Do Alcohol Excise Taxes Reduce Motor Vehicle Fatalities? Evidence from Two Illinois Tax Increases.

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ABSTRACT

This study examines the effect of alcohol excise taxes on alcohol-related fatal traffic crashes. In 2015, 29.3 percent of the 35,092 traffic fatalities in the United States involved someone driving under the influence of alcohol. Raising federal or state excise taxes on alcoholic beverages could reduce demand for alcohol and alcohol-related traffic fatalities, but within the large literature on the behavioral effects of alcohol prices and excise taxes there is no clear answer on the effect of tax increases on drunk driving deaths. We examine two large increases in excise taxes in Illinois that occurred in 1999 and in 2009. Using the synthetic control method, we do not find evidence that the tax increases led to a long-term reduction in alcohol-related traffic fatalities. Our results are robust across several specifications, although we do find evidence that, following the 2009 increase, Illinois counties that do not share a border with another state experienced a temporary drop in alcohol-related traffic fatalities.

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INTRODUCTION

Alcohol-impaired driving, legally defined as operating a vehicle with a blood alcohol content (BAC) of 0.08 grams per deciliter (0.08 percent) or higher, is illegal at both the federal and state level in the United States. Over the past four decades, the number of fatal alcohol-related motor vehicle crashes (FARMVCs), has dropped over time both in the United States and Illinois (figure 1). Yet in 2015, out of 35,092 motor vehicle fatalities, 10,265 (or 29.3 percent) occurred in an accident in which a driver had a BAC at or above 0.08 percent (NHTSA 2016).

One commonly proposed solution to reducing FARMVCs and other alcohol-related harms is to increase excise tax rates on alcoholic beverages. This approach shows promise because research suggests that when excise taxes are raised, retail prices can increase by at least as much as the amount of the tax increase (Young and Bielińska-Kwapisz 2002, Conlon and Rao 2016). However, although extensive research has demonstrated that alcohol consumption responds to price changes (Wagenaar, Salois, and Komro 2009; Hansom and Sullivan 2015), the evidence on the effects of alcohol taxation on motor vehicle fatalities is mixed. For example, in the 12 published estimates described in the literature review of Elder and colleagues (2010), five are positive, implying that raising excise taxes paradoxically increases traffic fatalities. Nelson and McNall (2016) note that one source for these varied results lies in the difficulty in creating quasi-random experiments of control and treatment states. Further, motorists in some states may be avoiding tax increases by crossing borders to purchase less expensive alcohol in neighboring states (Beatty, Larsen and Sommervoll 2009).

In this report, we examine the effect on FARMVCs of two substantial alcohol tax increases imposed by Illinois in 1999 and in 2009. Using the synthetic control methodology (SCM) described by Abadie, Diamond, and Hainmueller (2010), we combine the FARMVC trends of other states to create a synthetic version of Illinois with a FARMVC trend like the trend before the tax increases. We then evaluate the effect of tax increases on FARMVCs by comparing the trend in the actual Illinois with the trend in our synthetic state of Illinois in the years after the tax increases.

We rely on the US National Highway Traffic Safety Administration's Fatality Analysis Reporting System (FARS) to measure driving fatalities both with and without alcohol by state from 1982 through 2015. These data, combined with demographic, economic, and medical data from a range of sources, allow us to create a synthetic state that matches Illinois along several important dimensions.

We find little evidence that the tax increases led to a sustained drop in either the share of fatal crashes that involved alcohol or the FARMVC rate per driver. These results are robust across a range of specifications and modeling choices. This contrasts with the results of Wagenaar, Livingston, and Staras (2015), who find that the excise tax increase in 2009 led to a substantial decline in FARMVCs. However, we find a transitory sharp decline in the counties that do not border other states, suggesting that some individuals living near a state line may have been crossing it to avoid the tax increase on alcoholic beverages. This suggests that the excise tax was temporarily successful in reducing fatal crashes in the interior counties of Illinois but, like the results found in Beatty, Larsen, and Sommervoll (2009), drivers in border counties could purchase alcohol in other states, reducing or eliminating the effect of the tax increase.

BACKGROUND

LIQUOR TAXATION AND THE STATES

Federal and state governments can levy taxes on specific goods or services, notably on cigarettes, gasoline, and alcohol. [1] These excise taxes are often collected at the producer or wholesale level, after which the cost of the tax is incorporated into the price of the final good or service. Beer, wine, and beverages with an alcohol content of 20 percent or higher (spirits) are generally taxed at different rates.[2]

The modern system of federal alcohol excise taxes was enacted following Prohibition's end with the ratification of the 21st Amendment to the US Constitution. The federal government currently levies an excise tax on alcoholic beverages with per unit rates based on the type of beverage and the percentage of alcohol by volume. Those rates have been increased several times since 1933, most notably in 1951, 1985, and 1991. However, the revenue from the tax has been falling as a percentage of GDP since the 1940s (figure 2).

The 21st Amendment gave states broad latitude in the regulation of alcohol sales, meaning that states are often the major actors in alcohol regulation. Most states use a "three-tier system," in which a single firm or entity (with a few exceptions) must act as only an importer or producer, a distributor, or a retailer. Alternatively, in "monopoly states," the government is the sole retailer and wholesaler of spirits and sometimes wine (Xu and Chaloupka 2011).[3] These states set prices directly rather than levying taxes or restricting supply by licensing retailers, limiting distributors, or other regulatory actions.[4]

Although most responsibility for regulation rests with the states, the federal government can influence how states regulate alcohol. Two prominent examples involve the prohibition on driving while intoxicated and the designation of the legal drinking age. In 1982, Congress created a program that offered basic grants to states enacting per se drunk-driving laws, which establish a specific BAC level (0.10 percent in this case) that serves as evidence that a driver is intoxicated (US Department of Transportation 2001).[5] Additional funds were available to states that, among other criteria, enacted a 0.08 percent BAC per se law. In 1988, Congress created a second grant program for states that met five of seven criteria, including establishment of a 0.10 percent BAC per se law. After three years, additional funds were available to per se states that lowered the

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[1] See http://www.taxpolicycenter.org/briefing-book/what-are-major-federal-excise-taxes-and-how-much-money-do-they-raise for a description of the major federal excise taxes.
[2] See http://www.taxpolicycenter.org/briefing-book/how-do-state-and-local-sales-taxes-work for a description of selective sales taxes in the context of state and local finance.
[3] These states are also called "control states".
[4] For a full list of the various alcohol policies by state and year, see: https://www.niaaa.nih.gov/alcohol-health/alcohol-policy
[5] In non-*per se* states, a driver could argue that even with a high BAC, they were not under the influence of alcohol.

BAC limit to 0.08 percent. Illinois established a 0.08 percent BAC per se law in 1997 and by May 2001, 49 states had per se limits, 25 at 0.08 percent and the remainder at 0.10 percent. States also set a minimum age for alcohol consumption, purchase, and possession. In 1984, the federal government required states to set 21 as the minimum age for purchasing alcohol or risk losing some federal highway funding. Consequently, by 1988 every state had a minimum purchasing age of 21, although some states continue to vary on regulations of consumption and possession.[6]

States mainly levy excise taxes on beer, wine, and spirits, although some states levy taxes on additional beverages (such as sparkling wine and cider). Because the tax rates are raised infrequently, the real levels of taxation for all three categories are slowly declining as the cost of living increases. Some states, such as Wyoming, Oregon, and Idaho, have not substantially increased their tax rates since 1982 and consequently have the lowest per capita alcohol revenue for 2014 at between $3 and $5 per capita. The states with the highest per capita alcohol tax revenue in 2014 are monopoly states (such as Alabama), states with high tax rates in 1982 (such as Tennessee), or states that have increased taxes over time (such as Alaska and Washington)[7]. Overall, the average per capita revenue from state and local alcohol taxes has dropped from $40 in 1977 to $21 in 2014, representing a decline from 0.67 percent of state revenue to 0.19 percent.[8]

In 1999 and 2009, Illinois increased its excise tax on spirits, wine, and beer. Before July 1, 1999, Illinois imposed a tax on manufacturers or importing distributors of alcohol of $0.07 per gallon of beer, $0.23 per gallon of wine, and $2.00 per gallon of spirits. In 2000, those rates went up to $0.19 for beer, $0.73 for wine, and $4.50 for spirits, more than doubling the tax rates for all three categories. In 2009, the tax rates increased again to $0.23 per gallon for beer, $1.39 per gallon for wine, and $8.55 for spirits. These shifts have increased per capita alcohol tax revenue in Illinois from $7 in 1999 to $19 in 2002 and $27 in 2014.

LITERATURE REVIEW

There is a substantial literature on the effect of prices and taxation on alcohol consumption and harmful behavior related to intoxication. These studies have used many methodologies to examine a wide range of interventions on different populations and subgroups. Overall, the consensus is that the price elasticity of demand for alcoholic beverages is negative and that beer, wine, and spirits have different elasticities. However, summaries of the literature come to

[6] See a full description of drinking age laws here: https://alcoholpolicy.niaaa.nih.gov/UnderageDrinking.html
[7] Washington voted to break up the state monopoly on alcohol sales in 2012. Washington gave up its monopoly state status, and instead opted for a set of taxes and fees that – based on some reports – increased alcohol prices dramatically. https://www.washingtonpost.com/blogs/govbeat/wp/2014/06/30/two-years-after-liquor-privatization-booze-in-washington-state-costs-more/?utm_term=.424d7381aeb1
[8] Total Revenue and Alcoholic Beverage Tax Revenue data are from the State and Local Finance Initiatives Data Query System (DQS), which draws data from the U.S. Census Bureau's Annual Survey of State and Local Government Finances: http://slfdqs.taxpolicycenter.org/pages.cfm

conflicting conclusions regarding the magnitude of those elasticities, especially when it comes to certain subgroups. Xu and Chaloupka (2011) summarize the general differences by noting that individual-level studies (often using survey data on drinking habits) find larger reactions to price changes then results using aggregate data (which in US-based studies are state-level data). This separation into aggregate and individual studies is important given researchers' desire to examine subgroups. Some research separately estimates the response of younger individuals, who often face alcohol-related harm at higher rates, and heavy drinkers.

SURVEYS

Gallet (2007) examines 132 studies of the price elasticity of alcoholic beverages and concludes that price elasticities are generally negative but sensitive to many specifications. Further, elasticity varies greatly across types of alcoholic beverages and across subgroups of individuals. For example, beer consumption seems to have a smaller price elasticity than that of wine or spirit consumption, and younger people seem to be less responsive to prices than older people.

Wagenaar, Salois, and Komro (2009) perform a meta-analysis of 112 studies with estimates of price elasticity that cover a range of interventions and subgroups. They report that on average, spirits have the highest price elasticity, followed by wine and then beer. They also find that the average price elasticity across 10 studies of heavy alcohol use is substantially lower than the elasticity for spirits, wine, or beer.

Elder and colleagues (2010) conduct a literature review of studies published before 2005 that examined the reaction of alcohol consumption to price or tax changes. They conclude that the average price elasticity was negative and rested between -1.0 and -0.3. They also review 12 studies of traffic fatalities. Of the 12, three do not provide tests of significance or are insignificant. Of the remaining nine, six find negative effects and three find positive effects.

Nelson (2013) examines 19 individual-level studies of heavy drinkers and finds limited evidence of a price reaction (only two papers find a statistically and economically significant reaction of heavy drinkers to prices). Nelson also reviews how fatality rates from liver cirrhosis respond to price and tax increases: of nine studies, three have insignificant results, four have at least some estimates indicating that cirrhosis fall with price increases, two find negative relationships, and one finds a negative relationship not replicated by other studies.

Chaloupka, Grossman, and Saffer (1998) discuss a range of regulatory policies that increase the price of alcohol consumption, including taxation and reductions in availability. They conclude that these policies could reduce the harms associated with alcohol consumption. Wagenaar, Tobler, and Komro (2010) concur in their analysis of 50 articles on alcohol-related morbidity and mortality. They conclude that most alcohol-related harms, including fatal motor vehicle crashes, show an inverse relationship with prices and taxes. Some measures of harm, (such as sexually transmitted infection rates and suicide) likely suffer from measurement error, so the authors believe that the true effect is greater than estimates suggest. Roodman (2015) reviews the same papers, and although he questions the direction of causality, he concludes that a causal relationship exists between alcohol taxes and health.

Nelson and McNall (2016) state that the evidence based on natural experiments is notably less supportive of the effectiveness of excise taxes in reducing harms than reported in previous meta-analyses. They focus on 45 studies of alcohol prices and taxes that use various policy changes between 2003 and 2015 across nine countries. Ten studies focus on drunk driving arrests and traffic fatalities, but only a few of them examine changes in the United States. They note that many of the price changes in the study are relatively small and that the changes are often accompanied by other regulatory changes in the availability of alcohol. They conclude that caution is warranted when evaluating studies and that some studies suffer from confounding variables or weak methodologies. Further, small price changes may only change an individual's beverage of choice rather than alter their total consumption behavior. Overall, they argue that papers based on natural experiments may work best in evaluating large, rapid price changes.

INDIVIDUAL STUDIES

Ruhm (1996) examines several policies, including beer taxes, over 1982 through 1988. Drawing on state-level data from FARS, he estimates the effects of policies on fatal crashes per capita and fatal crashes per mile driven for both the total population and teen drivers. He finds that failure to control for fixed effects severely biases results and that failure to control for macroeconomic factors, such as per capita personal income and the unemployment rate, leads to a smaller bias. He concludes that teen fatalities are more responsive to policy changes than fatalities among the overall population, and that an 80 percent increase in beer taxes would lead to a seven to eight percent decline in fatalities across the entire population.

Dee (1999) examines how alcohol policies affect teen drinking and teen traffic fatalities. Arguing that previous estimates of how beer taxes affect traffic fatalities are implausibly large, Dee uses state-level FARS data from 1977 through 1992 to suggest that existing estimates may suffer from omitted variable bias. In a model with year and state fixed effects, beer taxes have a statistically significant and negative effect on traffic fatalities. Although the effect is larger in absolute value than estimated by Ruhm (1996), its significance disappears and the estimate changes sign when state-specific time trends are added. As Dee points out, however, because the time trends remove most of the variation from beer taxes, these results do not conclusively demonstrate the presence of omitted variable bias.

Mast, Benson, and Rasmussen (1999) show that the negative relationship between higher beer taxes and both alcohol consumption or traffic fatalities is not nearly as robust as previously shown. Those earlier results, which used data through the 1980s, are weakened when more current data are used. They attribute this to changes in laws, such as raising the minimum age for purchasing alcohol, and campaigns by grassroots organizations to educate the public on the consequences of drunk driving. They also show that the choice of controls strongly determines the statistical significance of the effect of excise taxes suggesting that some variation in results is caused by a researcher's choice of control variables.

Young and Likens (2000) use state-level FARS data for 1982 through 1990 and add many control variables to their model. Although some alcohol laws reduce both motor-vehicle fatalities per thousand and alcohol-involved driver deaths per thousand, neither beer price nor beer excise taxes have statistically significant effects on those measures. Young and Bielińska-Kwapisz (2006) use state-level data from 1982 through 2000 and, noting that alcohol prices are endogenous, use excise taxes as an instrument for prices. They find that higher alcohol prices tend to reduce traffic fatalities, although the size of that effect varies across populations and time of day. For the total population, higher prices reduce fatalities more during weekend nights then during other times. For drivers between the ages of 16 and 20, the results are different, with prices reducing fatalities on weekend nights more than other times (although the difference is not statistically significant).

Young and Bielińska-Kwapisz (2002) use state-level data from 1992 through to evaluate the relationship between alcohol excise taxes and prices. They find that alcohol taxes are over-shifted to consumers, meaning that the price of alcohol goes up by more than the amount of the tax. Additionally, they find that beer taxes are poor proxies for the price of alcohol, while spirit excise taxes are strongly correlated with prices.

Beatty, Larsen, and Sommervoll (2009) examine the consumption of heavily taxed goods (namely alcohol and tobacco) and drunk driving in Sweden and Norway. The authors exploit tax differences between the two countries and measure the effect on sales and outcomes by proximity to the border. They find that large tax differences across international borders induces cross-border tax avoidance and that outlets near borders report lower revenue from the sale of highly-taxed goods, like alcohol and tobacco. Further, in nations with increased excise taxes, households near international borders reported higher consumption of beer than households in the interior. Because Illinois has two large population centers (Chicago and Greater St. Louis) near borders, a similar effect could moderate the effectiveness of the excise tax increase.

Cook and Durance (2013) examine the effect of the 1990 increase in federal excise taxes on alcoholic beverages. Because this increase affected all states simultaneously, they identify the effect on traffic fatalities by assuming that reductions in traffic fatalities in states will vary with preexisting levels of alcohol consumption—that is, states where people drink more will see a larger absolute change than states where people drink less, leading to larger effect on crime and traffic fatalities. A federal tax increase will not lead to cross-state tax avoidance behavior. The authors conclude that the tax increase did correlate with reductions in injury deaths, which includes homicides, suicides, and traffic fatalities, to a greater degree in states where people drink more. In their separate estimates for traffic fatalities, they do not find a statistically significant effect when using their preferred panel models.

Wagenaar, Livingston, and Staras (2015) focus on the September 2009 Illinois tax increase and use monthly crash data from FARS from 104 months before the increase to 28 months after the increase. They regress monthly fatalities in which a driver had a positive BAC on a dummy variable equal to one in months after the tax increase and equal to zero in months prior to the increase. To control for secular trends in fatal crashes, they include the number of non-alcohol-related crashes. They conclude that the tax increase led to a statistically significant 26 percent reduction in average monthly fatal alcohol-related crashes (or a reduction of about 9.9 crashes per month). They also estimate the response for subgroups, finding a greater reduction for young drivers (under 30 years old). The reduction in crashes is greater for those with BACs greater than 0.15 percent than for those with BACs between 0 and 0.15 percent. The authors also find that when controlling for the effects of the Great Recession, fatal alcohol-related crashes fell only 15 percent.

METHODOLOGY AND DATA

SYNTHETIC CONTROL METHOD (SCM)

In this article, we follow Wagenaar, Livingston, and Staras (2015) in examining the 2009 excise tax increase in Illinois, as well as the 1999 increase that preceded it. We use the SCM of Abadie, Diamond, and Hainmueller (2010) on annual data to uncover the effect on the long-run trend in FARMVCs in Illinois. We use the SCM because it does not rely on a quasi-randomized controlled trial for consistent estimation. Focusing on two very large changes addresses both the problem of multiple regulatory changes that can be conflated with excise tax increases and the problem that relatively small changes in prices and tax rates may be too small to change behavior.

In 1999 and 2009 Illinois dramatically increased its alcohol excise tax rates relative to other states. Figures 3 through 5 compare the Illinois tax rates for beer, wine, and spirits with the median state tax rate. Before to the 1999 tax increase, Illinois had the 29th highest spirit tax, the 42nd highest wine tax, and the 48th highest beer tax. Starting in 2000, those rankings rose to 9th, 19th, and 26th, respectively. After 2009, those rankings rose again to 2nd, 9th, and 22nd, respectively. Table 1 compares the 2009 change in excise taxes in Illinois and surrounding states. Before the increase, the Illinois excise tax on spirits was slightly higher than that of Tennessee and substantially higher than those of other states. After the increase, the Illinois rate is almost twice that of Tennessee and more than triple the rate in Indiana. Comparisons of wine and beer taxes show a similar but less dramatic pattern.

Few states substantially changed their alcohol taxes between 1982 and 2015. In real terms, only Alaska, Delaware, Hawaii, Florida, and New Mexico changed their spirit tax during that period to a greater degree than Illinois did (figure 6). Illinois's tax increases are unique compared with those states because Delaware partially reversed a 1991 tax increase in 1998, generalizing results from Alaska and Hawaii to the contiguous 48 states is difficult, and the changes in Florida and New Mexico occurred too early in the study period; the synthetic control methodology requires a longer period before a policy change (or treatment). California, Connecticut, Florida, Iowa, New Jersey, New York, Nevada, and Oklahoma also had substantial but smaller excise tax changes.

The SCM estimates the effect of a policy change or treatment in a single state by comparing the actual outcomes (FARMVCs in this study) to the outcomes of a synthetic state used as a control. That synthetic state is formed as a weighted sum of states chosen from a pool of potential donors. The weighted sum is created by matching predictors in the pre-treatment period of the donor states to the predictors for Illinois, the treated state. The predictors typically include outcomes in the pre-treatment period as well as additional variables, such as share of the population under age 25, that are related to the outcome.

Valid implementation of the SCM requires that outcomes in the synthetic control during the pre-treatment period track the outcome of the treated state before treatment. If the two closely match, the path followed by the synthetic control after treatment is presumed to follow the path that the treated state would have taken in the absence of treatment. The effectiveness of the treatment is measured by how much the two series diverge. The SCM is similar in spirit to a difference-in-differences estimator, but the SCM can be used when the treatment effect varies over time. Because the method requires that the pre-treatment outcomes of the synthetic control state credibly match those of the treated state, Abadie, Diamond, and Hainmueller (2015) recommend that the method not be used if the match is poor or if the pre-treatment period is short.

To form a synthetic control state, the SCM first creates state-level weights. The weights are contingent on other weights that indicate the power of the chosen predictor variables. These predictor weights are typically chosen to minimize the outcome's root mean squared prediction error (RMSPE) between the synthetic and treatment state in the pre-treatment years. Outcomes from the pre-treatment period are powerful predictors, and in practice they are often highly weighted. In fact, using all lagged values for every year in the pre-treatment period will yield the lowest possible RMSPE. However, Kaul and coauthors (2016) show that if all outcomes in the pre-treatment period are used as predictors, other predictors receive no weight and are ignored.[9] They further show that this can produce a potentially serious degree of small sample bias. Consequently, our primary model uses a small number of pre-treatment outcomes and several other variables as predictors. We also use an alternative model using outcome lags from all pre-treatment years as a way to measure the fit of our model.

Consistency of the model requires that a fixed combination of donor states must be capable of approximating the counterfactual outcome of the treated state. If the counterfactual cannot be approximated, interpolation bias can result. As one solution, Abadie, Diamond, and Hainmueller (2015) recommend restricting the pool of potential donor states to those states with similar values for predictors. Reducing the size of the potential donor pool also reduces the risk of "overfitting": a larger donor pool increases the chance that donor states are matched because of idiosyncratic noise rather than because they share an underlying trend with the treated state. In our study, we use a relatively small donor pool, and as predictors, we carefully select outcomes from years that represent the overall trend of the outcome. We then test the sensitivity of our results to the choice of donors in the pool and the choice of years selected.

[9] This follows from the fact that assigning a positive weight to non-outcome predictors cannot lower a MSPE measured solely with outcome predictors.

The SCM does not create standard confidence intervals or inference tests available from regression methods; we instead rely on the placebo tests described in Abadie, Diamond, and Hainmueller (2010, 2015). In the first of those tests, the SCM is separately run on each state in the donor pool as though it is a treated state. The resulting placebo state is compared with its synthetic match, and the test is repeated on the next state in the donor pool. Because none of the donor pool states were affected by the policy change in question, differences between the placebo state and its synthetic control occur randomly around zero. The difference in outcomes between the treated state and its synthetic match is unlikely to arise from chance if it exceeds most of the differences among the placebos, and treatment would therefore appear to be effective.

The placebo test can act as a type of inference test, but analyses in recent work such Ferman and Pinto (2016) and Hahn and Shi (2016) have questioned the validity of the former approach. Ferman and Pinto (2016), for example, point out the distribution of the placebo differences is not the same as the difference in the actual treated state: although analysts use the SCM if the synthetic control closely matches the treated state during the pre-treatment period, the placebo states are fitted by rote, regardless of the tightness of the fit. Nevertheless, we perform a graphical comparison of the treated state and the placebo states to provide evidence about the potential effectiveness of the treatment. When necessary, we test the sensitivity of our conclusions to the problem by omitting the worst-fitting placebo states (Abadie, Diamond, and Hainmueller 2015).

In the second test, Abadie, Diamond, and Hainmueller (2015) examine the ratio of the RMSPE before treatment to the RMSPE after treatment for the treated state and for each of the placebo states. If the treatment is effective, the actual state will diverge away from the synthetic control, so the RMSPE after treatment will be large relative to its value before treatment. However, the model is only used if the synthetic state tightly fits the treated state before treatment but the SCM is applied by rote to the placebo states. Therefore, in the pre-treatment period, the RMSPE of the treated state will tend to be smaller than the RMSPE of the placebo states, so the ratio of post-treatment to pre-treatment RMSPE for the treated state will tend be higher than the same ratio for the placebo states. As the difference in outcomes between the synthetic control state and treated state declines to zero in the pre-treatment period, the ratio increases toward infinity. Consequently, we use both the RMSPE in the post-treatment period and the ratio of the pre-treatment to post-treatment RMSPE when evaluating our results.

DATASET

As with many previous studies, we use FARS to track the total number of fatal motor vehicle crashes. This dataset records information about motor vehicle crashes that took place on a public motorway and led to the death of a vehicle occupant or nonoccupant (NHTSA 2016). The data are collected from state documentation of crashes, which the NHTSA staff converts into a range of data elements about the time, location, and circumstances of the crash. These data have been collected since 1975, but because certain adjustments to the data made in 2002 were only applied to the dataset after 1982 and because of improvements in data collection that started in the same year, we only use data from 1982 through 2015.

For each crash, FARS provides data at the crash level, the vehicle level, and the person level. For each crash, FARS offers data on where and when the crash took place, the make and model of each vehicle in a crash, and the number of people involved as well as their demographic characteristics. Most importantly, FARS also records the BACs for most individuals involved. One potential issue with FARS dataset is that BACs are not available for every crash. In 2001, FARS addressed this issue by implementing a multiple imputation technique for calculating missing BACs for all available years of data. The methodology works in two stages, first using a logit model to separate out persons with BACs of 0 and those with BACs greater than 0, then using a regression model to determine BACs conditional on the first model predicting a positive value (Subramanian 2002). This methodology creates 10 imputed BACs per person-level record, and we use the average across those values as in our model. This methodology was implemented in 2002 and retroactively applied to FARS data starting in 1982.

We use the person-level file to identify which accidents involved a driver with a BAC at or above 0.08 percent, and then we aggregate the number of total accidents as well as the number of alcohol-related accidents by year and state. Next, we calculate our two variables of interest by year and state: the share of total fatal crashes with a BAC at or above 0.08 percent and the number of fatal crashes with a BAC at or above 0.08 percent divided by the total number of drivers. To calculate the second value, we use data from the US Department of Transportation's Highway Statistics on the number of licensed driver by state and year.[10]

To eliminate states from the donor pool with similar tax increases, we construct a dataset of excise taxes on beer, wine, and spirits.[11] We begin with the Urban-Brookings Tax Policy Center's records of state alcohol tax rates for select years between 2000 and 2017[12] and the Tax Foundation's state sales, gasoline, cigarette, and alcohol tax rates by state from 2000 to 2014.[13] We extend this dataset back to the 1990s using the Book of the States from 1995 through

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[10] US Department of Transpiration, Federal Highway Administration, Highway Statistics 2014: Table 6.2.2. Licensed drivers, by State, 1949-2014 and Highway Statistics 2015: Table 6.3.3. Licensed Drivers, by State, sex, and age group. We use these tables to collect state-year data on the number of licensed drivers. The data can be found at: https://www.fhwa.dot.gov/policyinformation/statistics/2014/ and from https://www.fhwa.dot.gov/policyinformation/statistics/2015/

[11] Urban-Brookings Tax Policy Center, Alcohol Excise Tax Rates: http://www.taxpolicycenter.org/statistics/state-alcohol-excise-taxes

[12] Urban-Brookings Tax Policy Center, Alcohol Rates 2000-2010, 2013-2017: http://www.taxpolicycenter.org/statistics/alcohol-rates-2000-2010-2013-2017

[13] Tax Foundation, State Sales, Gasoline, Cigarette, and Alcohol Tax Rates by State, 2000-2014: https://taxfoundation.org/state-sales-gasoline-cigarette-and-alcohol-tax-rates/.

1998[14] and Significant Features of Fiscal Federalism for 1990 through 1995.[15] Using a record of legal changes from the Distilled Spirits Council of the United States, we combined the records into a unified set of tax rates as of January 1 of any given year. In cases where the sources differed on rates, we referred to the official state office of revenue or the legislation itself.

To complete our dataset, we use data from the US Bureau of Economic Analysis on personal income per capita and price indexes for personal consumption expenditures, data from the Centers for Disease Control on mortality from alcoholic cirrhosis of the liver, data from the US Department of Transportation on state gasoline tax rates, and the Urban Institute's State and Local Finance Initiative's data on state unemployment rates. We also obtained census data on population by year, age group, and state from the Bureau of Economic Analysis and Centers for Disease Control. We use the Bureau of Economic Analysis's PCE deflator to convert personal income and gasoline tax rates to 2015 dollars.

MODEL SPECIFICATION

When using the synthetic control methodology, we first determine the pool of potential donor areas (in our case, states), starting with the 48 contiguous states (figure 6). We eliminate states that changed alcohol excise tax rates by more than one dollar (in 2015 dollars) from 1982 through 2015, leaving a donor pool of 35 states.[16] We then exclude monopoly states, leaving 20 states. As part of our specification testing, we also use an expanded donor pool that includes the monopoly states.

We separately analyze the two tax increases in Illinois. We start with the 1999 increase, using 1982 through 1998 as the pre-treatment period. Because we find no evidence of a decrease in FARMVCs in Illinois, we study the 2009 increase using a pre-treatment period of 1982 through 2008.

As noted, we use two variables to measure outcomes. In both cases, we start with the count of fatal motor vehicle crashes in which drivers had BACs at or above the legal limit of 0.08 percent. For the first outcome variable, we divide the number of FARMVCs by the total number of fatal motor vehicle crashes to get the FARMVC share. This is meant to control for factors that affect all types of driving fatalities. For example, changes in macroeconomic variables, such as the unemployment rate or the price of gasoline, might affect the number of miles driven in a state. Improvements in automobile safety and medical technology and changes in state spending on infrastructure could also affect fatality rates for those in motor vehicle crashes. Macroeconomic

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[14] Book of the States 1997 to 1998 and Book of the States 1995to 1996 – Table 6.21 - State Excise Tax Rates http://knowledgecenter.csg.org/kc/content/book-states-archive-1935-1999.
[15] Significant Features of Fiscal Federalism Volume 1, 1990-1995. http://www.library.unt.edu/gpo/acir/BrowseTitles.htm
[16] Conlon and Rao (2016) show that because nominal price rigidities, changes in alcohol excise taxes less than $1 do not lead to price changes unless the prices are far from optimal.

variables could, however, affect sober driving rather than alcohol-impaired driving. For example, such variables could reduce the amount of driving to and from work much more than driving to and from drinking establishments. In that case, changes in the share of fatalities involving alcohol might reflect changes in employment-related driving more than changes in alcohol-impaired driving. We therefore use a second outcome variable, the number of FARMVCs per driver in a state, and we add several predictors to account for macroeconomic factors.

RESULTS

POWER ANALYSIS

Before using the SCM to analyze the Illinois tax increases, we first confirm its ability to detect changes in FARMVCs. Specifically, we perform two series of Monte Carlo simulations, one in which there is no effect and one in which there is the 26 percent decline estimated in Wagenaar, Livingston, and Staras (2015). Although that decline is over a short period, Xu and Chaloupka (2011) note that the price elasticity of consumption for alcohol seems to be higher over the long run. If FARMVCs follow a similar pattern, a 26 percent decline would be a lower bound of the observed effect.

We first establish a benchmark by calculating 1,000 Monte Carlo simulations in which there is no treatment. To simulate data that has the same characteristics as our actual sample, all iterations follow the same overall trend found in the pool of donor states, but iterations vary by a randomly chosen state-level fixed effect and independently and identically distributed noise. To accomplish this, we use estimates from a simple two-way fixed effect model estimated on our donor pool:

$$S_{it} = \delta_t + \upsilon_i + \varepsilon_{it} \tag{1}$$

where S_{it} is the share of FARMVCs in state *i* in year *t*. Estimates of the year fixed effects $\hat{\delta}_t$ are used as a trend that is the same in each simulation. In each iteration, we randomly select one of the estimated entity fixed effects $\hat{\upsilon}_i$ and make *t* random draws $\hat{\varepsilon}_{it}$ from a normal distribution with the same standard deviation as ε_{it}. The series in each iteration is then as follows:

$$\hat{S}_{it} = \hat{\delta}_t + \hat{\upsilon}_i + \hat{\varepsilon}_{it} \tag{2}$$

We then use the synthetic control method to predict $\hat{S}_{it}$ and calculate the RMSPE in the years 2000 through 2015. The 95th percentile of the 1,000 RMSPEs determines the 5 percent critical value in the second set of simulations.

For each iteration of the second set of simulations, each simulation is created as before, except we impose a 26 percent decline in crash fatalities in the years after 2000:

$$\hat{S}_{it} = \begin{cases} \hat{\delta}_t + \hat{v}_i + \hat{\varepsilon}_{it}, & \text{if } t < 2000 \\ .74 * (\hat{\delta}_t + \hat{v}_i + \hat{\varepsilon}_{it}), & \text{if } t \geq 2000 \end{cases}$$

(3)

We again use the synthetic control method to predict $\hat{S}_{it}$ and calculate the RMSPE from 2000 through 2015. If the resulting RMSPE lies above the 5 percent critical value, the SCM can detect the 26 percent decline at the 5 percent point. In our simulations, we find clear evidence that the SCM can detect the change: 93.3 percent of the RMSPEs from the treated simulations exceed the 5 percent critical value calculated from the initial, untreated, set of simulations.

1999 ILLINOIS TAX CHANGE

We begin by examining the effect of the July 1999 excise tax increase in Illinois. Our first measure is the FARMVC share of all fatal motor vehicle crashes. Because the tax took effect in July 1999, we use 2000 as the first treatment year and 1998 as the last pre-treatment year. In our first analysis, we examine the FARMVC share using our donor pool of states.

As predictors, we use the FARMVC share of all fatal motor vehicle crashes in the years the 1983, 1985, 1991, 1993, and 1998. These are chosen to represent the long-run trend in the pre-treatment period, and in a later section of this report we test the sensitivity of our results to those choices. Because previous research has demonstrated the importance of young drivers when studying FARMVCs, we use the share of the population ages 15 to 24 as an additional predictor. McCarthy (2003) finds that crashes by older drivers are affected by alcohol prices, so we also use the share of the population over 65 as a predictor. Finally, as a measure of heavy drinking, we use the share of the population that died from alcoholic cirrhosis as a predictor variable. Following the standard approach for the SCM, the non-outcome predictors are averaged over the entire pre-treatment period (1982–98). [17]

The result is shown in figure 8.[18] The synthetic control tightly fits the actual Illinois in the pre-treatment period and the RMPSE is 0.0090. During the post-treatment period, the synthetic Illinois lies below the actual Illinois for most years. Taken at face value, that result suggests that the tax increase raised fatalities rather than lowering them. More likely is that the excise tax increase had no effect, and the relative positions of the synthetic and actual Illinois are due to chance.

[17] In the appendix (appendix figures 1 – 28) we check for the potential for interpolation bias by comparing the values for these variables across all 50 states plus DC and the US. There are no years where either the share or driver values for Illinois is a clear outlier.
[18] The selected states and their weights are listed in the appendix.

Our synthetic Illinois is composed of a weighted average of eight states (appendix table 1)[19]. Minnesota forms almost 40 percent of the synthetic control, Indiana forms nearly 25 percent, and North Dakota forms 10 percent. The remaining states each form less than 10 percent.

The list of all predictors and their weights is included in appendix table 2. As is typical for this method, the outcome lags are the most heavily weighted predictors, with weights between 15 and 20 percent. The other predictors have weights of less than 5 percent.

Although our results do not suggest that the excise tax increase in 1999 decreased FARMVC in the following years, we conduct the placebo test developed by Abadie, Diamond, and Hainmueller (2010). The results, shown in figure 9, plot the difference between the actual Illinois and its synthetic analogue and perform the same experiment on every state in the donor pool.

The difference between the synthetic and actual Illinois is well within the range of difference in other states in the donor pool. The RMSPE in the post-treatment period for Illinois is 0.0220, which is lower than the RMSPE of any other state, although the ratio of post-treatment to pre-treatment RMSPE is higher than most states. Undoubtedly that occurs because the synthetic Illinois tightly fits the actual Illinois in the pre-treatment period, and it should not be taken as evidence that the excise tax increase raised fatalities. Because many of the synthetic placebos poorly match the actual states, we follow Abadie, Diamond, and Hainmueller (2010) and eliminate any state with a pre-treatment RMSPE greater than 10 times that of Illinois and reexamine the resultant figure. We then repeat the experiment by eliminating any state with a pre-treatment RMSPE greater than five times that of Illinois. Both exercises led to no notable change in the results (appendix figures 29 and 30).

If our results are influenced by the choice of pre-treatment lags, using other lags may change our results. Figure 10 shows the percentage differences between actual and synthetic Illinois in models in which we perturb the outcome years used as predictors backward by one and two years. We also use a completely novel approach to reduce the risk that the donor states were matched because of idiosyncratic shocks and eliminate researcher judgement. We first eliminate idiosyncratic shocks by smoothing the pre-treatment outcome series for each state using a five-year moving average (two years forward and back, including the current year). We then create a synthetic control using evenly spaced lags from the smoothed outcome histories for Illinois and each state as predictors.

Overall, the percent difference between the three synthetic Illinois variations discussed above and the synthetic Illinois with our chosen lags are similar. One exception arises when we

[19] Supplemental tables and figures are available here: https://github.com/johniselin/Liquor-Taxation-and-Alcohol-Related-Driving-Fatalities

offset the lags by two years, which eliminates the negative difference in most years and creates positive spikes at the recessions in 2001.

Because of changes in state regulations during the 1980s, outcomes from those years may not be representative of current outcomes. Consequently, we chose 1985, 1990, and 1995 as alternative start dates for our pre-treatment periods (although this runs the risk of inducing small sample bias). The results for the synthetic Illinois constructed using a 1985–98 pre-treatment period (shown in figure 11) closely resemble our original mode, but using shorter periods noticeably reduces the fit of the synthetic control in the pre-treatment period. The period between 2000 and 2003 in which a synthetic control formed using just the 1994–98 period lies above the actual Illinois likely reflects the poor fit of the synthetic control. Overall, the evidence does not strongly indicate that the increased excise tax effectively reduced traffic fatalities.

Results up to this point are based on a donor pool of 20 states that implemented no major tax increases and that are not monopoly states. Our exclusion of the latter group from the donor pool might limit the SCM's ability to create synthetic Illinois. To test the effect of that exclusion, we rerun our analysis on this expanded donor pool (figure 12).

With the larger set of donor states, the post-treatment RMSPE counterintuitively increases to 0.0161. This occurs because the fit is determined using predictors that include the share of young drivers, the share of old drivers, and the fatality rate for cirrhosis of the liver, but the RMSPE is measured using only the share of FARMVCs during the pre-treatment period. The plot of the placebo test shows that results are qualitatively similar (figure 13).

Our final sensitivity test for the effect of the 1999 tax increase on the FARMVC share is to sequentially drop each chosen donor state from the donor pool (figure 14). For most states, this produced no effect. Dropping Indiana, however, appears to change the difference between the actual and synthetic FARMVC share. Omitting Indiana leads to a positive difference for 2000 through 2003, meaning that there appears to be a small apparent decrease in the FARMVC share. This result is likely spurious, however, because that effect disappears by 2004. We conclude that our results are not sensitive to the donor pool or the states chosen.

To reduce the possibility of conflating the effect of economic changes or changes in motor vehicle safety with the effects of excise tax changes on FARMVCs, we estimate a model using FARMVC per driver as an outcome. For this outcome variable, we include several additional macroeconomic variables as predictors. Following Ruhm (1996), we include real personal income per capita and state annual unemployment rates. We also include the real price of gasoline, which should affect miles driven.

The 1999 tax increase does not appear to lower FARMVC per million drivers (figure 15). To confirm this result, we run the placebo test (figure 16). The difference between the actual and synthetic Illinois is well within the bounds of the placebo states. However, when the pre-treatment period is shortened to 1990–98 or 1995–98 (figure 17), the tax increase appears to reduce FARMVC per million drivers. Because a similar trend is present going backwards from 1994, it may be caused by the very small number of years used to fit the synthetic control.

A placebo test using the 1990–98 pre-treatment period (figure 18) verifies that the results in figure 16 are caused by the synthetic control poorly fitting the treated state. Further, both the post-treatment RMSPE (0.071) and the ratio of pre- to post-treatment RMPSE (1.364) are near the average values across the potential donor states. We also apply this model to the expanded donor pool of states (appendix figure 31) and find similar results to those found in figure 8, where in the post-treatment period the synthetic Illinois has slightly fewer FARMVCs per million drivers than the actual Illinois.

2009 ILLINOIS TAX CHANGE

We next examine the Illinois increase its excise tax in 2009. Because the 1999 tax increase appears to have had little effect on either outcome, we add the years between 1999 and 2008 to the pre-treatment period for the 2009 increase. The years 2010 through 2015 then become the treatment period. For predictors, we now use outcomes for the years 1983, 1985, 1991, 1997, and 2008.

The resultant synthetic Illinois fits the actual Illinois in the pre-treatment period, with an RMSPE of 0.0146.[20] The outcome of the synthetic Illinois tracks the actual Illinois' outcome in the pre-treatment period, though not as well as it tracks the pre-treatment period of the 1999 increase. In the post-treatment period, the actual Illinois has a small temporary decrease in the FARMVC share of total fatal crashes in 2010 and 2011 that is not mirrored by the synthetic Illinois (figure 19). The effect, however, is small and temporary, and the actual and synthetic Illinois converge in 2012. This result is slightly different than the 1999 results.

The SCM chose a different group of states to construct our synthetic Illinois in this case, with almost 50 percent of the weight falling on Missouri, which was not included in the 1999 synthetic Illinois, and almost 21 percent of the synthetic state coming from Maryland, which contributed 1.8 percent in the 1999 model (appendix table 3).

The placebo test (figure 20) confirms that the difference between the actual and synthetic Illinois did not diverge dramatically from the differences of the placebo states in the post-treatment period. Both the post-treatment RMPSE and the ratio of the pre-and post-RMPSE sit in the middle of the distributions of those produced by the placebo states.

[20] Our synthetic state constructed using all of the pre-treatment years of FARMVC share has a pre-treatment RMSPE of 0.0084

We next run through the same series of sensitivity tests as before with no notable results. Expanding the donor pool did not alter the null result (appendix figure 32) nor did changing the selected lagged outcome variables (appendix figure 33) or adjusting the pre-treatment period (appendix figure 34). Leaving heavily weighted states out of the potential donor pool also did not alter the results.

Next, we examine the effect of the 2009 tax increase on the rate of FARMVCs per driver. We use the same model that we employed for the 1999 tax increase, but instead we use outcomes for the years 1983, 1985, 1991, 1997, and 2008 as predictors. This model creates a synthetic Illinois with a lower number of FARMVCs per million drivers than the actual Illinois during most of the pre-treatment period. Although we still have confidence in this model, this synthetic Illinois fits poorly relative to the previous synthetic states created in our work. With that in mind, the there is no noticeable effect of the tax increase in the post-treatment period (figure 21), and that result is confirmed by the placebo tests (figure 22).

These results are confirmed by our sensitivity tests. As with the 1999 tax increase, removing the 1980s from the pre-treatment period appears to produce a substantial difference between the actual Illinois and the synthetic states created using 1990–2008 and 1995–2008 that was not present using the longer time frames (figure 23). But as before, the effect is mirrored in the early years, suggesting that the result is spurious.

DISCUSSION

To our knowledge, our study is the first to use the SCM to examine the effect of alcohol excise taxes on an alcohol-related harm such as FARMVCs. We find no compelling evidence that either the 1999 or 2009 tax increase led a long-term decline in FARMVCs. Although a short-term decline in the FARMVC share occurs following the 2009 increase, it appears to be within the variation found in placebo states. We conduct a variety of sensitivity analyses, and overall our results are robust to the tests. One exception is divergence between synthetic and treated outcomes that occur when the pre-treatment set is shortened to omit the 1980's. However, given the caution by Abadie, Diamond, and Hainmueller (2010, 2015) to avoid short pre-treatment time periods and given the similar divergence that occurs before the shortened pre-treatment period, we believe that result is spurious.

Our results are consistent with the literature review conducted by Nelson and McNall (2016), who argue that the responses to alcohol regulations are more nuanced than earlier studies suggest. They are also broadly consistent with the results of Dee (1999); Mast, Benson, and Rasmussen (1999); and Young and Likens (2000), who find that prices and excise taxes have little apparent effect after accounting for omitted variable bias. In addition, our results align with those of Cook and Durrance (2013), who find no evidence of an effect of a federal tax increase on drunk driving fatalities in their preferred models. Because the magnitude of the 1991 federal excise tax increase is similar to the 1999 Illinois tax increase, the result is not surprising. However, the 2009 spirit excise tax increase was larger than either of those two tax changes and Wagenaar, Livingston, and Staras (2015) find a strong, statistically significant decline in FARMVCs from the Illinois 2009 tax increase. The nominal rigidities described by Conlon and Rao (2016) could prevent small tax changes from passing through to consumers. Further, Chetty, Looney, and Kroft (2009) show that consumers tend to underreact to alcohol taxes that are not salient. Illinois' substantial increase in the excise tax for spirits may have overwhelmed those effects.

However, our results for 2009 find no effect that approaches the 26 percent decline in FARMVC's reported by Wagenaar, Livingston, and Staras (2015). Our power test demonstrates the ability of the SCM to detect effects of the magnitude they estimate, so the difference is almost certainly not caused by any weakness of our methodology. The outcome variables in the two studies are slightly different: monthly FARMVCs defined by a positive BAC in their study versus FARMVCS as a share of total fatal motor vehicle crashes and FARMVCs per driver defined by a BAC greater than 0.08 in our study. The difference in BAC thresholds is unlikely to be the source of the difference, because they find similar results using a BAC threshold of 0.15. Over the short run, the number of drivers is relatively fixed, so their definition should be a multiple of our definition, eliminating that difference as the source of the varied results.

Wagenaar, Livingston, and Staras (2015) also only look at a short period following the tax increase, so they do not measure the longer-term effect of the tax change. For example, our model that examined FARMVCs per million drivers found a small short-term effect, but that was within the range of our placebo states, and that faded quickly.

The work of Beatty, Larsen, and Sommervoll (2009) suggest a potential reason we do not find a substantial effect of the Illinois tax increases. Their study—as well as similar research into sugared beverages, tobacco, and high-fat products—points to the role of cross-border tax avoidance in our results. Drivers closer to the border might be purchasing alcohol in states with lower excise taxes on beer, wine, and spirits (see table 1). We explore how this may have affected our analysis by re-estimating the SCM on the interior counties of Illinois.[21] The results, shown in figure 23, show a sharp decline in the share of FARMVCs. In 2008, the interior counties of Illinois had a FARMVC share of 31 percent, but by 2010 this had fallen to 22 percent, a 9-percentage point change over two years. This sharp drop is not reflected in the synthetic interior Illinois. Notably, the reaction is short-lived and the outcomes of the synthetic and actual Illinois converge again; in 2012 the FARMVC share spiked above previous levels to 35 percent before reverting to the previous trend of around 28 percent for the remaining years of the sample.

The placebo test in figure 24 shows that the interior counties of Illinois saw the largest-magnitude difference between its actual and synthetic versions in 2010. States without a major tax change did not deviate as dramatically from their synthetic counterparts. Notwithstanding the results in Xu and Chaloupka (2011), the short-lived response of motorists living in the interior of Illinois partially reconciles our analysis with the powerful short-run effect found in Wagenaar, Livingston, and Staras (2015). On the one hand, our result shows that there was a substantial and temporary reaction to the 2009 alcohol tax hike. On the other, our result only holds for part of Illinois while Wagenaar, Livingston, and Staras (2015) found a similar change across the entire state.

This result should be treated cautiously. The decline in fatalities may be caused in part by the recession rather than the excise tax increase. In addition, the smaller increase in fatalities in 2012 suggests that the changes may simply be idiosyncratic noise rather than the effect of changing excise taxes. However, the placebo test suggests that the results are unlikely to be due solely to noise. That Illinois should be such an outlier in the placebo test only supports the recession argument if Illinois was effected more dramatically by the recession relative to other states. It is also worth noting that the 2012 spike in the interior-counties' FARMVC share was not an outlier in the placebo test, unlike our main finding, indicating that it is more likely to be noise. But the sharp decline in the interior of Illinois does not explain why Wagenaar, Livingston, and Staras (2015) find a larger decline over the entire state. Further exploration of these results is the goal of future research.

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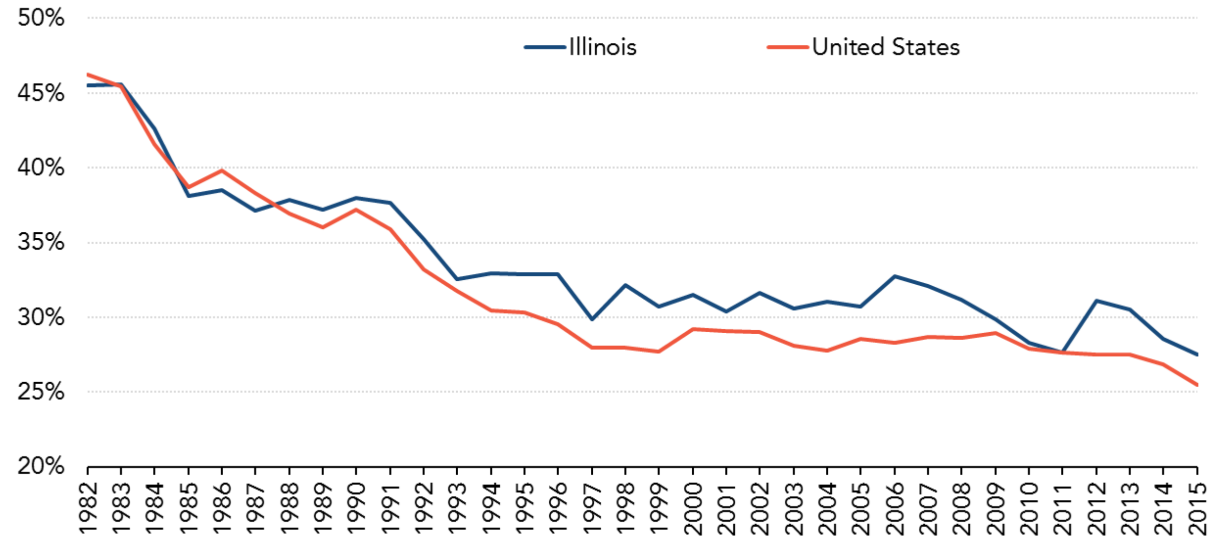
[21] Because we do not have county-level predictors such as gas prices we use the all-lags model in which the predictors are simply the outcomes in the pre-treatment period.

FIGURE 1

FARMVCs as a Share of Total Fatal Crashes

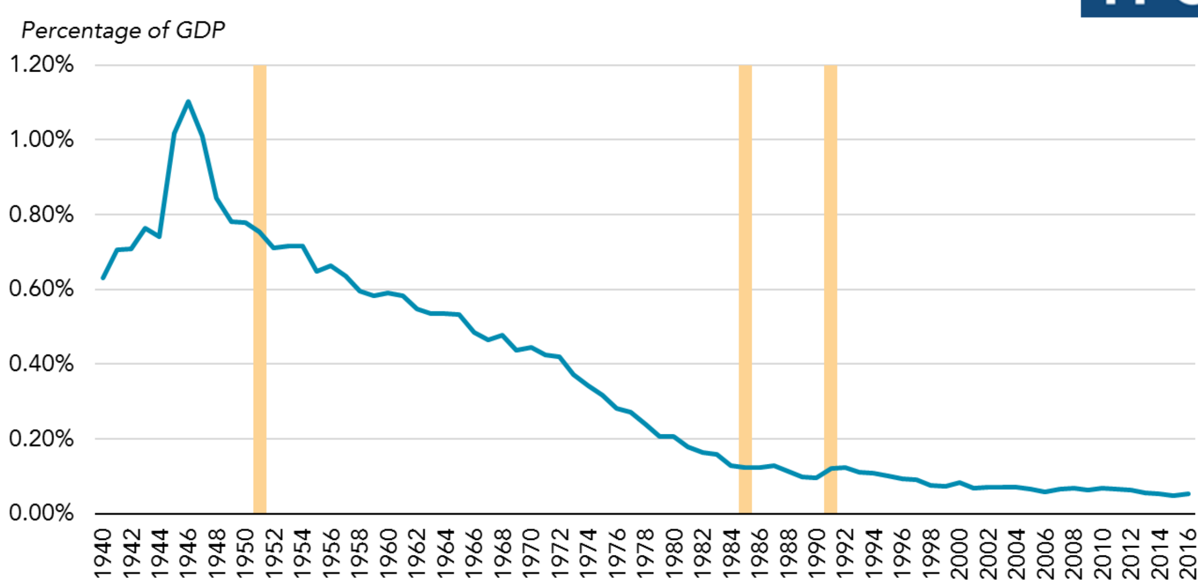
US and Illinois FARMVC share



Source: National Highway Traffic Safety Administration Fatality Analysis Reporting System data and author's calculations.
Note: FARMVC = fatal alcohol-related motor vehicle crash. "Alcohol-related" indicates that a driver involved in the crash had a blood-alcohol content at or above 0.08 percent.

FIGURE 2

Federal Alcohol Excise Tax Revenue

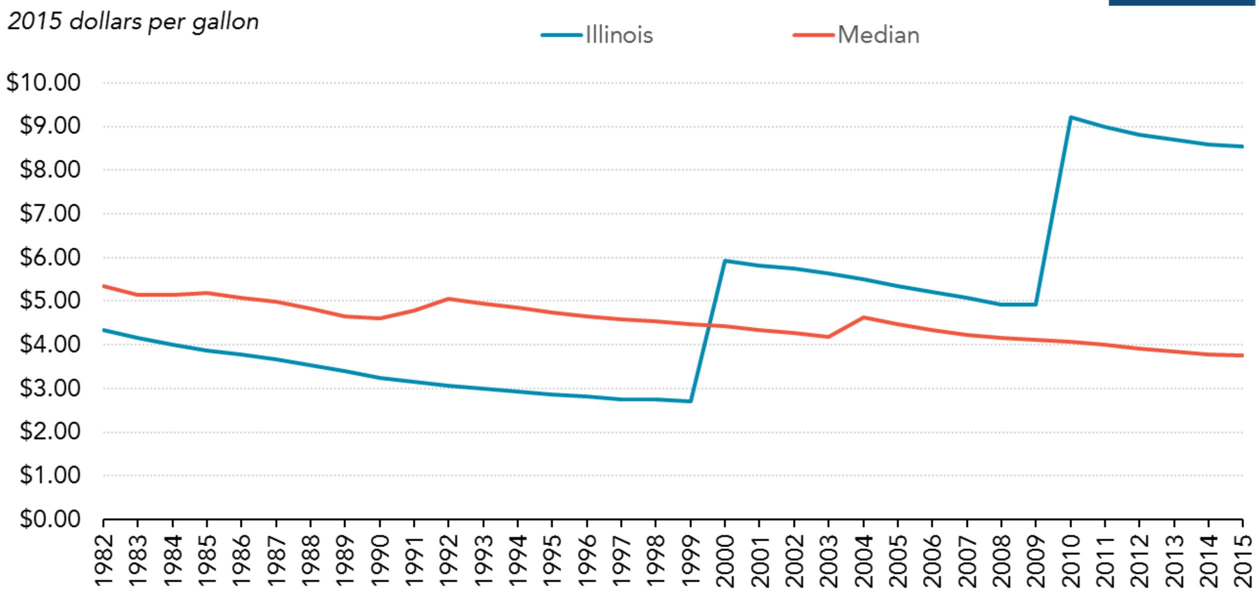


Source: Authors' calculations and the Office of Management and Budget, Historical Tables: Tables 2.3 and 2.4.
Notes: Orange markers indicate the three major federal alcohol excise tax changes.

FIGURE 3

State Spirit Tax Rates

Illinois and the median tax rate

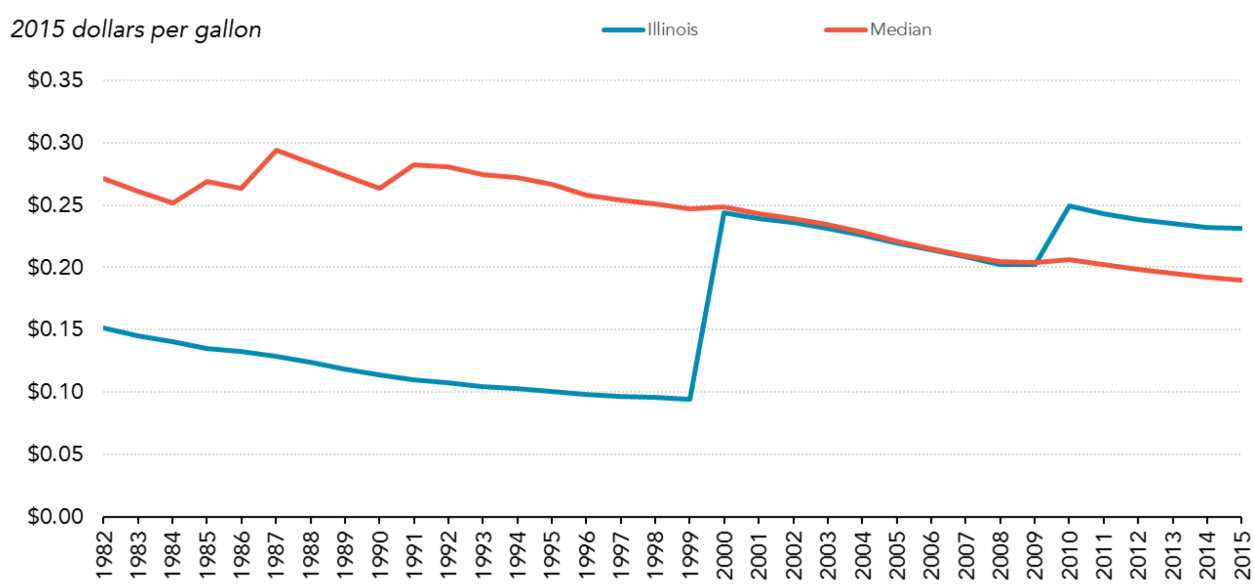


Source: Urban-Brookings Tax Policy Center data on state alcohol excise taxes and authors' calculations.
Note: Median tax rate of all noncontrol states.

FIGURE 4

State Beer Tax Rates

Illinois and the median tax rate

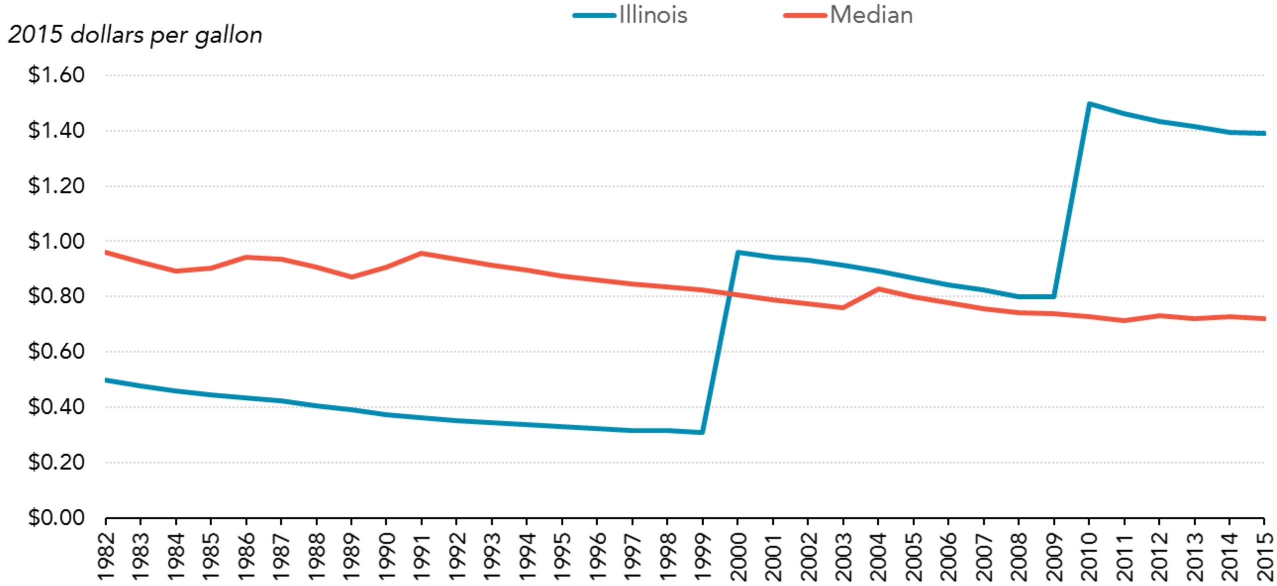


Source: Urban-Brookings Tax Policy Center data on state alcohol excise taxes and authors' calculations.

FIGURE 5

State Wine Tax Rates

Illinois and the median tax rate

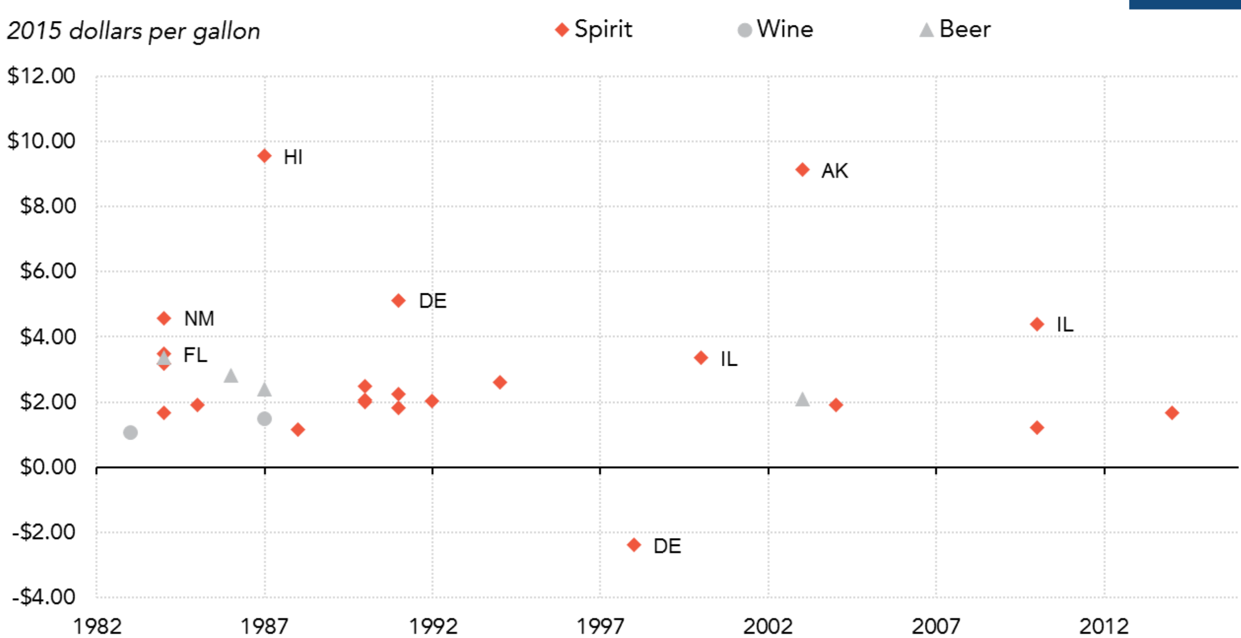


Source: Urban-Brookings Tax Policy Center data on state alcohol excise taxes and authors' calculations.
Note: Median tax rate of all noncontrol states.

FIGURE 6

State Alcohol Excise Tax Changes Greater Than $1

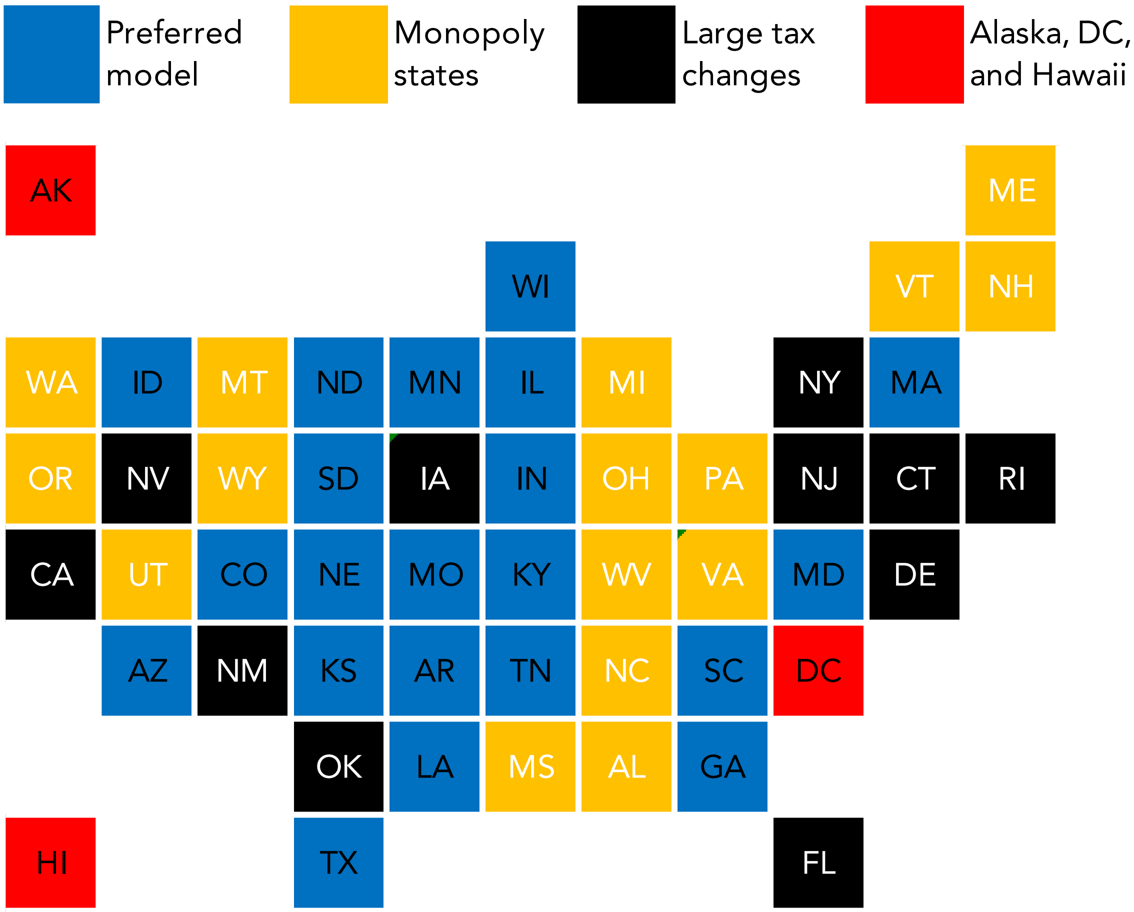
Spirit, wine, and beer taxes



Source: Urban-Brookings Tax Policy Center data on state alcohol excise taxes, the Bureau of Economic Analysis, and authors' calculations.

FIGURE 7

Classification of States in Donor Pool



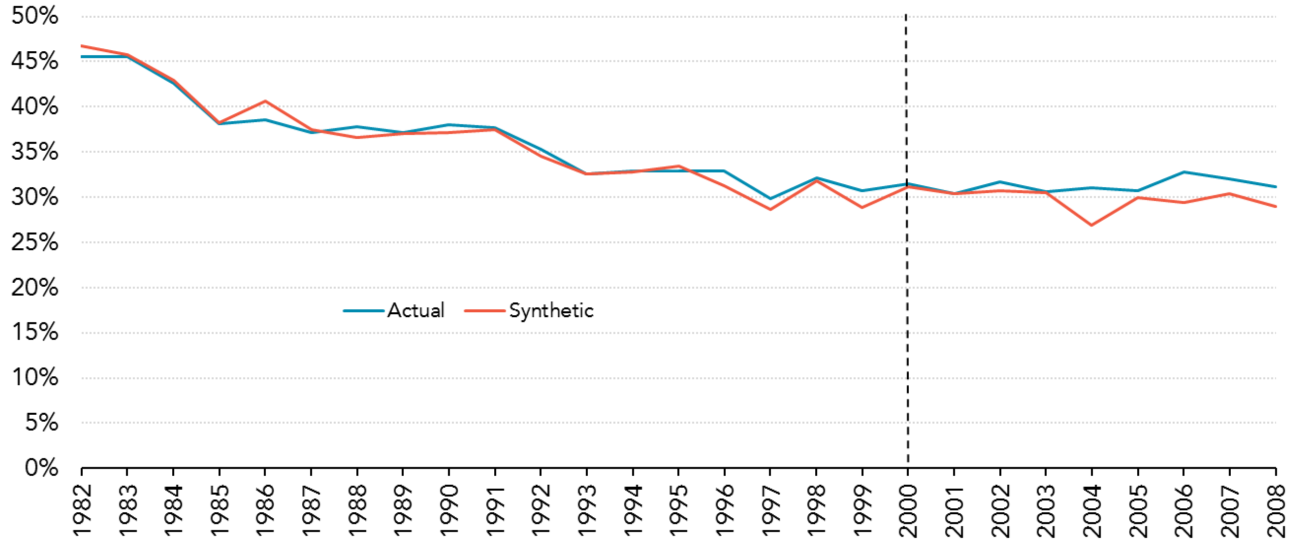
Source: Authors' calculations
Note: Narrow-model states are included in the donor pool in every version. Monopoly states have direct state sales of liquor and therefore have no direct liquor tax rates. States with large tax changes had an increase or decrease in alcohol excise taxes of at least one 2015 dollar per gallon.

FIGURE 8

Actual versus Synthetic Illinois FARMVC Share of Total Fatal Crashes

Model with chosen predictor and lagged variables, 1999 tax increase

Percentage of fatal motor vehicle crashes that were alcohol related



Source: Authors' calculations based on synthetic control methodology

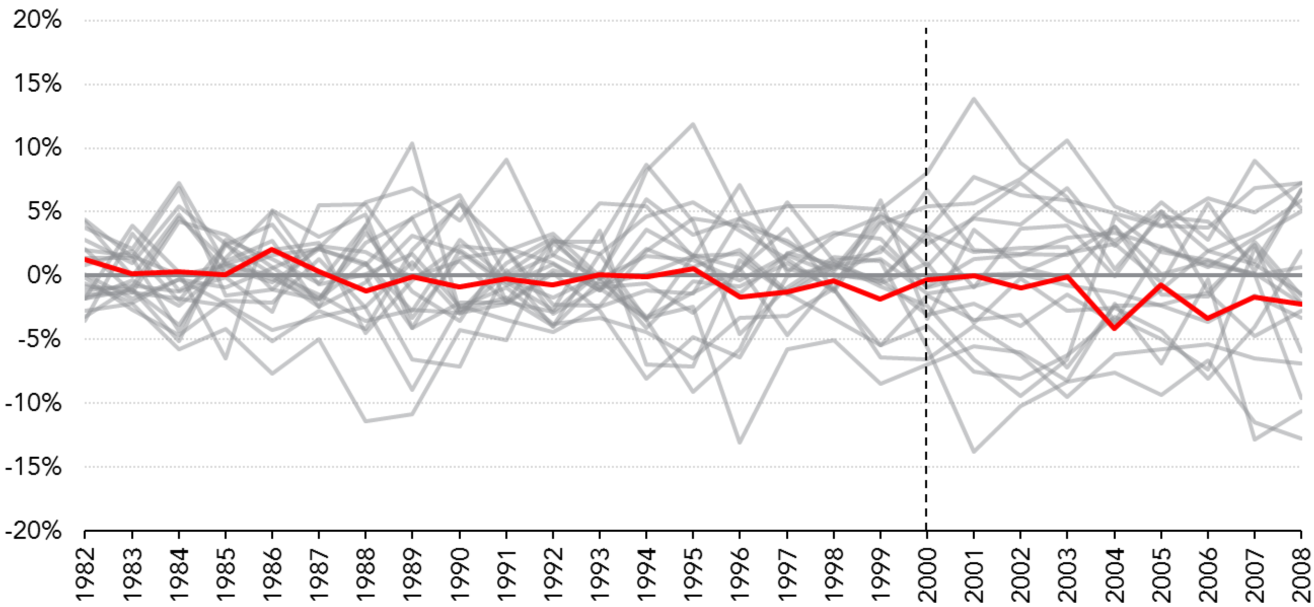
Notes: FARMVC = fatal alcohol-related motor vehicle crashes. We use 2000 as the treatment year. Predictor variables are selected lagged values of FARMVC shares as well as the averages from 1982 to 1998 of: the share of the population ages 15 to 24, the share of the population age 65 and over, and the number of deaths from alcoholic cirrhosis of the liver per 100,000 people. "Alcohol-related" indicates that a driver involved in the crash had a blood-alcohol content at or above 0.08 percent.

FIGURE 9

Placebo Synthetic Control Test for 1999 Tax Increase

Results for each potential donor state run through our chosen model

Actual state share minus synthetic state share (%)



Source: Authors' calculations based on synthetic control methodology.

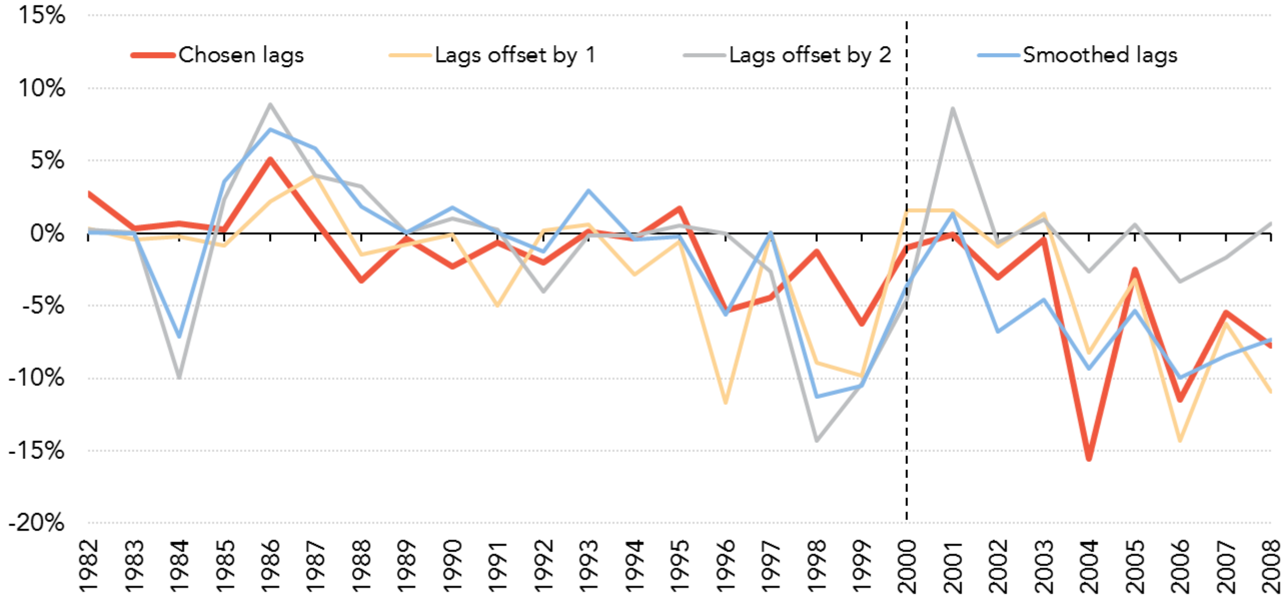
Note: The placebo test is designed to compare the difference between our outcome variable in the real and synthetic Illinois to that same difference in our 20 potential donor states. The red line is the Illinois difference.

FIGURE 10

Alternative Lagged Predictor Test

FARMVC share model, 1999 tax increase

Percentage difference between actual IL and synthetic models



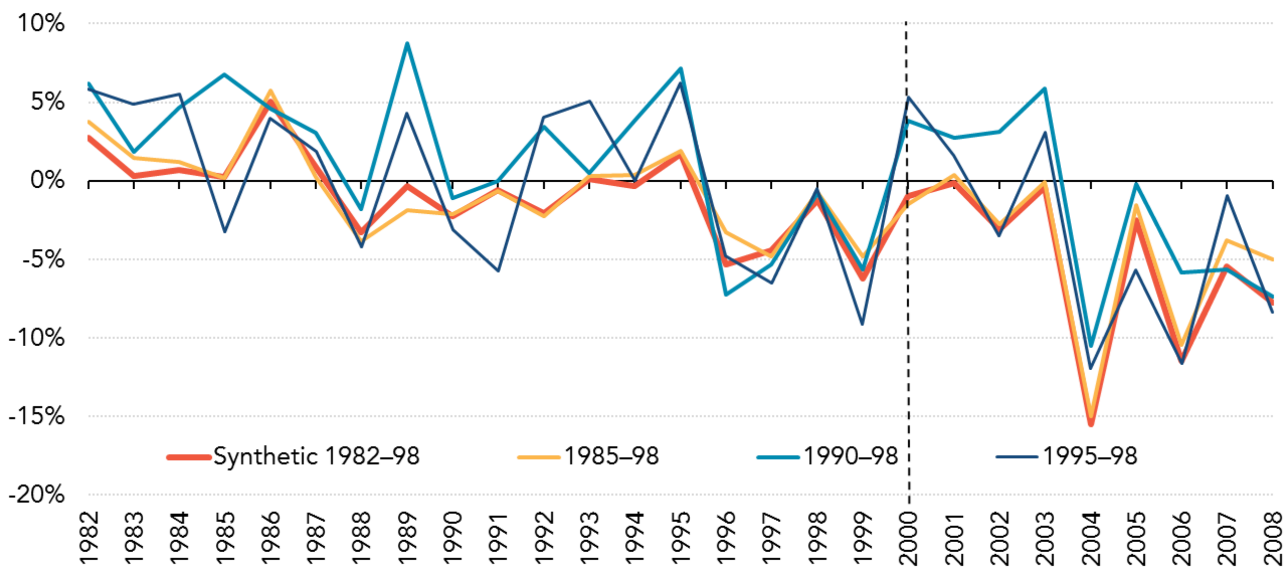
Source: Authors' calculations based on synthetic control methodology.
Note: FARMVC = fatal alcohol-related motor vehicle crash. The smoothed lag model uses evenly spaced lags from a smoothed version of the dependent variable. "Alcohol-related" indicates that a driver involved in the crash had a blood-alcohol content at or above 0.08 percent.

FIGURE 11

Alternative Pretreatment Test

FARMVC share model, 1999 tax increase

Percentage difference between actual IL and synthetic models



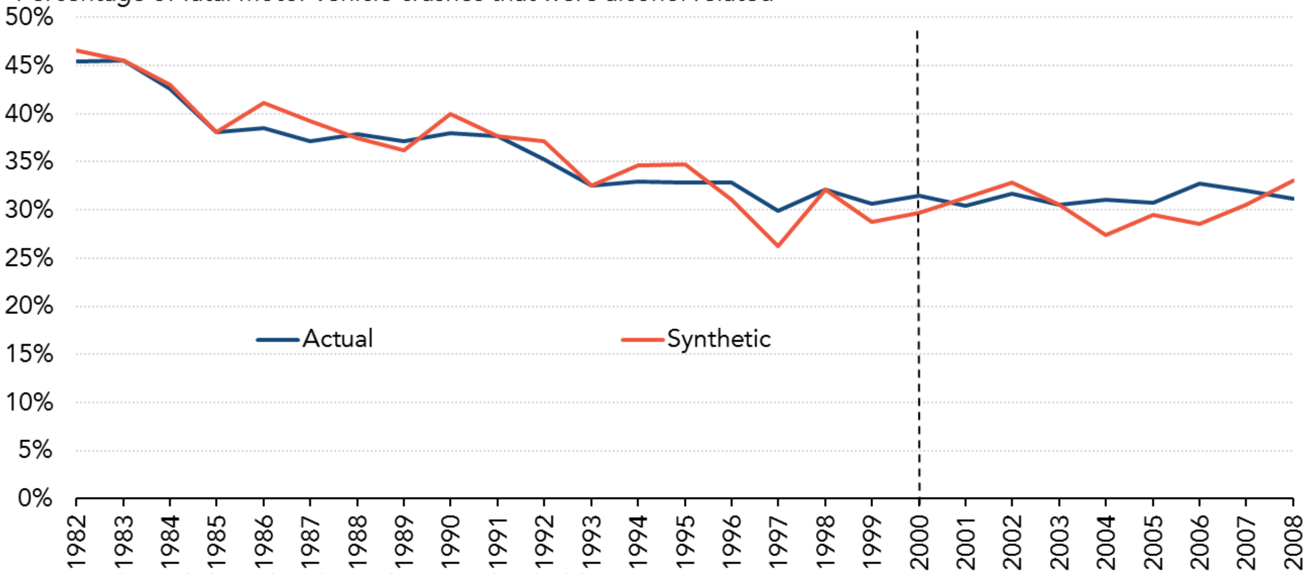
Source: Authors' calculations based on synthetic control methodology.
Note: FARMVC = fatal alcohol-related motor vehicle crash. "Alcohol-related" indicates that a driver involved in the crash had a blood-alcohol content at or above 0.08 percent.

FIGURE 12

Actual versus Synthetic Illinois FARMVC Share of Total Fatal

Model with chosen predictor and lagged variables, expanded donor pool, 1999 tax

Percentage of fatal motor vehicle crashes that were alcohol related



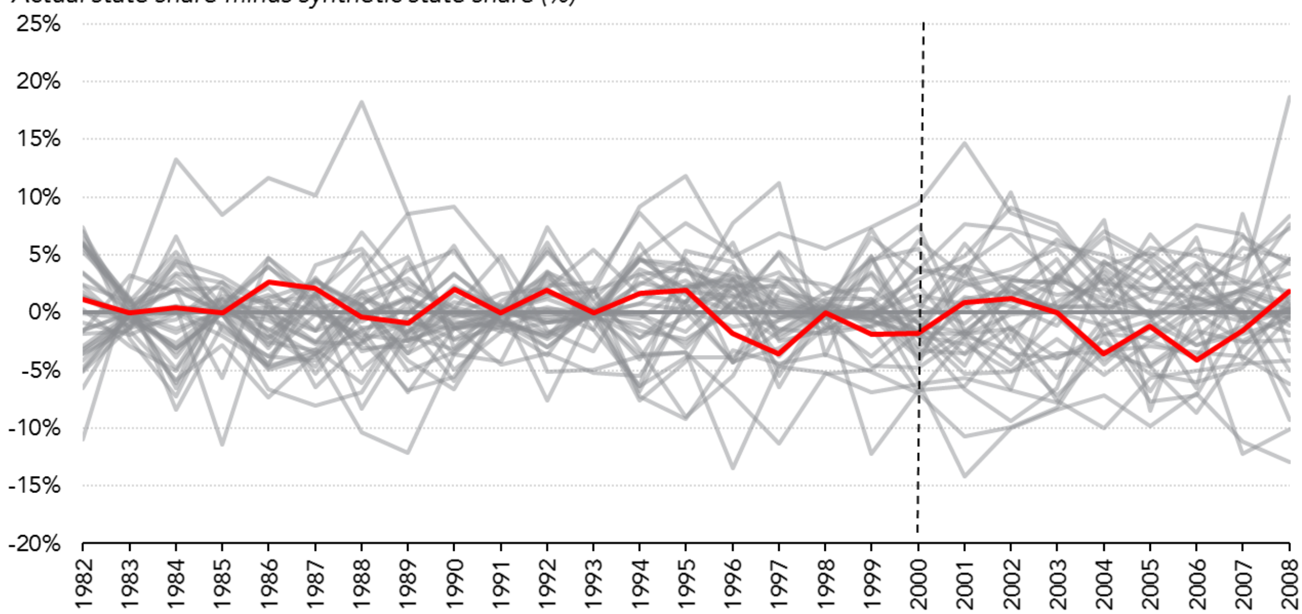
Source: Authors' calculations based on synthetic control methodology.
Notes: FARMVC = fatal alcohol-related motor vehicle crash. We use 2000 as the treatment year. Predictor variables are selected lagged values of FARMVC shares as well as the averages from 1982 to 1998 of: the share of the population ages 15 to 24, the share of the population age 65 and over, and the number of deaths from alcoholic cirrhosis of the liver per 100,000 people. "Alcohol-related" indicates that a driver involved in the crash had a blood-alcohol content at or above 0.08 percent.

FIGURE 13

Placebo Synthetic Control Test for 1999 Tax Increase

Results for each potential donor state in our expanded donor pool

Actual state share minus synthetic state share (%)



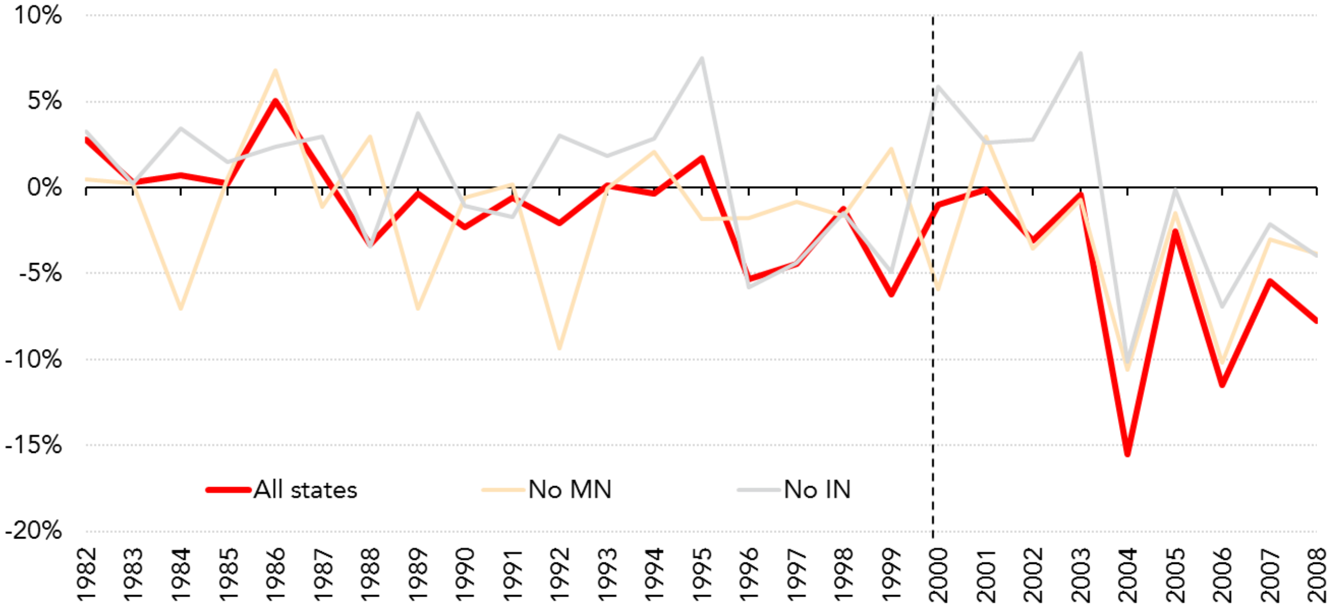
Source: Authors' calculations based on synthetic control methodology.
Note: The placebo test is designed to compare the difference between our outcome variable in the real and synthetic Illinois to that same difference in our 20 potential donor states and monopoly states. The red line is the Illinois difference.

FIGURE 14

Leave-One-Out Test for 1999 Tax

FARMVC share model, 1999 tax increase, model separately omitting MN and

Percentage difference between actual and synthetic Illinois



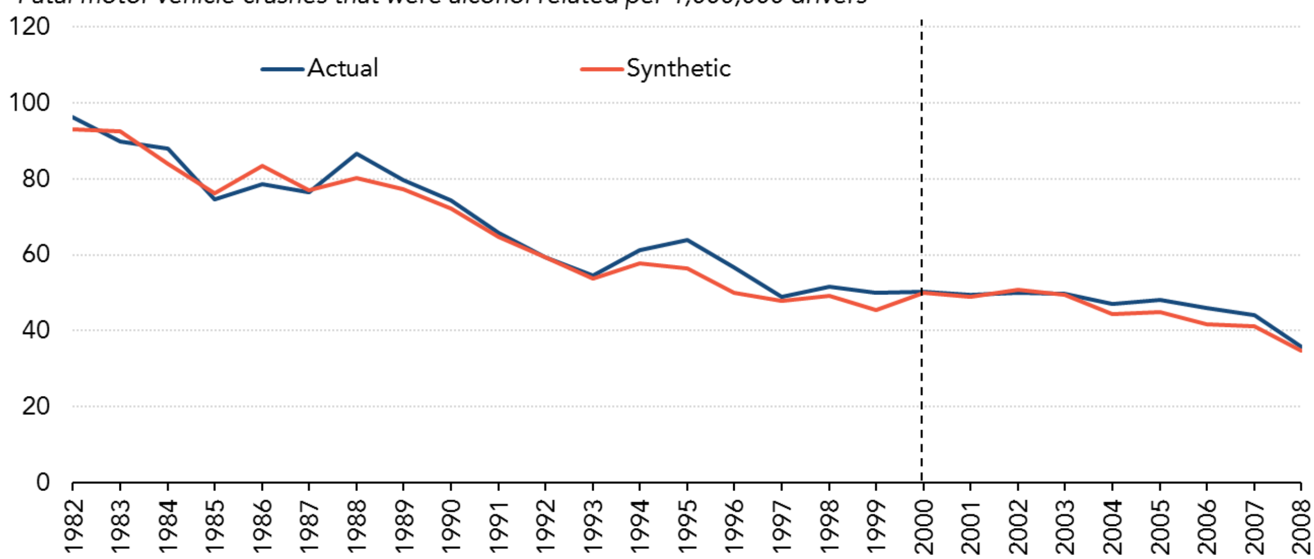
Source: Authors' calculations based on synthetic control methodology.
Note: FARMVC = fatal alcohol-related motor vehicle crash. "Alcohol-related" indicates that a driver involved in the crash had a blood-alcohol content at or above 0.08 percent.

FIGURE 15

Actual versus Synthetic Illinois FARMVCs per Million Drivers

Model with chosen predictor and lagged variables, 1999 tax increase

Fatal motor vehicle crashes that were alcohol related per 1,000,000 drivers



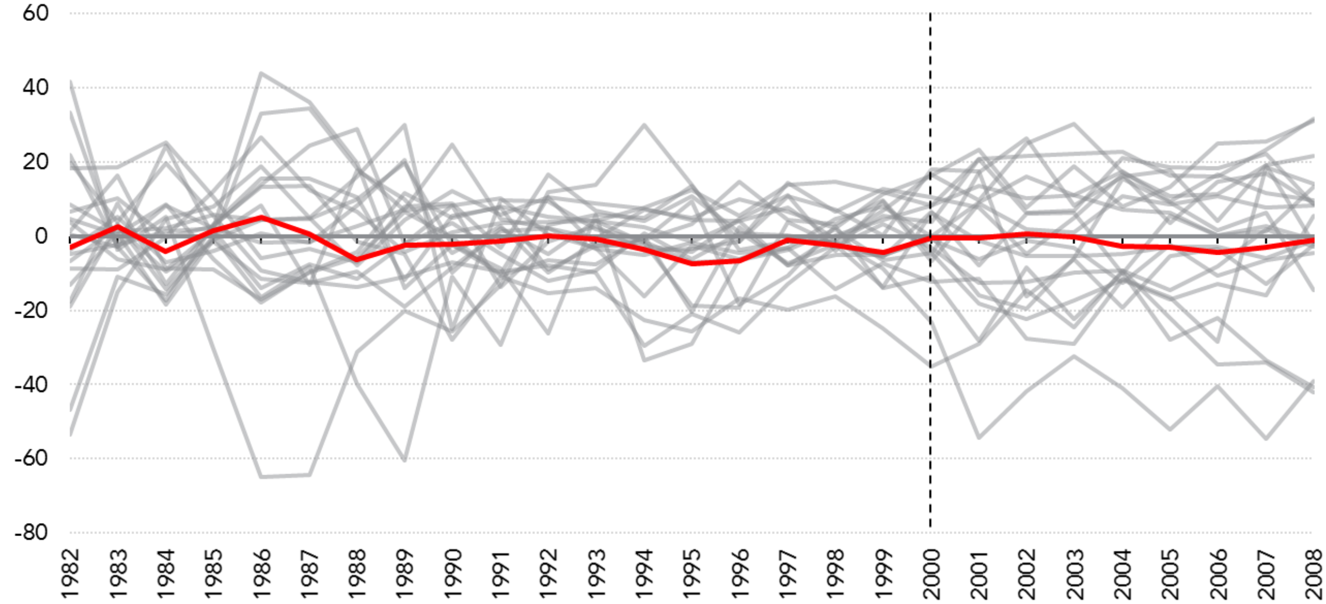
Source: Authors' calculations based on synthetic control methodology.
Notes: FARMVC = fatal alcohol-related motor vehicle crash. We use 2000 as the treatment year. Predictor variables are selected lagged values of FARMVC per driver as well as the averages from 1982 to 1998 of: the share of the population ages 15 to 24, the share of the population age 65 and over, real per capita personal income, real gas tax rates, unemployment rates, and the number of deaths from alcoholic cirrhosis of the liver per 100,000 people. "Alcohol-related" indicates that a driver involved in the crash had a blood-alcohol content at or above 0.08 percent.

FIGURE 16

Placebo Synthetic Control Test for 1999 Tax Increase

Results for each potential donor state run through our chosen model

Actual minus synthetic fatal motor vehicle crashes that were alcohol related per 1,000,000 drivers



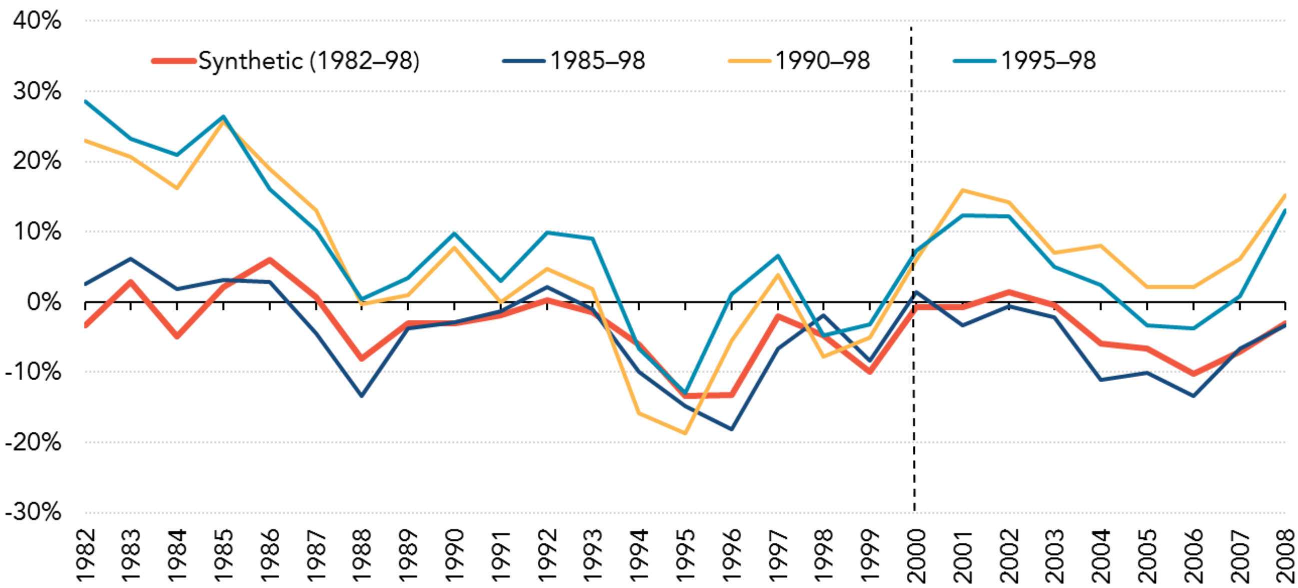
Source: Authors' calculations based on synthetic control methodology.
Note: The placebo test is designed to compare the difference between our outcome variable in the real and synthetic Illinois to that same difference in our 20 potential donor states. The red line is the Illinois difference.

FIGURE 17

Alternative Pre-Treatment Test

FARMVC per driver model, 1999 tax increase

Fatal motor vehicle crashes that were alcohol related per 1,000,000 drivers



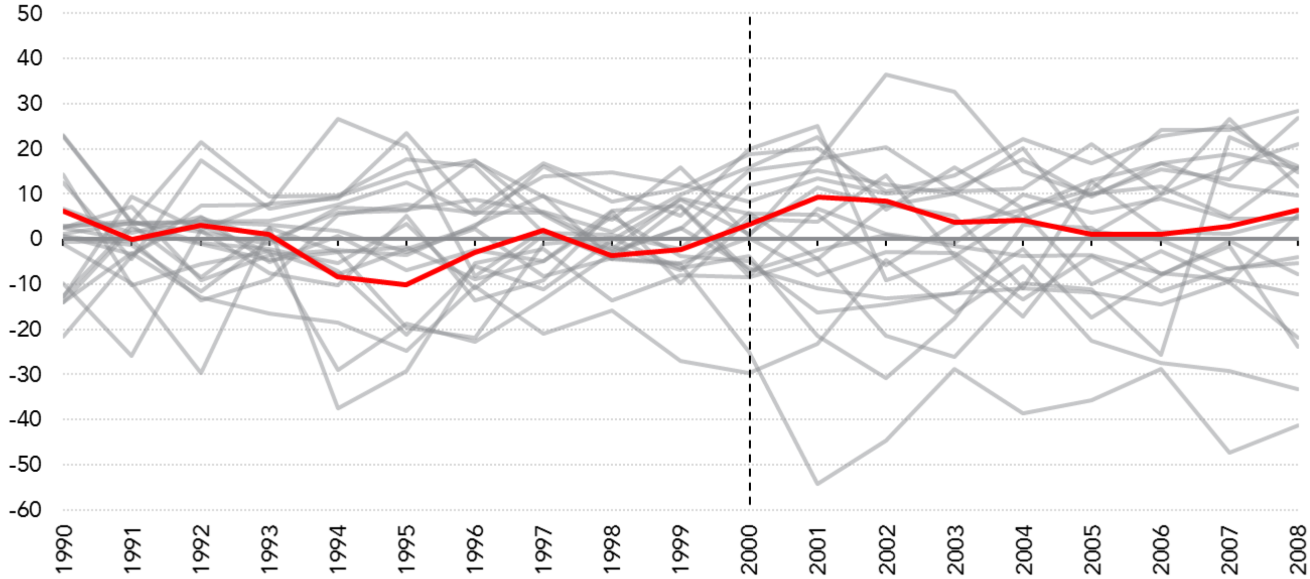
Source: Authors' calculations based on synthetic control methodology.
Note: FARMVC = fatal alcohol-related motor vehicle crash. "Alcohol-related" indicates that a driver involved in the crash had a blood-alcohol content at or above 0.08 percent.

FIGURE 18

Placebo Synthetic Control Test for 1990–98 Pretreatment Period

Results for each potential donor state run through our chosen model

Actual minus synthetic fatal motor vehicle crashes that were alcohol related per 1,000,000 drivers



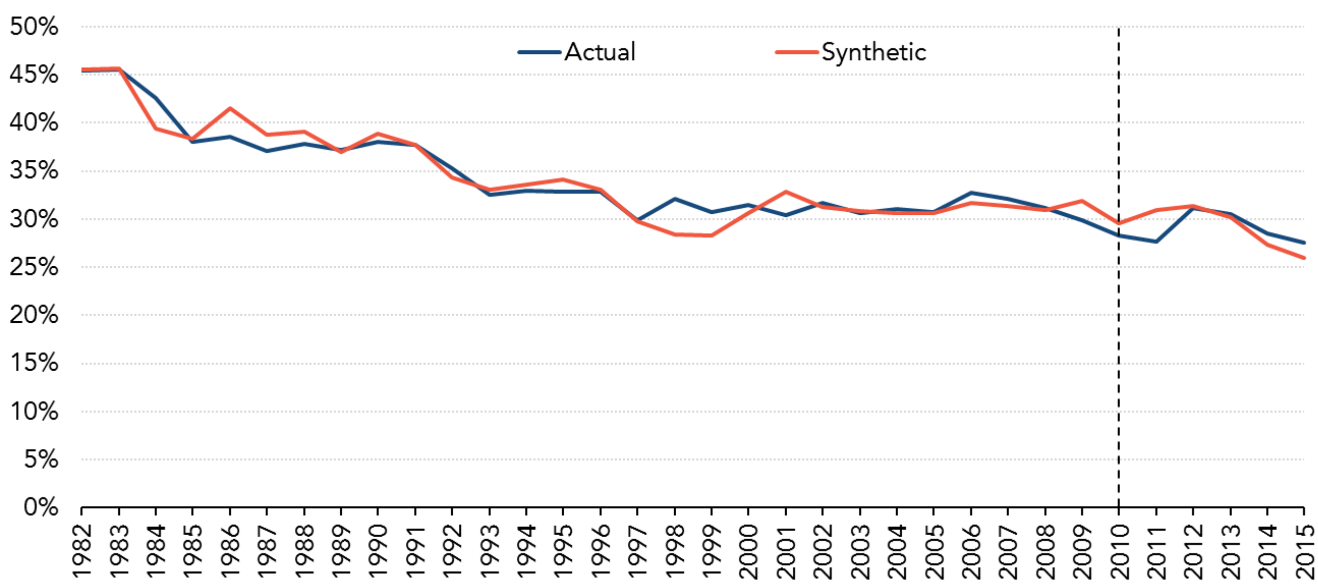
Source: Authors' calculations based on synthetic control methodology
Note: The placebo test is designed to compare the difference between our outcome variable in the real and synthetic Illinois to that same difference in our 20 potential donor states. The red line is the Illinois difference.

FIGURE 19

Actual vs. Synthetic Illinois FARMVC Share of Total Fatal Crashes

Model with chosen predictor and lagged variables, 2009 tax increase

Percentage of fatal motor vehicle crashes that were alcohol related



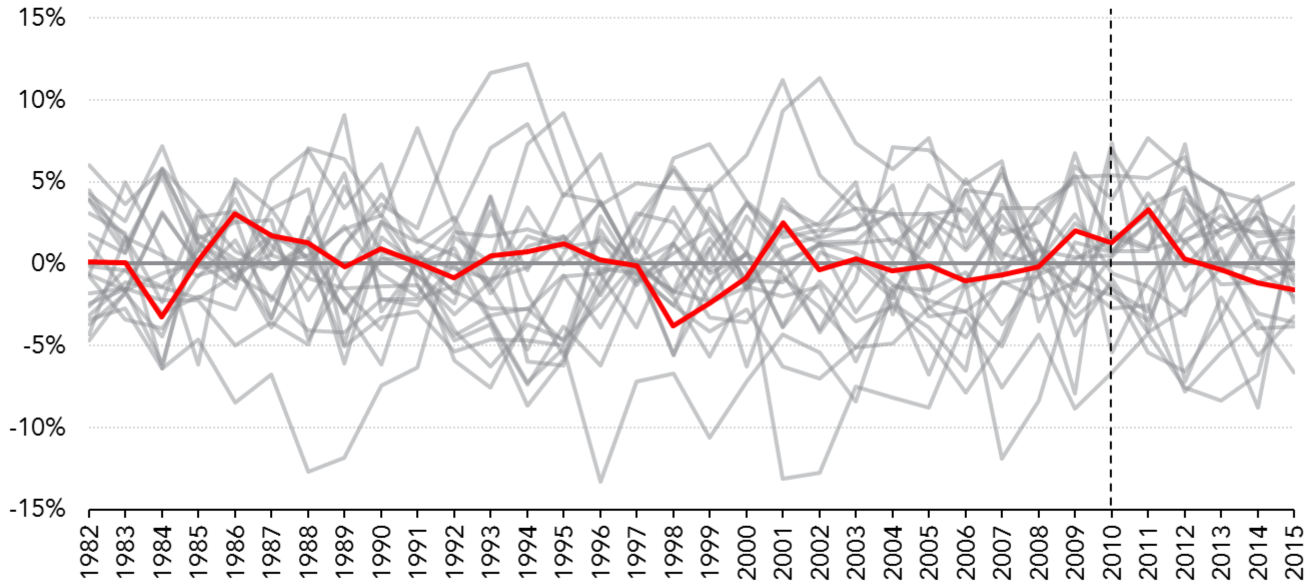
Source: Authors' calculations based on synthetic control methodology.
Notes: FARMVC = fatal alcohol-related motor vehicle crash. We use 2010 as the treatment year. Predictor variables are selected lagged values of FARMVC shares as well as the averages from 1982 to 2008 of: the share of the population ages 15 to 24, the share of the population age 65 and over, and the number of deaths from alcoholic cirrhosis of the liver per 100,000 people. Alcohol-related incidents involve someone with a blood

FIGURE 20

Placebo Synthetic Control Test for 2009

Results for each potential donor state run through our chosen model

Actual state share minus synthetic state share (%)



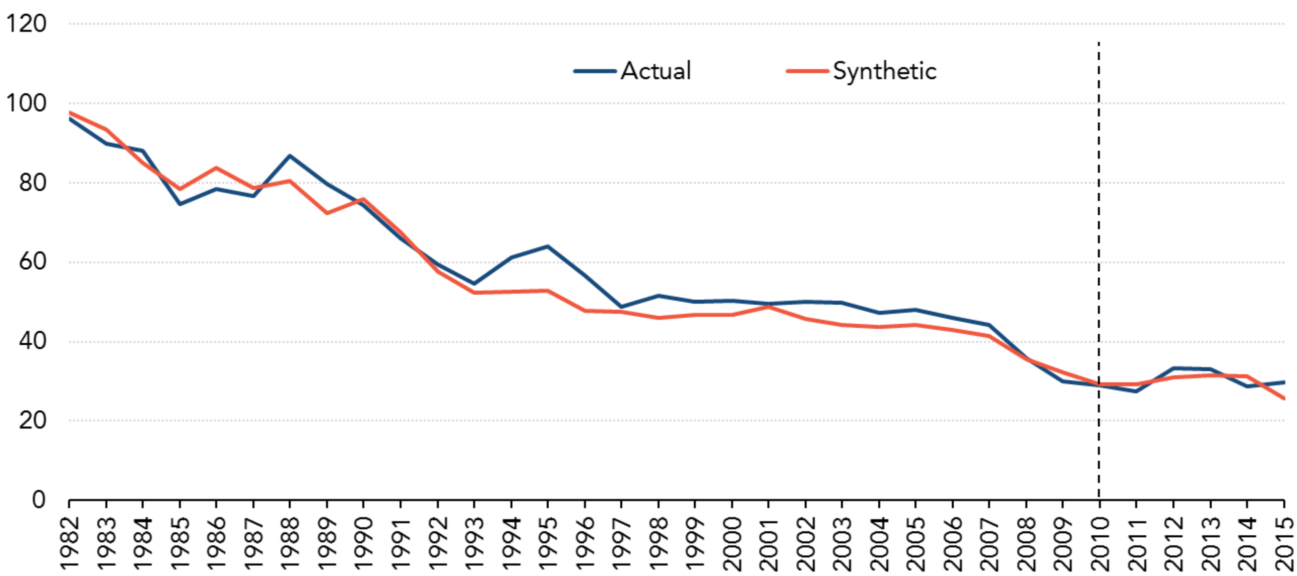
Source: Authors' calculations based on synthetic control methodology.
Note: The placebo test is designed to compare the difference between our outcome variable in the real and synthetic Illinois to that same difference in our 20 potential donor states. The red line is the Illinois difference.

FIGURE 21

Actual vs. Synthetic Illinois FARMVC per Million Drivers

Model with chosen predictor and lagged variables, 2009 tax increase

Fatal motor vehicle crashes with BAC values at or above 0.08 per 1,000,000



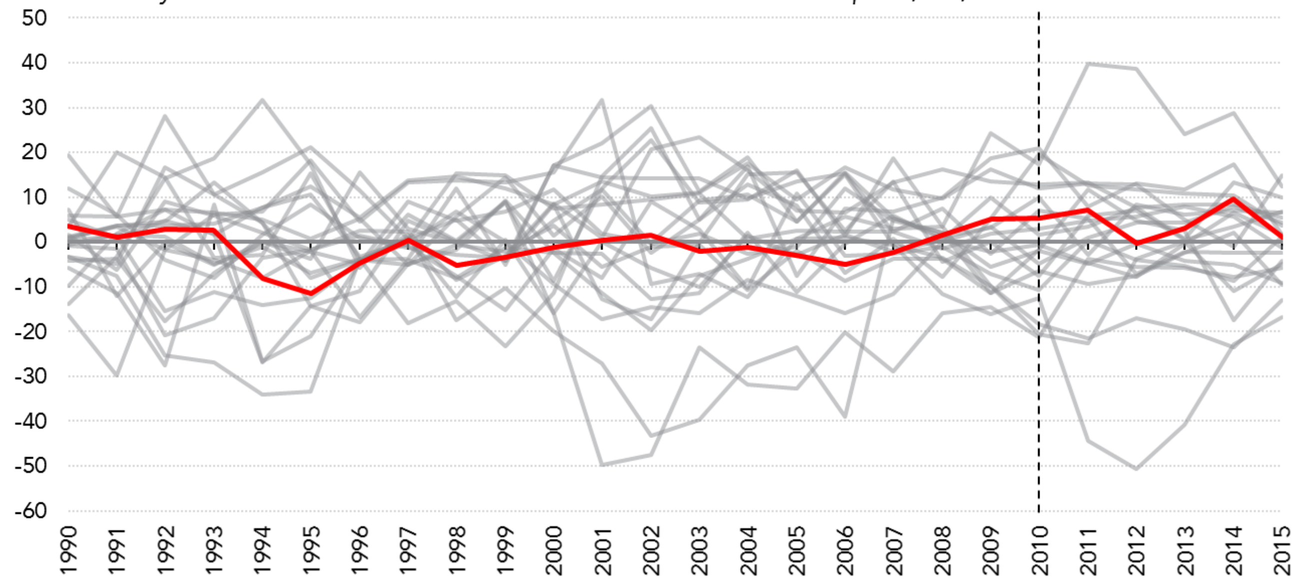
Source: Authors' calculations based on synthetic control methodology
Notes: We treat 2010 as the treatment year. Predictor variables are selected lagged values of FARMVC per driver as well as the averages from 1982-2008 of: the share of the population ages 15 to 24, the share of the population age 65 and over, real per capita personal income, real gas tax rates, unemployment rates, and deaths from alcoholic cirrhosis of the liver per 100,000 people. FARMVC = fatal alcohol-related motor vehicle crash. "Alcohol-related" indicates that a driver involved in the crash had a blood-alcohol content at or above 0.08 percent.

FIGURE 22

Placebo Synthetic Control Test for 1990-2008 Pretreatment Period

Results for each potential donor state run through our chosen model

Actual minus synthetic fatal motor vehicle crashes that were alcohol related per 1,000,000 drivers



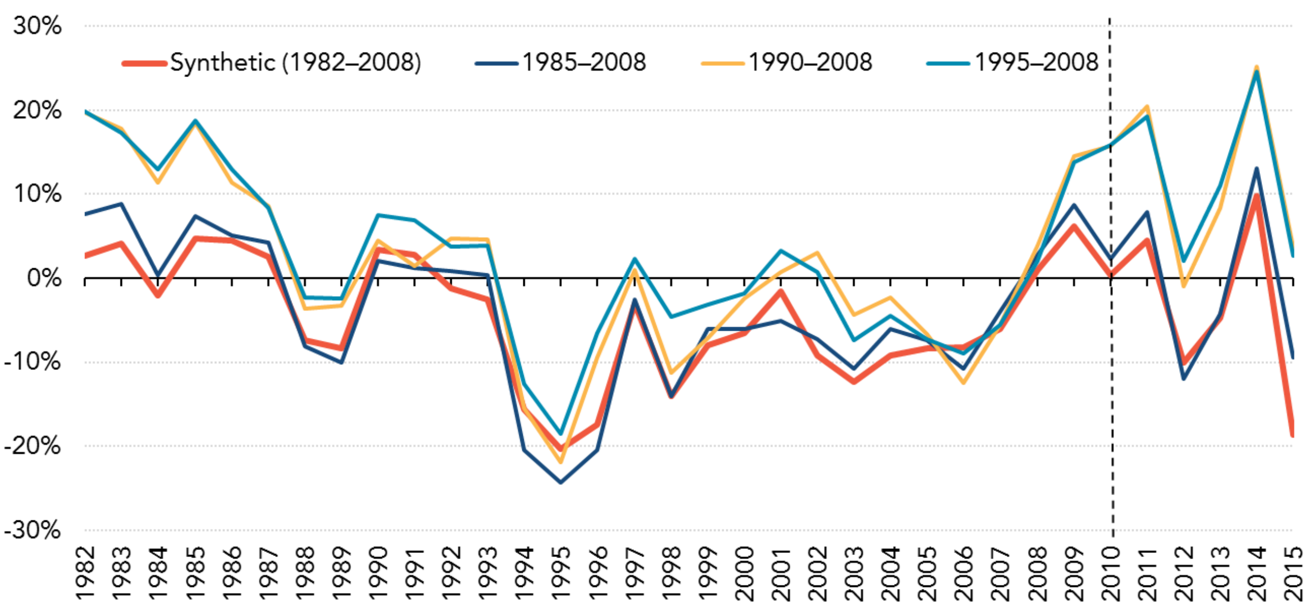
Source: Authors' calculations based on synthetic control methodology.
Note: The placebo test is designed to compare the difference between our outcome variable in the real and synthetic Illinois to that same difference in our 20 potential donor states. The red line is the Illinois difference.

FIGURE 23

Alternative Pretreatment Test

FARMVC per driver model, 2009 tax increase

Fatal motor vehicle crashes that were alcohol related per 1,000,000 drivers



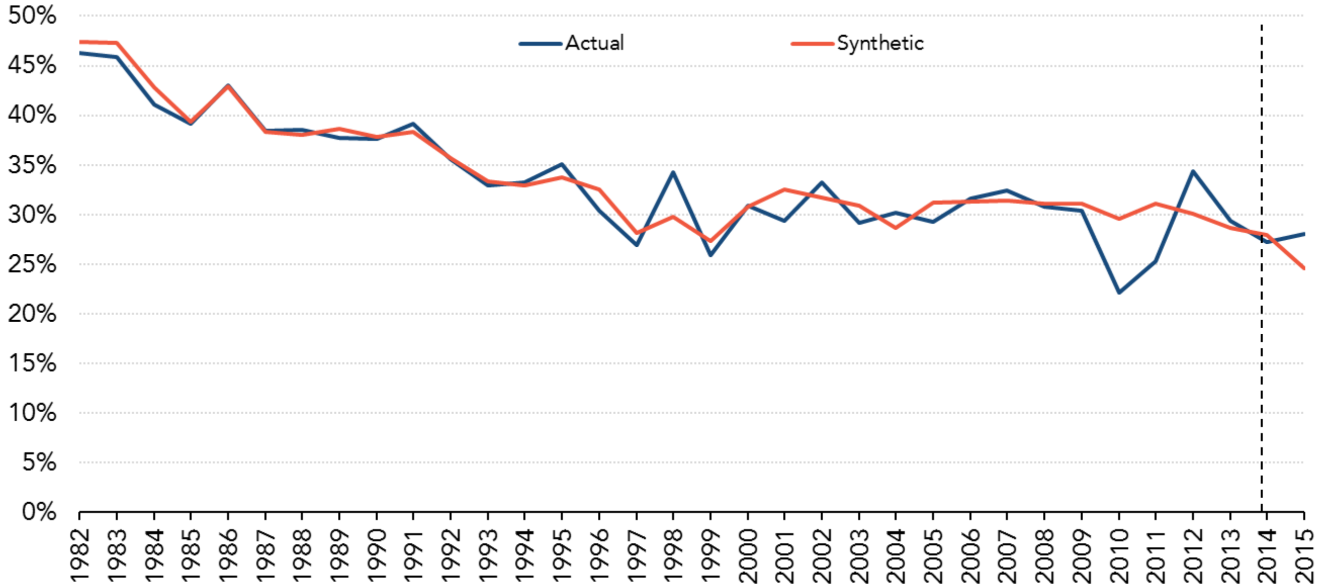
Source: Authors' calculations based on synthetic control methodology.
Note: FARMVC = fatal alcohol-related motor vehicle crash. "Alcohol-related" indicates that a driver involved in the crash had a blood-alcohol content at or above 0.08 percent.

FIGURE 24

Actual versus Synthetic Illinois FARMVC Share of Total Fatal

All lagged model with no border counties, 2009 tax increase

Percentage of fatal motor vehicle crashes that were alcohol related



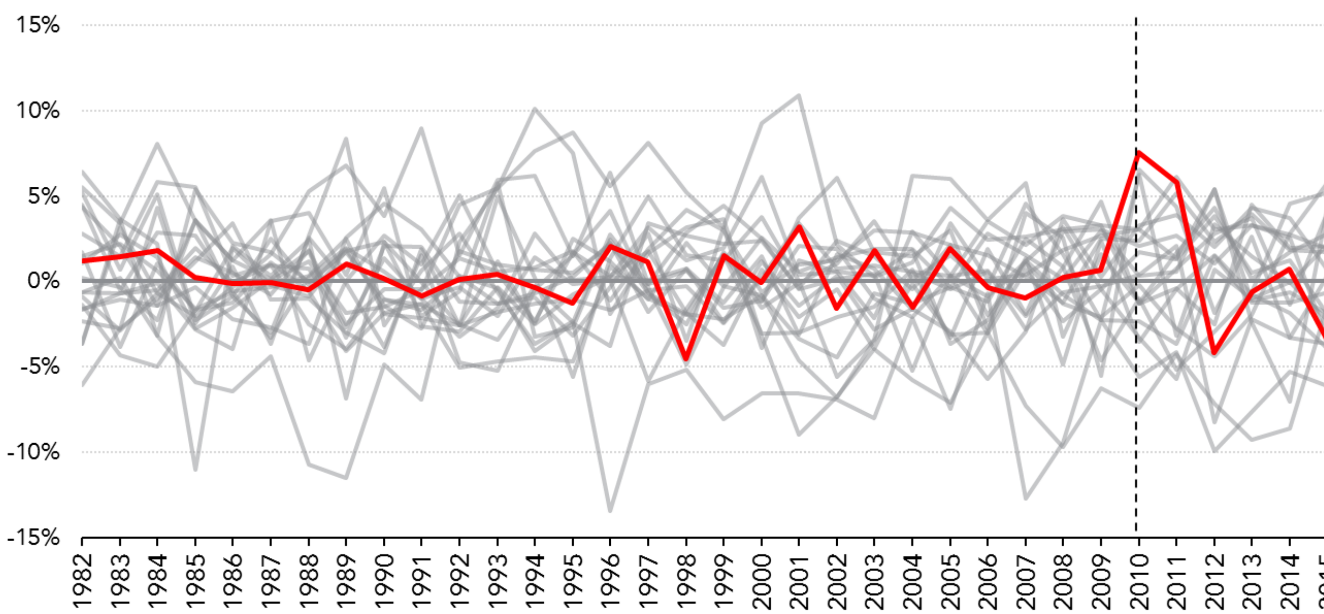
Source: Authors' calculations based on synthetic control methodology.
Notes: FARMVC = fatal alcohol-related motor vehicle crash. We use 2010 as the treatment year. Predictor variables are selected lagged values of FARMVC shares from 1982 to 2008. "Alcohol-related" indicates that a driver involved in the crash had a blood-alcohol content at or above 0.08 percent.

FIGURE 25

Placebo Synthetic Control Test For 2009

Results for each potential donor state, all lags model with no border counties

Actual state share minus synthetic state share (%)



Source: Authors' calculations based on synthetic control methodology.
Note: The placebo test is designed to compare the difference between our outcome variable in the real and synthetic Illinois to that same difference in our 20 potential donor states. The red line is the Illinois difference.

TABLE 1

Alcohol Tax Rates in Illinois and Surrounding States 2008 and 2010

2015 dollars per gallon

State	2008			2010		
	Beer	Wine	Spirits	Beer	Wine	Spirits
IL	0.20	0.80	4.50	0.25	1.50	8.55
IN	0.13	0.51	2.68	0.12	0.51	2.68
IA	0.21	1.92	N/A	0.20	1.89	N/A
KY	0.09	0.55	1.92	0.09	0.54	1.92
MI	0.22	0.56	N/A	0.22	0.55	N/A
MO	0.07	0.46	2.00	0.06	0.45	2.00
TN	0.15	1.32	4.40	0.15	1.30	4.40
WI	0.07	0.27	3.25	0.07	0.27	3.25

Source: Urban-Brookings Tax Policy Center data on alcohol excise taxes and authors' calculations.

TABLE 2

Summary Statistics

All states plus DC from 1982 through 2015

Variable	Observations	Mean	Standard deviation	Min	Max
FARMVC share of fatal crashes	1734	0.33	0.08	0.09	0.61
Alcohol-related fatal crashes per million drivers	1734	72.00	37.30	7.48	276.30
Share of population ages 15 to 24	1734	0.15	0.01	0.12	0.20
Share of population age 65 or older	1734	0.13	0.02	0.03	0.19
Personal income per capita (2015 dollars)	1734	36385.05	9118.05	18070.42	73504.78
Gasoline tax rates (cents per gallon, 2015 dollars)	1734	25.49	6.62	7.53	53.37
Deaths from alcoholic cirrhosis of the livers per 100,000 people	1734	3.13	1.40	0.00	11.54
Unemployment rate	1734	6.02	2.11	2.30	17.79

Source: FARS, BEA, CDC, Department of Transportation, and authors' calculations.

Note: FARMVC = fatal alcohol-related motor vehicle crash. FARMVCs are measured per million drivers to ease data interpretation for readers. In our model, we use FARMVCs per driver. "Alcohol-related" indicates that a driver involved in the crash had a blood-alcohol content at or above 0.08 percent.

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