ESSAYS ON HEALTH ECONOMICS: EFFECT OF ECONOMIC FORCES ON DRINKING AND SMOKING-RELATED OUTCOMES

By

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To Vita and Simon

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CHAPTER I

INTRODUCTION

Excessive alcohol consumption and cigarette smoking are two activities that may have large negative impacts on health. Countless laws and research have focused on these activities because of the severity of their outcomes. In this dissertation, I address three economic forces that influence drinking and smoking behaviors directly and indirectly. I investigate how the opening of a Native American casino changes the drinking behavior of different populations. I research how tax competition affects cigarette prices and whether beer excise taxes fully pass through to prices.

In chapter 2, I provide new evidence on the effect of Native American casino openings on alcohol behaviors of Native Americans and non-Native Americans. I use data from Centers for Disease Control's Behavioral Risk Factor Surveillance System (BRFSS) and difference-in-difference models with county and year fixed effects to measure these effects. I find that Native American casino openings are associated with significant increases in drinking participation in the past month and the average number of drinks consumed per occasion for non-Native Americans. I also find that Native American casinos are associated with a large increase in binge drinking among non-Native Americans. Effects on Native Americans are inconclusive. The casino-related increases in binge drinking for non-Native Americans are larger among men, 18-40 year olds, and individuals with a high school degree or less. These findings provide a mechanism for results from previous work that casinos increase drunk driving fatalities and crime.

Multiple studies have shown that cigarette taxes are more than fully passed through to cigarette prices and that access to a nearby state with a lower cigarette tax also reduces local cigarette prices (Sullivan and Dutkowsky 2012, Hanson and Sullivan 2009, and Harding, Leibtag, and Lovenheim 2012). Chapter 3, co-authored with Christopher S. Carpenter, provides new evidence on the price effects of two previously unstudied sources of tax competition: sales from nearby Native American reservations and online sales. Using quarterly data on local cigarette prices from 1976-2003, we show the openings of nearby Native American casinos and increases in state internet penetration are associated with significantly lower local cigarette prices. Specifically, the opening of a Native American casino within 25 miles of a city center is associated with a 1.6-2.7 cent lower per-pack price, while a 50 percentage point increase in internet penetration is associated with a 22-26 cent per-pack price reduction. These effects are robust and are not observed for other local prices for which there is no potential tax savings. Our results further our understanding of how tax competition affects local cigarette prices and provide important context to multiple studies linking Native American reservations and internet penetration to cigarette smuggling behavior.

Hundreds of studies in health and public economics control for state beer taxes to capture a plausibly exogenous measure of local alcohol availability, yet very little is known about how beer taxes are shifted to beer prices. Chapter 4, co-authored with Christopher S. Carpenter, presents new evidence on beer tax pass-through using newly digitized data on the prices of over 30,000 six-packs of beer in 756 cities from 1982-2012. In standard two-way fixed effects models we estimate that a $1 increase in state beer taxes significantly increases local beer prices by $0.68-$1.10, much less than previously thought.

These three chapters provide new empirical evidence regarding drinking behavior and the effect of taxes on cigarette and beer prices. The results show that the opening of a Native American casino leads to an increase in drinking participation and intensity among non-Native Americans. Cigarette tax price competition from Native American reservations and internet sales lead to cheaper cigarette prices in nearby cities. Beer excise taxes are passed through to beer prices at a lower rate than previous research has shown. These findings provide important, new information for regulators regarding the direct and indirect effect of casinos, taxes, and tax competition on drinking and smoking behavior.

CHAPTER II

EFFECTS OF CASINOS ON ALCOHOL BEHAVIORS: EVIDENCE FROM NATIVE AMERICAN CASINO OPENINGS

Introduction

Native American casinos in the United States earned a total of $28 billion dollars in revenue in 2013, while Las Vegas earned $6.5 billion dollars over the same year.[1] Native American casinos are currently operating in 29 states with plans to open additional casinos in New York and Arizona. Planned Native American casinos lead to fierce political debate as states and communities weigh the potential benefit of increased tax revenue against the potential negative externalities associated with casinos.

This chapter provides an important potential mechanism that explains why casinos are associated with social ills. I show that Native American casinos are associated with increases in alcohol behavior, particularly binge drinking, which may lead to crime, drunk driving, and violence. Of the few studies that have analyzed the effects of casinos, most highlight loss of money through gambling and tourism as the mechanisms for negative outcomes. However, there has been extensive work that shows increased alcohol consumption leads to the same social ills with which casinos are associated. By bridging these two separate literatures, I provide a better understanding of how casinos may lead to social ills, and I identify an area where policy makers may focus efforts to mitigate the negative impact of casinos.

[1] http://www.nigc.gov/Gaming_Revenue_Reports.aspx; http://gaming.unlv.edu/reports/NV_1984_present.pdf

Native American casinos have been in operation in the United States since Congress passed the Indian Gaming Regulatory Act (IGRA) in 1988, yet little work has been done to measure the effect of such establishments on alcohol behavior. Several small scale behavioral studies have found a strong correlation between gambling and increased alcohol consumption, but none have utilized the nationwide variation in geographic location and timing following Native American casino openings to examine the effect casinos on alcohol behavior.[2] The IGRA stipulates that Native American casinos may be operated only on reservation land, and as a result, variation in the presence of casinos over time and across space provides a natural experiment to test claims that Native American casinos lead to potentially harmful alcohol behavior.

In this paper, I provide the first evidence of the effect of Native American casino openings on alcohol behaviors of non-Native Americans and Native Americans from 2004-2012. I use the Centers for Disease Control's Behavioral Risk Factor Surveillance Survey (BRFSS) to estimate difference-in-difference models that include fixed effects for county and year and yearly county-specific unemployment rates. I provide one of the first comprehensive analyses on drinking outcomes that include: consumption of alcohol in the past month, average number of drinks consumed on an occasion, number of days in the past month when alcohol was consumed, participation and frequency of binge drinking, and participation and frequency of drunk driving.[3]

[2] Stewart et.al. (2002) monitor alcohol behavior while gambling in a controlled experiment and find increased participation in and consumption of alcohol relative to a control group. Walker, Clark, and Folk (2012) use data from the National Youth Longitudinal Survey and report that individuals who gambled in a casino were more likely to binge drink.

[3] "Participation" refers to any time in the past month that an alcohol behavior was reported. "Frequency" refers to the number of days in the past month that the alcohol behavior was reported.

I find that counties with a Native American casino opening were more likely to have increases in drinking behavior. There is a 3% increase in non-Native Americans having any alcoholic drink in the past month and an 11% increase in binge drinking due to the presence of a Native American casino. Binge drinking results are inconclusive for Native Americans. Further analysis reveals that of the non-Native American residents, males, residents with a high school diploma or less, and residents ages 18-40 have the largest increases in binge drinking. Results are robust to several specifications including the addition of state-year interactions, the inclusion of casino size measured by square footage, and altering the measurement of casino openings within varying distance measures to an individual's county of residence.

My research expands the knowledge of the effects of casinos and makes an important contribution to the literature. It bridges an existing body of research that shows casinos are associated with bad health and social outcomes and an independent body of research that finds consuming alcohol increases the likelihood of those same bad health and social outcomes. My research suggests that Native American casino lead to increases in binge drinking and other measures of alcohol consumption, which provides a potential mechanism for findings that casinos lead to bad health and social outcomes. I also explore the effect of Native American casino openings on several alcohol behaviors in greater detail, with several controls and robustness checks. Finally, I extend the time period of analysis beyond previous research to 2004-2012.

My work informs the decision to open casinos that occurs in the present day.[4] States and communities that have approved new Native American casino construction have accounted for the benefits of a new source of tax revenue, but the unintended externality of increased binge drinking may lead to a non-optimal decision if ignored. This chapter identifies an effect that could be ameliorated with policy action.

The rest of the chapter is as follows: Section II describes previous literature, Section III details the empirical approach and data, Section IV describes results, and Section V provides a discussion and concludes the paper.

Previous Literature

Following the 1988 Indian Gaming Regulatory Act (IGRA), there was a rapid increase of Native American casino openings. Most of the literature exploring the effects of Native American casinos has focused on the effect on crime rates and financial well-being, and by comparison, few studies have examined the effect on the health outcomes and behaviors. A large body of research has examined the economic impact of Native American casinos on neighboring communities. Research has shown that casinos openings decrease housing values (Huang, Humphreys, and Zhou, 2014; Gazel, Rickman, and Thompson, 2001) and increase personal bankruptcy (Garrett and Nichols, 2008; Barron, Staten, and Wilshusen, 2002). Other scholars have explored the effects of Native American casino openings on illegal behavior, finding that casinos increase drunk driving (Cotti and Walker, 2010). Wolfe et al. (2012) is one of the few papers that investigate the

[4] http://www.nativetimes.com/index.php/business/gaming/10497-casino-study-renews-hope-for-maine-s-indian-tribes

effect of Native American casinos on health outcomes on Native Americans exclusively. Using BRFSS data from 1988-2003, they find small improvements in health outcomes and a small increase in the number of days of binge drinking.[5]

Studies have also analyzed the potential negative effects of casino openings more generally (not limited to Native American casinos). Huang, Humphreys, and Zhou (2014) show a 6-7% decline in housing values as a result of a casino opening in an urban location. Grinols and Mustard (2006) study casino openings from 1977-1996 and find that a casino is associated with an 8% increase in crime in a county. Gazel, Rickman, and Thompson (2001) perform a case study of Indian casino openings in Wisconsin and find increases in aggravated assault and automobile theft in counties with a casino. Wilson (2001) and Stitt, Nichos, and Giacopassi (2003) examine case studies of riverboat casino openings on crime in surrounding counties. They find mixed results that are likely due to the limited scope of the data. Cotti and Walker (2010) examine casino openings from 1990-2000 and the effect on drunk driving rates. They find that casino openings are associated with a 9% increase in drunk driving. Each of these papers highlight gambling losses and tourism as potential sources of increased crime associated with casino openings.

[5] Researchers have also examined the effect of Native American casino openings on employment and income. Evans and Topoleski (2002) report the change in employment for Native Americans and non-Native Americans in counties with Native American casino openings between 1988-2000. They find that four years after an Indian casino opening, there is a 12% increase in the employment to population ratio, a 14% decrease in the number of working poor for the Native American population, and a 2% decrease in mortality for the casino county. Akee et al. (2010) and Akee et al. (2013) use the Great Smoky Mountain Study to analyze the effect of unconditional income transfer payments from casino income sharing on children's health outcomes. Akee et al. (2010) finds that a $4,000 transfer payment leads to an additional year of education and a lower probability of committing a minor crime. Akee et al. (2013) reports the same transfer payment increases the BMI of children from low SES families relative to children from high SES families.

Unlike a typical drinking establishment, Native American casinos may lead to more intense drinking behavior as a result of the "Las Vegas-style" gaming activities they provide. Two studies explore the biological mechanism for the frequent association of casino gaming with alcohol consumption. Stewart et al. (2002) examine differential levels of alcohol consumption among gamblers and non-gamblers. They report that individuals engaged in gambling activities were much more likely to drink alcohol over non-alcoholic beverages, compared to a control group that did not gamble. Grant, Kushner, and Kim (2002) describe the neurological process that may cause gambling to lead to increased alcohol consumption. They report that if the brain develops an inability to regulate serotonin or dopamine, there may be a biological disposition or inclination toward drinking while gambling. These studies provide support for my work examining Native American casino openings and their effect on binge drinking.

There are a variety of social ills that have been associated with casinos. Casinos have been linked to psychiatric disorders, prostitution, hard drug use, and adverse outcomes for children. Walker, Clark, and Folk (2012) use the National Longitudinal Study of Adolescent Health to estimate the association of gambling with drinking and paying for sex. They find that individuals that gambled increased the probability that they participated in drinking by 7 percentage points and that they were 2.4 percentage points more likely to have paid for sex. The negative effects of gambling expand beyond individual behavior. Jacobs et al. (1989) reports that children of compulsive gamblers are more likely to smoke and drink. Cunningham-Williams et al. (1998) find that men are three times as likely to develop a gambling problem and that having a gambling problem is associated with an increased risk of alcoholism and tobacco dependence. They report that casual gamblers (individuals who had placed at least two bets in their lifetime) are more likely to be male and that they have a higher likelihood of suffering from periods of major depression and phobias. These papers underscore the importance of studying the results of Native American casinos beyond increased tax revenue. They have found that there are detrimental health outcomes associated with the main activity that Native American casinos provide, gambling.

A separate literature studies the casual effects of alcohol consumption in general on social ills such as property damage and crime. Carpenter (2007), Joksch and Jones (1993), Markowitz and Grossman (2000) use a variety of empirical strategies to isolate plausibly exogenous variation in alcohol consumption and its effect on property crime, vandalism, and child abuse, respectively. Other studies have reported associations with alcohol consumption and self-reported arrests,[6] physical violence,[7] and spousal abuse by husbands.[8] Dee (2001) finds a 7% decrease in traffic fatalities after states lowered blood alcohol content standards from 0.10 to 0.08. Dee and Evans (2001) report a similar relationship between "Zero Tolerance" laws and alcohol-related traffic fatalities. This area of research clearly establishes an association between alcohol use and negative health outcomes and social ills. These same negative health outcomes and social ills are associated with the opening of casinos, yet no one has examined the effect of Native American casinos on non-Native Americans and Native Americans.

[6] Saffer (2001)
[7] Markowitz (2001)
[8] Markowitz (2000)

My work contributes to the prior literature in three important ways. By finding that Native American casinos cause increases binge drinking and alcohol consumption, I provide a mechanism for how the opening of a Native American casino leads to bad outcomes. This result bridges the literature finding that casinos increase bad outcomes and the literature finding that alcohol use increases bad outcomes. This result is unique because I focus on alcohol behaviors related to Native American casino openings that have not been previously studied in detail: binge drinking participation and intensity, consumption of any alcohol in the past month, the average number of days when an individual consumed alcohol, the average number of drinks consumed on an occasion, and drunk driving participation and intensity.[9] In addition, I analyze the effects of Native American casino on both the Native American and non-Native American populations, and I study a more recent period (2004-2012), which witnessed 58 Native American casino openings in 16 different states.

Data and Empirical Approach

I use variations in the opening and closing of Native American casinos from 2004-2012 to examine county level changes in alcohol behaviors. I gathered the universe of Class III Native American casinos that opened and those that are in operation from 2004 through 2012. The Native American casino data include the name of the casino, tribe that controls the casino, opening dates, number of slot machines, approximate square

[9] Wolfe (2012) examines the effect of a Native American casino opening on Native American binge drinking.

footage of the gaming floor, location by county, and zip code.[10] Bill Evans, Barbara Wolfe, and Jessica Jakubowski provided this information for 1988-2005.[11] I updated the casino information to include 2006-2012 using a variety of sources. I started with the list of tribes and their casinos available on the National Indian Gaming Commission website.[12] The document provides the name of the tribe, the name of the casino, casino address, and telephone number. I used several gambling websites to confirm and augment the list with new opening dates, closing dates, and casino information.[13] The collection of data represents a comprehensive list of Native American casino opening and closing dates for the entire United States.[14] There are 362 Native American casinos located in 156 unique counties that have a median population size of 44,618 inhabitants.

Figure 1 provides a map of Native American casinos that opened in 2004-2012 in the United States. Most Native American casino openings over this period are located in the Midwest, the West Coast, and the Southwest. Figure 2 shows the cumulative number of new casino openings by year. There are a consistent number of casinos opening from year to year from 2004 to 2012. The majority of Native American casinos are smaller than the typical casino found in Las Vegas or Atlantic City. However, the largest casino in North America is a Native American casino. The Foxwood casino, located in

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[10] I am grateful to Bill Evans, Barbara Wolfe, and Jessica Jakubowski for providing data on Native American casino openings.

[11] Brad Humphreys provided the data on non-Native American casinos, which includes riverboat casinos, "cruises to no where", and land based casinos.

[12] http://www.nigc.gov/Reading_Room/List_and_Location_of_Tribal_Gaming_Operations.aspx: accessed May 1, 2014

[13] The main website used was http://500nations.com/Indian_Casinos_List.asp : accessed May 1, 2014.

[14] While the list is comprehensive, I did not include all casinos in the data set. I removed "casinos" with names that clearly indicated their sole business purpose was something other than gaming. For example, the Dyno-Mart with 20 slot machines and TJ's Variety Store with 16 slot machines were not included as casinos.

Connecticut, has over 300,000 square feet of gaming space. The mean casino size is 40,000 square feet of gaming space along with typical casino features that include a hotel, restaurants, and an entertainment venue.

One concern is that the placement of Native American casinos depends on underlying county factors. Native American casinos may be placed in counties with lower average income or higher unemployment which would violate the exogeneity assumption necessary for a causal interpretation of the empirical regression. Appendix Table 1 reports summary statistics for counties that have ever gained a casino, counties that have ever bordered a casino (the implicit assumption being that these counties are similar to counties that gain a casino), and counties that have never gained a casino or bordered a county that has a casino. Column 1 shows that counties with a casino have a slightly lower average household income and a higher unemployment rate than border counties or counties that will never have a casino.

Appendix Table 2 reports regression results for the effect of average county household income and county unemployment rate on the probability of opening a Native American with county and year fixed effects and standard errors clustered at the county level. The results show coefficients that are statistically insignificant and small in magnitude which indicates that Native American casinos are not opened in counties based on these economic characteristics, when conditioning on state and time fixed effects.

In addition to the Native American Casino data, I use the Behavioral Risk Factor Surveillance System (BRFSS) for individual- and household-level outcomes. The survey is a national telephone survey conducted by the Centers for Disease Control (CDC) and administered by each state. The survey is a repeated cross section of the United States from 1984 to the present day. The annual survey consists of a "core" questionnaire and a group of "modules." All states administer the core questions and each state may choose whether to ask the module questions. A CDC employee calls a household and asks each question as it relates to the individual or the household of that individual. These include a wide variety of questions that relate to demographics, self-reported health, drinking and smoking behaviors, health insurance coverage, and other health related questions. I have a sample of 48,199 self-reported Native Americans and 3,177,679 non-Native Americans living in 2,412 counties from 2004-2012.[15] I am able to examine the effect of Native American casinos openings on both populations over the 2004-2012 time period.[16]

The summary statistics of the Native American and non-Native American populations from 2004-2012 with sample weight adjustments are presented in Table 1. The differences in means for demographic and alcohol behavior variables highlight key differences between the two populations. A higher proportion of non-Native Americans have some college education or more and are more likely to be married or co-habiting relative to Native Americans. Native Americans are 6 years younger relative to their

[15] The CDC restricts the FIPS county codes from public use data if the county has a population smaller than 10,000 people or if the number of people sampled from the county is under 50. It is the current policy of the CDC that only an employee of the CDC may gain access to this restricted data, despite my numerous attempts. This will cause some bias in the estimates although the direction of the bias is unclear. If casinos cause larger increases in binge drinking in small counties, my estimates are under reporting their effect. Likewise, if the effect is smaller in these smaller counties, my estimates are over reporting their effect.

[16] I highlight the motivation for studying 2004-2012 in appendix Figure 1. Wolfe et.al. (2012) has previously studied 1988-2003 using restricted access BRFSS data. Before 2004, there are fewer individuals in the Public Use BRFSS data with geographic indicators in counties that gain a casino. As it is shown in Appendix Figure 1, I have a much better coverage rate over the 2004-2012 time period that provides better specification of the model.

non-Native American counterparts. 30% of Native Americans in the sample live in a county with a Native American casino and only 15% of non-Native Americans live in a county with a Native American casino. Non-Native Americans are more likely to have consumed alcohol in the past month (8 percentage points) and drink more frequently (1.03 days) than Native Americans. Native Americans drink 0.30 more drinks on an average occasion than non-Native Americans, which translates to 2 more drinks consumed per month. Native Americans and non-Native Americans are equally likely to have engaged in binge drinking activity, however, Native Americans binge drink more frequently. Non-Native Americans and Native Americans have the same rates of drunk driving in the past month, while Native Americans have higher frequency of driving under the influence in the past month than non-Native Americans.

I use a difference-in-difference fixed effects model to measure the effect of a casino opening on the drinking behavior of Native Americans and non-Native Americans. I follow the model in Cotti and Walker (2010), and I include fixed effects for each county and year, demographic variables, year-county specific unemployment rate, and a control for non-Native American casinos. Specifically I estimate the following difference-in-difference model:

(1) $Y_{ict} = \alpha + \beta_1(Any\ NA\ Casino\ In\ County)_{ict} + \beta_2 X_{ict} + \beta_3 Z_{ct} + C_c + T_t + \varepsilon_{ict}$

where Y_{ict} includes outcome variables for alcohol behavior. The variable $(Any\ NA\ Casino\ In\ County)_{ict}$ refers to any Native American casino opening or existing Native American casino within a county and in a particular year and X_{ict} is a collection of control variables including sex, education, age, and marital status. Z_{ct} is a vector of county year varying controls that includes county unemployment rate and the presence of non-Native American casinos in a county. C_c and T_t are county and year fixed effects. ε_{ict} is an independently distributed standard error clustered at the county level (Bertrand, Duflo, Mullinathan 2004). The fundamental assumption is that Native American casinos are uncorrelated with unobservable characteristics that may affect drinking behavior. I use county unemployment by year from the Bureau of Labor Statistics to control for county-specific economic factors.[17] I have an additional specification that adds a variable to the baseline model that indicates whether a county bordered a county that opened a Native American casino.

Results

I find that non-Native Americans living in a county when a Native American casino opens show substantial, statistically significant increases in binge drinking and other alcohol behavior. These increases are concentrated among men, 18-40 year olds, individuals with high school education or less, and non-smokers. I present detailed effects of Native American casinos on drinking that provide a plausible mechanism for previous work that shows casinos are associated with social ills.

Table 2 presents the results of the reduced form model described in equation (1) for years 2004-2012. The first row reports outcomes for all individuals in the sample, the second row focuses on Native Americans, and the third row presents outcomes for non-Native Americans. Columns 1, 3, and 4 suggest that the opening of a Native

[17] Ruhm and Black (2002) show that the intensity of drinking for existing drinkers is pro-cyclical, therefore, I use county unemployment by year as a control.

American casino in a county leads to higher rates of drinking participation and consumption. Column 1 reports that non-Native Americans are 1.5 percentage points more like to have had a drink in the past month if they live in a county with a Native American casino. Native Americans have a large, but statistically insignificant increase. Coefficient estimates in column 3 show increases in the number of drinks consumed by Native Americans and non-Native Americans. Column 4 presents an 11% increase in binge drinking for the non-Native American population after a Native American casino opening.[18]

There are potential differences between drinkers and non-drinkers. Table 3 presents coefficients for estimates of equation 1 limited to individuals that indicated having consumed alcohol in the past month. Column 4 highlights a 17% increase in binge drinking among non-Native Americans who drank in the past month due to a Native American casino in their county of residence. The number of drinks consumed by non-Native Americans also increases, but only marginally. In contrast, the average number of days both groups drank last month decreases, by a large magnitude for Native Americans, but without statistical significance. The effect of a Native American casino opening on drunk driving participation is inconclusive for both non-Native Americans and Native Americans. Drunk driving intensity for both groups increases, but this result

[18] Percentage changes reported were calculated using the regression coefficient divided by the mean rate reported in the table. Appendix Tables 5 & 6 presents additional regression results by race: White, Black, Asian, Hawaiian, Pacific Islander, and Hispanic individuals. After a Native American casino opens in a county there are large increase in Any Past Month Drinking for Asian, Hawaiian, Pacific Islander and Hispanic individuals respectively. Black and Hispanic individuals report drinking 0.5 and 5.4 more days in a month. Black, Asian, Hawaiian, Pacific Islander, and Hispanic individuals report large increases in Any Binge Drinking in the Past Month. White individuals report small increases in the Number of Drinks Consumed in the Past Month, Any Binge Drinking in the Past Month, and Drunk Driving, although none are statistically significant.

is not statistically significant. An explanation for the findings on drunk driving are explored further in the next section.[19]

The robustness of the large, statistically significant increase in binge drinking by non-Native Americans after a casino opens is further examined in Table 4. Column 1 provides the baseline measure for non-Native Americans from Table 2 for the effect of a Native American casino opening on non-Native American binge drinking. Column 2 reports results for specifications that control for the size of the casino, and these results show similar effects of small and large Native American casinos on non-Native American binge drinking.[20] I add-census region-year interactions, census-division-year interactions, and state-year interactions to the baseline model and report the coefficient on Native American casinos from these specifications in Columns 3, 4, 5. The interactions provide an unrestricted control for any differences in each geographic measure over time and controls for confounding factors that may influence binge drinking and change over time within the census region, census division, or state. The results for non-Native American binge drinking are similar in magnitude and statistical significance to the baseline level. The final two columns restrict the sample to only states (Column 6) and counties (Column 7) that have or ever had a Native American casino

[19] I consider cross county travel in Appendix Table 7 by adding an indicator for "county bordering a NA casino county" to the base specification. All drinking outcomes remain statistically significant and increase slightly in magnitude. The results on drunk driving in the past month remains unchanged, however, the number of times someone drove drunk has a statistically significant 2.3 percentage point increase for non-NA individuals.

[20] I have information on the square footage of the gaming floor of the casino. 10% of the sample has missing square footage data. I considered the missing data to indicate a "small" casino. The median size of casinos is 40,000 square feet. Native American casinos were considered "large" if they were above the median and "small" if they were below. I ran a regression where casinos with missing data were dropped, and results were similar. I also include information on the number of slot machines in the casino. Some casinos focus on card and table games, such as poker, which would provide a poor representation of the casino's size by using slot machines.

between 2004 and 2012. The results are consistent with the baseline estimate. I perform similar robustness checks for non-Native American's drinking participation in the past month and average drinks consumed on an occasion.[21] The results are provided in Appendix Tables 3 and 4.

Table 5 reports results in row 1 for drinking in the past month and in row 2 for past month binge drinking, broken down by four demographic variables: gender, education, age, and smoker status. Women increase drinking participation in the past month as well as binge drinking in the past month. Men have a statistically significant 11% increase in binge drinking in the past month.[22] Individuals with a high school degree or less present an increase in drinking participation in the past month and a 19% increase in binge drinking in the past month. Non-Native Americans with a college degree or more have small, statistically insignificant increases in both drinking outcomes. Non-Native Americans under 41 years old increase binge drinking in the past month by 24% and individuals 41 years old and older are 4% more likely to have had alcohol in the past month. Non-smokers are more likely to have consumed alcohol in the previous month and binge drinking in the past month by 9%, whereas smokers have statistically insignificant increases in both drinking outcomes.

I further investigate the increase in non-Native American drinking frequency in Table 6 by four demographics: gender, education, age, and smoker status. The first row reports coefficient estimates of a Native American casino opening in a county for the

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[21] I completed an additional robustness check for the effect of a Native American casino opening within 50, 25, 10, and 5 miles of a county. I used ArcGIS to measure the distance from the centroid of a casino zip code to the centroid of each county. The results are similar to the baseline specification and are available upon request.

[22] Reported percentage is calculated from the reported mean of 0.16 for binge drinking in the past month.

average number of drinks consumed on an occasion and the second row presents the results for the number of days of binge drinking in the past month. I find that men, individuals with a high school degree or less, and 18-40 year old have large, statistically significant increases in both outcomes. Men have a 7% increase in the number of days of binge drinking in the past month and women have a marginally significant increase in the number of drinks consumed on an occasion.[23] Column 4 reports a marginal increase in the average number of drinks consumed for individuals with a high school education or less and a 26% increase in the number of binge drinking days for the same group. Non-smokers drink more drinks on an occasion after a casino opening while smokers have a statistically insignificant increase. Non-Native American 18-40 years olds drink 0.19 more drinks per occasion and have a 28% increase in the number of days of binge drinking. I find that individuals with a college degree or more, smokers, and 41 year olds and older have statistically insignificant changes in drinking frequency.

Discussion and Conclusion

My results suggest that the opening of Native American casinos significantly increases binge drinking and other alcohol behavior among non-Native Americans. These findings are large and statistically significant, and supported by evidence in other areas of research in alcohol consumption.

The 11% increase in binge drinking is likely a causal mechanism that helps explain the research that finds casinos lead to increased crime, prostitution, and drunk

[23] Reported percentage is calculated from the reported mean of 0.63 for the number of times binge drinking occurred in the past month.

driving.[24] While large, this is a plausible increase for several reasons. Walker, Clark, and Folk (2012) use the Youth Longitudinal survey and find that individuals who had gambled in a casino in the last year showed a 20% increase in binge drinking over the last month. While their study finds a correlation of gambling and binge drinking, the results show an increase of similar size to the results I find using a difference-in-difference approach.

Native American casinos tend to open in smaller counties where their novel attraction may be greater than in a larger county.[25] Scribner, Cohen, and Fischer (2000) show that drinking rates climb as the density of drinking establishments increases. Since most Native American casinos have a full cocktail service for gamblers, at least one bar, and a restaurant, the casino may be considered a very large drinking establishment.

The increase in binge drinking that arises as a result of the mix of activities is further supported by behavioral and medical literature. Stewart et al. (2002) show that individuals participating in gambling activities are much more likely to choose to drink alcohol and drink in larger quantities than individuals in a non-gambling situation. Medically speaking, the physiological response to the act of gambling leads to changes in brain chemistry that increases the desire for alcohol (van Holst et al. 2010). All this is to say that a casino is not a typical establishment that has alcohol to offer its patrons; rather, it is an establishment that makes them likely to consume larger quantities of alcohol than they would otherwise.

[24] See Walker, Clark, and Folk (2012), Grinols and Mustard (2006), Adams and Cotti (2010), respectively.
[25] The median population of a county with a Native American Casino is 44,618 inhabitants.

The fact that I find no result for drunk driving maybe due to the non-specific question asked in the BRFSS survey.[26] Without a precise definition of what "perhaps too much to drink" means compared to the clear definition for binge drinking, I find it plausible that the results could be inconclusive.[27] The ideal question would ask, "Have you driven one hour after having consumed 5 or more drinks over the course of one sitting?" Cotti and Walker (2010) find a 9.2% increase in drunk driving after any casino opening and an even larger effect due to casinos that open in smaller counties. My finding of an 11% increase in binge drinking after a Native American casino opening in the county is within a reasonable range of Cotti and Walker's findings and highlights an additional potential causal mechanism.

I am able to show several interesting results of binge drinking broken down by demographics. Women are likely to increase participation in drinking in the past month at the one percent level, while men do not have a statistically significant change. Thombs, Wolcott, and Farkash (1997) reports that women are more likely to drink to feel sociable in surroundings with alcohol. That is to say, they are more likely to have "a drink in hand" to be social, as opposed to drinking a large quantity of alcohol. This is also consistent with my findings for binge drinking participation and frequency for women. I find a statistically significant 2 percentage point increase in having 5 or more drinks on an occasion for women, but no significant increase in binge drinking frequency. When one considers that an evening at the casino could consist of dinner, a performance,

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[26] The BRFSS drinking and driving question is: "During the past 30 days, how many times have you driven when you've had perhaps too much to drink?

[27] The BRFSS binge drinking question is: "Considering all types of alcoholic beverages, how many times during the past 30 days did you have 5 or more drinks on an occasion?

and gambling afterward, the "occasion" when an individual consumes alcohol lasts much longer and provides a more casual setting with which to consume alcohol.

My results find that men in counties that gained a casino increase binge drinking participation and frequency compared to men in counties that do not gain a casino. Carpenter, Dobkin, and Warman (2014) show that young, Canadian men are twice as likely to participate in "extreme drinking" as women once they reach the minimum legal drinking age.[28] Similarly, I find that men that have access to a Native American casino in their county are much more likely to engage in binge drinking.

Several other studies support the difference in binge drinking by gender. Thombs (1997) reports that men react to situations with alcohol by consuming a larger quantity than women. Giacopassi, Stitt, and Vandiver (1998) show that men are more likely than women to gamble in a casino and drink more. It is possible that the effect of a casino on men's alcohol behavior after a Native American casino opening is much stronger because they are more inclined to drink heavily while gambling than their female counterparts.

Native American casinos appear to have a much strong effect on 18-40 year olds than those over 41. There is a statically significant increase in drinking participation in the past month among the younger cohort and that is most likely attributable to the wide range of activities offered by the casino in conjunction with alcohol service. However, the younger population is much more likely to increase their drinking intensity after a Native American casino has opened. It could be that the younger demographic group is less risk averse than the older demographic group. Cox (1998) finds that gambling

[28] Extreme drinking is defined as 10 or more drinks for men and 8 or more drinks for women.

increases with age up to age 39 and then begins to decline. Given the connection to gambling and increased alcohol consumption, this could be the reason that the younger demographic group consumes more drinks on an occasion and increases binge drinking participation and frequency.

Finally, individuals with a high school diploma or less and non-smokers increase all aspects of their drinking behavior. The result for those with less education is consistent with work by Muthen and Muthen (2000) that finds high school dropouts have a much higher rate of heavy drinking than those that attend college. Non-smokers increasing their consumption of alcohol without any similar statistically significant increase in smoker's alcohol behavior is interesting compared to the findings in the literature that test the cross-price elasticity of drinking and smoking. Dee (1999) finds that alcohol and cigarettes are compliments among teens by using an increase in the minimum legal drinking age to show a decrease in cigarette use. Decker and Schwartz (2000) show that higher alcohol prices decrease drinking and cigarette consumption, but that increases in cigarette prices lead to an increase in drinking. Picone, Sloan, and Trogdon (2004) study smoking bans and find that there is a positive effect of cigarette prices on alcohol consumption. The act of smoking may have already led to a higher consumption of alcohol so that a Native American casino does not lead to an individual changing their drinking behavior. For the non-Smoker, the Native American casino may provide a unique experience that leads to a statistically significant increase in alcohol use.

There are a few limitations to the paper. I cannot analyze earlier periods in the BRFSS data because of a lack of coverage of casino openings. Appendix Figure 1 shows very low coverage of casino openings from 1988-2003 that would result in a misspecification of the model if the time period were included. I am able to identify Native Americans in the BRFSS data, but the small sample size prevents a precise estimate of the effect of Native American casino openings on drinking behavior. Finally, all drinking outcomes are self-reported which could lead to bias in the estimates. This type of self-reported data is used throughout the literature and I have no reason to believe there is a systematic difference in self-reported drinking habits in counties with a Native American casino versus counties without a Native American casino.

While I have shown strong evidence to support that Native American casino openings increase alcohol behavior and binge drinking, this paper is one part of the overall research on Native American casinos. I have attempted to better inform the policy making discussion by pointing out a potential mechanism for many of the documented social ills associated with casinos. This information could be used to mitigate harmful externalities in a similar manner to sporting events that stop selling alcohol before a game has ended.

My results provide an important piece of insight into the larger discussion of Native American casino openings. Many states and communities have been in favor of these new establishments as a source of increased tax revenue that does not require the passage of politically unfavorable tax increases since the Great Recession. I point out the unintended social impact of these casinos by showing an increase in binge drinking and other alcohol behaviors that have been tied to numerous social ills such as: drunk driving, crime, and prostitution. I plan to use a similar design for future work that will examine the effect of Native American casino openings on: hospitalizations, birth outcomes, substance use treatment facility admissions, and other non-drunk driving fatalities.

Figure 1
Native American Casino Openings from 2004-2012

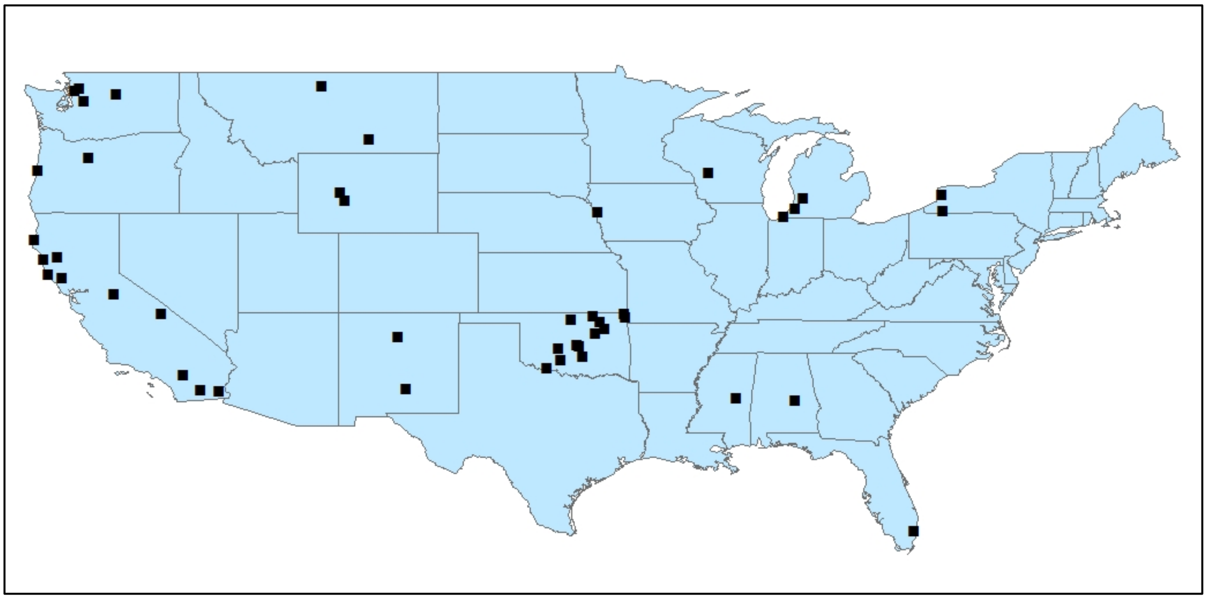


Figure 2
Cumulative Number of Casinos Open and Operating by Year

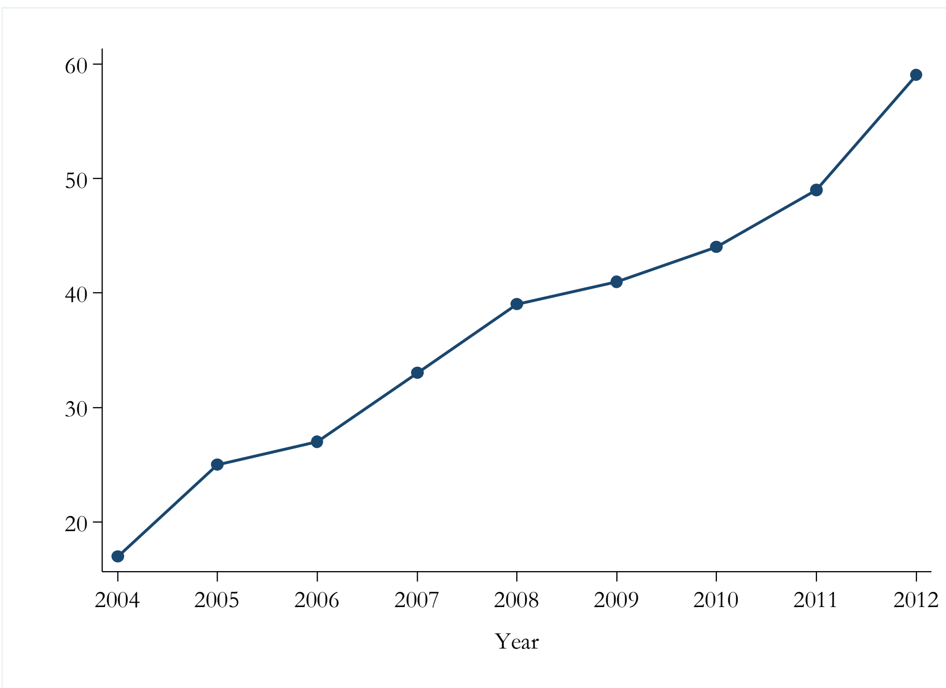


Table 1
Summary Statistics

Variable	Native Americans	Non-Native Americans
Any Past Month Drinking	0.46	0.54
Number of Days Drank Last Month	3.45	4.48
Avg. Number of Drinks Consumed on an Occasion	1.61	1.32
Number of Drinks Consumed in the Past Month	14.35	11.95
Any Binge Drinking Past Month	0.19	0.16
Number of Times Binge Drinking Last Month	0.94	0.63
Any Drunk Driving Past Month	0.02	0.02
Number of Times Drunk Driving Last Month	0.06	0.04
Percentage of Observations in a County with a Native American Casino	0.26	0.14
Proportion with High School Degree or Less	0.57	0.40
Proportion with Some College or More	0.43	0.60
Married or Cohabiting	0.53	0.62
Single	0.25	0.20
Female	0.44	0.51
Age	42.5	46.4
Sample Size	48,199	3,177,697

Weighted means are reported. Sample is the 2004-2012 BRFSS.

Table 2
Native American Casinos and Alcohol Behaviors
BRFSS Adults 2004-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any past month drinking	Avg # days drank past month	# drinks consumed on days drank past month	Any binge drinking past month	# times binge drinking past month	Any drunk driving past month	# times drunk driving past month
Full sample							
Any Native American Casino in county	0.019***	0.058	0.097***	0.018**	0.030	0.000	0.017
	(0.003)	(0.145)	(0.036)	(0.003)	(0.032)	(0.003)	(0.011)
Mean Rates	0.505	4.584	1.060	0.115	0.471	0.016	0.036
R^2	0.107	0.077	0.075	0.080	0.036	0.019	0.010
Observations	3,158,163	3,138,006	3,127,382	3,129,785	3,129,785	1,683,117	1,683,117
Native Americans Individuals Only	0.193	0.223	0.348*	0.015	0.149	-0.034	0.068
Any Native American Casino in county	(0.134)	(0.912)	(0.185)	(0.031)	(0.311)	(0.036)	(0.131)
Mean Rates	0.382	2.842	1.173	0.136	0.673	0.016	0.059
R^2	0.180	0.178	0.169	0.173	0.213	0.214	0.282
Observations	46,981	46,690	46,379	46,407	46,407	24,667	24,667
Non-Native American Ind. Only	0.015***	0.052	0.090***	0.019***	0.028	0.001	0.017
Any Native American Casino in county	(0.003)	(0.141)	(0.037)	(0.003)	(0.034)	(0.003)	(0.011)
Mean Rates	0.507	4.611	1.058	0.115	0.468	0.016	0.036
R^2	0.107	0.077	0.075	0.080	0.036	0.019	0.011
Observations	3,111,182	3,091,316	3,081,003	3,083,378	3,083,378	1,658,450	1,658,450

Note: Values presented are the results of a difference-in-differences model with fixed effects for year and county. I include controls for age, education, marital status, race, and county-year unemployment rate. All observations are weighted using the BRFSS sample weight. Binge drinking is defined as 5 or more drinks on an occasion for men (4 or more for women). Number of Days Drinking and Number of Days Binge Drinking are defined as the number of days individuals participated in the past month. Number of drinks is the number of drinks consumed on average on an occasion when an individual drank. Mean Rates are the averages from 2004-2012 for the group defined in the row. Significance: *** p<0.01, ** p<0.05, * p<0.10

Table 3
Native American Casinos and Alcohol Behaviors among Past Month Drinkers
BRFSS Adults 2004-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any past month drinking	Avg # days drank past month	# drinks consumed on days drank past month	Any binge drinking past month	# times binge drinking past month	Any drunk driving past month	# times drunk driving past month
Full sample							
Any Native American Casino in county	N/A	-0.235 (0.244)	0.109* (0.061)	0.024*** (0.005)	0.018 (0.064)	0.000 (0.007)	0.033 (0.022)
R^2		0.082	0.118	0.123	0.059	0.028	0.019
Observations		1,572,612	1,561,150	1,563,471	1,563,471	836,667	836,667
Native Americans only							
Any Native American Casino in county	N/A	-2.266 (2.227)	0.497 (0.407)	0.032 (0.081)	0.478 (0.927)	-0.144 (0.156)	0.201 (0.418)
R^2		0.266	0.271	0.259	0.316	0.302	0.369
Observations		17,621	17,288	17,316	17,316	9,092	9,092
Non-Native Americans only							
Any Native American Casino in county	N/A	-0.189 (0.231)	0.109* (0.066)	0.026*** (0.006)	0.023 (0.064)	0.002 (0.007)	0.034 (0.021)
R^2		0.082	0.177	0.122	0.058	0.028	0.019
Observations		1,554,991	1,543,862	1,546,155	1,546,155	827,575	827,575

Note: Values presented are the results of a difference-in-differences model with fixed effects for year and county. I include controls for age, education, marital status, race, and county-year unemployment rate. All observations are weighted using the BRFSS sample weight. Binge drinking is defined as 5 or more drinks on an occasion. Number of days drinking and Number of days binge drinking are defined as the number of days in the past month. Number of drinks is the number of drinks consumed on average on an occasion when an individual drank. Significance: *** p<0.01, ** p<0.05, * p<0.10

Table 4
Native American Casinos and Binge Drinking among Non-Native Americans
Robustness Analyses
BRFSS Adults 2004-2012

	(1) Baseline model: control for whether county has a NA casino	(2) Large vs. small NA casinos	(3) (1) + region by year interactions	(4) (1) + division by year interactions	(5) (1) + state by year interactions	(6) States with NA casinos	(7) Counties that will have NA casinos
Any Native American Casino in county	0.019*** (0.003)	--	0.019*** (0.003)	0.020*** (0.004)	0.018*** (0.004)	0.020*** (0.003)	0.019*** (0.004)
Large NA Casino in county (by square footage)	--	0.019*** (0.003)	--	--	--	--	--
Small NA Casino in county (by square footage)	--	0.018*** (0.007)	--	--	--	--	--
R^2	0.080	0.080	0.080	0.080	0.080	0.078	0.072
Observations	3,083,378	3,083,378	3,083,378	3,083,378	3,083,378	1,489,545	377,248

Note: Values presented are the results of a difference-in-differences model with fixed effects for year and county in addition to specification listed in each column. I include controls for age, education, marital status, race, and county-year unemployment rate. All observations are weighted using the BRFSS sample weight. Binge drinking is defined as 5 or more drinks on an occasion. Significance: *** p<0.01, ** p<0.05, * p<0.10

Table 5
Native American Casinos and Past Month Drinking among Non-Native Americans
Results by Subgroup
BRFSS Adults 2004-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Baseline model: all adults	Men	Women	Hs or less	Some college or more	18-40	41+	Smoker	Non-Smoker
Any Drinking in the Past Month									
Any Native American Casino in county	0.015***	0.009	0.022***	0.0227***	0.009	-0.009	0.023***	0.012	0.012**
	(0.003)	(0.007)	(0.007)	(0.006)	(0.007)	(0.019)	(0.009)	(0.011)	(0.006)
R^2	0.107	0.109	0.076	0.068	0.058	0.076	0.109	0.094	0.107
Observations	3,111,182	1,918,878	1,192,304	1,193,394	1,912,869	715,688	2,395,494	534,292	2,563,348
Any Past Month Binge Drinking									
Any Native American Casino in county	0.019***	0.020***	0.019*	0.031***	0.008	0.039***	0.002	0.017	0.015***
	(0.003)	(0.006)	(0.011)	(0.007)	(0.005)	(0.009)	(0.003)	(0.016)	(0.004)
R^2	0.080	0.051	0.067	0.056	0.065	0.037	0.039	0.095	0.045
Observations	3,083,378	1,906,204	1,177,174	1,179,685	1,898,932	709,102	2,374,276	525,479	2,544,743

Note: Values presented are the results of a difference-in-differences model with fixed effects for year and county in addition to specification listed in each column. I include controls for age, education, marital status, race, and county-year unemployment rate. Any drink in the past month refers to having had at least one drink in the past 30 days. Any Past Month Binge Drinking is defined as consuming 5 or more drinks for men (4 or more drinks for women) on at least one occasion in the past month. All observations are weighted using the BRFSS sample weight. Significance: *** p<0.01, ** p<0.05, * p<0.10

Table 6
Native American Casinos and Drinking Frequency among Non-Native Americans
Results by Subgroup
BRFSS Adults 2004-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Baseline model: all adults	Men	Women	HS or less	Some college or more	18-40	41+	Smoker	Non-Smoker
Avg. number of drinks consumed on an occasion									
Any Native American Casino in county	0.090**	0.025	0.173*	0.145*	0.035	0.191**	-0.008	0.021	0.091***
	(0.037)	(0.064)	(0.093)	(0.075)	(0.045)	(0.092)	(0.037)	(0.127)	(0.031)
R^2	0.075	0.058	0.050	0.048	0.051	0.031	0.040	0.077	0.041
Observations	3,081,003	1,904,565	1,176,438	1,179,259	1,897,008	707,920	2,373,083	525,454	2,542,452
# times binge drinking in past month									
Any Native American Casino in county	0.028	0.058***	0.014	0.166***	-0.088	0.180**	-0.088	0.133	-0.017
	(0.034)	(0.022)	(0.086)	(0.058)	(0.066)	(0.083)	(0.081)	(0.211)	(0.053)
R^2	0.036	0.021	0.028	0.022	0.026	0.024	0.015	0.035	0.016
Observations	3,083,378	1,906,204	1,177,174	1,179,685	1,898,932	709,102	2,374,276	525,479	2,544,743

Note: Values presented are the results of a difference-in-differences model with fixed effects for year and county. I include controls for age, education, marital status, race, and county-year unemployment rate. All observations are weighted using the BRFSS sample weight. # of times Binge Drinking in the Past Month is defined as the number of times in the past month an individual consumed 5 or more drinks for men (4 or more drinks for women). Significance: *** p<0.01, ** p<0.05, * p<0.10

Appendix A

Appendix Table 1
Pre-Native American Casino County Characteristics

	(1) Income ($2012)	(2) Unemployment Rate	(3) Any Binge Drinking in Past Month
County that will Gain a Casino	$53,669	7.00	0.121
Observations	2,297	2,251	2,146
County that will Border a County with a Casino	$55,626	6.52	0.128
Observations	4,442	4,364	4,122
County that will Never Border a County with a Casino	$58,983	6.29	0.123
Observations	3,193	3,152	2,925

Note: Figures presented are averages. "County that will Gain a Casino" is an observation for any year before a county gains a casino. "County that will Border a County with a Casino" is an observation for a county any year before any neighboring county gains a casino. "County that will never Border a County with a Casino" is an observation for a county in each year for counties that will never have a casino or border a county with a casino. Data for Income and Any Binge Drinking are computed from the BRFSS and Income is adjusted to 2012 dollars. Data for County Unemployment Rates are from the Bureau of Labor Statistics.

Appendix Table 2
Exogeneity of the Establishment of a Native American Casino

	(1) Casino Established in a County		(2) Casino Established in a County
County Unemployment Rate	-0.001	Avg. Household Income ($2012)	-0.002
	(0.001)		(0.001)
R^2	0.982	R^2	0.983
Observations	17,387	Observations	17,522

Note: Values presented are the results of a difference-in-differences model with fixed effects for year and county. Data for average household income is from the BRFSS and is adjusted to $2012 and a log scale. County Unemployment Rate is from the Bureau of Labor Statistics. Standard errors are clustered at the county level. Significance: *** p<0.01, ** p<0.05, * p<0.10

Appendix Table 3
Native American Casinos and Past Month Drinking among Non-Native Americans
Robustness Analyses
BRFSS Adults 2004-2012

	(1) Baseline model: control for whether county has a NA casino	(2) Large vs. small NA casinos	(3) (1) + region by year interactions	(4) (1) + division by year interactions	(5) (1) + state by year interactions	(6) States with NA casinos	(7) Counties that will have NA casinos
Any Native American Casino in county	0.015*** (0.003)	--	0.017*** (0.003)	0.017*** (0.003)	0.017*** (0.004)	0.013*** (0.003)	0.019*** (0.004)
Large NA Casino in county (by square footage)	--	0.010*** (0.004)	--	--	--	--	--
Small NA Casino in county (by square footage)	--	0.022*** (0.006)	--	--	--	--	--
R^2	0.107	0.107	0.107	0.107	0.108	0.099	0.079
Observations	3,111,182	3,111,182	3,111,182	3,111,182	3,111,182	1,503,251	380,744

See notes to Table 4.

Appendix Table 4
Native American Casinos and # Drinks Consumed on Days Drank among Non-Native Americans
Robustness Analyses
BRFSS Adults 2004-2012

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline model: control for whether county has a NA casino	Large vs. small NA casinos	(1) + region by year interactions	(1) + division by year interactions	(1) + state by year interactions	States with NA casinos	Counties that will have NA casinos
Any Native American Casino in county	0.090*** (0.037)	--	0.085** (0.036)	0.087** (0.036)	0.081** (0.038)	0.069* (0.041)	0.112*** (0.042)
Large NA Casino in county (by square footage)	--	0.078*** (0.026)	--	--	--	--	--
Small NA Casino in county (by square footage)	--	0.108 (0.086)	--	--	--	--	--
R^2	0.075	0.075	0.075	0.075	0.076	0.075	0.064
Observations	3,081,003	3,081,003	3,081,003	3,081,003	3,081,003	1,488,271	376,970

See notes to Table 4.

Appendix Table 5
Native American Casinos and Alcohol Behaviors by Race
BRFSS Adults 2004-2012

	(1) Any past month drinking	(2) Avg # days drank past month	(3) # drinks consumed on days drank past month	(4) Any binge drinking past month	(5) # times binge drinking past month	(6) Any drunk driving past month	(7) # times drunk driving past month
White Individuals Only							
Any Native American Casino in county	-0.001	-0.166	0.001	0.001	-0.047	0.002	0.020
	(0.010)	(0.203)	(0.043)	(0.008)	(0.043)	(0.005)	(0.016)
Mean Rates	0.531	5.044	1.083	0.118	0.481	0.016	0.036
R^2	0.124	0.084	0.090	0.095	0.042	0.022	0.011
Observations	2,539,108	2,523,052	2,516,744	2,518,595	2,518,595	1,350,639	1,350,639
Black Individuals Only							
Any Native American Casino in county	0.040	0.523**	0.482***	0.105***	0.355***	-0.021**	-0.027
	(0.033)	(0.239)	(0.168)	(0.035)	(0.058)	(0.010)	(0.019)
Mean Rates	0.368	2.372	0.789	0.084	0.349	0.011	0.032
R^2	0.096	0.069	0.078	0.068	0.047	0.043	0.027
Observations	265,869	263,939	262,199	262,209	262,209	144,728	144,728
Asian, Hawaiian, and Pacific Islander Only							
Any Native American Casino in county	0.163*	1.513	0.307**	0.105**	0.103	0.005*	0.017
	(0.087)	(1.000)	(0.140)	(0.041)	(0.157)	(0.003)	(0.0144)
Mean Rates	0.427	3.093	0.989	0.107	0.449	0.015	0.040
R^2	0.103	0.070	0.108	0.102	0.090	0.051	0.074
Observations	72,700	72,393	72,069	72,068	72,068	37,898	37,898

Note: Values presented are the results of a difference-in-differences model with fixed effects for year and county. I include controls for age, education, marital status, and county-year unemployment rate. All observations are weighted using the BRFSS sample weight. Binge drinking is defined as 5 or more drinks on an occasion. Number of Days Drinking and Number of Days Binge Drinking are defined as the number of days an individual participated in the activity in the past month. Number of drinks is the number of drinks consumed on average on an occasion when an individual drank. Mean Rates are the averages from 2004-2012 for the group defined in the row. Significance: *** p<0.01, ** p<0.05, * p<0.10

Appendix Table 6
Native American Casinos and Alcohol Behaviors by Race Continued
BRFSS Adults 2004-2012

	(1) Any past month drinking	(2) Avg # days drank past month	(3) # drinks consumed on days drank past month	(4) Any binge drinking past month	(5) # times binge drinking past month	(6) Any drunk driving past month	(7) # times drunk driving past month
Hispanic Individuals Only							
Any Native American Casino in county	0.240**	5.398*	0.832***	0.094***	0.448***	0.017**	0.042
	(0.097)	(2.856)	(0.368)	(0.029)	(0.102)	(0.007)	(0.043)
Mean Rates	0.419	2.609	1.142	0.127	0.456	0.015	0.036
R^2	0.133	0.095	0.095	0.100	0.071	0.057	0.127
Observations	176,120	175,299	173,805	174,129	174,129	93,566	93,566
Native American Individuals Only							
Any Native American Casino in county	0.192	0.223	0.344*	0.014	0.144	-0.034	0.069
	(0.134)	(0.915)	(0.187)	(0.030)	(0.312)	(0.036)	(0.132)
Mean Rates	0.381	2.842	1.173	0.136	0.673	0.016	0.059
R^2	0.181	0.179	0.169	0.172	0.213	0.214	0.282
Observations	46,338	46,052	45,743	45,771	45,771	24,328	24,238
Non-Native American Individuals Only							
Any Native American Casino in county	0.015***	0.052	0.090***	0.019***	0.028	0.001	0.017
	(0.003)	(0.141)	(0.037)	(0.003)	(0.034)	(0.003)	(0.011)
Mean Rates	0.507	4.611	1.058	0.115	0.468	0.016	0.036
R^2	0.107	0.077	0.075	0.080	0.036	0.019	0.011
Observations	3,111,182	3,091,316	3,081,003	3,083,378	3,083,378	1,658,450	1,658,450

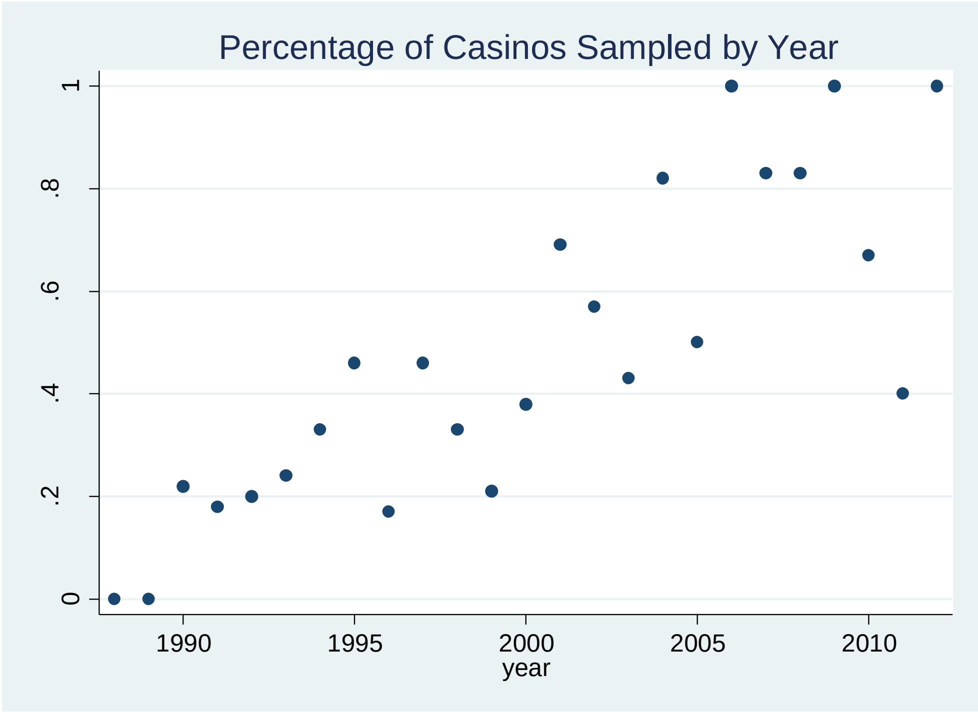
Note: See Appendix Table 5

Appendix Table 7
Native American Casinos and Alcohol Behaviors for Casino Counties and Border Counties
BRFSS Adults 2004-2012

	(1) Any past month drinking	(2) Avg # days drank past month	(3) # drinks consumed on days drank past month	(4) Any binge drinking past month	(5) # times binge drinking past month	(6) Any drunk driving past month	(7) # times drunk driving past month
Full Sample							
Any Native American Casino in County	0.024***	0.126	0.097**	0.017***	-0.002	0.001	0.024**
	(0.007)	(0.161)	(0.041)	(0.005)	(0.045)	(0.003)	(0.011)
County Bordering a	0.010	0.120	0.003	-0.002	-0.054	0.002	0.012*
County with NA Casino	(0.007)	(0.107)	(0.029)	(0.006)	(0.048)	(0.002)	(0.007)
R^2	0.107	0.077	0.075	0.080	0.036	0.019	0.010
Observations	3,158,163	3,138,006	3,127,382	3,129,785	3,129,785	1,683,117	1,683,117
Native American Individuals Only	0.178	0.495	0.613*	0.048	0.987*	-0.055	0.147
Any Native American Casino in county	(0.115)	(0.949)	(0.317)	(0.046)	(0.599)	(0.046)	(0.170)
County Bordering a	-0.022	0.386	0.392	0.050	1.242	-0.042	0.144
County with NA Casino	(0.107)	(0.942)	(0.462)	0.052	(0.773)	(0.036)	(0.120)
R^2	0.180	0.178	0.169	0.173	0.214	0.214	0.282
Observations	46.981	46,690	46,379	46,407	46,407	24,667	24,667
Non-Native American Individuals Only	0.020***	0.121	0.087**	0.017***	-0.013	0.002	0.023**
Any Native American Casino in county	(0.005)	(0.156)	(0.042)	(0.005)	(0.049)	(0.003)	(0.011)
County Bordering a	0.010	0.121	-0.001	-0.003	-0.069	0.003	0.011
County with NA Casino	(0.007)	(0.109)	(0.030)	(0.006)	(0.057)	(0.002)	(0.007)
R^2	0.107	0.077	0.075	0.080	0.036	0.019	0.011
Observations	3,111,182	3,091,316	3,081,003	3,083,378	3,083,378	1,658,450	1,658,450

Note: Results are from a difference-in-differences model with fixed effects for year and county. I include controls for age, education, marital status, race, and county-year unemployment rate. All observations are weighted using the BRFSS sample weight. Binge drinking is defined as 5 or more drinks on an occasion. Number of Days Drinking and Number of Days Binge Drinking are defined as the number of days in the past month. Number of drinks is the number of drinks consumed on average on an occasion when an individual drank. Mean Rates are the averages from 2004-2012 for the group defined in the row. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Appendix Figure 1
The Percentage of Casinos with Sample Observation by Year



CHAPTER III

NEW EVIDENCE ON THE PRICE EFFECTS OF CIGARETTE TAX COMPETITION

with Christopher S. Carpenter

Introduction

A large literature in public finance has studied the price effects of various sales and excise taxes (e.g., Besley and Rosen 1999, Poterba 1996). Cigarettes have received perhaps the most attention of any individual commodity, in part due to the very high excise tax burden on cigarettes that has grown even higher since the 1998 Master Settlement Agreement.[29] Multiple studies have documented the effects of excise taxes on cigarettes on cigarette prices (e.g., Keeler et al. 1996), and several papers also relate the presence of a nearby lower tax border to cigarette prices (Hanson and Sullivan 2009, Harding et al. 2012). Sullivan and Dutkowsky (2012), for example, show both that cigarette taxes are more than fully passed through to prices and that the presence of a nearby state border with a lower cigarette tax also puts downward pressure on local cigarette prices.

In this paper we provide the literature's first evidence on the local price effects of two other sources of competition from low-tax and tax-free cigarettes: online purchases (which enabled tax-free sales over our time period) and purchases on Native American

[29] Numerous studies have linked cigarette excise taxes to youth and adult smoking and related outcomes. See, for example, Carpenter and Cook (2008), Hansen et al. (2013), and others.

reservations (where cigarettes can typically be purchased tax-free).[30] We use state internet penetration rates to proxy for online availability, and we use Native American casino openings to proxy for availability on reservations. Prior work has studied tax-free online sales with respect to cigarette consumption and the elasticity of taxable sales (Goolsbee et al. 2010), but we are not aware of any research that has directly linked internet penetration to local cigarette prices. Similarly, prior work has studied cigarette purchasing behavior on Native American reservations (DeCicca et al. 2015), but we are not aware of any research directly linking Native American casino openings to local cigarette prices.

To preview, we use quarterly data on local cigarette prices for 723 cities from 1976-2003 and two-way fixed effects methods to replicate the main results of prior work that taxes are more than fully shifted to prices and that a nearby state with a lower cigarette tax also reduces cigarette prices. We then provide the literature's first evidence that internet penetration rates and Native American casinos also significantly reduce local cigarette prices. For online sales, we estimate that a fifty percentage point increase in the state internet penetration rate (roughly the increase observed over our sample period) is associated with a significantly lower per-pack cigarette price of about 22-26 cents. For Native American reservations, we estimate that the opening of a Native American casino within 25 miles of the city center (of which there were hundreds over our study period) is

[30] Strictly speaking cigarettes are not completely tax-free on all Native American reservations, though in practice there are very large price differentials driven mainly by tax exemptions to tribes who are sovereign entities in the eyes of the government. For details, see Lovenheim (2008) and DeCicca et al. (2015). Over our time period (1976-2003) cigarettes could be obtained tax-free from the internet, though this policy has recently changed. For simplicity we refer here to 'tax-exempt' or 'tax-free' cigarette sales in each of these contexts.

associated with a significantly lower per-pack cigarette price of 1.6-2.7 cents. Consistent with an interpretation of tax competition, we also demonstrate that the casino effect on prices is unique to Native American casinos: there is no price effect for openings of nearby non-Native American casinos (which may be expected to provide similar economic shocks but without the differential cigarette tax treatment). Finally, we document that these price effects of cigarette tax competition are not observed for prices of other services (e.g., a haircut) for which there should be no differential tax competition. Overall, our findings provide the first evidence on the price effects of two previously unstudied sources of cigarette tax competition.

The paper proceeds as follows: Section II briefly describes the previous literature. Section III outlines the data and empirical approach. Section IV presents the results, and Section V concludes.

Previous Literature

Most empirical research on cigarette taxes and prices has estimated rates of tax pass-through; interested readers can find papers in the literature finding no effects of cigarette taxes on prices (Ashenfelter and Sullivan 1987), positive effects of cigarette taxes on prices that do not reflect full shifting (Harding et al. 2012, Chiou and Muehlegger 2010, DeCicca et al. 2013), and positive effects of cigarette taxes on prices that reflect more than full shifting (Sullivan and Dutkowsky 2012, Hanson and Sullivan 2009, Keeler et al. 1996). We restrict our brief review here to the handful of recent studies that are most relevant to our work.

Our study is most closely related to Sullivan and Dutkowsky (2012), as we use the same local cigarette price data and two-way fixed effects empirical specifications. They use ACCRA/C2ER and other data to demonstrate that state excise taxes are more than fully shifted to prices and that the presence of a nearby state border with a lower cigarette tax was negatively related to local cigarette prices. Specifically, they estimate that a $1 tax increases local cigarette prices by approximately $1.10. They also find that cities within one mile of a lower tax state have cigarette prices that are nearly 8 cents cheaper, consistent with a price effect of tax competition. Our paper builds on their work by replicating their basic pass-through and nearby low-tax state results using a longer time series (back to 1976) and adding new controls to capture tax competition from online sales and sales at Native American reservations.

Two other studies have also examined how cigarette tax pass-through estimates vary with distance to a state with a lower tax border. Hanson and Sullivan (2009) study a cigarette tax hike in Wisconsin and use variation in cigarette prices across stores in the state that are differentially located near borders with Minnesota (which had a lower cigarette tax than Wisconsin following the reform) and Illinois (which had a higher cigarette tax than Wisconsin following the reform). They find that the cigarette tax was more than fully passed through to cigarette prices overall but that this effect was smaller for stores near the Minnesota border. They interpret this as evidence that Wisconsin retailers near the Minnesota border faced stiffer tax competition which reduced their ability to pass on the state tax hike to consumers. Harding, Leibtag, and Lovenheim (2012) use a similar strategy but examine national scanner data from a large grocery store chain from 2005 and 2006. They observe millions of cigarette price observations and find that tax hikes over this period are less than fully passed through to prices. They also find that pass-through rates are lower for stores located near a lower tax border state, again consistent with a role for tax competition in reducing retailers' ability to pass cigarette excise taxes on to consumers. Results from these studies on border-state tax competition are broadly consistent with other papers in health economics that examine the effects of cross-state tax and price differentials on smuggling behavior using consumption data, sales data, or both (see, for example, Stehr 2005).

We are not aware of any prior studies that have studied the effects of availability of tax-free and low-cost cigarettes from online sales or Native American reservations on prices, but a handful of studies have examined the consumption and sales effects of such sources of tax competition. For example, Goolsbee et al. (2010) consider the impact of tax-free online cigarette sales on the elasticity of taxable cigarette sales. They use the CPS Computer Use Supplements to measure the rate of internet penetration by state from 1989-2003 as a proxy for online sales. In a two-way fixed effects model with controls for neighboring state cigarette tax and population, state income, and wholesale prices of cigarettes, they find that an increase of internet penetration from 0-50% nearly doubles the elasticity of taxable cigarette sales, making individuals more sensitive to price changes in their home state's cigarette prices.

Multiple studies have examined the role of Native American reservations in cigarette tax competition. Lovenheim (2008) incorporates tax-free cigarette sales on Native American reservations into the more commonly used cross-state tax differentials as incentives for cross-border cigarette smuggling. While not the central focus of his paper, Lovenheim's results using the Tobacco Use Supplements to the Current Population Survey demonstrate the importance of accounting for tax-free sales on Native American reservations in estimating the effects of tax differentials on casual cigarette smuggling. DeCicca et al. (2015) investigate the impact of Native American reservations on cigarette smuggling in New York. Specifically, they examine whether tax-free cigarette savings are fully passed on to the consumer. Using data from the New York State Tobacco survey from 2003-2009, they find that individuals who purchased cigarettes from reservations received a reduction in price nearly equal to the difference in taxation. An important exception occurs for one particular Native American tribe in New York State that faces no tribal competition in cigarette sales; on that reservation, they estimate the tribal monopoly captures about half of the tax differential.

Data and Empirical Approach

Our primary source for local prices is ACCRA/C2ER. ACCRA data were originally designed to provide cost of living estimates for urban professionals and have been collected quarterly since 1976. Typically volunteers from local Chambers of Commerce would collect information on prices of several local goods in multiple locations, and ACCRA/C2ER compiled these into a city-specific index. These data have been used extensively in the existing literature on tax pass-through in part because of the large number of participating cities. From 1976-2003 we observe the price for a carton of

Winston Kings cigarettes in each of several hundred cities.[31] This price includes any cigarette taxes but excludes sales taxes that are added on at the cash register. We measure all prices in real 2012 U.S. dollars and divide each carton price by 10 to arrive at a local real per-pack price. We compile information on state excise taxes on cigarettes from various years of The Tax Burden on Tobacco (Orzechowski and Walker). Our Native American casino data include all 'Class III' Las Vegas style gaming casinos from 1986-2003. We use Native American casinos as a proxy for the ability to purchase cigarette tax-free on reservation land. Although it is possible that individuals may have been able to purchase tax-free cigarettes on some reservations prior to casino openings, we argue that the casino opening provides a shock that drives economic activity to the reservation which may lead to an information transfer to store owners in nearby cities and consumers that cigarettes may be purchased tax-free. Our data on state internet penetration rates come from various years of the Computer Use Supplements to the Current Population Surveys from 1987-2003.[32]

To estimate the price effects of tax competition from Native American casinos and online sales, we follow previous research and estimate two-way fixed effects models controlling for city and time fixed effects and linear city-specific trends. These models take the following form:

[31] We note that the sampling of the ACCRA data results in some limitations: we do not observe prices in rural markets, nor do we observe prices of generic cigarettes over our sample period.
[32] We linearly interpolate an internet penetration rate for each state in the non-survey years.

(1) $Y_{cst} = \beta_0 + \beta_1$(CIGARETTE EXCISE TAX)$_{st}$ + β_2(LOWER TAX BORDER WITHIN 25 MILES)$_{ct}$ + β_3(NATIVE AMERICAN CASINO WITHIN 25 MILES)$_{ct}$ + β_4(NON-NATIVE AMERICAN CASINO WITHIN 25 MILES)$_{ct}$ +β_5(STATE INTERNET PENETRATION RATE)$_{st}$ + β_6COST CONTROLS$_{ct}$ + β_7CITY$_c$ + β_8TIME$_t$ + β_9CITY TREND$_{ct}$ + ε_{cst}

where Y_{cst} is the real cigarette price in city c in state s at time t. CIGARETTE EXCISE TAX is the state excise tax on a pack of cigarettes.[33] LOWER TAX BORDER WITHIN 25 MILES is an indicator variable equal to one if the ACCRA/C2ER city center is within 25 miles of a state with a lower cigarette tax. NATIVE AMERICAN CASINO WITHIN 25 MILES is an indicator variable equal to one if the ACCRA/C2ER city center is within 25 miles of a Native American casino. In robustness analyses we vary the relevant radii for the lower tax border and casino variables to be 50, 10, and 5 miles. NON-NATIVE AMERICAN CASINO WITHIN 25 MILES is an indicator variable equal to one if the ACCRA/C2ER city center is within 25 miles of a non-Native American casino. This variable is meant to test whether any observed casino-cigarette price relationship is unique to Native American casinos, which should be true if our hypothesis is correct since there is no differential tax treatment for casinos opening on non-tribal lands. STATE INTERNET PENETRATION RATE is the fraction of respondents to the Computer Use Supplements of the CPS who report that they are connected to the internet. COST CONTROLS is a vector of input costs measured in the ACCRA data and include: gasoline, apartment rent, and repair cost (a combination of television repair from 1976 to

[33] For the small number of ACCRA cities spanning multiple states, we assign the relevant tax for the state in which the majority of the city is located. Excluding such cities altogether returns similar results.

1979 and washing machine repair from the second quarter of 1979 to 2003 due to data coverage).[34] CITY and TIME are a full set of city and year-quarter dummies, respectively. CITY TREND is a vector of linear city-specific time trends constructed by taking each city fixed effect and interacting it with a variable TREND that equals 1 in the first quarter of our data, 2 in the second quarter of our data, and so forth. We report standard errors clustered at either the state or city level throughout (Bertrand, Duflo, and Mullainathan 2004).

Results

Figure 1 shows trends in the inflation adjusted price of a pack of cigarettes and in state excise taxes on cigarettes. Cigarette prices have steadily increased since 1976. The graph indicates sharp spikes in both cigarette prices and taxes following the Master Settlement Agreement, resulting in an increase from $2.50 a pack to almost $5.00 a pack in just five years. Figure 2 shows trends in Native American casinos and state internet penetration rates over our sample period. Native American casinos increased sharply following the 1988 Indian Gaming Regulatory Act which created the conditions for Las Vegas style casino gaming by Native American tribes. The total number of casinos increased from a handful in 1980 to nearly 200 by 2003. Internet penetration steadily increased over our sample period from a very low base. In the empirical work we set all pre-1989 internet observations equal to zero, which Figure 1 shows is a reasonable

[34] These variables were chosen to most closely match the controls used in prior work and because they were consistently measured in the largest number of quarters of the ACCRA data.

approximation: in 1989 less than 10 percent of CPS respondents report home internet use. By the end of our sample period, this had increased to well over half of all households.

We present descriptive statistics of key variables in Table 1. Table 1 shows that the mean price for a pack of Winston Kings in the ACCRA/C2ER data over our period was $2.54, adjusted for inflation. This average reflects considerable variation, as in some cities toward the early part of our sample period a pack of cigarettes cost less than 25 cents while in one city (New York City) toward the end of our sample a pack of cigarettes cost over $10. Average inflation adjusted excise taxes on a pack of cigarettes were 56 cents over the entire period but ranged from 2 cents (North Carolina) to $3.46 (Rhode Island). Internet penetration rates averaged 16 percent. Table 1 also shows sample means for the other prices used in our analyses: monthly apartment rent, gasoline, and appliance repair costs (used for local cost shifters); a month subscription to a local newspaper, a movie ticket, dry cleaning for a two piece suit, and a man's haircut (used for placebo/falsification tests).

Table 2 presents the key results on cigarette tax pass-through and cigarette tax competition. Columns 1 and 2 present the results on the key variables from equation (1) for the sample that uses any ACCRA/C2ER observation in the data, even if the city was only intermittently participating. We show results for the period 1990-2003 in column 1 in the spirit of replicating prior work by Sullivan and Dutkowsky (2012), and we show results from the full 1976-2003 period in column 2. In the subsequent columns, we present results from similarly specified models on the full 1976-2003 period but where we impose increasingly strong requirements about the number of year-quarter observations for which we require the city to have participated in the ACCRA/C2ER data collection (i.e., 25%, 50%, and 75% of year-quarter observations in Columns 2, 3, and 4, respectively). Doing so allows us to be more confident that the city fixed-effects and linear city trends are appropriately controlling for unobserved sources of city-specific bias in the relationship between our tax-free sources of competition and local cigarette prices.[35] Additionally, it demonstrates the robustness of our results to different samples based on the percentage of data that are missing.[36] For each sample we show the coefficient estimates on the cigarette tax, the presence of a lower tax border within 25 miles, the presence of a Native American casino within 25 miles, the presence of a non-Native American casino within 25 miles, and the state internet penetration rate.

The results in Table 2 replicate the well-documented finding that cigarette taxes are more than fully passed through to prices. Specifically, we estimate that a one dollar increase in cigarette taxes increases local cigarette prices by between one dollar and eight cents and one dollar and eleven cents, and this result is statistically significant at the one percent level in every sample. Moreover, the sample period that most closely matches previously published work (column 1, which restricts attention to 1990-2003) returns a pass-through estimate of $1.11, which is very similar to the $1.13 estimate from a model with slightly different controls reported in Sullivan and Dutkowsky (2012).[37] We also

[35] The city and year-quarter fixed effects and city trends explain the vast majority of variation in local cigarette prices, as evidenced by the high R-squareds for these models, which are standard in this literature (see, for example, Goolsbee et al. 2010 and Sullivan and Dutkowsky 2012).

[36] We also estimated models that used state and year quarter fixed effects with state linear time trends (instead of city fixed effects and trends). These models returned quantitatively similar estimates of the relationship between internet penetration and local cigarette prices.

[37] Specifically, Sullivan and Dutkowsky (2012) include controls for local excise taxes (which we do not observe) and exclude the controls for casinos and internet penetration. Their baseline models also do not

replicate the literature's previous finding that cities nearby states with lower cigarette taxes – whose variation is driven by cigarette tax changes in the fixed-effects models – have lower cigarette prices, consistent with greater cigarette tax competition reducing prices. These border state effects are statistically significant at the ten percent level or better in all models with clustering at the city level and in two of the four models when we cluster at the state level.

We next turn to the two sources of tax competition that have not been previously studied in the literature on local cigarette prices: Native American casinos and internet penetration rates. We find that the presence of a Native American casino within 25 miles of an ACCRA/C2ER city – whose variation in the fixed-effects models is generated by numerous casino openings over our sample period – is associated with lower local cigarette prices. These estimates are generally sizable and statistically significant at the ten percent level or better in models with state level clustering. Notably, the coefficients on the non-Native American casino indicator variable are smaller in magnitude and are statistically insignificant in all models, consistent with our interpretation that the increased tax competition due to the Native American casino is driving the downward pressure on local prices. Moreover, the point estimates on the Native American casino indicator are broadly similar to those for the variable indicating the presence of a nearby lower tax border. Turning to internet penetration rates, we find that higher internet penetration is associated with substantially lower local cigarette prices. We estimate that

include linear city trends; excluding the trends from our specification returns a statistically significant state excise tax pass through estimate of $1.15, again very close to their published estimate of $1.13 (column 3 of Table 6 of their paper).

a fifty percentage point increase in state internet penetration is associated with a reduction in local cigarette prices by 22 to 25 cents, and these estimates are statistically significant.[38] Taken together, the results in Table 2 confirm previously published findings and provide new evidence on the importance of two previously unstudied sources of cigarette tax competition.

In Table 3 we present the robustness of the main findings in Table 2 to alternative choices about what constitute 'nearby' lower tax borders and Native American casinos. For all models in Table 3 we restrict attention to cities observed in 75% of all year-quarter observations (i.e., the sample from column 4 of Table 2). Column 2 of Table 3 reprints the estimates using a radius of 25 miles, and we present results from other choices in Columns 1 (50 miles), 3 (10 miles), and 4 (5 miles). If our hypotheses about lower tax borders and Native American casinos reflecting cigarette tax competition are correct, we might expect a plausible monotonicity in the relationship between relatively closer tax-free sources compared to relatively farther tax-free sources. Of course, a challenge with this exercise is that the number of cities with nearby sources of tax-free competition gets smaller at smaller radii, so precision is an issue. Despite this challenge, the results in Table 3 are consistent with both lower tax borders and Native American casinos reflecting real effects of tax competition on local prices. At either 50 or 25 mile radii around the ACCRA/C2ER city center, we estimate significant effects of nearby lower tax borders and casinos on prices, and while the coefficients are not statistically

[38] It could be that purchasers of individual packs of cigarettes might respond differently (probably less) to these tax incentives than more addicted smokers who are more likely buying cigarettes by the carton, which is the only price measure we observe in the ACCRA data.

significant at 10 or 5 miles, the point estimates are either similar in magnitude or plausibly larger than those from the model based on 50 and 25 mile rules. In all specifications the cigarette tax pass-through estimates and the internet penetration estimates remain unchanged, as expected.[39]

Finally, in Table 4 we demonstrate that the tax competition effects we identify are unique to cigarette prices. We present the coefficient estimates on the 'placebo' outcomes from estimation of equation (1) for the full sample for each of other services whose prices were tracked over the same period as cigarettes: a month subscription to a local newspaper, a movie ticket, dry cleaning for a man's 2-piece suit, and a man's haircut.[40] For each of these items we find no consistently significant relationship between either the cigarette tax or any of the sources of lower-tax competition and the prices of the other goods.[41] These null findings lend further support to our evidence uniquely linking Native American casinos and internet penetration to local cigarette prices.

[39] We also estimated models where we interacted the cigarette tax with the other variables reflecting tax competition to see if pass-through varies systematically with these lower-tax sources as suggested by prior work (Harding et al. 2012, Hanson and Sullivan 2009). The results from these models were uninformative due to insufficient precision. We also examined whether the relationships differed before or after the Master Settlement Agreement (November 1998) using separate city time trends. These results showed that state excise tax pass-through was greater in the pre-MSA period, though the coefficients on the other key tax competition variables did not differ significantly before and after the MSA.

[40] The ACCRA data also tracks prices of a number of other commodities (e.g., tennis balls, man's shirt), but it is plausible that some of those goods could have had prices affected by ability to buy the items online so we focus on services which are not easily purchased online as our placebo outcomes.

[41] The same was true when we restricted the sample to cities observed in 75% of year-quarter observations.

Conclusion

The results presented above provide new evidence on the local price effects of two previously unstudied sources of tax competition: online sales (as proxied by state internet penetration rates) and Native American reservations (as proxied by Native American casino openings). In each case, we find that greater tax competition puts significant downward pressure on local cigarette prices. Notably, we find no significant relationship between non-Native American casinos and local cigarette prices, suggesting that the differential tax treatment at Native American casinos is driving the price effect. These results come from models that replicate the literature's basic findings on tax pass-through and access to lower tax borders, suggesting that our models are generally well specified. Moreover, the effects we observe are not observed for local prices of other commodities that do not share similar tax competition dynamics as do cigarettes.

Our study has implications for interpreting series of studies that have looked at the effects of tax competition on local cigarette sales and consumption. Lovenheim (2008) find that distance to a lower tax border (measured by neighboring state or Native American reservation) significantly increases the home-tax-elasticity of sales, suggesting smuggling behavior. Stehr (2005) finds that cigarette sales respond both to own-tax as well as neighboring tax, consistent with widespread smuggling. Goolsbee et al. (2010) find that the tax elasticity of taxable sales is higher when there is more internet penetration, suggesting that people reduce purchases of home state cigarettes when taxes increase because tax-free sources such as online sales become increasingly attractive in the presence of tax hikes. Our new findings on local prices uncover important mechanisms underlying these effects on sales and consumption.

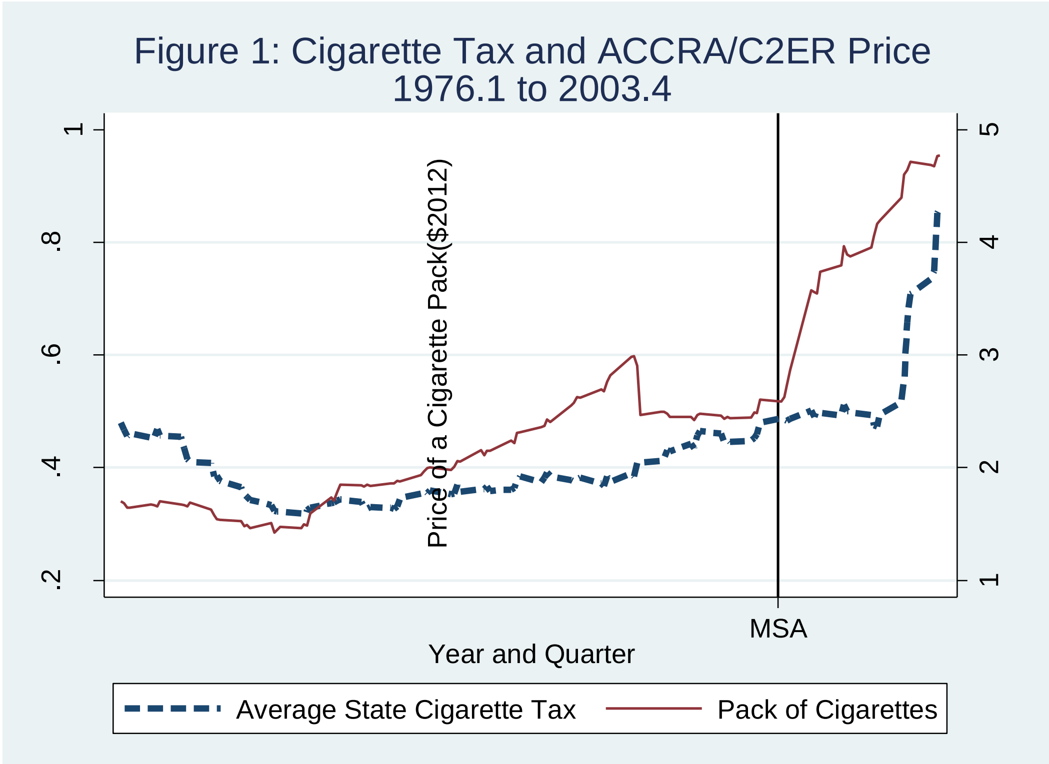


Figure 1: Cigarette Tax and ACCRA/C2ER Price
1976.1 to 2003.4
.2
.4
.6
.8
1
1
2
3
4
5
Price of a Cigarette Pack($2012)
MSA
Year and Quarter
Average State Cigarette Tax
Pack of Cigarettes

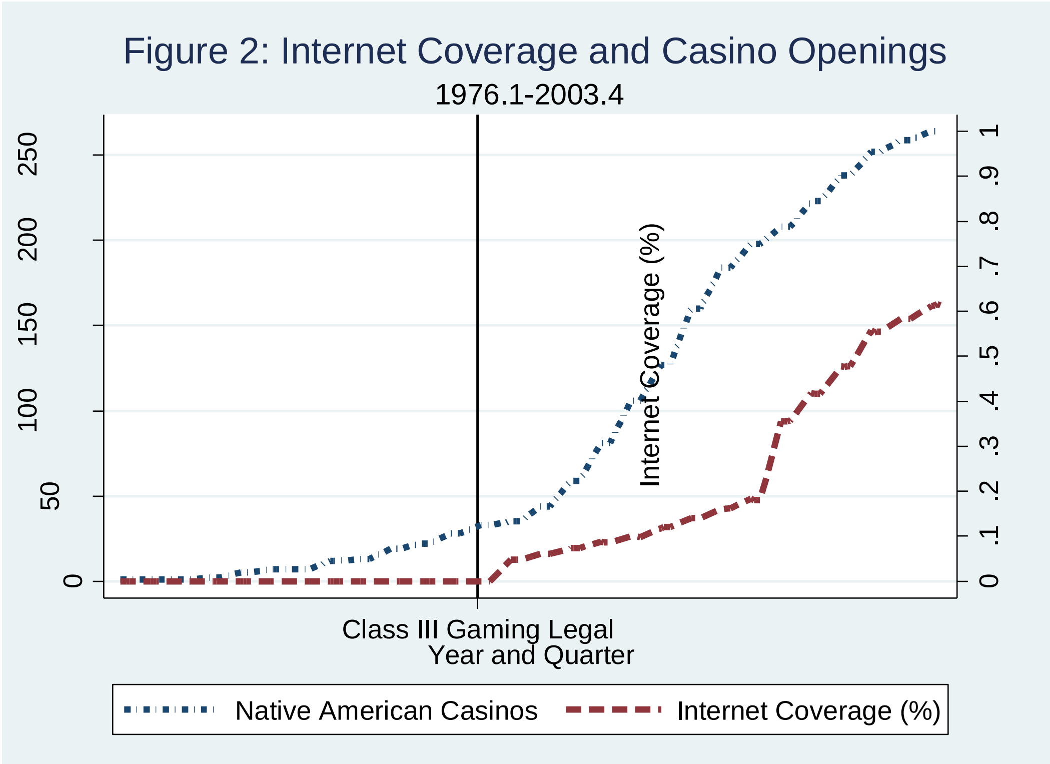


Figure 2: Internet Coverage and Casino Openings
1976.1-2003.4
0
50
100
150
200
250
0
.1
.2
.3
.4
.5
.6
.7
.8
.9
1
Internet Coverage (%)
Class III Gaming Legal
Year and Quarter
Native American Casinos
Internet Coverage (%)

Table 1
Descriptive Statistics
ACCRA/C2ER

Variable	All ACCRA/C2ER cities	Cities with a lower tax border within 25 miles	Cities with a Native American casino within 25 miles
Cigarette price	$2.54	$2.56	$3.30
	(0.96)	(1.02)	(1.11)
Cigarette tax	$0.42	$0.49	$0.59
	(0.26)	(0.31)	(0.35)
Internet penetration	0.16	0.14	0.29
	(0.20)	(0.19)	(0.22)
Monthly apartment rent	$800.11	$799.30	$886.18
	(249.13)	(345.83)	(261.81)
Gasoline price (One Gallon Unleaded)	$2.05	$2.09	$1.85
	(0.56)	(0.56)	(0.39)
Appliance repair cost	$52.83	$52.35	$54.42
	(19.14)	(9.24)	(9.12)
Monthly newspaper subscription price	$17.43	$17.85	$17.15
	(4.28)	(5.16)	(3.48)
Movie ticket price	$8.73	$8.75	$9.07
	(1.39)	(1.25)	(1.05)
Dry cleaning price – man's 2 piece suit	$9.95	$9.67	$10.49
	(1.50)	(1.30)	(1.49)
Man's haircut price	$12.71	$12.43	$12.80
	(2.50)	(2.76)	(2.31)

Notes: ACCRA/C2ER means (standard deviations).

Table 2
Tax Competition and Local Cigarette Prices
ACCRA1976-2003

	(1)	(2)	(3)	(4)	(5)
	Sullivan & Dutkowsky (2012) period: 1990-2003	Full sample: 1976-2003	Cities observed in at least 25% of possible year-quarters, 1976-2003	Cities observed in at least 50% of possible year-quarters, 1976-2003	Cities observed in at least 75% of possible year-quarters, 1976-2003
State cigarette excise tax	1.11	1.08	1.08	1.09	1.11
	(.049)***	(.038)***	(.038)***	(.038)***	(.040)***
	[.031]***	[.028]***	[.028]***	[.030]***	[.038]***
Lower tax border within 25 miles	-0.012	-0.020	-0.020	-0.026	-0.032
	(.023)	(.013)	(.014)	(.015)*	(.015)**
	[.022]	[.011]*	[.012]*	[.013]**	[.014]**
Native American casino within 25 miles	0.010	-0.016	-0.017	-0.015	-0.027
	(.019)	(.009)*	(.009)*	(.010)	(.009)***
	[.018]	[.015]	[.015]	[.016]	[.019]
Non-Native American casino within 25 miles	-0.022	-0.008	-0.010	-0.013	-0.014
	(.019)	(.016)	(.016)	(.015)	(.015)
	[.016]	[.015]	[.015]	[.015]	[.016]
State internet penetration rate	-0.377	-0.440	-0.436	-0.482	-0.508
	(.312)	(.265)	(.264)	(.277)*	(.281)*
	[.178]**	[.154]***	[.154]***	[.161]***	[.187]***
R^2	.98	.99	.99	.99	.99
Observations	15,407	27,168	24,950	21,545	15,995

Notes: ACCRA/C2ER data from 1976-2003. All models also include controls for city-specific cost controls, city fixed effects, year-quarter fixed effects, and linear city-specific trends. Standard errors clustered at the state level are in parentheses; standard errors clustered at the city level are in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3
Robustness to Choice of Distance
ACCRA1976-2003

	(1)	(2)	(3)	(4)
	Cities observed in at least 75% of possible year-quarters	Cities observed in at least 75% of possible year-quarters	Cities observed in at least 75% of possible year-quarters	Cities observed in at least 75% of possible year-quarters
Distance measure used (in miles):	50	25	10	5
State cigarette excise tax	1.11	1.11	1.11	1.11
	(.039)***	(.040)***	(.040)***	(.041)***
	[.037]***	[.037]***	[.037]***	[.038]***
Lower tax border within X miles	-0.037	-0.032	-0.031	-0.039
	(.015)**	(.015)**	(.026)	(.036)
	[.015]**	[.014]**	[.023]	[.033]
Native American casino within X miles	-0.024	-0.027	-0.043	-0.089
	(.012)**	(.009)***	(.045)	(.088)
	[.015]	[.019]	[.041]	[.088]
Non-Native American casino within X miles	0.000	-0.014	0.014	0.024
	(.014)	(.015)	(.017)	(.018)
	[.013]	[.016]	[.022]	[.027]
State internet penetration rate	-0.505	-0.508	-0.497	-0.509
	(.280)*	(.281)*	(.273)*	(.273)*
	[.186]***	[.187]***	[.184]***	[.184]***
R^2	.99	.99	.99	.99
Observations	15,995	15,995	15,995	15,995

Notes: ACCRA/C2ER data from 1976-2003. All models also include controls for city-specific cost controls, city fixed effects, year-quarter fixed effects, and linear city-specific trends. Standard errors clustered at the state level are in parentheses; standard errors clustered at the city level are in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4
Tax Competition and Other Local Prices
ACCRA1976-2003

	(1)	(2)	(3)	(4)
	Monthly newspaper subscription	Movie ticket	Dry Cleaning (2-Piece Suit)	Man's haircut
State cigarette excise tax	0.295	-0.263	-0.061	0.106
	(0.451)	(0.115)**	(0.121)	(0.195)
	[0.458]	[0.078]***	[0.108]	[0.235]
Lower tax border within 25 miles	-0.153	0.000	0.046	0.056
	(0.240)	(0.040)	(0.074)	(0.143)
	[0.256]	[0.041]	[0.072]	[0.141]
Native American casino within 25 miles	-0.154	-0.071	0.127	0.240
	(0.313)	(0.139)	(0.093)	(0.250)
	[0.373]	[0.137]	[0.126]	[0.263]
Non-Native American casino within 25 miles	0.668	-0.012	-0.071	0.473
	(0.342)*	(0.117)	(0.076)	(0.151)***
	[0.295]**	[0.140]	[0.089]	[0.171]***
State internet penetration rate	-2.254	1.057	1.392	3.251
	(2.717)	(0.926)	(1.270)	(2.092)
	[2.394]	[0.675]	[1.002]	[1.737]*
R^2	.829	.615	.722	.698
Observations	22,843	27,375	27,378	19,923

Notes: ACCRA/C2ER data from 1976-2003. All models also include controls for city-specific cost controls, city fixed effects, year-quarter fixed effects, and linear city-specific trends. Standard errors clustered at the state level are in parentheses; standard errors clustered at the city level are in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

CHAPTER IV

NEW EVIDENCE ON BEER PRICES AND BEER TAXES

with Christopher S. Carpenter

Introduction

Large literatures in health, labor, and public economics have studied the role of local alcohol availability in determining alcohol consumption, mortality, earnings, education, family structure, health, risky sexual behavior, and crime (for reviews, see Cook and Moore 2001, and others). To proxy for local alcohol availability, it has become standard for researchers to control for state excise taxes on beer, with the idea that taxes are more plausibly exogenous to outcomes than are prices (since prices themselves are outcomes of supply and demand forces).[42] The result is that there are now hundreds of published studies that include controls for state beer taxes as a plausibly exogenous measure of local alcohol availability.[43]

Importantly, nearly *all* of this literature relies on a critical assumption that beer taxes are fully passed through to beer prices, and thus beer prices reflect meaningful variation across location and time from beer excise taxes. To our knowledge, there is

[42] Taxes on beer – as opposed to taxes on liquor or wine – are commonly justified because: 1) beer is the drink of choice among young adults (where much of this literature is focused); and 2) beer taxes are well defined and measured without error for individuals in all states, unlike liquor taxes (since 18 states operate under government-controlled liquor monopolies).

[43] This literature is not without disagreement. The earliest studies leveraged cross-state variation in beer taxes (see, for example, Grossman et al. 1987, 1994; Coate and Grossman 1988; and others) and was subsequently criticized for the concern that such differences are likely to reflect unobserved state-specific sentiment toward alcohol as opposed to cross-state differences in alcohol availability per se (Dee 1999). Studies employing fixed-effects methods have produced a range of conclusions (see, for example, Dee 1999, Cook and Moore 2001, and others), and as a result there is a longstanding and ongoing debate that centers on the degree to which there is sufficient within-state variation in beer taxes in previous studies to credibly identify tax effects on outcomes.

only one peer-reviewed study that directly addresses this question. Young and Bielinska-Kwapisz (henceforth YBK) (2002) use state average prices from the ACCRA Chamber of Commerce price surveys for the period 1982-1997 and find in two-way fixed effects models that a one dollar increase in state beer taxes significantly increases average state beer prices by $1.74. Thus, they find that beer taxes are strongly over-shifted to beer prices. This study has been cited hundreds of times in the large literature on alcohol control (including many studies using more recent data outside the YBK sample period) as the best evidence that taxes are more than fully reflected in prices.[44] But the relative lack of empirical research on this question is notable given that economic theory does not deliver a clear prediction on what to expect regarding tax pass-through in imperfectly competitive markets like the beer industry.[45]

In this note we revisit this critical and largely untested question and provide updated estimates on the relationship between beer excise taxes and beer prices using recently digitized price data for 756 cities from ACCRA/C2ER going back to 1982 (i.e., the city-level price data underlying the state averages used in YBK). We build on YBK

[44] Some early work also references Cook and Moore 1983 and Cook 1981 as evidence that alcohol taxes are fully shifted to prices, but neither of these studies leverages changes in alcohol taxes over time which has been shown to be important in other contexts such as cigarette tax pass-through (Sullivan and Dutkowsky 2012).

[45] While our study is most strongly related to YBK, three other studies are quite relevant. Chetty, Kroft, and Looney (2009) (henceforth CKL) study the relationship between beer prices and beer taxes as part of their analysis of tax salience. They report that the coefficient on the state excise tax in a regression of beer prices using ACCRA data from 1982-2000 (versus 1982-1997 used in YBK) is ‘approximately one’, though they do not report these results directly (as they are not the main focus of the paper). Kenkel (2005) estimates the effect of a large increase in Alaska’s alcohol taxes in 2002 on quoted prices from a telephone survey for alcohol in both on-premises and off-premise locations performed one year before and one year after the tax hike. For off-premises beer sales – similar to the beer prices measured in our ACCRA data – he estimates significant overshifting of taxes to prices for five of six brands. Kenkel’s study design did not, however, collect prices in other states, and as such he is unable to disentangle the tax effect from other secular changes in prices. Hanson and Sullivan (2014) use a geographic regression discontinuity design for four southern states where alcohol taxes vary sharply across state borders. Using a telephone survey of alcohol prices, they find evidence of *negative* pass-through of excise taxes to prices.

(2002) in several important ways. In addition to using the city-specific prices, we also explicitly adjust our standard errors to allow for state level clustering (Bertrand, Duflo, and Mullainathan 2004), and we explore sensitivity of our main findings to inclusion of smooth state-specific time trends. Finally, we update previous estimates from 1982-1997 with data from 1998-2012.

We report two key findings. First, although we are able to very closely replicate the beer tax and beer price means reported in YBK (2002), we are unable to replicate their over-shifting result. Over the period 1982-1997 and using the same specification as reported in YBK, we obtain a pass-through estimate of \$0.98 – much lower than the \$1.74 reported in YBK (2002).[46] Moreover, once we adjust for state clustering, the estimate is not statistically significant. Second, using the full 1982-2012 sample with 33,660 local price observations we estimate a pass through rate of between \$0.68 and \$1.10 once we account for smooth city-specific time trends. These estimates are statistically significant even after accounting for state clustering. Taken together, our results provide the strongest evidence to date that beer prices reflect significant variation across space and time in the level of state excise taxes on beer, though the estimated pass-through rates are much lower than previously thought.

[46] Although YBK generously shared their data with us, we have been unable to precisely determine why we cannot replicate their findings. At the time of this writing, we are actively working with YBK to understand the difference in pass-through estimates. We note that we can nearly exactly match the reported price and tax averages reported in their paper, with small differences likely attributed to transcription and computation errors from creating state specific averages from the ACCRA hard copy printouts. For example, the data provided to us from YBK do not include state average prices for Hawaii, but the hard copy surveys include several year-quarter observations for Hilo, HI and Honolulu, HI. All of our models use the data either provided directly by ACCRA/C2ER in digitized form (for 2000-present) or that was double-entered by hand with guaranteed accuracy of 99.9% (for 1982-1999).

This note proceeds as follows: Section 2 briefly outlines the data and empirical approach. Section 3 presents the results, and Section 4 concludes.

Data Description and Empirical Approach

We use data on local beer prices from the Associated Chambers of Commerce Researchers Association (ACCRA/C2ER). ACCRA data were originally designed to provide cost of living estimates for urban professionals and have collected city-specific prices since 1982. Typically volunteers from local Chambers of Commerce would collect information on prices of several local goods, and ACCRA/C2ER compiled these into a city-specific index. These data have been used extensively in the existing literature on tax pass-through in part because of the large set of types of goods for which price information was collected and because of the large number of participating cities (see, for example, Sullivan and Dutkowsky 2012 who study cigarette tax pass through using ACCRA). We observe prices for a six-pack of various brands of beer from 1982-2012.[47] Our data on beer taxes comes from the University of Michigan World Tax Database; beer taxes are measured in dollars per gallon of beer.

To estimate the pass-through of beer taxes to beer prices, we begin by following the core specification in YBK using state level average prices (i.e., aggregating up the city level price observations to the state level) from 1982-1997. This model takes the form:

[47] The actual brands of beer changed over our sample period. From 1982.Q1 until 1989.Q3 it was a six pack of Budweiser or Schlitz. From 1990.Q1 until 1999.Q4 it was a six pack of Budweiser or Miller Lite. From 2000.Q1 to present it has been a six pack of Heineken.

(1) $PRICE_{st} = \beta_0 + \beta_1(EXCISE\ TAX)_{st} + \beta_2(COST\ CONTROLS)_{st} + \beta_3 STATE_s + \beta_4 TIME_t + \varepsilon_{st}$

where $PRICE_{st}$ is the real price (in 1997 dollars) of a six pack of beer in state *s* at time *t*. TAX is the relevant federal and state excise tax on beer converted to dollars per gallon of pure ethanol.[48] COST CONTROLS is a vector of input costs of energy, production space, and labor measured in the ACCRA data and includes: gasoline, apartment rent, and washing machine repair cost.[49] Like PRICE, COST CONTROLS are also constructed using simple state averages. STATE and TIME are a full set of state and year-quarter dummies, respectively. Equation (1) follows YBK and weights the regressions by the number of cities underlying each state price observation. In robustness models we also investigate sensitivity of the tax pass-through estimate (i.e., β_1) to inclusion of smooth state-specific time trends (i.e., interacting a variable TREND that equals 1 in 1982.Q1, 2 in 1982.Q2, etc., with each state fixed effect). We cluster standard errors at the state level throughout (Bertrand et al. 2004).

We then move to our preferred specification which leverages the underlying city-specific microdata and follows Sullivan and Dutkowsky (2012) in which we estimate two-way fixed effects models controlling for city (as opposed to state) and time fixed effects. These models take the following form:

(2) $PRICE_{cst} = \beta_0 + \beta_1(TAX)_{st} + \beta_2(COST\ CONTROLS)_{ct} + \beta_3 CITY_c + \beta_4 TIME_t + \varepsilon_{cst}$

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[48] Federal beer taxes were raised substantially in 1991 but because there is no sub-national variation in such taxes they contribute nothing to the estimation of the tax pass-through estimate, as they are perfectly collinear with time dummies.

[49] These variables were chosen to most closely match the controls used in prior work and because they were consistently measured in the largest number of quarters of the ACCRA data.

where $PRICE_{cst}$ is the real beer price (again 1997 dollars for comparability) in city *c* in state *s* at time *t*. TAX and COST CONTROLS are as defined above, except COST CONTROLS in equation (2) are measured at the city level. CITY and TIME are a full set of city and year-quarter dummies, respectively. As above, we also investigate sensitivity of the tax pass-through estimate (i.e., β_1) to inclusion of smooth *city*-specific time trends (i.e., interacting TREND with each city fixed effect).

Results

We begin by reporting descriptive statistics for the tax and price data in Table 1. Real beer prices over our sample period averaged $7.37 per six pack, with prices substantially higher in the late period than in the early period. The real value of federal plus state beer taxes averaged 65 cents per gallon over the sample period, with real taxes actually falling in the later period (1998-2012) relative to the earlier YBK period (1982-1997) because states did not increase them in line with inflation.

Table 2 shows our first set of estimates of the pass through of beer taxes to beer prices. Our first goal is to try to replicate prior findings using the same data from YBK who find in standard difference-in-differences models that a one dollar increase in state excise tax on beer is associated with a $1.74 increase in ACCRA beer prices from 1982-1997 (as reprinted in column 1 of Table 2). Recall that YBK used as their outcome the average price within the state based on the cities reporting beer prices in any quarter, and they weighted their core estimates by the number of cities contributing to that average in any individual quarter. Column 2 shows the results of our attempted replication using the data generously provided by YBK. We obtain a statistically significant but much lower estimate: 1.01, suggesting that beer taxes are fully shifted to beer prices.

In column 3 of Table 2 we re-estimate the same specification as reported in column 2 but we use our underlying micro-data on city-prices, aggregated up to the state level for the same period (1982-1997) and weighted by the number of cities contributing to the state average in each quarter. Apart from transcription error, these findings should be very similar to those using the data directly provided by YBK, and indeed this is the case; we estimate in column 3 of Table 2 a statistically significant pass-through estimate of 0.98, very close to the 1.01 obtained using YBK's data.

The remaining columns of Table 2 investigate the robustness of the basic 1982-1997 estimate using state averages based on the digitized microdata. Column 4 reports estimates from an identically specified model to that in column 3 but where we additionally cluster standard errors at the state level, as is now standard in the literature (Bertrand et al. 2004). Doing so causes the standard error on the beer tax estimate to increase dramatically (consistent with extensive serial correlation within states) and renders the point estimate statistically indistinguishable from zero. In columns 5 and 6 we retain the specification of column 3 (i.e., with state clustering) but allow for linear and quadratic state-specific time trends, respectively. Doing so has dramatic effects on the point estimate on the beer tax from 0.98 to 0.331 (with linear state trends) and 0.944 (with linear and quadratic state trends), and notably the last estimate is statistically significant even with state clustering.[50] Overall, the results in Table 2 indicate that estimates of the pass-through of state beer taxes to state average beer prices are sensitive both to state clustering and to inclusion of smooth state trends, though in models with quadratic state trends we estimate statistically significant pass through on the order of 0.944 for every one dollar increase in state beer tax.

In Table 3 we turn to our preferred specification using the city-specific microdata on beer prices for the entire period 1982-2012 and replacing state fixed effects and state trends with city fixed effects and city trends, and as above we also explore robustness to state clustering. Column 1 of Table 3 presents beer tax estimates from equation (2) with city and year-quarter fixed effects and cost controls along with standard errors that are robust to heteroskedasticity but not state clustering. Column 2 replicates column 1 but reports standard errors adjusted for state clustering. Column 3 adds linear city-specific time trends, and column 4 adds quadratic city-specific time trends.

The results in Table 3 document several new and interesting patterns. First, accounting for state clustering matters a great deal for inference. Standard errors are an order of magnitude larger in column 2 compared to column 1. Second, estimates of the effect of beer taxes on beer prices are quite sensitive to city-specific time trends, similar to the pattern observed in Table 2.[51] Third, once we account for smooth city-specific time trends, we obtain statistically significant pass-through estimates on the order of $1.095 in column 3 (with linear city trends) and $0.684 in column 4 (with liner and

[50] In results not reported but available upon request, we found that the added state trends were always jointly significant predictors of state average beer prices.
[51] As above, the added trends are always jointly statistically significant predictors of local alcohol prices. Results not reported but available upon request.

quadratic city trends).[52] Both of these estimates are substantially smaller than the $1.74 pass through estimate reported in YBK (2002).

Conclusion

In this note we have revisited the important but understudied question of how state excise taxes on beer are passed through to beer prices. This question is important to both public finance and health economics literatures because numerous studies control for state beer taxes to capture a plausibly exogenous measure of local alcohol prices. Using recently digitized city-specific price data for the period 1982-2012, our best estimate is that a one dollar increase in state beer tax is shifted to prices at a rate of $0.68 to $1.10. These estimates are much lower than estimated in previous work, but they are statistically significant. Overall, our research supports the use of state beer taxes as meaningful measures of local alcohol prices and thus one measure of local alcohol availability.

[52] In results not reported but available upon request we also explored sensitivity of our main estimates to restricting attention to the sample of cities observed in 25%, 50%, or 75% of all year-quarter observations to address concerns that cities come in and out of ACCRA participation over time. Doing so returned similar patterns to those reported in Table 3.

Table 1
Descriptive Statistics, Prices and Taxes
ACCRA/C2ER, 1982-2012

	(1) Years	(2) Mean	(3) SD	(4) Min	(5) Max
Real beer price, full sample	1982.Q1-2012.Q1	$7.37	$1.50	$3.29	$13.41
Real beer price, YBK period	1982.Q1-1997.Q4	$6.23	$0.69	$3.29	$11.07
Real beer tax, full sample	1982.Q1-2012.Q1	$0.61	$0.24	$0.28	$1.76
Real beer tax, YBK period	1982.Q1-1997.Q4	$0.65	$0.27	$0.28	$1.76

Notes: Prices are from ACCRA, various years 1982.Q1-2012.Q1. Beer taxes are measured in dollars per gallon of pure ethanol and are compiled from various sources, including the Brewer's Almanac and the World Tax Database. The actual product varied for beer over our sample period: from 1982.Q1-1989.Q3 it was a six pack of Budweiser or Schlitz; from 1990.Q1-1999.Q4 it was a sick pack of Budweiser or Miller Lite; from 2000.Q1-present it has been a six pack of Heineken.

Table 2
Attempted YBK Replication and Extension of Beer Tax Pass-Through Using State Averages
ACCRA/C2ER, 1982-1997

	(1)	(2)	(3)	(4)	(5)	(6)
Specification ➔	State and time fixed effects, as reported in YBK (2002)	Attempted replication with state and time fixed effects, using data YBK provided	Attempted replication with state and time fixed effects using city level microdata collapsed to state level	(3), cluster on state	(4), add linear state trends	(5), add quadratic state trends
Beer tax	1.74***	1.01***	0.982***	0.982	0.331	0.944***
	[16.7]	(.097)	(.090)	(.627)	(.360)	(.301)
R^2	.79	.78	.81	.81	.88	.90
Observations	2,891	2,887	2,901	2,901	2,901	2,901

Note: Prices and taxes measured in 1997 dollars. T-statistics in brackets; robust standard errors in parentheses. Standard errors in columns 4-6 have also been adjusted for clustering at the state level. All models also include cost controls described in text (washing machine repair, monthly apartment rent, and gallon of gasoline) that have been aggregated to the state-quarter level, not shown here. Models are weighted by the number of cities on which the state-quarter price observation is based. *** significant at 1%; ** significant at 5%; * significant at 10%.

Table 3
New Estimates of Beer Tax Pass-Through Using City-Specific Prices
ACCRA/C2ER, 1982-2012

	(1)	(2)	(3)	(4)
Specification ➔	City and year-quarter fixed effects, no state clustering	City and year-quarter fixed effects, with state clustering	(2) + linear city trends	(3) + quadratic city trends
Beer tax	0.494***	0.494	1.095***	.684**
	(.045)	(.479)	(.408)	(.323)
R^2	.91	.91	.93	.94
Observations	33,660	33,660	33,660	33,660

Note: Prices are measured in 1997 dollars. Robust standard errors clustered at the state level are reported in parentheses for columns 2-4; standard errors in column 1 are not adjusted for state clustering. All models also include city-specific cost controls described in text (washing machine repair, monthly apartment rent, and gallon of gasoline), not shown here. *** significant at 1%; ** significant at 5%; * significant at 10%.

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