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The impact of casinos on fatal alcohol-related traffic accidents in the United States

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ABSTRACT

Casinos have been introduced throughout the U.S. to spur economic development and generate tax revenues. Yet, casinos may also be associated with a variety of social ills. One issue that has not been empirically tested in the literature is whether there is a link between casino expansion and alcohol-related fatal traffic accidents. We suspect a link may exist since casinos often serve alcohol to their patrons and, by their dispersed nature, could impact driving distances after drinking. Using the variation in the timing and location of casino openings over a 10-year period, we isolate the impact of casino introduction on alcohol-related fatal accidents. Results indicate that there is a strong link between the presence of a casino in a county and the number of alcohol-related fatal traffic accidents. However, this relationship is negatively related to the local-area (county) population. Results prove durable, as we subject them to robustness checks.

1. Introduction

With the exception of Nevada and Atlantic City, NJ, casinos had no significant presence in the United States until Congress passed the Indian Gaming Regulatory Act (IGRA) in 1988. The IGRA opened the door for formalized Indian casinos by allowing gaming to exist on tribal lands, subject to a compact agreement with the state government.[1] Shortly after the IGRA passed, several states also began to legalize commercial casinos. Together these changes in the legislative landscape surrounding casinos led to a tremendous increase in the presence of casinos across the United States. By the end of 2008 commercial casinos were operating in 12 states with annual revenues exceeding $32 billion (American Gaming Association, 2009), while tribal casinos had opened in 29 states with annual revenues exceeding $26 billion (National Indian Gaming Commission, 2009). Collectively, the casino sector has a significant economic presence.

While the casino industry is one of the fastest growing entertainment industries in the U.S., its growth is not without controversy. Casino opponents argue that casinos bring a variety of social problems, including increases in crime, bankruptcy, and divorce. Recently claims of casinos leading to higher drunk driving prevalence have also been noted. For example, newspaper reports often link DUI arrests and/or alcohol-related traffic fatalities to casinos that serve alcohol (e.g., Cornfield, 2009; Smith, 2010). Many casinos follow a "destination resort" model; they include restaurants, bars, shows, shops, and a hotel. Other casinos cater more to a local clientele. At a minimum, both types of casino typically include a bar service and casino customers often enjoy drinking alcohol while they socialize and play casino games. The fact that alcohol is readily available at many casinos suggests that casinos may, in fact, be a catalyst for increased drunk driving and hence, increased alcohol-related traffic fatalities. However, a more detailed look at the possible impact of casinos on drunken driving behavior demonstrates that there could be an inverse relationship between casinos and drunk driving under the right circumstances. Regardless, we are aware of no previous study that rigorously examines the possibility of such a link.

The purpose of this study is to test whether there is, in fact, a relationship between the spread of casinos and the number of alcohol-related fatal traffic accidents. Our analysis utilizes U.S. county-level data from 1990 to 2000, a period of time that saw the overwhelming majority of casino openings in the last 30 years. Overall, this presents a natural laboratory to test the effects of

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[1] See Light and Rand (2005) for a comprehensive discussion of tribal casinos and relevant law.

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casino entry on accident risk. In the next section we provide background information and discuss various theoretical issues and predictions surrounding possible effects.

In general, our estimates reveal that casino entry does significantly impact the danger posed by drunk drivers, but that the direction and size of this effect is related to the size of the population where the casino opens. Specifically, our best estimate indicates that alcohol-related fatal accidents increase by about 9.2% for casino counties with the mean log population, yet this estimated effect declines as population increases. Although this is a striking result, we will demonstrate below that our 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 regulations), as well as changes in factors that may influence overall driving risk separate from drinking behavior (e.g., road construction, weather). Furthermore, these estimates are also robust to several alternative definitions of the control group, the dependent variable, and to the estimation method selected (e.g., weighted least squares, Poisson, probit).

2. Background and theoretical considerations

The principle motivation by governments to allow casinos to open in their jurisdictions is the hope that casinos will create economic growth and increase tax revenues at the state level. The casino expansion of the early 1990s had mostly died off until the 2007–09 recession compounded state-level fiscal crises. Consequently, much of the existing research focuses on the pre-2000 period of time that saw the vast majority of casino openings in the U.S. Given the typical motivation for casinos, research has often focused on evaluating the impacts of casino introduction on economic development or government revenue generation (e.g., Elliott and Navin, 2002; Mason and Stranahan, 1996; Siegel and Anders, 2001). While less numerous, other studies have looked at how casino introduction has impacted consumers' behavior with respect to related sectors of the local economy, such as hotels, restaurants, bars, and property values (e.g., Anders et al., 1998; Popp and Stehwien, 2002; Siegel and Anders, 1999; Wenz, 2007). Of course, other researchers have also recognized that this large increase in the presence of casinos and gambling could have important impacts on crime, bankruptcy, divorce, and other social ills (e.g., Barron et al., 2002; Curran and Scarpitti, 1991; Garrett and Nichols, 2008; Grinols and Mustard, 2006; Stitt et al., 2003; Thalheimer and Ali, 2004). However, little attention has been paid to how the introduction of casinos into a community or region impacts drinking and driving habits and their effects. This lack of research is surprising, given the degree to which alcohol use often accompanies casino gambling.

There is an extensive literature that estimates the impacts of changes in public policies, such as minimum legal drinking age laws, beer taxes, and zero-tolerance policies, on drunk driving behavior (e.g., Carpenter, 2004; Chaloupka et al., 2002; Dee, 1999; Ruhm, 1996). The motivation behind these policy changes is that they will impact individual behavior and reduce drunk driving. Of course, any factor that changes drinking behavior or the location of drinking activities can impact drunk driving outcomes, whether intended or not. The introduction of casinos into an area may be one such factor.

One can imagine a variety of ways by which casinos might impact drunk driving behavior. For example, there are several reasons to suspect that casino presence may lead to an increase in drunk driving. First, the location of a casino could promote an increase in the total number of miles driven after drinking, which could lead to an increase in automobile accidents in an area following the opening of a casino. Existing literature on consumer behavior supports the contention that small differences in consumer utility can prompt changes in driving habits. For example, the cross-border shopping literature indicates that people will consume what they desire in an alternate location when their own jurisdiction has limits or restrictions on consumption, or relatively high costs (Asplund et al., 2007; Ferris, 2000). Some Canadians, for example, drive great distances to consume health services in the U.S. In the case of casinos, their presence may draw people from a large surrounding area to gamble. However, this effect on drunk driving fatalities would depend on the extent to which the introduction of casinos actually does lead to a net increase in the number of people driving and the average distance to casinos. The distance to casinos is likely to decrease as casinos become more widespread, but the introduction of casinos could increase the number of people driving in the area immediately surrounding the casino. If this is the case, we would expect that the introduction of a casino will likely increase the number of miles driven in a county, which could also increase the amount of drunk driving accidents, *ceteris paribus*, as drinking and gambling often go together.

Similarly, a product differentiation effect could also lead to greater distances driven after drinking. Specifically, Lee (1997) applies a Löschian location model (Lösch, 1954) to describe the hexagonal market areas created by bar service differentiation. He posits that bar differentiation leads to more drunk driving. As casinos can act as a substitute for bars in many ways, yet allow for extensive gambling activities while drinking, the introduction of a casino may increase the degree of product differentiation among drinking options in an area. So, one can assume that consumers will drive to the casino if their additional transportation and time costs do not cause their total costs to exceed their benefits from being able to gamble and drink. Therefore, the casino represents a new option for some consumers and may be likely to increase the proportional miles driven drunk as a result.

Of course, the impact of casinos on drunk driving could be negative, and this alternative possibility must be considered. The attraction of a nearby casino may cause a substitution effect, as many individuals substitute away from other discretionary pursuits, such as a night out at the local bar or club, to spend an evening gambling at a casino. As a result, if the ability to gamble at a casino creates a sufficient substitute to drinking at a bar, or if casino patrons drink less at the casino than they would have without the casino option, then we may see a decrease in alcohol-related accident risk in an area after the introduction of a casino. Moreover, while many casinos must follow local "bar time" laws when it comes to serving alcohol, the casinos themselves are typically open 24 h. This could give intoxicated individuals the opportunity to sober up before driving home.[2] We should also point out that, unlike casinos in Las Vegas or Atlantic City, which give complementary alcoholic beverages to those gambling, many casinos charge for alcoholic beverages, so a gambler would have to "sacrifice" some of their gambling dollars in order to purchase a drink. This might lead patrons to drink less at the casino than they might have otherwise at some bar or nightclub.[3] Lastly, if we assume that some

[2] We see professional sporting events actively facilitating this behavior as they frequently stop alcohol sales after the third quarter of a football game or after the 7th inning in a baseball game, for example.

[3] Casinos' policies with respect to alcohol vary by market; some states have a law that prohibits casinos providing free alcohol to patrons. That said, there is extensive complexity involved in identifying the casino specific treatment of these policies, which prohibited us from being able to specifically control for casino alcohol policies in our model. This exclusion would only impact our findings significantly if there was correlation between the county population and the likelihood of offering free

drinkers choose to frequent the closest drinking establishment to their residence, by increasing the number of drinking options in a county, the casino could reduce the distance driven after drinking among some intoxicated drivers.

Regardless of the economic theory, the literature discussed above would support the idea that the relatively dispersed nature of casino locations across the country could lead to an increased accident risk due to greater distances traveled by drunk driving gamblers. Indeed, some casinos have acknowledged such problems. For example, the Connecticut-based Mohegan Sun Casino admitted that there is a problem with drunk drivers leaving their casino (WFSB Hartford, 2009). A few studies have indirectly examined the link between casinos or gambling and DUI arrests (e.g., Reuter, 1997; Stitt et al., 2003; Stokowski, 1996; Wilson, 2001), but drunk driving is not their primary focus. Furthermore, none of these studies addresses the potential link between casinos and *alcohol-related fatal accidents*. We can find no study that has previously tested for such a link.

In addition to the economic literature on drinking and driving, the gambling and psychology literatures provide an anecdotal link between casinos and drunk driving. In particular, a large proportion of problem gamblers[4] have coexisting disorders ("comorbidity"), including alcohol abuse, which may affect the relationship between casinos and drunk driving. For example, Welte et al. (2001) find that problem drinkers (alcoholics) are 23 times more likely to have a gambling problem than individuals who do not have a drinking problem. Petry et al. (2005) have estimated that over 70% of pathological gamblers in the U.S. also have an alcohol use disorder. Since gamblers are the individuals we would most expect to increase their driving after the introduction of a casino, and since a disproportionate number of alcoholics are gamblers, then it is plausible to expect a casino to encourage travel disproportionately by the individuals who are most likely to drive while intoxicated. Of course, casino patrons are not all problem gamblers and alcoholics, but there is a small proportion of the population that has drinking and gambling problems, and this may have an impact on any relationship between casinos and drunk driving and therefore, on alcohol-related fatal accidents.

Given the discussion above about the potential impacts of casino introduction on drinking and driving behavior, we must consider what factors we anticipate will impact the strength of a particular effect on drunk driving. Specifically, we believe that the largest factor is likely to be population of the area where the casino locates. In large cities, casino patrons will disproportionately be locals, who do not need to travel great distances, or who may have public transportation options. Indeed, the opening of a casino in an urban area may not be expected to have any impact on miles driven, since the casino represents one new entertainment option out of many existing ones. Yet, in the case of rurally located casinos, with small local populations, a large proportion of the casino's customers are likely to have driven longer distances, relative to patrons at urban casinos. Therefore, we might expect miles driven and the number of alcohol-related fatal accidents following the introduction of a casino to be greater in rural than in urban areas. To the extent that casinos – either rural or urban – attract new tourists to a particular area, then we would expect an increase in miles driven. Overall, we believe that the *net* impact of casino introduction on alcohol-related traffic risk will depend on the population or "urbanicity" of the area where the casino locates, and this hypothesis is reflected in our empirical specification.

In the remainder of the paper, we investigate whether these theoretical predictions are verified by observing the how local alcohol-related fatal accidents were impacted by casino entry. We find substantial evidence that the number of fatal accidents involving alcohol is impacted by casino entry, but the magnitude and direction of the effect is indeed dependent on the size of the local population.

3. Data and methods

In order to analyze any relationship that might exist between casinos and alcohol-related fatal accidents (ARFAs, hereafter), we must choose appropriate data. Although data are readily available at a state level, such aggregation would likely not foster a good analysis since many states with casinos have few of them, which means the locations of the casinos would be a necessary control for the analysis. County-level data are available on casinos and on ARFAs, and we view this to be the ideal level for our analysis.

3.1. Casino and fatal accident data

The vast majority of the expansion in the U.S. casino industry occurred during the 1990–2000 period. Between 2000 and 2008, only one state (Pennsylvania) legalized commercial casinos. We are interested in analyzing whether and how the spread of casinos has affected ARFAs, so like most casino-focused studies we choose the 1990–2000 period of time for our analysis. A set of 131 counties saw casinos open within their borders between 1990 and 2000. These casino counties represent the treatment group for our primary estimates.[5] We link these data on casino location to data on fatal vehicle crashes obtained through the National Highway Traffic Safety Administration's (NHTSA) Fatality Analysis Reporting System (FARS).[6] 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. The legal maximum BAC is set by state government and every state currently has a maximum legal BAC of 0.08.

Although Federal law requires that BAC levels be obtained from every fatal crash, this is frequently not done and can lead to substantial bias in any estimation. The NHTSA is aware of this issue and provides imputed measures of the BAC for all drivers who were not tested. The NHTSA creates the imputed values using a multitude of characteristics in each case, including factors such as time of day, day of week, contents of the police report, and position of car in the road (NHTSA, 2002).[7] 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 this new NHTSA procedure (e.g., Villaveces et al., 2003; Hingson et al., 2005; Cummings et al., 2006).

drinks, and our anecdotal research suggests this is not the case. We do, however, recognize this limitation of our analysis.

[4] A "problem gambler" is defined as a person that gambles to such an extent that it disrupts their professional or personal life. Psychologists have estimated that about 1–3% of U.S. adults have a gambling problem (American Psychiatric Association, 1994). However, it is beyond the scope of our study to address the various levels of problem gambling severity.

[5] For clarity, Atlantic County, NJ and all counties in Nevada were excluded from the analysis due to the unique nature of the casino industry in these areas. Results are robust to this restriction. The list of treatment casino counties is available from the authors upon request.

[6] To be clear, the NHTSA reports fatal accidents on all roadways, not just "highways."

[7] This follows suggestions from Rubin et al. (1998) and improves on the former procedure based on discriminant analysis (Klein, 1986; NHTSA, 2002).

Table 1
County-year means and proportions of key variables in balanced-sample analysis.

	All counties	Casino counties	Non-casino counties
Number of annual fatal accidents involving a driver with a blood alcohol content (BAC) above 0.08	31.83	39.29	30.51
Number of fatal accidents involving a driver with a positive blood alcohol level	37.71	46.54	36.15
Number of fatal accidents involving no alcohol	63.89	78.52	61.30
Population (unweighted, from U.S. Census Bureau)	150,471	270,803	139,501
County unemployment rate (from Local Area Unemployment Statistics)	5.68%	5.98%	5.63%
Prevailing beer tax per gallon (in 2000 dollars)	$0.24	$0.23	$0.24
BAC law specifying minimum of 0.08	29.1%	36.03%	27.84%
Zero-tolerance laws	56.23%	59.48%	55.66%
Number of observations (number of counties)	17,248 (1,568)	1,441 (131)	15,807 (1,437)

Notes: (1) As the primary estimation is weighted by county population, the above means and proportions are weighted similarly, unless noted. (2) To maintain consistency with the primary sample utilized in the analysis, the above values are from a balanced sample of counties, and they exclude data from the state of Nevada and from Atlantic County, NJ.

We aggregate NHTSA counts of fatal accidents involving a driver with a BAC content exceeding 0.08 by county and year. We can link annual fatal accident counts to other available county-level annual data (i.e., population data from the Census Bureau). Moreover, annual counts provide us with a sufficient number of accidents for each county upon which to base the analyses.

Unfortunately, county authorities sometimes fail to report any accident data for a particular year, leaving us with an unbalanced panel. For our main estimates we include only counties for which FARS data were available for all 11 years of our analysis (1990–2000). We do, however, test the robustness of this restriction. Table 1 reports means and proportions of variables included in the analysis for both the treatment counties and counties without a casino. The second column in the table, casino counties, includes all county-year observations for counties that have a casino present within their borders for at least 1 year in the sample time period. In many cases there are small differences between the treatment and control counties, although some variables, such as county population and the prevailing beer tax, are very similar. There are two notable differences between the casino and non-casino counties. First, higher unemployment rates are observed in the treatment counties. This is consistent with the idea that some municipalities or states attempt to utilize casinos as a form of economic development in depressed areas. The second difference is that there is a larger number of fatal automobile accidents (alcohol-related and non-alcohol-related) in the casino counties.

3.2. Methodology

We first pool a balanced sample of all of the counties in which a casino was open (the treatment group) and the remaining counties in the U.S. that did not have a casino present during the sample period (the control group). We experiment later with alternative samples and the results prove robust. Our basic analysis begins with the following fixed effects regression model:

$$\mathrm{ARFA}_{ct} = \alpha_c + \tau_t + \beta_1 C_{ct} + \beta_2 P_{ct} + \beta_3 CP_{ct} + \gamma' X_{ct} + \varepsilon_{ct}, \quad (1)$$

where subscript c denotes counties and t denotes years. ARFA is the number of alcohol-related fatal accidents; α_c and τ_t are county and time fixed effects, respectively; C is a dichotomous variable indicating the presence of a casino; P is the log of county population; CP is an interaction term between the casino variable and the log of the county population; X is a vector of additional variables, explained in more detail along with the other variables, below; and ε is the error term.

ARFA is defined in most estimates as the log of the number of fatal accidents involving a driver whose measured BAC exceeded 0.08 in a given county-year cell. Specifically, in constructing ARFA we add one to the number of ARFAs in each county-year to prevent losing the very small counties that may have zero accidents when the values are logged. Results prove robust to this approach. We judge logs to be the most appropriate scale for the dependent variable because the median estimated number of fatal accidents for the county-years in the sample is less than the mean.

Given that the number of accidents may be highly variable in smaller counties and that we use data aggregated to the county-year level, we weight the OLS estimates by county-year population size obtained from the Census Bureau. Estimation of Eq. (1) will therefore use weighted least squares (WLS). We also correct all standard errors to allow for non-independence of observations from the same state through clustering. This follows Arellano (1987) and Bertrand et al. (2004). We show later that redefining the dependent variable or using a different estimation model yields qualitatively identical results.[8]

Variable C is a county indicator that is set to one if the county has a casino present in a given year.[9] Variable CP is the interaction of the casino dummy and the log of the county population. To allow for a more meaningful interpretation, we will also estimate CP as the interaction of the casino dummy and the demeaned log of the county population. Thus, the estimate of β_3 can be read as an estimate of the percent increase in ARFAs after a casino opens in a county with an average log population, relative to a control group of counties that did not have a casino open at any point during the sample period. As mentioned earlier in the paper, we believe that drinking and driving outcomes are likely to be affected by the population of the counties impacted, hence variable CP, capturing the casino-population interaction, will help to identify whether such a relationship does exist.[10]

The inclusion of county fixed effects (α_c) and time fixed effects (τ_t) are imperative to proper identification when utilizing this empirical research design. Specifically, the inclusion of county fixed effects captures differences in accident prevalence across coun-

[8] For example, a Poisson regression (Hausman et al., 1984) could be used given the discrete measurement of the dependent variable (before logging). 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 of which are later reported in Table 4.

[9] We recognize that utilizing a dichotomous variable to indicate whether there is a casino present in a county or not ignores any differences in the size of the casino environment across counties and over time. Unfortunately we were unable to obtain any reliable or comprehensive measure of casino size at the county level or for individual casinos. This is a limitation of our analysis.

[10] It is important to note that the inclusion of the log of population is equivalent to the inclusion of the log of population per square mile, given that county fixed effects are included and that the area size of counties does not vary over time.

ties that are time-invariant. Therefore, the inclusion of fixed effects allows us to compare counties with persistent differences in accident prevalence, without concern that these differences will impact our estimates. On the other hand, time fixed effects capture changes in accident prevalence over time that is common in all counties.

We recognize the recent empirically rigorous studies that evaluate the determinants of drunk driving (e.g., Dee, 1999; Baughman et al., 2001; Eisenberg, 2003) and understand that our empirical strategy should isolate the impact of casinos from the other determinants of ARFAs. We know that population growth will likely increase the number of accidents, so one control is the log of the county's population (*P*), obtained from the Census Bureau. Although we think casino openings are likely exogenous in the context of our study, there may exist some correlation between casino presence and some other factors. Our empirical approach addresses this in a number of ways. First, 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 ARFAs over time and include them in the *X* vector.

Second, Ruhm and Black (2002) showed that downturns in the economy have a small negative net impact on drinking behavior. So, county unemployment rates collected from the Local Area Unemployment Statistics (LAUS) program are included in *X*.

Third, we are concerned that there may be an underlying propensity for *all* traffic accidents to change in a county (or state) over time because of differences in speed limits, gas prices, general economic activity, highway construction, weather patterns, insurance rates, or other factors that might confound the interpretation of our estimates of ARFAs. To capture these, we employ an approach employed by Adams and Cotti (2008), which utilizes the log number of accidents per county that were *not* alcohol-related (also measured in the FARS data). This control isolates the effect of the independent variables (including the casino variables) apart from the many potentially omitted factors that make it more dangerous to drive in any particular locality. 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.

Another issue that must be addressed in this analysis is the concern that the opening of a casino in a county is correlated with other government policies that are meant to deter drunk drivers. We use data from 1990 to 2000, however, which is a time period beyond the point that most states had engaged in most of their legislative activity aimed at deterring drunk driving. For example, since 1988 the minimum legal drinking age has been 21 in all states. This alleviates the concern that casino passage tended to coincide with legislation aimed to deter intoxicated drivers. The fact that our sample includes casinos from every region of the U.S. further supports the experimental nature of our study.

Nevertheless, during our sample period, there were three state-level variables that changed enough to raise concern that they might confound the interpretation of the estimated casino effect. 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 1 shows that more counties in our treatment group than the control group were affected by this reduction. Dee (2001) and Eisenberg (2003) use somewhat older data to show that stricter BAC requirements reduce 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, many states passed zero-tolerance laws on teen drivers during our sample time-frame. Carpenter (2004) shows that these laws play an important role in reducing drinking and driving among young drivers, so we include a dummy variable indicating if a state had a zero-tolerance alcohol policy in place. Third, alcohol excise taxes varied over our sample period, as some states increased or decreased their rates. 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 such an effect from beer taxes. We include controls for the log of beer taxes (in 2000 dollars) to capture any tax effect. However, a look at Table 1 shows little differential variation in beer taxes between the treatment and control states.

There are obviously other minor state and local laws and regulations aimed at deterring drunk driving, many of which might be effective in certain areas. We find that adding control variables for BAC laws, zero-tolerance policies, and beer taxes does not substantially change our estimated casino effect. So, if these much more visible and effective policies are not correlated with the introduction of casinos, it is unlikely our results would be affected by less visible policies.[11]

4. Results

4.1. Basic results

We begin by estimating Eq. (1) for a balanced sample of all the treatment and control counties. Results are shown in column (1) of Table 2 and indicate that, for counties of near the mean logged population, the opening of a casino increases alcohol-related fatal accidents (ARFAs) by a statistically significant 9.2%. Consistent with our expectations, the casino-population interaction shows that this effect declines as population size increases. Recall, we estimate the casino effect where the casino-population interaction is defined as the interaction of the casino dummy and the demeaned log of the county population.[12] So, the estimates on the casino and casino-population interaction variables provide evidence that casino presence does impact ARFAs, but the population of a county determines the magnitude and the direction of the effect. For example, the estimates in Table 2, column (1) suggest that smaller/rural counties with casinos, such as Sauk County, WI (average sample population = 17,339; log population = 9.76) would see a statistically significant increase in ARFAs of 16.9% (*p*-value = 0.014), while much larger/urban counties with casinos, such as Milwaukee County, WI (average sample population = 936,589; log population = 13.75) would see a statistically significant *decline* in ARFAs of 6.1% from the introduction of a casino (*p*-value = 0.064). In light of our earlier theoretical discussion of the possible effects of casinos on ARFAs, our results may indicate that in rural counties, casinos tend to increase miles driven by intoxicated drivers (potentially from residents of the county and by out-of-county visitors), and therefore make ARFAs more likely. In urban settings, however, it appears that this effect may be more than offset by a substitution of casino patronage for other drinking establishments, coupled with other aspects of urban living, such as a much greater availability of public transportation.

With regard to the other variables in the regression, as expected, all else equal, population growth increases the number of accidents. Also as expected, the number of fatal accidents involving no alcohol is also positive and highly significant. We believe this captures the general accident trend in a county, which is driven by factors that impact the relative driving danger of an area separate from alcohol,

[11] We also included interaction terms of the casino variable and the policy variables. However, none of these interaction terms was significant and they did not affect the overall results.

[12] Average (unweighted) log population in the sample is 11.095.

Table 2
Effects of casino entry on ARFAs, 1990–2000.

Explanatory variables	Dep. variable: Nat. log alcohol-related fatal accidents (ARFAs) WLS	
	(1)	(2)
Casino dummy (C)	0.092**	0.117***
	(0.041)	(0.041)
Casino-population interaction (CP)[a]	−0.058**	−0.081***
	(0.023)	(0.028)
Border county dummy (B)	–	0.107***
	–	(0.033)
Border county-population interaction (BP)[b]	–	−0.069***
	–	(0.017)
Nat. log county population (P)	0.488***	0.449***
	(0.171)	(0.175)
Nat. log non-alcohol-related fatal accidents	0.148***	0.135***
	(0.031)	(0.024)
Zero-tolerance law dummy	−0.052**	−0.056***
	(0.021)	(0.020)
0.08 blood alcohol content (BAC) dummy	0.034	0.029
	(0.044)	(0.038)
Nat. log beer tax (in 2000 dollars)	−0.087	−0.069
	(0.074)	(0.069)
Nat. log county unemployment rate	−0.085*	−0.095*
	(0.051)	(0.054)
Observations	17,248	17,248
Counties	1568	1568
States	50	50
R-squared	0.940	0.941

Notes: (1) Robust standard errors are in parentheses. (2) Estimates are clustered at the state level to allow for non-independence of observations from the same state. (3) Estimates are weighted by county population. (4) Nevada and Atlantic County, NJ have been excluded. (5) Only counties where observations were available for all 11 years are included.

[a] The casino-population interaction is demeaned for interpretation at a meaningful population and is defined as (casino dummy) × [ln(population) − ln(mean population)].

[b] The border county-population interaction is demeaned for interpretation at a meaningful population and is defined as (border county dummy) × [ln(population) − ln(mean population)].

* $p < 0.1$.

** $p < 0.05$.

*** $p < 0.01$.

such as road construction or weather. It is important to note that, although changes in non-alcohol-related accidents are highly correlated with ARFAs, the effect of the casino and casino-population interaction is still significant.[13] Estimates on the remaining controls are as anticipated or are insignificant.

The identification strategy utilized to this point is predicated on the assumption that after the inclusion of fixed effects and time-varying controls, the casino counties are comparable to the non-casino counties. Yet, even though we have controlled for changes in non-alcohol-related trends, there is always the concern that casino openings are somehow correlated with some unobserved trend in ARFAs. Although we view this to be unlikely, in light of the aforementioned controls and the exogenous nature of casinos with regard to drunk driving, we do test for the presence of such a correlation in two ways. First, we fail to reject the null hypothesis that the pre-casino trends of ARFAs in the treatment and control groups are identical, 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 casino entry (p-value = 0.562). Second, we look at the effect of casinos over time by introducing lead and lagged effects, as well as a contemporaneous effect of the casino entry. The lead effects are informative in that we can determine whether the estimates of the casino dummy variable (C) are indeed stemming from the opening of casino, as opposed to the effect of a previously existing trend. The results, presented in Table 3, indicate the expected pattern as the lead effects are not significant and have opposing signs, while estimates only become statistically significant and consistently positive after the casino opens.[14] Overall, these results provide no evidence to suggest that the estimates in Table 2, column (1) are the result of trending differences between the treatment and control counties; instead they appear to be real effects of casinos.

4.2. Robustness checks

Although we view our empirical decisions thus far as reasonable, we recognize there are several alternative definitions of the sample, the dependent variable, the policy variables, and estimation methods that we could have employed. In order to verify that the results are not sensitive to our choices, we next engage in a series of robustness checks, which we summarize in Table 4. For comparison, row (1) repeats the primary results from Table 2, column (1), a 9.2% increase in ARFAs after casino entry, with a −0.058 estimated coefficient on the demeaned casino-population interaction.

Our first set of robustness checks tests the robustness of our chosen estimation model. We have been using weighted least squares

[13] One could envision a falsification exercise where the log of non-alcohol-related accidents is the dependent variable. However, we find no evidence of a casino effect on accidents with no alcohol involved (Coef. = 0.019, SE = 0.035). Likewise, the estimated effect of the casino-population interaction is both statistically and absolutely insignificant (Coef. = −0.001, SE = 0.018). It is only the *alcohol*-related crashes that are impacted by casino entry.

[14] A test of the joint significance of leads fails to reject the null hypothesis that leads jointly equal zero (p-value = 0.5506). Test of the joint significance of lags successfully rejects the null hypothesis that lags jointly equal zero (p-value = 0.0636).

Table 3
Effects of casino entry on ARFAs, leads and lags.

	Dep. variable: Nat. log alcohol-related fatal accidents (ARFAs) WLS Casino year effects
Lead 3 years+	0.049
	(0.070)
Lead 2 years	−0.074
	(0.063)
Lead 1 year	−0.036
	(0.078)
Year of casino opening	0.057
	(0.052)
Lag 1 year	0.126**
	(0.046)
Lag 2 years	0.090
	(0.060)
Lag 3 years +	0.126**
	(0.059)
p-value: test joint significance of leads	0.5506
p-value: test joint significance of lags	0.0636*
Observations	17,248
Counties	1568
States	50
R-squared	0.940

Notes: (1) Results are analogous to those presented in the first column of Table 2. Hence, all control variables from Table 2, column (1), as well as interactions between the lead/lag dummies and log of demeaned population were included in this regression. (2) Robust standard errors are in parentheses. (3) Estimates are clustered at the state level to allow for non-independence of observations from the same state. (4) Estimates are weighted by county population. (5) Nevada and Atlantic County, NJ have been excluded. (6) Only counties where observations were available for all 11 years are included.

* $p < 0.1$.

** $p < 0.05$.

estimation with a log transformed dependent variable. However, several alternative estimation methods are also potentially good options. For example, given the discrete count-nature of the accident data, a Poisson approach may be appropriate. Row (2) of Table 4 provides estimates using a fixed-effect Poisson estimation approach and shows similar inference to the WLS estimates.[15] Next we note that frequently in the accident literature the dependent variable will be divided by a measure of population to generate an accident rate and a logit or probit approach will be utilized. In row (3) we have taken this approach, using a probit model to estimate the effects of casino entry. Again results prove robust as the estimated marginal effects are very similar to the WLS estimates. The last alternative estimation approach tests the sensitivity of the basic results to the use of county fixed effects. We recognize that three of the control variables we use are measured at the state level: beer tax, zero-tolerance laws, and lower BAC requirements. In row (4) we employ state rather than county fixed effects; the estimates remain very similar to our original estimates.

Next, we checked the robustness of our chosen specification. First, to this point, we have considered a county to be a "casino county," with the casino dummy variable equal to one if a casino was open within a county's borders at any point during a calendar year. We could have weighted the casino dummy differently for the first year a casino is present in a county, because the impact may be lessened if the casino was not operating for the entire year. Alternatively, we could have considered a county as only having been affected by the casino's presence for a given year if the casino was present before the beginning of that year. So, in order to test our results to the sensitivity of the first year weighting we generate estimates where the year the casino opens is given half-weight ($C = 0.5$) or no weight ($C = 0$). As detailed in rows (5) and (6) of Table 4, the overall impact of casinos remains both quantitatively and qualitatively the same regardless of how we treat the casino dummy variable and the corresponding casino-population interaction during the first year of a casino presence. Next we consider the robustness of our dependent variable definition. Instead of using the log of ARFAs where driver BAC exceeded 0.08, we could have chosen the log number of fatal accidents involving any alcohol. When we do this the outcome is nearly the same, as shown in row (7).

In our final set of robustness tests, we test whether the sample group we have been using is unduly influencing the results. We test three alternative samples. In the first alternative, we restrict the control group to only those states with a casino present at some point during the sample time-frame (1990–2000). From the perspective of cultural or regional driving norms, the non-casino counties from states with some casino presence may provide a better control group.[16] The results of this test are reported in row (8); the story remains virtually unchanged. Next, on a similar theme, we used a logistic regression to calculate propensity scores for each county as a means of matching the treatment counties to the most similar control counties in the sample. Results of this examination are presented in row (9) and also prove robust, albeit less precisely measured. Finally, thus far we have been using a balanced sample of counties, which has imposed a strong restriction on the data. So, in our last robustness check we replicate the analysis from Table 2, column (1), utilizing the much larger unbalanced sample. Although the estimated effect of casino entry on the mean population is larger, the inclusion of these additional counties does not alter our qualitative findings.

Overall, the results detailed in Table 4 provide us with a broad and comprehensive picture of the nature of the measured effects. Under most of the alternatives, we estimate an effect that is slightly stronger than the basic estimates. Under a few of the alternatives, the precision is smaller, but, regardless of empirical assumptions, the qualitative conclusions of the primary model remain intact. We therefore regard our results presented in Table 2, column (1) as being robust and fairly conservative estimates for the impact of casinos on ARFAs.

4.3. Border county analysis

In Section 2 of the paper we advanced several potential mechanisms that might explain how opening a casino might impact alcohol-related fatal accidents. One such mechanism for an increase in drunk driving rates comes from the existing literature on consumer behavior which suggests that small differences in consumer utility can prompt changes in driving habits. In particular, if casinos act as a destination and attract people from a wide area, we could see an increase in accident deaths in counties near a casino county, as well as in the county in which the casino is located. Returning to Table 2, we address this possibility by testing for casino effects on fatal accidents in counties adjoining the casino counties. If there are increases in ARFAs in the adjoining counties after casinos open, this is suggestive that people are driving greater distances in response to this change in their incentives.

Our specification of this analysis, presented in Eq. (2) below, is nearly identical to that presented in Eq. (1), except we now include

[15] Due to a limitation in the Stata programming, the estimation is not weighted and the standard errors from these estimates were clustered at the county, rather than the state level.

[16] For these estimates, we exclude counties from states such as Maine and South Carolina, which do not have any casinos present between the years 1990 and 2000.

Table 4
Robustness checks of the basic results.

	Model	Casino dummy (C)	Casino-population interaction (CP)[a]
(1)	Basic specification (repeated from Table 2, column 1)	0.092**	−0.058**
		(0.041)	(0.023)
	Alternative estimation method		
(2)	Unweighted Poisson fixed effects	0.088***	−0.068***
		(0.029)	(0.015)
(3)	Fixed effects probit (dep. variable is ARFA rate) (marginal effects shown)	0.069**	−0.049**
		(0.029)	(0.020)
(4)	State fixed effects (instead of county)	0.097**	−0.048*
		(0.046)	(0.028)
	Alternative specifications		
(5)	Casino dummy given half-weight during year casino opened, one thereafter	0.107**	−0.061**
		(0.050)	(0.024)
(6)	Casino dummy given zero-weight during year casino opened, one thereafter	0.099**	−0.053**
		(0.050)	(0.021)
(7)	Dep. variable is log of number of accidents involving any alcohol	0.096**	−0.054**
		(0.042)	(0.023)
	Alternative samples		
(8)	Only counties from a state with a casino (1,002 total counties)	0.099**	−0.055**
		(0.047)	(0.022)
(9)	Propensity score analysis (701 counties)	0.133*	−0.109**
		(0.072)	(0.048)
(10)	Unbalanced panel (3114 total counties)	0.120**	−0.054**
		(0.048)	(0.021)

Notes: (1) Each row represents a separate regression on the dependent variable ARFAs. County and year fixed effects, as well as controls for accidents not involving alcohol, population, beer tax, a zero-tolerance dummy, the local area unemployment rate, and minimum BAC levels are included in all regressions. For the sake of brevity, these other variables are not shown here. Unless otherwise noted, the number of counties in consistent: 1568.
(2) Coefficient estimates and robust standard errors (corrected to allow for non-independence of observations within a state through clustering) are reported.
[a] (Casino dummy) × [ln(population) − ln(mean population)].
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

variable B, which is a dummy variable that is equal to one if a county borders a county with a casino, and variable BP, which is an interaction between the border county indicator and county population:

$$\text{ARFA}_{ct} = \alpha_c + \tau_t + \beta_1 C_{ct} + \beta_2 P_{ct} + \beta_3 CP_{ct} + \beta_4 B_{ct} + \beta_5 BP_{ct} + \gamma' X_{ct} + \varepsilon_{ct} \qquad (2)$$

This specification allows for two distinct treatment groups, counties with a casino and counties that border counties with a casino, and a control group that consists of all remaining counties. This approach provides us not only the ability to estimate if any potential spillover effects of casinos exist in bordering counties, but, in the event spillovers are present, to also re-estimate the impact of casinos on drunk driving accidents in the casino counties against a potentially more appropriate control group.

Results presented in the second column of Table 2 indicate that, for counties of near the mean logged population, the opening of a casino increases ARFAs in border counties by a statistically significant 10.7% and in the casino county itself by 11.7%. Moreover, both the county-population interaction variables are negative and significant, indicating that again the size of the county plays an important role in outcomes. We should point out that, while the estimated border county interaction suggests that highly populated border counties could see a decline in ARFAs, given on the actual border county populations, these estimates would predict an *increase* in ARFAs in nearly 90% of the border counties in the sample. With this in mind, these results suggest that there are generally relevant spillover costs onto neighboring counties, as residents seem to drive to and from casinos.

Overall, findings from this border county analysis seem to indicate that increases in visitors from nearby areas are at least partially responsible for any net increases in ARFAs observed in the casino counties. And, from a policy perspective, this result suggests that jurisdictions that border casino counties should be aware of a heightened risk of drunk drivers returning along major highways from the locations which have operating casinos.

5. Conclusion

This paper is the first of which we are aware to show that casinos impact the fatal accident risk posed by drunk drivers. Specifically, we find that the magnitude and direction of the effect is dependent on the size of the population where the casino is opened. Thus, on average, rural or moderately sized counties will likely see an increase in alcohol-related fatal traffic accidents when casinos are present, but urban or greater-than-average populous counties may be expected to see a decrease in alcohol-related fatal traffic accidents when casinos are present. Among other factors, we believe the net effect lies in the tradeoff between increases in the total number of miles driven while intoxicated in a county (increasing risk), and the potential that casinos may act as a substitute to other venues at which alcohol may be served (decreasing risk), with the former being stronger in all but the most urban areas.

We have shown that this 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 laws), as well as changes in factors that may influence overall driving risk separate from drinking behavior (e.g., construction, weather). Furthermore, these estimates are also robust to several alternative definitions of the control group, the dependent variable, and to the estimation method selected (e.g., weighted least squares, Poisson, probit). Lastly, evidence from an analysis of border counties is consistent with the idea that the dispersed nature of casinos creates a destination effect – particularly in less urban areas – that attracts people from surrounding jurisdictions to drink and gamble, which leads to an increase in ARFAs in the casino county, as well as in the bordering counties.

Overall, this study provides an important new piece of information on the effects of casinos on local communities. This information can be helpful to jurisdictions currently weighing the casino option, as well as existing casino jurisdictions attempting to address the social impacts from casinos. In particular, we hope that this study will provide increased awareness about the potential problems that casino introduction can create, especially on rural highways, and that local communities will take the appropriate steps necessary to increase the private costs associated with the decision to drink and drive.

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