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Drunk driving after the passage of smoking bans in bars ☆

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Abstract

Using geographic variation in local and state smoke-free bar laws in the US, we observe an increase in fatal accidents involving alcohol following bans on smoking in bars that is not observed in places without bans. Although an increased accident risk might seem surprising at first, two strands of literature on consumer behavior suggest potential explanations — smokers driving longer distances to a bordering jurisdiction that allows smoking in bars and smokers driving longer distances within their jurisdiction to bars that still allow smoking, perhaps through non-compliance or outdoor seating. We find evidence consistent with both explanations. The increased miles driven by drivers wishing to smoke and drink offsets any reduction in driving from smokers choosing to stay home following a ban, resulting in increased alcohol-related accidents. This result proves durable, as we subject it to an extensive battery of robustness checks.
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1. Introduction

Levitt and Porter (2001) show that drunk drivers impose an externality per mile driven of at least 30 cents because of their greater likelihood of causing fatal accidents. In this paper, we identify a source of increased alcohol-related car accidents that has previously gone unnoticed — the prohibition of smoking in bars. The list of countries that have bans on smoking in public places continues to grow and now spans six continents. Even within countries without bans, many states, provinces, counties, cities, and villages have banned smoking.[2] Although other aspects of smoking bans have been studied, we are the first to investigate the effect of bans on drunk driving.

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[2] The Americans for Non-Smokers' Rights (www.no-smoke.org) maintains a frequently updated list of such bans.

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The expected effect of smoking bans on drunk driving is ambiguous. Many would suspect a decline as smokers go to bars less often. Recent studies (Adams and Cotti, 2007; Adda et al., 2007) indeed show evidence consistent with bar patronage falling as a result of smoking bans being implemented. A closer look reveals that an increase in alcohol-related accidents might be more likely, however. First, coverage of smoking bans is not universal. In the US, many communities with smoking bans border counties without bans. Bars subject to bans have reported losing customers to nearby communities (McCormick-Jennings, 2007). Moreover, even within communities or states with smoking bans, anecdotal evidence of rampant non-compliance by bars abound (e.g. Widenman, 2006; Falgoust, 2004). Given this non-compliance, along with the fact that most bans exempt outdoor seating and patios, it would be hardly surprising if the private cost associated with finding one of these other numerous locations to smoke and drink falls short of the private benefit some consumers get from continuing to be able to smoke and drink. When one further considers that, according to the National Institute on Alcohol Abuse and Alcoholism (NIAAA), approximately 46 million adults used both alcohol and tobacco in 2006 (roughly 24% of licensed drivers who are of legal drinking age),[3] and approximately 6.2 million adults reported *both* an alcohol-use disorder (AUD) and dependence on nicotine (NIAAA, 2007), it is certainly plausible that there might be an increase in drunk driving risk even if the average bar's business declines. If the miles driven by smokers to avoid a ban are great enough to offset the potential reduction in driving by smokers who stay home to avoid the ban, then there will be in increase in alcohol-related accident risk.

To test whether we observe a change in alcohol-related accidents following smoking bans, we use county-level data from the US, a country without a national smoking ban but a large number of states, counties, and municipalities that have individually passed bans at different points in time, the vast majority of which were implemented over the past 6 years. This presents a natural laboratory to demonstrate the effects of smoking bans. Previous research has used this variation to test for effects of smoking bans on bar and restaurant employment (Adams and Cotti, 2007). In this paper we use this variation to test for changes in fatal accidents involving alcohol following smoking ban implementation. Our estimates reveal a significant increase in the danger posed by drunk drivers following the passage of bans. Specifically, our preferred estimate indicates that fatal accidents involving a drunk driver increase by about 13%. This is approximately 2.5 fatal accidents a year for a typical county.[4] Although a striking result and one that must be considered by policymakers, we show later in the paper that the effect is plausible in light of estimates in the literature regarding the effects of other policies.

As we will demonstrate below, these estimates are robust to the inclusion of controls for area and time fixed effects, changes in population, changes in other policies that may impact drunk driving behavior (e.g. beer taxes, blood alcohol content regulation), as well as changes in factors that may influence overall driving risk separate from drinking behavior (e.g., construction, weather, etc.). Furthermore, these estimates are also robust to several alternative definitions of the control group, the dependent variable, the policy variable, the level of analysis, and to the estimation method selected (e.g., weighted least squares, negative binomial, etc).

We test the underlying mechanism of the smoking ban-drunk driving relationship by investigating specific cases where smokers might reasonably be expected to cross into another jurisdiction where smoking and drinking are allowed, and consistent with the expectations, we find pronounced evidence of increased fatal accidents in border jurisdictions as well. We also engage in additional case studies where crossing a border is not possible due to the geographic isolation of the jurisdiction passing the laws. In many of these cases, we find increases in alcohol-related accidents, suggesting that non-compliance within a jurisdiction is potentially causing some accidents as well.

Ultimately, we view the findings in this paper as a warning that the heightened risk posed by drunk drivers must be addressed by local and state governments when they ban smoking in their bars. If not, they face an unanticipated social cost of an ordinance that is intended to improve people's health. Alternatively, the results are suggestive that a well-enforced ban that covers the entire nation, thereby eliminating the attractiveness of crossing a border to smoke and drink, might avoid the heightened risk posed by drunk drivers.

2. Background on US smoking bans and theoretical considerations

According to the Americans for Nonsmokers' Rights group, nearly one-third of the US population lives in communities that have banned smoking in bars. More bans in the US are inevitable. Ordinances were initially enacted at the local level,

[3] Number of licensed drivers obtained from U.S. Department of Transportation, Federal Highway Administration, *Highway Statistics 2003*.

[4] Details on this calculation will be provided in the paper.

but as of January 2007, California, Colorado, Connecticut, Delaware, Hawaii, Maine, Massachusetts, Ohio, New Jersey, New York, Rhode Island, Vermont, and Washington had prohibited smoking in bars. Although the prevalence of these laws is higher in urban areas, statewide bans have resulted in many rural bars being smoke-free as well.

Most existing research has focused on the impact of smoking bans on bar business. Because bans are a relatively new phenomenon in the US, the initial evidence was mixed and often depended on subjective opinions of bar owners (Hyland et al., 1999; Dunham and Marlow, 2000). The only fixed effects estimation on a panel of US jurisdictions passing smoking bans on bars observed a decrease in bar staffing following bans that is not observed in a control group of counties without bans (Adams and Cotti, 2007). Outside the US, one recent study of Scottish public houses found evidence consistent with a reduction in bar patronage following the Scottish smoking ban. This reduction was relative to a comparison group of pubs in northern England (Adda et al., 2007). The long run impact on bar business will likely take more years of data and study to resolve.[5]

As mentioned in the introduction, at first glance one might be inclined to think that the danger posed by drunk drivers might decrease following bans if smokers choose not to go out as often or not stay at their favorite bar as long. It may also be likely, however, that many consumers will choose to find alternative locations to smoke and drink. Even if the number of bar patrons falls, the patrons choosing to find new locations to drink and smoke may still increase the total number of miles driven to and from bars. This will result in a heightened accident risk.

Two strands of existing literature on consumer behavior support this contention. First, the cross border shopping literature informs that people will consume what they desire in another location in the presence of limits or relatively high costs on consumption in their own jurisdiction (e.g., Asplund et al., 2007 and Ferris, 2000). When this is legal, establishment enclaves bordering high cost jurisdictions can flourish, as with shopping malls and outlet stores in Pennsylvania near the border of New Jersey (Pennsylvania has no sales tax on clothing). When cross border shopping is not legal, smuggling occurs, as is the case with cigarettes (e.g., Chaloupka and Warner, 2000 and Gruber et al., 2003). Some products cannot be smuggled, however, so Canadians cross the border to consume health services in the US and, in the case of smoking bans in bars, people may want to consume cigarettes and alcohol across the border of their jurisdiction. For example, the state of Delaware banned smoking in bars and their neighbor Pennsylvania did not. It is possible that some people in Wilmington, Delaware might wish to cross the border to smoke and drink. This might impose an increased danger to drivers in border counties of both states. In fact, some bar owners have blamed their loss of business on cross border shopping (McCormick-Jennings, 2007).

Second, although cross border shopping would cause greater distances driven by intoxicated motorists, drunk driving might increase even if consumers remain within their jurisdiction. If sufficient demand exists, bars will risk non-compliance with a law or set up special outdoor areas where smoking and drinking are allowed. Ample anecdotal evidence suggests that some bars indeed risk non-compliance (e.g., Widenman, 2006; Falgoust, 2004). Others build outdoor patios following bans (e.g., Rolland, 2006). Therefore, after a ban, there is more product differentiation between bars. Smokers will drive to another bar if their additional costs do not cause their total costs to exceed their benefits from finding a bar where smoking is permitted. In fact, Lee (1997) applied a Loschian (1954) location model to describe the hexagonal market areas created by bar service differentiation. He posits that bar differentiation leads to more drunk driving. Non-compliance and outdoor seating represent sources of differentiation in terms of ability to smoke, so a heightened drunk driving risk is anticipated as consumers drive farther to find the bar characteristic they desire.

Both the cross border and product differentiation hypotheses are bolstered by the fact that cigarettes and alcohol are highly complementary (Dee, 1999a). Moreover, smokers are perhaps more likely to pose a danger on the road than typical bar patrons. According to NIAAA, between 80 and 95% of alcoholics smokes cigarettes — a rate that is three times higher than among the population as a whole. Approximately 70% of alcoholics are *heavy* smokers (i.e., smoke more than one pack of cigarettes per day), compared with 10% of the general population (NIAAA, 1998). Moreover, Di Franza and Guerrera (1990) finds individuals who smoke are about ten times more likely to be alcoholics. Since smokers are the individuals we would most expect to increase their driving to evade a smoking ban and a disproportionate number of alcoholics are smokers (heavy smokers at that), then it is logical to suggest that a smoking ban encourages travel by the individuals who are most likely to drive while exceedingly intoxicated.

[5] Another strand of evidence investigates the relationship between smoking bans and heart attacks. The initial work on this shows a decrease in heart attacks following passage of an indoor smoking ban (Sargent et al., 2004). We recognize that the increase in drunk driving deaths following smoking bans must be weighed against other health gains from bans, both potentially short term in the case of a reduction in heart attacks, and long term in terms of less exposure to second hand smoke by patrons and employees. For this reason, we do not think our findings should be considered an indictment of smoking bans. Rather, potential drunk driving risks must be addressed by communities passing bans.

Looking beyond the economic literature, there is growing evidence from laboratory experiments in neuroscience that nicotine inhibits the intoxicating effects of alcohol in the brain (Bachtell and Ryabinin, 2001; Rohrbaugh et al., 2006). So, if smoking typically mitigates a smoker's level of intoxication, then the removal of the nicotine from the bar could increase the level of intoxication of consumers leaving the bar. Psychological experiments by Palfai et al. (2000) find that nicotine deprivation increases the urge to drink and the volume of alcohol consumed. This would suggest that smokers would be inclined to drink more in a smoke-free environment then would otherwise be the case and, therefore, an increased prevalence of impaired driving results. If smokers drink more in the absence of smoking or are influenced by the alcohol in a stronger way, or both, the passage of a smoking ban could increase the number of alcohol-related accidents by increasing the number of intoxicated drivers. Although none of the aforementioned studies test driving ability specifically, the interactive effect of alcohol consumption and nicotine are pronounced enough that a connection between smoking and drunk driving is reasonable speculation.

In the remainder of the paper, we investigate whether these theoretical predictions are indeed verified by observing the US experience of banning smoking in bars. We find substantial evidence that the number of fatal accidents involving alcohol increases after smoking bans. We also provide additional evidence consistent with both the cross border shopping and product differentiation hypotheses.

3. Data and methods

3.1. Data sources on smoking bans and fatal accidents

Smoking bans have been passed in every region of the US at the state and local level. For this study, we identify the set of jurisdictions that enacted smoking bans from 2000 to 2005, a period in which many of the bans in existence in the US were passed.[6] Information on dates and coverage of smoking bans was obtained from the Americans for Nonsmokers' Rights. Table 1 lists the jurisdictions with smoking bans that we study, which compose the treatment group for our estimates.

We link these data on state, county and city smoking bans to data on fatal vehicle crashes obtained through the Fatality Analysis Reporting System (FARS) of the National Highway Traffic Safety Administration (NHTSA). Our primary variable of interest is the annual number of fatal accidents in a county for which a driver's imputed blood alcohol content (BAC) exceeds 0.08. Federal law requires BAC levels be obtained from every fatal crash, but this frequently is not done. Therefore, using only cases with measured BAC drastically understates the number of crashes where alcohol was a factor and this understatement will vary across jurisdictions, causing substantial bias in any estimation. The NHTSA is aware that this poses a problem to empirical research and public discourse. As a result, the FARS data allow for imputation of the BAC for all drivers who were not tested via a multiple imputation procedure yielding ten different simulated BAC measures for each driver in every accident. These values are obtained using the multitude of characteristics in each case, including factors such as time of day, day of week, contents of the police report, position of car in the road, etc. (NHTSA, 2002). This follows suggestions from Rubin et al. (1998) and improves on the former procedure based on discriminant analysis (Klein, 1986; NHTSA, 2002). While previous studies using counts generated from older FARS data used imputed values based on discriminant analysis or relied on counts generated from accidents that were more likely to be alcohol-related (e.g., crashes on weekend evenings), more recent studies use data generated by the new procedure (e.g., Villaveces et al., 2003; Hingson et al., 2005; Cummings et al., 2006). Although we are certain that our counts of fatal accidents involving alcohol by county-year are the most accurate possible, one must still be aware that estimated effects of policies may be biased if the rate of the need to impute information is systematically related to the policy in question. There is little concern that this is the case here, as smoking bans should not affect how officers investigate a crash scene, as opposed to changing BAC requirements for example.[7]

[6] The cutoff for 2005 was the enactment of a ban by mid-year. Locations enacting bans later in 2005 are excluded from the sample due to insufficient information to analyze effects of the ban.

[7] As noted above, other studies restrict attention to certain types of accidents, such as whether an accident took place on the weekend, during the evening, age of drivers, etc. to estimate whether policies affect alcohol-related crashes or fatalities. We do not need to do this because our multiple imputation procedure uses a combination of all of this information to generate highly accurate counts of alcohol-related accidents in a county-year directly. Moreover, to the extent that some heavy drinkers who smoke may choose to evade smoking bans during off peak times where enforcement or bar compliance might be more lax, we do not want to restrict our attention to any one characteristic of an accident.

Table 1
Jurisdictions with effective smoking bans large enough to be analyzed in the data, 2000–2005

Jurisdiction passing ban	Effective dates of ban for purposes of this paper's analysis	Treatment County(ies)
Effingham County (Georgia)	2002–2005	Effingham County
El Paso County (Texas)	2002–2005	El Paso County
Lincoln County (West Virginia)	2002–2005	Lincoln County
Delaware	2003–2005	Delaware counties
New York	2003–2005	New York counties
Lexington (Kentucky)	2004–2005	Fayette County
Maine	2004–2005	Maine counties
Montgomery County (Maryland)	2004–2005	Montgomery County
Connecticut	2004–2005	Connecticut counties
Burlington (Vermont)	2004–2005	Chittenden County
Lawrence (Kansas)	2004–2005	Douglas County
Massachusetts	2004–2005	Massachusetts counties
Roswell (New Mexico)	2004–2005	Chavez county
Webster County (West Virginia)	2004–2005	Webster County
Boulder County (Colorado)	2005	Boulder county
Columbus (Ohio)	2005	Franklin County
Lincoln (Nebraska)	2005	Lancaster County
Minneapolis (MN)	2005	Hennepin County
Rhode Island	2005	Rhode Island counties
Tift County (Georgia)	2005	Tift County

Note: Only counties for which we have sufficient data for 2000–2005 are analyzed in the sample. Any bans enacted in the latter half of 2005 are not included in the analysis and the information from the county is dropped. Several counties (e.g., Suffolk County, New York and Suffolk County, Massachusetts) are included in the treatment group as soon as they pass the ban, with the remainder of the state added later. A total of 117 counties compose the treatment group. Benton County, OR and California passed bans prior to 2000. Given they provide us with no identifying information, they are not included above.

Following NHTSA procedures that are used to generate their official statistics, we aggregate our counts of fatal accidents involving a driver with a BAC content exceeding 0.08 by county. We can link annual fatal accident counts to other data available by county annually (e.g., population data from the US Census Bureau). Moreover, annual counts provide us with a sufficient number of accidents for each county upon which to base the analyses.

Our policy variable is a dummy variable indicating that a county has a smoke-free bar law in place for a given year. Since counties are subsets of states, the only problem may emerge with city bans, where the city is a subset of the county population. In each case, we attempted to only identify those counties that we judged to be predominantly smoke-free because of a ban. The cutoff we use is whether half of the county's population fell under the provisions of the law.[8] Since we expect going smoke-free to be a process for some bar owners, especially for those wishing to build patio seating, we suspect that some adjustments could be made after passage of a ban but before actual enactment of the ban. For this reason, we code bans enacted by the summer to be effective for a given year. This seems most appropriate to detect effects of the policy, but later in the paper we check whether alternative measures of the timing of laws influence our results. We also test for the presence of lead and lagged effects. Our results will prove robust.

For our main estimates, we include only counties that have greater than zero fatal accidents for the six years of our study to facilitate our use of logs of the dependent variable in the basic estimation. This excludes some small counties with zero estimated accidents. We verify their exclusion causes no meaningful change in the results. We also exclude counties that are difficult to classify as treatment or control counties (e.g., the counties mentioned above that have small communities within them passing bans; these are too small to consider the county smoke free but also render the county questionable as a control case). We exclude a few additional counties that passed bans later in 2005 — too late to be

[8] For example, Columbus, Ohio went smoke-free and we identified Franklin County as smoke-free, but we did not code Pitkin County, Colorado smoke-free when Snowmass Village banned smoking in bars because it is a much smaller percentage of the county's overall population. We checked to see whether coding only counties with 100% bans changed the results. It actually makes the results stronger in the rest of the paper. Thus, we are reporting more conservative estimates.

Table 2
Means and proportions of key variables in analysis

	All counties	Counties where a ban becomes effective		Counties with no ban	
		Pre-ban	Post-ban	2000–2002	2003–2005
Number of annual accidents involving a driver with a blood alcohol level above 0.08	18.93	14.20	15.11	20.32	19.16
Number of accidents involving a driver with a positive blood alcohol level	23.65	19.43	19.90	25.15	23.68
Number of accidents involving no alcohol	36.81	38.98	37.79	36.29	36.70
Population	677,101	922,145	1,001,812	620,827	629,087
Prevailing beer tax (2005 cents per gallon)	22	13	13	22	24
BAC law specifying minimum of 0.08	72.9%	22.97%	99.07%	55.66%	96.55%
Number of observations	13,238	393	289	6,276	6,280
(Number of Counties)	(2,452)	(117)	(117)	(2,335)	(2,335)

Note: Since logs are used for the main specifications, we only calculate the means for counties with positive numbers of fatal accidents for a given year. We verify that this does not bias the results. All estimates are weighted by county-year population.

considered a treatment case but clearly not a control case either. California, which passed a state ban in 1998 and contains several counties, cities, and villages that passed individual bans prior to 1998, and Benton County, Oregon (for the same reason) are both excluded from the basic analysis. We anticipate there may be lagged effects of smoking bans. Therefore, California's and Benton County's inclusion would only confound the interpretation of the estimates. Nevertheless, our results will prove robust to California's and Benton County's inclusion or exclusion, as we demonstrate later in the paper. With these restrictions, we are left with a set of 2452 treatment and control counties for which we have sufficient data and we are comfortable classifying as a treatment or control county.

Table 2 reports means and proportions of variables in the analysis for both the treatment counties and counties without a ban for different periods of time. Looking at the first several rows of the table, there is an increase in fatal traffic accidents involving a seriously inebriated driver (BAC greater than 0.08) from 14.20 per year to 15.11 per year following the passage of a smoking ban in bars. Given that accidents involving no alcohol were declining during this time period for the counties passing a ban and that counties with no smoking ban were seeing a reduction in alcohol-related accidents, this is a substantial relative increase in alcohol-related accidents for counties enacting a smoking ban that requires closer examination.[9]

3.2. Methodology

We first pool all of the jurisdictions passing bans (the treatment group) and the remaining counties in the US that did not have smoking bans during the sample period (the control group). We experiment later with narrower control groups and the results prove robust. Our basic analysis begins with the following fixed effects regression model:

$$FA_{ct} = \alpha_c + \tau_t + \beta L_{ct} + \gamma' X_{ct} + \varepsilon_{ct}, \tag{1}$$

where subscript c denotes counties and subscript t denotes years. The terms α_c and τ_t are the county and time fixed effects. The inclusion of fixed effects ensures that differences in accidents across counties that are time-invariant and differences in accidents across time that are common in all counties will not bias estimates.

FA is defined primarily as the log of the number of fatal accidents involving a driver whose BAC exceeded 0.08 in a given county-year cell. We judge logs to be the most appropriate measure of the dependent variable because the median estimated number of fatal accidents for the county-years in the sample is less than the mean. Estimation of Eq. (1) will therefore be by weighted least squares, with standard errors corrected to allow for non-independence of observations

[9] Given that the number of accidents may be highly variable in smaller counties and we use data aggregated to the county-year level, we weight all estimates by county-year population size obtained from the Census Bureau.

from the same county through clustering (Arellano, 1987; Bertrand et al., 2004). We show later that redefining the dependent variable or using a different estimation model yields qualitatively identical results.[10]

The variable L is set to one if the county has a smoke-free ban in effect by at least the summer of a given year.[11] Thus, the estimate of β can be read as an estimate of the percent increase in alcohol-related traffic accidents after a smoking ban is passed relative to a control group of counties that did not have a smoking ban at any point during the sample period.

We recognize the recent empirically rigorous studies that evaluate the determinants of drunk driving (e.g., Dee, 1999b; Baughman et al., 2001, and Eisenberg, 2003) and understand that our empirical strategy should isolate the impact of smoke-free bar laws from the other determinants of alcohol-related crashes. Although we think smoking bans are likely exogenous in the context of our study, there may exist some correlation between ban passage and some of these other factors. Our empirical approach addresses these in a number of ways. The county fixed effects capture differences in counties that might affect accidents and are constant over time. We also add various covariates that capture county-specific changes in a county's alcohol-related crashes over time and include them in the X vector. The first control is the log of the county's population obtained from the US Census Bureau. Population growth will likely increase accidents. In Table 2, the population in our treatment counties is a bit larger than in the control group. Later on in our robustness checks, we verify that this difference does not compromise our research design by restricting the control group to larger counties and observing no substantive change to the results. Second, we might also be concerned that the underlying propensity for *all* traffic accidents might change over time in a county due to things like economic activity, highway construction, weather patterns, insurance rates, number of drivers, age composition of drivers, etc., and hence influence our estimates of alcohol-related accidents. To capture these, we employ a unique approach that constructs a variable measuring the log number of accidents per county that were not alcohol related, also from the FARS. This control allows us to isolate the effect of smoking bans apart from the many potentially omitted factors that make it more dangerous to drive in a particular locality and is included in the X vector. Given that this captures underlying traffic trends in the data, it would capture any differences in general accident risk that may arise between the treatment and control groups during the sample period analyzed, and as such is a very powerful control.

There may be concerns that the passage of smoking bans in bars is correlated with government policies that are meant to deter drunk drivers. We use data from 2000–2005, however, which is a time period beyond the point where most states had engaged in the bulk of their vigorous legislative activity. This limits the concern that smoking ban passage tended to coincide with legislation aimed to deter intoxicated drivers. Studying later smoking bans also limits the concern that more progressive locations are likely to pass bans first (like the coastal California counties). Pakko (2005) advocated studying later smoking bans for this reason. The fact that our sample includes bans from every region of the US further supports the experimental nature of our study.

Nevertheless, during our sample period, there were two state level variables that changed enough to think that they might confound the interpretation of the estimated effect of smoking bans. First, a number of states lowered the minimum BAC used to determine whether a driver was legally intoxicated from 0.10 to 0.08. Table 2 shows that more counties in our treatment group were affected by this reduction than the control group. Dee (2001) and Eisenberg (2003) show using older data that stricter BAC requirements reduced drunk driving accidents. For this reason, we include controls for whether the county is located in a state that had a 0.08 statute for a given year (the remainder of the counties had 0.10 BAC laws during this time period). Second, alcohol excise taxes varied over this time frame as some states increased or decreased their rates or tax rates declined in real terms. Ruhm (1996) finds beer taxes to be effective in deterring drunk driving for at least a subset of the population. Eisenberg (2003), however, finds limited evidence of the effect of beer taxes. We include controls for beer taxes in 2005 cents to capture any tax effect. A quick look at Table 2 shows little differential variation in beer taxes over the sample period for the treatment and control groups, however.

Other regulations that have been shown to reduce drunk driving are various provisions that hold those who sell alcohol legally responsible for potential harm caused by customers. The extent of this liability, whether the regulations

[10] For example, a Poisson regression (Hausman et al., 1984) could be used given the measurement of the dependent variable. Given the potential over-dispersion of the dependent variable, however, the Poisson might be inappropriate. Therefore, a negative binomial model might be more appealing, but the conditional negative binomial model correcting for over-dispersion has recently been criticized on the grounds of failing to be a true fixed effects estimator (Allison and Waterman, 2002). We settle on weighted least squares as the least problematic and most easily interpretable measure to use in presenting the basic results. We conducted a multitude of robustness checks to ensure our choice of model is not driving the result (many but not all of which are later reported in Table 5).

[11] As this definition is somewhat arbitrary, we will test the robustness of this definition against other alternative definitions of the policy variable later in the paper.

Table 3
Determinants of fatal accidents involving alcohol, 2000–2005

	Full sample				Sample excluding college towns enacting their own bans	Sample excluding bans in effect for less than 18 months
	(1)	(2)	(3)	(4)	(5)	(6)
Smoking ban	0.1178 (0.0459)	0.1361 (0.0458)	0.1444 (0.0466)	0.1344 (0.0457)	0.1438 (0.0457)	0.1895 (0.0513)
Log of Population	…	0.8845 (0.2085)	0.8246 (0.2032)	0.8792 (0.2140)	0.8741 (0.2143)	0.8123 (0.2151)
Log of non-alcohol related accidents	…	…	0.0819 (0.0183)	0.0833 (0.0183)	0.0834 (0.0183)	0.0844 (0.0185)
Beer tax in 2005 cents	…	…	…	−0.0070 (0.0026)	−0.0067 (0.0027)	−0.0061 (0.0029)
BAC law specifying minimum of 0.08	…	…	…	−0.0073 (0.0221)	−0.0103 (0.0220)	−0.0040 (0.0225)
Sample (number of counties)			13,238 (2,452)		13,220 (2,449)	13,106 (2,429)

Note: Each column is from a separate weighted least squares regression that includes both county and year fixed effects. The dependent variable is the log of the estimated number of fatal accidents involving a driver with a BAC exceeding 0.08 in a county-year. The standard errors in parentheses are corrected to allow for non-independence of observations within a county through clustering. The fifth column excludes bans passed in the college towns of Lawrence (Kansas), Lexington (Kentucky), and Lincoln (Nebraska) that did not cover the rest of the county. The final column (6) excludes the 23 (of 117) counties for which a ban became effective after July 1, 2004 (Massachusetts through Tift County in Table 1). The remaining bans therefore have been in place for at least 18 months and two years of post-ban alcohol-related accidents are reliably identified.

are codified, and whether the liability extends beyond serving minors, varies by locality, but all such regulations have been collected under the moniker of dram shop laws. According to many studies, dram shop laws have a strong effect on decreasing drunk driving (Eisenberg, 2003). We uncovered insufficient variability in such laws to identify an effect in our sample, given our fixed effects panel data approach.[12]

The identification strategy outlined to this point is predicated on the assumption that after the inclusion of fixed effects and time-varying controls, the counties that pass a ban (the treatment group) are comparable to the counties not passing a ban (the control group). Yet, even though we have controlled for changes in non-alcohol related trends, there is always the concern that smoking ban implementation is correlated with some unobserved trend in alcohol-related accidents. Although we view this to be unlikely, in light of the aforementioned controls and the exogenous nature of smoking bans with regard to drunk driving, we do test for the presence of trends in a couple of different ways. First, we failed to reject the null hypothesis of a test of whether the pre-ban trends of alcohol-related accidents in the treatment and control group are different, thus providing no evidence to indicate that there is a difference in accident trends between the control group and treatment group in the years prior to the implementation of smoking bans. We also later engage in an analysis of leads and lags, with the estimates of the former shedding light on questions of policy exogeneity. Specifically, the basic approach in Eq. (1) can also be used to study whether there are lead and lagged effects of legislation by replacing the dummy policy variable with a series of lead and lagged dummy variables indicating the years before and after passage of smoking bans. The lead effects will detect whether we are comparing counties with differential trends prior to ban enactment, thus testing our policy exogeneity assumption.

4. Results

4.1. Basic estimates

We begin by estimating Eq. (1) for a sample consisting of all treatment and control counties. Results are reported in Table 3. Column (1) provides the result using only fixed effects and shows that a smoking ban increases alcohol-related accidents by a statistically significant 11.78%. In the second column, we add a control for population and observe little

[12] There are obviously dozens of other state, national, and local laws and regulations aimed at deterring drunk driving, many of which might be effective in certain areas. As controls, however, these likely will have little effect on our results. As we will show, adding controls for BAC laws and beer taxes do not substantially change our estimates of the effect of smoking bans. Thus, if these much more visible and effective policies are not correlated with the passage of smoking bans, it is unlikely our results are affected by less visible policies.

change in the policy's effect. As expected, population growth increases accidents. In the third column, we add our control for the number of fatal accidents involving no alcohol, which we believe captures general accident trends in a county that are driven by congestion, construction, or other county specific factors. Even though changes in non-alcohol related accidents are highly correlated with alcohol-related accidents, the effect of smoking bans remains positive and significant at the 0.01 level.[13]

In the fourth column, we add the remaining controls for beer taxes and lower BAC laws. Increased beer taxes and lower BAC laws lower alcohol-related fatal accidents, which is consistent with the literature that commonly points to these as a negative determinant of accidents, although the latter estimate is not statistically significant.[14] Most importantly, the effect of smoking bans on fatal drunk driving accidents remains a substantial 13.44% increase, and continues to be statistically significant at the .01 level. The durability of the smoking ban result across columns supports our fixed effects research design and the appropriateness of our treatment and control groups.

It is useful to convert this 13.44% into an actual number of additional fatal accidents after smoking bans are passed. Given that the average annual number of accidents in a county involving a seriously inebriated driver is 18.93 in our sample, the 13.44% increase would amount to 2.54 additional accidents for a typical county in our sample of 677,101 people. This is a large (but not implausibly large) effect. There have been numerous studies of other policies affecting drunk drivers, especially using data from the 1980s and 1990s, with two policies frequently showing strong impacts on fatal accident rates — laws that hold establishments liable for damages caused by an inebriated driver served at their bar (dram shop laws) and laws that establish legal maximums for BAC levels. It is useful to put our 2.54 accidents in the context of these studies. This will allow us to gauge the sensibility of our results. To do so, we appeal to the estimates in Table 4 of Eisenberg (2003), a relatively recent publication and a comprehensive look at policies passed in the 1980s and 1990s to deter drunk driving. Although his level of analysis is the state and he measures his dependent variable as accidents per 10,000 licensed drivers, we can use his estimates as a rough guide in placing our estimates in some context. Given that we have population data and about 67% of the population had a valid driver's license in 2005, we can easily translate his estimates to our typical county of 677,101 people.[15] Dram shop laws, in his study, decreased accidents by a rate of .1484 per 10,000 licensed drivers. For a population of 677,101, this would amount to about 6.7 accidents averted due to this policy.[16] Initiating a BAC requirement of .10 or .08 reduced alcohol-related accidents by .0482 and .1061, respectively, compared with no BAC requirement at all. These translate to crash reductions in our typical county of 2.19 and 4.81. Perhaps more interesting, the difference in these two BAC estimates is 2.62, which could be interpreted as the reduction in alcohol-related accidents from moving from a .10 to .08 BAC law.[17] These calculations therefore suggest that our estimate of 2.54 additional accidents, although slightly smaller than the measured effects of policies that target drunk driving directly, still represent a meaningful increase in drunk driving behavior.

One could also frame our result in terms of the increased number of miles driven by seriously inebriated drivers post smoking ban. To do this, we start with Levitt and Porter's (2001) externality estimate of 30 cents per mile driven caused by a seriously inebriated driver and our estimated 2.54 additional accidents in a typical county.[18] FARS data and the direct estimate on fatalities we report in Table 5 suggest that approximately six fatalities occur in 2.54 accidents. About 1 in 5 deaths enter Levitt and Porter's externality calculation because the rest were a drunk driver or passengers of a drunk driver, which they exclude. So, about 1.25 lives lost from our estimates should be considered when applying Levitt and Porter's 30 cents/mile. If we assume a life lost is worth $3,000,000, which is low by 2007 standards but is the assumption that generated the 30 cents number in their 2001 paper, a smoking ban would require at the most 12,500,000 more miles driven in a county in an entire year to generate our estimated 2.5 additional accidents or 6

[13] One could envision a falsification exercise where the log of non-alcohol-related accidents is the dependent variable. We find no positive effects of smoking bans on accidents with no alcohol involved. In fact, the number of non-alcohol-related crashes following a smoking ban are lower. It is only the alcohol-related crashes that increase.

[14] This insignificant effect is not necessarily at odds with other work in the literature on BAC requirements, which studied much earlier enactment and changes in BAC laws and found strong negative effects. Our sample is from after 2000 and is picking up states that lagged behind the national move to a .08 BAC standard. These states may in general be more lax in their enforcement of drunk driving regulations.

[15] This 67% was calculated from published figures from the Federal Highway Administration (www.fhwa.dot.gov).

[16] $6.7 = .1484 \times (677{,}101/(10{,}000 \times (1/.67)))$.

[17] The likely reason for the stronger BAC impact in Eisenberg's study, as compared with our Table 3, is the earlier time frame. Moreover, looking at earlier bans, he estimated both effects of moves from having no BAC law to a BAC regulation and changes from a .10 to .08 standard.

[18] We thank a referee for this suggestion.

Table 4
Effects of smoking bans over time

	Full sample (sample and control variables from column (4) of Table 3)	Restricted sample (sample and control variables from column (6) of Table 3)
Two years prior to ban	0.0269 (0.0541)	0.0241 (0.0743)
One year prior to ban	−0.0382 (0.0568)	−0.0025 (0.0743)
Effective year	0.1288 (0.0608)	0.2161 (0.0769)
Year after ban	0.1379 (0.0774)	0.2001 (0.0978)
Two years after ban	0.1139 (0.0644)	0.1583 (0.0739)
Sample size	13,238	13,106
Number of counties	2,452	2,429

Note: Each column is from a separate weighted least squares regression. The dependent variable is the log of the estimated number of fatal accidents involving a driver with a BAC exceeding 0.08. County and year fixed effects, as well as controls for accidents not involving alcohol, population, beer tax level, and minimum BAC levels, are included in all regressions. The standard errors in parentheses are corrected to allow for non-independence of observations within a county through clustering.

fatalities.[19] This turns out to be a sensible number. If, as reported earlier, 46 million adults (about 15.33% of the US population) both drink and smoke, this would mean 103,800 people in our typical county. If only 13.5% of these people (the percentage of smokers who have an alcohol abuse problem) decide to look for a bar where they can smoke and drink at the border of the county or a non-compliant bar, even if no one else drives longer distances, that would be 14,013 people we might expect to drive longer distances drunk following a smoking ban. Considering only these people, the average person in this group would have to drive 892 more miles in a year to their new bar. If he makes the trip twice a week, this would mean he found a new location to drink about 8.5 additional miles away. This suggests a clearly large risk created by a smoking ban in bars but one that is within reason.[20]

We view the estimate in the fourth column of Table 3 as our preferred estimate. In columns (5) and (6), we illustrate that the results become even stronger when we remove from the treatment group several cases that we would reasonably expect to have a smaller impact on accidents. First, three college towns passed their own bans during the sample period that did not cover the rest of their counties. In these places, (namely Lexington, Kentucky, Lawrence, Kansas, and Lincoln, Nebraska) bars are concentrated close to the campuses and many people walk rather than drive to them.[21] Moreover, college students have a strong preference for college bars, which cater to their unique tastes. Thus, they might not respond to smoking bans by driving to other locations. Indeed, removing them from the sample increases the effect of smoking bans by nearly a percentage point.

In column (6), we remove counties with bans that are relatively new. Given that we code a county as smoke-free if a law goes into place by the summer, we may be biasing our estimates toward zero by including bans in the sample that have been in place for less than 18 months. Only bans in effect for 18 months are assured to have a full year of accidents upon which to estimate an effect of a smoking ban. Removing the 23 (of 117) cases from the sample with a ban in place for less than 18 months results in a stronger estimated effect of the bans — a nearly 19% increase in accidents.[22] Given the exercise we employed earlier, this translates into 3.59 additional accidents in our average community following these smoking bans. We carry the estimate from this sample and specification throughout the rest of the paper, as well as the sample and specification from column (4). The column (4) estimate from Table 3 will still serve as the preferred estimate of the effect of bans and the column (6) estimate might be considered a possible upper bound of the effect.

In Table 4, we briefly look at the effect of smoking bans over time by introducing two year lead effects and two year lagged effects, as well as a contemporaneous effect of the bans. The estimates in Table 4 include all control variables used in the fourth and sixth columns of Table 3. The lead effects are informative in that we can determine whether the positive effects from Table 3 are indeed stemming from the banning of smoking in bars, as opposed to the effect of a previously existing trend. In both columns of Table 4, the two lead effects are small, not significant, and have opposing signs, thus suggesting that the estimates in Table 3 are not the result of trending differences between the treatment and

[19] $12{,}500{,}000=(1/.30)\times 1.25\times 3{,}000{,}000$.

[20] Any assumption we change in this externality-based calculation, such as allowing for extra driving by smokers without an alcohol abuse problem, "bar hopping," non-smoking friends driving more, etc., would make the miles per driver necessary to generate our results less.

[21] Note that we consider Minneapolis and Columbus too large of cities and the University of Vermont too small of a student body to remove any of the corresponding counties from this illustration.

[22] The counties removed are the counties from the bottom of Table 1, starting with Massachusetts counties and ending with Tift County (Georgia).

control counties but real effects of smoking bans. The effects of the policy during the effective year of the law are positive and significant. There is a modest upswing in the non-restricted sample in the year following the ban. Two years after the ban there are still effects on fatal accidents but the impacts show some sign of waning. In short, there are no discernable differences in fatal alcohol-related accidents before the ban in treatment and control counties, but after passage of the ban the effects are positive, significant, and last for at least several years.

4.2. Robustness checks

Although we view our empirical decisions thus far as reasonable, we recognize there were several alternative definitions of the control group, the dependent variable, the policy variable, and estimation methods that we could have employed. So, in order to verify that the results are not sensitive to our choices, we next engage in an extensive series of robustness checks, which we summarize in Table 5. For comparison, row (1) repeats the primary results from Table 3 (columns 4 and 6) of a 13.44% and 18.95% increase in fatal alcohol-related accidents.

Our first series of robustness checks test whether the broad control group we have been using is unduly influencing the results. We test three alternative samples. The first adds a population size restriction on the sample, allowing only all

Table 5
Robustness of the basic results

		Full sample (sample and control variables from column (4) of Table 3)	Restricted sample (sample and control variables from column (6) of Table 3)
(1)	Basic specification (repeated from Table 3)	0.1344 (0.0457)	0.1895 (0.0513)
Alternative control groups			
(2)	Counties with populations greater than 100,000 people (508 total counties)	0.1312 (0.0496)	0.1912 (00561)
(3)	Only counties from a state with a smoking ban (960 total counties)	0.1552 (0.0490)	0.2145 (0.0622)
(4)	Including California and Benton County, OR (2,512 total counties)	0.1257 (0.0454)	0.1808 (0.0518)
Alternative dependent variables			
(5)	Log of fatalities (not accidents) in crashes involving a driver with a BAC>.08	0.1244 (0.0550)	0.2093 (0.0574)
(6)	Number of accidents involving a driver with a BAC>.08 (ie., in levels not in logs)	2.56 (0.82)	3.14 (0.88)
(7)	Log of number of accidents involving any alcohol	0.0899 (0.0301)	0.1371 (0.0325)
(8)	Alcohol-related crashes per 100,000 people	0.3169 (0.0857)	0.4253 (0.0966)
Alternative policy variable			
(9)	Half-weight placed on laws passed mid-year	0.1413 (0.0441)	0.1944 (0.0476)
(10)	Zero weight placed on laws passed mid-year	0.1082 (0.0359)	0.1482 (0.0375)
Alternative estimation methods			
(11)	Unweighted OLS	0.0975 (0.0579)	0.1306 (0.0655)
(12)	Unweighted negative binomial fixed effects	0.0879 (0.0318)	0.1389 (0.0379)
(13)	State fixed effects (state instead of county)	0.1316 (0.0475)	0.2065 (0.0518)
(14)	Alternative clustering of standard errors (state instead of county)	0.1344 (0.0936)	0.1895 (0.0778)
Alternative level of analysis:			
(15)	State (all data aggregated to state level except California and states with a county or city with a law; also, observations are clustered by state)	0.1286 (0.0861)	0.1872 (0.0697)

Note: Each cell is from a separate regression. County and year fixed effects, as well as controls for accidents not involving alcohol, population, beer tax, and minimum BAC levels, are included in all regressions. In the robustness checks using alternative control groups, the sample size will differ. We note the number of counties used in parentheses in these cases. Coefficient estimates and standard errors (corrected to allow for non-independence of observations within a county through clustering unless otherwise noted) are reported.

counties with more than 100,000 people to compose the control group (rather than all counties with at least one traffic fatality each year). Summary statistics from Table 2 demonstrate that the average population in the treatment counties is larger than in the non-ban counties, and this restriction allows us to compare counties in the treatment and control group that are more similar, at least with regard to population. The second row estimates yield nearly identical results as the primary results from Table 3. As a second alternative, we restrict the control group to only those states with an existing smoking ban. From the perspective of cultural or regional driving norms, the non-ban counties from states which do have at least one smoking ban in place, may provide a better control group.[23] The results of this test are reported in the third row. Despite the smaller sample size, the story of a heightened risk posed by drunk drivers remains, perhaps even strengthening with the more similar control group. Finally, we add California and Benton County, OR to our sample. California and Benton County's smoking bans were in effect the entire sample period. Therefore, we identify no effects from their bans, so they effectively become additional control cases in the estimates. Their inclusion does not alter our findings.

We next test for the robustness of our dependent variable. First, we redefine our dependent variable as the log of *fatalities*, rather than accidents, which has an appeal for those interested in interpreting the magnitude of the social cost of drunk driving post smoking ban for a typical county. The fatalities increase by 12.44% in the full sample. Given a typical county in our sample has 48.63 traffic fatalities involving a drunk driver each year, this amounts to just below six extra annual deaths following a smoking ban. This matches the accident estimates almost exactly, as about 2.5 people die per alcohol-related crash in our FARS data. Second, to test the sensitivity of using log accidents (instead of levels) in our primary estimation, we use the number of alcohol related accidents as a measure. The results do not change our conclusions in any way. In fact, the 2.56 additional fatal accidents reported in the sixth row implies a 13.5% increase in accidents given the 18.93 annual average of fatal accidents per county involving alcohol. In Tables 3 and 4, we restricted the dependent variable to the log of accidents with a BAC of 0.08 or higher, which is now the legal limit in all states in the US. Alternatively, we could have chosen the log number of fatal accidents involving any alcohol. When we do, the outcome is similar but a bit smaller in magnitude, as one would expect if the policy change truly affects seriously drunk drivers. We also test whether defining the dependent variable as the number of alcohol-related accidents per 100,000 county residents affects the results, and the results are stronger.

Next, we checked the robustness of our coding of the policy variable. To this point, we have considered a law as effective for a given year if it was passed by the summer. We could have weighted laws enacted in the middle of the year by less. Alternatively, we could have considered a law as effective for a given year if it was enacted before the beginning of that year. As detailed in rows (9) and (10), the impact of smoking bans remains qualitatively the same regardless of how we treat the smoking ban indicator during the year it goes into effect. As we would expect if the effect is immediate and real, however, the zero-weighted policy variable weakens the estimate somewhat.

Next, we test the robustness of our choice of estimation model starting in the eleventh row of Table 5. We have been using a weighted least squares estimation to account for greater precision in our larger counties. As shown in row (11), this weighting does not drive the results. Using unweighted least squares does make the estimates a bit weaker and less precise. We also tested whether a negative binomial fixed effects model changes the inferred effect of the smoking ban in the twelfth row and it does not. The reported negative binomial coefficient estimates can be transformed to incidence rate ratios, which imply increases in fatal accident of 9% to 15%. The negative binomial model is not weighted, much like the OLS estimate in the preceding row. The coefficient estimate falls in each of the unweighted estimates because we are allowing very small counties to have equal weight to large counties, perhaps biasing the results toward zero. One might be concerned, however, that this means a few large counties are outliers and may be driving the basic results of the paper. We tested for the influence of outliers, by first removing cases where the number of accidents exceeds the treatment mean by more than one standard deviation. The results were similar. This was also the case when we removed the counties with populations exceeding the treatment mean by a standard deviation. Later in the paper, we analyze some case studies that suggest the effects of smoking bans exist in counties of all sizes and in different geographic areas.

Finally, we recognize that two of the control variables we use are measured at the state level (beer tax and lower BAC requirements). Moreover, over half of the affected counties in our sample are part of a ban at the state level. We already partially addressed this in row (3) by testing for effects on a sample of counties in states with at least one law.

[23] So for these estimates, we exclude counties from states such as Tennessee and the Dakotas, which do not have smoking bans in bars at any municipal level.

Table 6
Effect of smoking bans in places more and less prone to drunk driving

	Full sample (sample and control variables from column (4) of Table 3)	Restricted sample (sample and control variables from column (6) of Table 3)
Beer tax		
High tax jurisdiction (greater than sample mean of 21.62 cents; 854 counties)	0.0596 (0.1331)	0.0349 (0.1367)
Low tax jurisdiction (1598 counties)	0.1179 (0.0473)	0.1849 (0.0588)
P-value of test of difference	0.71	0.32
BAC		
Long-standing lower BAC requirement (943 counties)	0.0364 (0.1180)	0.0227 (0.1210)
More recent BAC reductions (1509 counties)	0.1418 (0.0482)	0.2049 (0.0546)
P-value of test of difference	0.37	0.14

Note: Each row is from a separate weighted least squares regression. The dependent variable is the log of the estimated number of fatal accidents involving a driver with a BAC exceeding 0.08. County and year fixed effects, as well as controls for accidents not involving alcohol, population, beer tax level, and minimum BAC levels, are included in all regressions. The standard errors in parentheses are corrected to allow for non-independence of observations within a county through clustering.

The effect was consistent with the basic estimates. In row (13) of Table 5, we employ state fixed effects (rather than county) and the estimates remain virtually the same. In the penultimate row, we adjust our standard errors to allow for correlation between all observations from the same state through clustering. Although this might be overdoing it because it allows for non-independence between observations from counties in the same state that should reasonably be considered independent observations, the estimate remains marginally significant for the full sample and highly significant in the restricted sample.[24] In the final row, we omit all local ban information and estimate purely off of state laws. Here we aggregate all variables up to the state level. The lack of observations make these estimates less precise but the point estimates of the effect of bans are similar to those in the rest of table and remain significant in the restricted sample, even though we are throwing out all local ban information.

Overall, the results detailed in Table 5 provide us with a broad and comprehensive picture of the nature of the measured effects. Under many of the alternatives, we estimate an effect that is larger than the basic estimates. Under a few of the alternatives, the magnitude of the effects or precision is smaller, but, regardless of empirical assumptions, the qualitative conclusions of the paper remain intact.

4.3. Effects in places where smokers are more (and less) likely to react to a ban

In addition to verifying that the basic results are robust, it is also useful to verify that they are intuitive. Given that the data only allow estimation of what amounts to a reduced form relationship between smoking bans and accidents, and that our result might seem counterintuitive to some readers, we next conduct a few tests of whether we observe greater effects of smoking bans in places we might think the propensity of smokers to react to bans by driving drunk is greater. In Table 6, we exploit geographic variation in beer taxes and commitment to lower BAC levels. We previously reviewed the literature that suggested that both beer taxes and a lower BAC tended to deter drunk drivers in the past, although that literature is hardly without disagreement. Thus, we might suspect that if the positive effect of smoking bans on drunk driving is real, it would be the jurisdictions with lower beer taxes that would experience the more heightened risk of drunk drivers following smoking bans. Likewise, states that lowered their BAC prior to the sample period are likely more pro-active in deterring drunk drivers. Thus, it is reasonable to suspect that drivers in such

[24] A referee suggested we condition on state-year fixed effects, which would be appealing in that it would completely rule out confounding influences of any state-level changes. Unfortunately, this asks too much of the existing data, as we hold constant the bulk of the meaningful variation in the data. When we do condition on state-year cells, we no longer observe a positive effects on accidents overall. However, when we look at the cases individually later in the paper and focus on those cases where we would expect cross-border shopping or within jurisdiction shopping, we do see an increase in accidents whether the control groups are composed of counties from across the nation or within state. In any case, our inability to completely rule out the possible confounding influence of state-level variation must be noted.

localities are more aware that they may be jailed or fined if they drive drunk. The greater effects of stricter BAC requirements in past studies are consistent with this idea. If this is the case, it should be those jurisdictions that had failed to lower their BAC by the beginning of our sample period to be more lax toward drunk drivers, thus facing a greater risk post smoking ban.

We divide our sample in the top panel between high tax and low tax jurisdictions, with high tax defined as a county in a state with a greater than mean beer tax of 21.62 cents (in 2005 dollars) and the remainder of counties as low tax. This bifurcation of the sample is imperfect, but the estimates suggest that it is indeed the low tax jurisdictions where we observe the more significant effect of smoking bans. In fact, the effects in the high tax jurisdiction are no longer significant. The estimates are too imprecise to judge the differences as statistically significant, however.

In the bottom panel, we break the sample up by long-standing BAC requirements. We find that it is counties in states that were lax in their implementation of the lower BAC requirements that experience more significant effects of smoking bans, consistent with expectations. States which had long standing lower BAC requirements appeared far less likely to experience the heightened drunk driving risk, almost significantly so (p-value of .14) in the restricted sample.

These estimates are highly suggestive that the reduced form relationship observed throughout the paper is indeed revealing a real relationship between smoking bans and drunk driving behavior. We are reluctant to make stronger statements concerning the Table 6 estimates, but we note that they do suggest that there are solutions at the disposal of local officials following smoking bans to reduce the threat of drunk drivers. As the decision to drive drunk presents itself to some smokers following a ban, they do appear to consider other factors, such as the price of alcohol (proxied by beer taxes) or the chance that they might be punished (proxied by long-standing lower BAC requirements).

5. Mechanisms by which bans increase drunk driving: a case study approach

5.1. Evidence of smokers driving to nearby counties without bans

Continuing with our attempt to disentangle what lies beneath the smoking ban-drunk driving relationship we observe in Tables 3 through 5, we next engage in a series of case studies that shed light on the mechanism by which bans on smoking lead to drunk driving. We advanced two hypotheses in Section 2 of the paper. The first suggests that consumers denied the opportunity to smoke might search for bars in nearby jurisdictions not subject to the ban, thus increasing the fatality risk in the location passing the ban, as well as the border county. In Table 7, we take up this question by analyzing the cases in our sample where border shopping could reasonably be expected and test for effects of smoking bans on fatal accidents in the adjoining county. If there are increases in accidents in the adjoining counties after legislation is enacted, this is suggestive that smokers are crossing borders in response to this change in their incentives.

In determining which counties from our treatment group should be included in the Table 7 analysis, we attempted to isolate treatment counties that seemed to provide the most obvious cross border shopping options. Specifically, we focused on treatment counties that had a bordering county with a sufficient enough population to be attractive for consumers to reasonably travel there to smoke and drink and a sufficient enough population itself to reasonably expect

Table 7
Border county case studies

Case	Border counties potentially affected	Effect of smoking ban in neighboring county
Delaware	Delaware County (Pennsylvania) and Chester County (Pennsylvania)	0.2622 (0.1672)
Boulder County (Colorado)	Jefferson County (Colorado)	0.4088 (0.0204)
Minneapolis (Minnesota)	Ramsey County (Minnesota)	0.1254 (0.0340)
Pierce County (Washington)	King County (Washington)	0.1101 (0.0150)
All border counties	All of the above	0.2098 (0.0863)

Note: The dependent variable is the log of the estimated number of fatal accidents involving a driver with a BAC exceeding 0.08. County and year fixed effects, as well as controls for accidents not involving alcohol, population, beer tax level, and minimum BAC levels, are included in all regressions. The standard errors in parentheses are corrected to allow for non-independence of observations within a county through clustering. The control group consists of all counties that do not have a smoke-free ordinance and are not a border county.

it to have an impact on drunk driving in the border county.[25] Looking at the Table 1 list of treatment group cases, we isolated three cases. We also add a fourth case that was previously excluded from the original analysis because the ban was repealed.

Our first case is Delaware. Its ban could reasonably be expected to influence the behavior of consumers in the populous northern portion of the state, which borders two Pennsylvania counties — Delaware County and Chester County. Both of these Pennsylvania counties are highly populated suburbs of Philadelphia. A test of whether accidents increased in these counties following Delaware's ban is sensible, and we report the results in the first row of Table 7. We find using the same methodology as in Eq. (1), with the full set of controls, that alcohol-related accidents increased by 26.22% in Delaware County (Pennsylvania) and Chester County (Pennsylvania), relative to all other counties without smoking bans.[26]

Our second case is Boulder County (Colorado), which borders the second most populated county in Colorado—Jefferson County (Colorado). Residents of Boulder wishing to drive to Denver will likely pass through Jefferson County on US-36 as well. We find that after Boulder banned smoking, fatal accidents in Jefferson County increased by over 40%.

Our third case deals with Minneapolis and St. Paul (Minnesota). Minneapolis passed a smoking ban in early 2005, but St. Paul did not have one in place. These two cities collectively make up one large metropolitan area. Yet, each is within a separate county, Minneapolis in Hennepin County and St. Paul in Ramsey County, which allows us to separate the two cities distinctly. Hence, this situation provides another excellent case study of cross border shopping as the obvious and convenient alternative location for Minneapolis residents to smoke and drink is St. Paul, which is the only large city in neighboring Ramsey County. We find that after Minneapolis enacted their smoking ban, fatal accidents in Ramsey County increased by over 12% relative to other counties with no smoking ban, a result which is again highly indicative of a cross border shopping effect.

Our last case is Pierce County (Washington). We did not include Pierce in our original estimates because it passed a ban in 2004 but then repealed it in 2005, only to have the entire state of Washington go smoke-free by the end of 2005.[27] There was, however, a full year where Pierce County had a ban and their neighbor King County did not. Thus, this allows for a very clean test of cross border effects. Tacoma, the only large city in Pierce, is adjacent to Federal Way, a town in King County that has many bars and attractions and potentially could draw in Tacoma customers. Seattle is farther away, but is still in King County and perhaps worth the trip for some Tacoma residents. Finally, the Emerald Queen Casino is also an option for Tacoma residents to smoke and drink (KOMO News Service, 2006), and access to this casino from I-5 takes many patrons through King County. We find a significant positive effect of 11% in King County in 2004, the year Pierce County's ban took effect.

In the final row of Table 7, we consider all border counties together and find a statistically significant 20.98% increase in fatal accidents in these counties relative to counties that did not pass smoking bans. As with the case by case analysis in the first four rows, we use a version of Eq. (1) testing for smoking ban effects in the border counties. Although we admit that identifying effects off of four counties has its limitations, the results are suggestive that the existence of cross border options drives up alcohol-related fatal accidents after bar smoking bans.

5.2. *Evidence of smokers driving within counties: case studies of "isolated" counties*

We next turn to the second hypothesis for increased accidents — within jurisdiction driving due to product differentiation. That is, some bars choose to build outdoor seating or simply not to comply with the smoking ban. Smokers drive to these bars. We cannot test for product differentiation directly, but we can identify a list of counties from Table 1 for which cross border shopping can be ruled out and within jurisdiction shopping is likely. It is logical to start with statewide bans. These present reasonable tests for the product differentiation hypothesis since most residents

[25] For example, the New Jersey counties bordering New York are likely not reasonable options for the latter to drive to just to drink and smoke. Moreover, Effingham County (Georgia) is a small county (typically experiencing only one or two fatal drunk driving accidents a year) bordering a larger county that contains Savannah. It is not surprising that we observe no positive effects on accidents in New Jersey border counties or Savannah following smoking bans.

[26] We also verified that limiting the control group to other Pennsylvania counties yields similar (albeit less precise) results. The same is true if we limit the control groups to within state counties in the rest of the Table 7 (and Table 8) cases. This provides us with confidence that smoking bans indeed have an effect on drunk driving accidents apart from state-level changes in accident trends or state policies to deter drunk driving.

[27] Including Pierce in the basic estimates only strengthens the results.

Table 8
Additional case studies; within jurisdiction effects

Case	Effect of smoking ban
Counties of New York	0.2755 (0.0526)
Counties of Maine	0.1104 (0.1455)
Counties of Rhode Island	−0.2258 (0.2306)
El Paso	0.2020 (0.0156)
Roswell	0.6010 (0.0174)
All isolated counties	0.2484 (0.0496)

Note: The dependent variable is the log of the estimated number of fatal accidents involving a driver with a BAC exceeding 0.08. County and year fixed effects, as well as controls for accidents not involving alcohol, population, beer tax level, and minimum BAC levels, are included in all regressions. The standard errors in parentheses are corrected to allow for non-independence of observations within a county through clustering.

would understand that they need to search for non-compliant bars within a jurisdiction, as opposed to cross border shop since the whole state is smoke-free. Among these states, Delaware is excluded because its population center has the potential to cross border shop in Pennsylvania. We also omit Massachusetts and Connecticut. Although border shopping is unlikely by residents in these states, it is also still possible, as Rhode Island, Vermont, and New Hampshire were options at various times. This leaves Maine, New York, and Rhode Island as clean tests. We keep all counties from Maine because the population is concentrated far enough from border counties. We keep all New York counties because they are sufficiently separate from any cross border shopping alternatives.[28] All of Rhode Island is surrounded by water or borders a state with a smoking ban already in effect.

Among the local bans in Table 1, only El Paso and Roswell qualified for the Table 8 test of the product differentiation hypothesis. We think that within jurisdiction shopping is the only options for El Paso residents, other than the very unlikely choice to cross into Mexico to smoke. Roswell (New Mexico) is the only city in Chavez County and very far from any other options.

Table 8 reveals that in four of the five cases where within jurisdiction shopping is a probable mechanism, there is an increase in alcohol-related accidents.[29] In the other case, there is a non-significant negative effect. Analyzing all these counties together, however, we find a 24.84% increase in alcohol-related accidents in those counties where product differentiation is the likely mechanism of increased drunk driving. We concede that the evidence from Table 8 is not conclusive and cannot point to non-compliance or outdoor seating options as the reason for the bar differentiation, but we think the estimates are suggestive that within jurisdiction bar search is at least partially a cause of the increased drunk driving risk following smoking bans in some jurisdictions.[30]

One might wonder about the cases we left out of both the Tables 7 and 8 analyses. Although we recognize some might view this as arbitrary, we thought it important to provide any evidence that helped explain the reduced form results in Tables 3–5. We think that the case studies we identified achieve this. We left some cases out of Tables 7 and 8 because we could not classify them as either cross border shopping cases or geographically isolated cases. We noted reasons for excluding several above (e.g. the college towns of Lawrence, Lexington, and Lincoln being fundamentally different; the non-isolated but questionable cross border shopping options of Connecticut and Massachusetts). In the other cases, we simply were not able to uncover enough information about the counties to take a stand on which mechanism (cross border or within jurisdiction shopping) a given case study would test. We assure readers that we tested for effects for all of these cases anyway, and there was a decidedly mixed bag of strong increases in drunk driving accidents (Lincoln County, Tift County, Montgomery County, and Columbus), strong decreases (the college towns, Effingham County and Burlington—perhaps indicating we should have considered Burlington like a college town), and a slew of inconclusive results (Connecticut counties, Massachusetts counties, and Webster County). A couple of points can be made. First, when all of these cases are included in the basic estimates in Table 3, the positive effect on drunk driving holds. Thus, we have established that there is a link between smoking bans and drunk driving on average.

[28] We did re-run tests excluding Maine and New York counties that border other states that boast a county with at least some potential attraction. These never altered the results in any meaningful way.

[29] New York has more than half the counties in the table and is therefore the most meaningful of the case studies.

[30] The overall reduction in bar business that has been found following smoking bans in recent studies (Adams and Cotti, 2007; Adda et al., 2007) likely rules out explanations for the basic results of the paper involving non-smokers driving more to bars. Thus, we truly are left with hypotheses involving smokers or their acquaintances driving longer distances to find alternatives as the only possible explanations.

Second, in the cases where we can make a stand, support for both potential hypotheses exists, thus providing credible intuitive reasons for the reduced form results.

6. Conclusion

This paper is the first to show that banning smoking in bars increases the fatal accident risk posed by drunk drivers. We have shown that the result is robust to the inclusion of controls for area and time fixed effects, changes in population, changes in other policies that may impact drunk driving behavior (e.g. beer taxes, BAC law), as well as changes in factors that may influence overall driving risk separate from drinking behavior (e.g. construction, weather, etc.). Furthermore, these estimates are also robust to several alternative definitions of the control group, the dependent variable, the policy variable, and to the estimation model employed. Our evidence is consistent with two mechanisms—smokers searching for alternative locations to drink within a locality and smokers driving to nearby jurisdictions that allow smoking in bars.

Recent legislative activity suggests that more smoking bans are inevitable, and our estimates suggest any state, county, or city should be aware that as a ban becomes effective in their jurisdiction, more attention must be paid to the threat of intoxicated drivers. Communities should step up enforcement measures that they find most effective. Alternatively, national governments should be aware that local legislation banning smoking in the absence of a well-enforced national ban may encourage drunk driving through cross border shopping. We do stress, however, that any increase in drunk driving estimated in this paper will eventually have to be weighed against the potential positive health impacts of smoking bans, something that may take years to determine.

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