0.003	0.001	0.001	-0.007	-0.001	-0.008
	(0.006)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.008)	(0.007)	(0.006)
Ban	-0.019^{***}	-0.009^{***}	-0.011^{*}	-0.007	-0.011^{***}	-0.011	-0.017^{**}	-0.010^{*}	-0.014^{*}
	(0.007)	(0.003)	(0.006)	(0.007)	(0.003)	(0.007)	(0.007)	(0.006)	(0.008)
Lagged Dep. Variable	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear Trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Quadratic Trend	No	No	Yes	No	No	Yes	No	No	Yes
Number of Cities	670	670	670	61	61	61	639	639	639
Number of Establishments	27,768	27,768	27,768	17,115	17,115	17,115	15,779	15,779	15,779
Observations	1,346,629	1,346,629	1,346,629	840,509	840,509	840,509	756,839	756,839	756,839

Notes: Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered at the municipality level. Linear and quadratic trends are establishment specific.

TABLE 8A

Over Time Estimates of Smoke Free Ordinances on Log Mixed Beverage Tax

	All Establishments			Only Adopters			Late Adopters		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
1st Quarter of Ban	-0.034***	-0.023***	-0.030***	-0.014	-0.020***	-0.025***	-0.024*	-0.017**	-0.024**
	(0.012)	(0.008)	(0.009)	(0.009)	(0.007)	(0.008)	(0.013)	(0.008)	(0.011)
2nd Quarter of Ban	-0.040****	-0.027****	-0.035****	-0.023**	-0.028****	-0.033****	-0.037**	-0.031***	-0.037***
	(0.009)	(0.007)	(0.008)	(0.009)	(0.006)	(0.008)	(0.016)	(0.010)	(0.013)
3rd Quarter of Ban	-0.025	-0.01	-0.019*	-0.005	-0.009	-0.015	-0.016	-0.007	-0.013
	(0.015)	(0.007)	(0.011)	(0.014)	(0.007)	(0.011)	(0.018)	(0.011)	(0.017)
4th Quarter of Ban	-0.042**	-0.024**	-0.036***	-0.02	-0.022**	-0.032**	-0.028	-0.019	-0.028
	(0.017)	(0.011)	(0.012)	(0.014)	(0.011)	(0.012)	(0.018)	(0.012)	(0.019)
5th Quarter of Ban	-0.044***	-0.026**	-0.040***	-0.023*	-0.027**	-0.037**	-0.022	-0.02	-0.026
	(0.016)	(0.010)	(0.015)	(0.014)	(0.010)	(0.015)	(0.019)	(0.014)	(0.022)
6th Quarter of Ban	-0.037***	-0.018	-0.031***	-0.017	-0.021	-0.030***	-0.061***	-0.065***	-0.064**
	(0.012)	(0.013)	(0.011)	(0.014)	(0.014)	(0.011)	(0.019)	(0.020)	(0.026)
7th Quarter of Ban	-0.035*	-0.013	-0.025*	-0.01	-0.012	-0.021	-0.035	-0.036	-0.037
	(0.018)	(0.014)	(0.013)	(0.018)	(0.015)	(0.015)	(0.024)	(0.032)	(0.036)
8th Quarter of Ban	-0.057***	-0.030*	-0.045***	-0.032*	-0.031*	-0.043**	-0.050**	-0.046*	-0.047
	(0.020)	(0.018)	(0.016)	(0.018)	(0.017)	(0.017)	(0.023)	(0.025)	(0.031)
9th Quarter of Ban	-0.056***	-0.029*	-0.047***	-0.033*	-0.031*	-0.046**	-0.054**	-0.057*	-0.06
	(0.019)	(0.016)	(0.016)	(0.018)	(0.016)	(0.018)	(0.025)	(0.030)	(0.038)
10th Quarter of Ban	-0.058****	-0.031**	-0.051****	-0.034**	-0.035**	-0.051***	-0.077****	-0.082***	-0.086**

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	(0.014)	(0.013)	(0.014)	(0.016)	(0.015)	(0.015)	(0.021)	(0.031)	(0.038)
11th Quarter of Ban	-0.052***	-0.023	-0.043**	-0.026	-0.024	-0.041**	-0.048	-0.054	-0.055
	(0.019)	(0.019)	(0.017)	(0.020)	(0.020)	(0.019)	(0.030)	(0.043)	(0.051)
12th Quarter of Ban	-0.072****	-0.041**	-0.062****	-0.044**	-0.041**	-0.058***	-0.066***	-0.073**	-0.072*
	(0.020)	(0.020)	(0.018)	(0.018)	(0.019)	(0.019)	(0.023)	(0.029)	(0.040)
13th Quarter of Ban and After	-0.074****	-0.032*	-0.059****	-0.039**	-0.033*	-0.057***	-0.065***	-0.069**	-0.066
	(0.020)	(0.017)	(0.017)	(0.016)	(0.018)	(0.019)	(0.024)	(0.035)	(0.050)
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear Trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Quadratic Trend	No	No	Yes	No	No	Yes	No	No	Yes
Number of Cities	670	670	670	61	61	61	639	639	639
Number of Establishments	27,768	27,768	27,768	17,115	17,115	17,115	15,779	15,779	15,779
Observations	1,380,486	1,380,486	1,380,486	861,357	861,357	861,357	776,072	776,072	776,072

Notes: Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered at the municipality level. Linear and quadratic trends are establishment specific.

TABLE 8B

Over Time Estimates of Smoke Free Ordinances on Log Mixed Beverage Tax

	All Establishments			Only Adopters			Late Adopters		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1st Quarter of Ban	-0.015***	-0.009**	-0.013***	-0.007*	-0.010****	-0.013***	-0.011*	-0.009*	-0.013*
	(0.005)	(0.004)	(0.005)	(0.004)	(0.003)	(0.004)	(0.006)	(0.005)	(0.007)
2nd Quarter of Ban	-0.017***	-0.012***	-0.016***	-0.009	-0.016***	-0.020***	-0.020**	-0.019***	-0.023***
	(0.006)	(0.004)	(0.005)	(0.007)	(0.005)	(0.006)	(0.009)	(0.007)	(0.008)
3rd Quarter of Ban	-0.006	0.002	-0.002	0.003	-0.001	-0.005	0.003	0.004	0.002
	(0.008)	(0.005)	(0.007)	(0.007)	(0.005)	(0.008)	(0.010)	(0.009)	(0.012)
4th Quarter of Ban	-0.023**	-0.014**	-0.021**	-0.013	-0.016**	-0.023**	-0.017*	-0.016**	-0.019**
	(0.009)	(0.007)	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)	(0.007)	(0.009)
5th Quarter of Ban	-0.017***	-0.009*	-0.016**	-0.008	-0.014**	-0.022**	-0.014	-0.014	-0.015
	(0.005)	(0.005)	(0.008)	(0.006)	(0.006)	(0.009)	(0.009)	(0.010)	(0.013)
6th Quarter of Ban	-0.018***	-0.007	-0.013	-0.008	-0.012	-0.020**	-0.035***	-0.044****	-0.044***
	(0.006)	(0.009)	(0.008)	(0.007)	(0.009)	(0.009)	(0.011)	(0.012)	(0.016)
7th Quarter of Ban	-0.012	0	-0.008	-0.001	-0.004	-0.012	-0.005	-0.014	-0.012
	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.012)	(0.013)	(0.021)	(0.025)
8th Quarter of Ban	-0.028***	-0.014	-0.022*	-0.016	-0.019	-0.028**	-0.029**	-0.031**	-0.026
	(0.010)	(0.011)	(0.012)	(0.010)	(0.011)	(0.013)	(0.012)	(0.015)	(0.020)
9th Quarter of Ban	-0.024****	-0.011	-0.020**	-0.014**	-0.018*	-0.028**	-0.024**	-0.033	-0.029
	(0.007)	(0.008)	(0.010)	(0.007)	(0.009)	(0.012)	(0.012)	(0.020)	(0.027)
10th Quarter of Ban	-0.023***	-0.011	-0.022*	-0.011	-0.017	-0.030**	-0.038***	-0.049**	-0.047

	(0.007)	(0.010)	(0.011)	(0.008)	(0.010)	(0.012)	(0.013)	(0.023)	(0.030)
11th Quarter of Ban	-0.021**	-0.006	-0.017	-0.008	-0.011	-0.024*	-0.013	-0.024	-0.021
	(0.010)	(0.012)	(0.013)	(0.010)	(0.013)	(0.014)	(0.012)	(0.027)	(0.035)
12th Quarter of Ban	-0.033***	-0.017	-0.030**	-0.019*	-0.022*	-0.037**	-0.034**	-0.043**	-0.038
	(0.011)	(0.012)	(0.013)	(0.010)	(0.012)	(0.014)	(0.013)	(0.017)	(0.027)
13th Quarter of Ban and After	-0.030****	-0.01	-0.024*	-0.013	-0.016	-0.035**	-0.026**	-0.036	-0.031
	(0.009)	(0.011)	(0.013)	(0.008)	(0.013)	(0.015)	(0.011)	(0.025)	(0.035)
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear Trend	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Quadratic Trend	No	No	Yes	No	No	Yes	No	No	Yes
Number of Cities	670	670	670	61	61	61	639	639	639
Number of Establishments	27,768	27,768	27,768	17,115	17,115	17,115	15,779	15,779	15,779
Observations	1,346,629	1,346,629	1,346,629	840,509	840,509	840,509	756,839	756,839	756,839

Notes: Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered at the municipality level. Linear and quadratic trends are establishment specific.

Appendix A

Definition and Construction of Alcohol Accessibility Index Variable

Definitions	Index Value
Dry: The term "dry" means the jurisdiction is dry throughout. No type of alcoholic beverage sales is permitted.	0
Wet: The term "wet" describes a jurisdiction where a type of alcoholic beverages is permitted.	1-7

Texas jurisdictions allow for the sale/consumption of alcoholic beverages under the following rules:
1. B-On: Sale of Beer for on premises consumption authorized.
2. B-Off: Sale of Beer for off-premises consumption authorized.
3. W-On: Sale of Wine for on-premises consumption authorized.
4. W-Off: Sale of Wine for off-premises consumption authorized.
5. DS-Off: Sale of distilled spirits for off-premises consumption authorized.
6. MB: Sale of mixed beverages authorized. Not used to describe areas where sale of mixed beverages only authorized in restaurants.
7. RM: Sale of mixed beverages authorized but only in restaurants.

We define and index as the summation of all the possible options of sale/consumption in a jurisdiction in a year. For example, a jurisdiction that allows beer and wine off premises would have an index value of 2. In contrast, a jurisdiction that allows beer and wine both on and off premises and the sale of distilled spirits for off premise consumption would take a value of 5.

Appendix B

Theoretically, if smoking bans in bars reduce clientele among smokers, but do not affect clientele among non-smokers, then we should observe that smokers are pushed to substitutes for bars – such a liquor stores. In our case, this would suggest that demand for alcohol in liquor stores increases after ban implementation. Table B1 summarizes results of what happens to log liquor sales in municipalities with a 100% smoking ban in bars. The fully specified results suggest no change in alcohol sales. Even though the parameter is negative, it is statistically insignificant. We also estimate the results for restaurant bans, another venue where alcohol is consumed, and find that smoking bans in restaurants do not lead to changes in economic activity for liquor stores.   Table B3 presents results over time. We find that these results reinforce our findings from those in tables B1 and B2. Restaurant bans do not affect liquor store sales. Bar results also suggest zero effect. Column 1 results find that bar bans increase clientele – nonsmokers now may be more likely to go out to bars. However, this effect goes away once we account for non-linear time trends.

TABLE B1

OLS Regression of Log Liquor Sales on Bar Ban

	(1)	(2)	(3)
Bar Ban	-0.268*	-0.222*	-0.215
	(0.150)	(0.132)	(0.181)
Log-Sales	0.044	0.033	0.051*
	(0.045)	(0.032)	(0.030)
Log-Population	0.804****	0.163	0.129
	(0.234)	(0.234)	(0.150)
Pre-Trends	Yes	Yes	Yes
City FE	Yes	Yes	Yes
Quarter-by-Year FE	Yes	Yes	Yes
City Specific Linear Trend	No	Yes	Yes
City Specific Quadratic Trend	No	No	Yes
Number of Cities	229	229	229
N	5618	5618	5618

Notes: Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered at the municipality level.

TABLE B2

OLS Regression of Log Liquor Sales on Restaurant Ban

	(1)	(2)	(3)
Restaurant Ban	-0.168	-0.105	-0.127
	(0.121)	(0.116)	(0.126)
Log-Sales	0.044	0.035	0.051*
	(0.046)	(0.032)	(0.030)
Log-Population	0.814****	0.167	0.122
	(0.243)	(0.232)	(0.151)
Pre-Trends	Yes	Yes	Yes
City FE	Yes	Yes	Yes
Quarter-by-Year FE	Yes	Yes	Yes
City Specific Linear Trend	No	Yes	Yes
City Specific Quadratic Trend	No	No	Yes
Number of Cities	229	229	229
N	5618	5618	5618

Notes: Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered at the municipality level.

TABLE B3

The Impact of Bans on Liquor Stores Over Time

	Bar Ban	Bar Ban	Restaurant Ban	Restaurant Ban
Quarter of Adoption	-0.118	-0.155	-0.007	-0.09
	(0.076)	(0.146)	(0.099)	(0.110)
2nd Quarter of Adoption	-0.236*	-0.277	-0.086	-0.179
	(0.121)	(0.236)	(0.123)	(0.168)
3rd Quarter of Adoption	-0.187	-0.235	-0.069	-0.163
	(0.122)	(0.255)	(0.129)	(0.180)
4th Quarter of Adoption	-0.245*	-0.291	-0.15	-0.212
	(0.126)	(0.266)	(0.131)	(0.195)
5th Quarter of Adoption	-0.253**	-0.303	-0.072	-0.146
	(0.120)	(0.269)	(0.129)	(0.200)
6th Quarter of Adoption	-0.231*	-0.297	-0.074	-0.168
	(0.133)	(0.294)	(0.133)	(0.213)
7th Quarter of Adoption	-0.206	-0.269	-0.087	-0.187
	(0.136)	(0.315)	(0.135)	(0.227)
8th Quarter of Adoption	-0.250*	-0.31	-0.098	-0.202
	(0.137)	(0.324)	(0.140)	(0.232)
9th Quarter of Adoption	-0.282**	-0.338	-0.116	-0.231
	(0.130)	(0.327)	(0.139)	(0.236)
10th Quarter of Adoption	-0.262*	-0.328	-0.094	-0.221
	(0.139)	(0.348)	(0.157)	(0.251)
11th Quarter of Adoption	-0.246*	-0.331	-0.085	-0.233
	(0.141)	(0.357)	(0.165)	(0.258)
12th Quarter of Adoption	-0.260*	-0.328	-0.12	-0.255
	(0.143)	(0.362)	(0.153)	(0.262)
13th Quarter of Adoption	-0.295**	-0.355	-0.134	-0.27

	(0.143)	(0.365)	(0.154)	(0.268)
14 or More Quarters After Adoption	-0.229	-0.34	-0.06	-0.244
	(0.160)	(0.390)	(0.174)	(0.287)
Pre-Implementation Trends	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Quarter-by-Year FE	Yes	Yes	Yes	Yes
City-Specific Linear and Quadratic Time Trends	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes
Number of Cities	229	229	229	229
N	5618	5618	5618	5618

Notes: Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Errors clustered at the municipality level.