NBER WORKING PAPER SERIES

THE EFFECT OF CHANGES IN ALCOHOL TAX DIFFERENTIALS ON ALCOHOL CONSUMPTION

Markus Gehrsitz
Henry Saffer
Michael Grossman

Working Paper 27117
http://www.nber.org/papers/w27117

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 2020

Research for the paper was supported by grant number 1R21AA025371A1 from the National Institute on Alcohol Abuse and Alcoholism to the National Bureau of Economic Research. This paper was presented at the Eighth Conference of the American Society of Health Economists, at the Second Hamburg Center for Health Economics Risky Behaviors Workshop, at the Applied Micro Workshop at the Hertie School of Governance in Berlin, and at seminars at the University of Dundee and the University of Essex. We would like to thank participants in those events, especially Maria Sanmartin, for helpful comments and suggestions. We also would like to thank Ege Aksu and Daniel Dench for research assistance and comments. The paper employs data from the A.C. Nielsen Company and was purchased from the Kilts Center at the University of Chicago Booth School of Business. Results are calculated (or derived) based on data from The Nielsen Company (US), LLC and marketing databases provided by the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. Information about the data and access are available at http://research.chicagobooth.edu/nielsen/. We are grateful to the A.C. Nielsen Company and the Kilts Center for providing the data and for instructions in its use. The conclusions drawn from the Nielsen data are those of the researchers and do not reflect the views of Nielsen. Nielsen is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein. Copyright © 2017, the Nielsen Company (US), LLC. All Rights Reserved The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

The Effect of Changes in Alcohol Tax Differentials on Alcohol Consumption
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NBER Working Paper No. 27117
May 2020
JEL No. I12,I18

ABSTRACT

We show that tax-induced increases in alcohol prices can lead to substantial substitution and avoidance behavior that limits reductions in alcohol consumption. Causal estimates are derived from a natural experiment in Illinois where spirits and wine taxes were raised sharply and unexpectedly in 2009. Beer taxes were increased by only a trivial amount. We construct representative and consistent measures of alcohol prices and sales from scanner data collected for hundreds of products in several thousand stores across the US. Using several differences-in-differences models, we show that alcohol excise taxes are instantly over-shifted by a factor of up to 1.5. Consumers react by switching to less expensive products and increase purchases of low-tax alcoholic beverages, thus all but offsetting any moderate, tax-induced reductions in total ethanol consumption. Our study highlights the importance of tax-induced substitution, the implications of differential tax increases by beverage group and the impacts on public health of alternative types of tax hikes whose main aims are to increase revenue.

Markus Gehrsitz
Department of Economics
University of Strathclyde
Strathclyde Business School
Duncan Wing
199 Cathedral Street
Glasgow G4 0QU
United Kingdom
and IZA
markus.gehrsitz@strath.ac.uk

Henry Saffer
National Bureau of Economic Research
5 Hanover Square, 16th Floor, Suite 1602
New York, NY 10004-2630
hsaffer@gc.cuny.edu

Michael Grossman
National Bureau of Economic Research
5 Hanover Square, 16th Floor, Suite 1602
New York, NY 10004-2630
and City University of New York Graduate Center
and IZA
mgrossman@gc.cuny.edu

1. Introduction

Alcohol taxes are thought to be a valuable tool to reduce heavy alcohol consumption. Alcohol tax rates are based on three product groupings: spirits, wine and beer. Each group has its own excise tax rate and when alcohol excise taxes are increased it inevitably is done in a way that changes relative alcohol prices. The three classes of alcohol are to some degree substitutable and differential tax increases create substitutions that reduce the decline in overall alcohol consumption. We show that even though taxes are over-shifted to consumers, these substitutions can have a far greater impact on changes in total alcohol consumption than was understood in the past. Indeed, the data used in prior studies of this subject were generally too imprecise to measure induced substitutions. This paper combines a quasi-experimental design with high-resolution retail scanner data to investigate substitution patterns within and across different types of alcoholic products, to estimate the pass-through of alcohol taxes and estimate the elasticities of demand for spirits and wine.

Most economic models predict that higher alcohol taxes, on either the producers, sellers, or consumers of alcohol, will translate into higher prices. Decades of research on the price elasticity of alcohol demand, in turn, have shown a negative correlation between alcohol prices and consumption. A recent meta-study by Wagenaar et al. (2009) found that the average elasticity estimates of this extensive literature are -0.46 for beer, -0.69 for wine, and -0.80 for spirits. However, past studies are subject to two major concerns. First, they use arguably unreliable price measures, often from a limited sample of stores and products, which have been shown to be prone to overstating elasticities and thus the effectiveness of alcohol taxes (Ruhm et al., 2012). Second, previous research used observational study designs, such as interrupted time series or simple panel models, which may well suffer from endogeneity issues.

This paper overcomes these issues. We leverage detailed retail scanner data on alcohol sales in thousands of stores across the US to construct price measures that are based on a representative product basket and are consistent over time. We obtain credibly causal effects from these novel data by exploiting a natural experiment in Illinois where spirits and wine taxes were raised sharply and unexpectedly in 2009. We deploy classic difference-in-differences designs, event-study specifications, and the synthetic control method to estimate the effects of alcohol taxes on alcohol prices and sales. Moreover, our data allow us to shed light on cross-product-category substitution as well as substitution along the price distribution within product categories.

Our analysis suggests that consumers engage in cross-product substitution towards products that are subject to lower taxes. The 2009 Illinois excise tax increase resulted in higher spirits and wine prices but no change in beer prices. This resulted in higher beer sales, which largely offset the reductions in wine and spirits sales, such that total ethanol consumption was all but unaffected by the tax increase. We also found that consumers switched to lower priced spirits and wine. Our study thus highlights the importance of substitutions induced by changes in relative tax rates. We also find that alcohol taxes are over-shifted by a factor of about 1.5. That is, a $1 increase in alcohol excise taxes on average translates into a price increase of up to $1.50. This pass-through also does not vary very much at different points in the price distribution for spirits whereas price increases for wine occur predominantly in the very top and very bottom price segments.

This paper proceeds as follows. Section 2 summarizes the previous body of research on the effects of alcohol taxes with a focus on the data and methods used in these studies. It also introduces the natural experiment that we will exploit in this paper. Section 3 describes our main data and outlines the construction of our price and sales measures. In Section 4, we introduce our methodology. Section 5 presents the results, which are subjected to a series of robustness tests before we conclude in Section 6.

2. Background

2.1 Past Research

According to the National Institute on Alcohol Abuse and Alcoholism (2020a), the economic cost of alcohol abuse in the United States was around $249 billion in 2010. These costs include over 88,000 deaths per year making alcohol the fourth leading cause of preventable mortality in the United States. Alcohol is also a major risk factor for various morbidities including heart disease and various forms of cancer and plays a significant role in traffic accidents (Saffer, 1997; Dee, 1999), crime (Carpenter, 2007), poor birth outcomes (Nilsson, 2017), risky sexual behavior (Markowitz et al., 2005), and unemployment (Cook and Moore, 2002). These direct and indirect costs have made the control of alcohol misuse a public health policy priority, especially since most of the costs of alcohol overuse and misuse are borne by the government and by individuals who do not abuse alcohol.

The taxation of alcohol has not been an important component of government policy to address the external costs of alcohol abuse (for example, Grossman, 2017). Instead, antidrinking campaigns have focused on the enactment of minimum legal drinking ages for the purchase and consumption of alcoholic beverages, the requirement that warning labels be placed on bottles and cans containing alcohol, and laws that call for stiff fines and penalties and that raise the probability of apprehension and conviction for the offense of driving under the influence of alcohol. Nevertheless, alcohol excise tax hikes, which have and continue to be used to raised revenue, are potential tools in this campaign.

Taxes affect both moderate consumers as well as heavy drinkers. Some prior studies show that the health costs of alcohol consumption are related to heavy consumption. For example, O'Keefe et al. (2018) argue that light or moderate alcohol consumption is associated with a lower risk for all-cause mortality, coronary artery disease, type 2 diabetes mellitus, heart failure, and stroke. In this case, alcohol taxes penalize moderate drinkers for the damage caused by heavy drinkers. This view has been challenged by Griswold et al. (2018) who argue that all-cause mortality, and cancer mortality specifically, rises with increasing levels of consumption and thus even moderate alcohol consumption is problematic. Others argue that policy induced changes in average consumption reflect similar changes in heavy consumption.

The effect of a tax-induced price increase on alcohol consumption depends on the pass-through and the elasticity of demand. The pass-through is the ratio of the price increase to the tax increase. A textbook Cournot oligopoly model (Scherer and Ross, 1990) predicts that high industry concentration and low price elasticities will result in over-shifting taxes to consumers.[1] Yet, there has been little past research on alcohol tax pass-throughs. One of the few studies that investigated this issue in the case of Alaska is Kenkel (2005), who found an alcohol tax pass-through greater than one. Shrestha and Markowitz (2016) confirm this result in the case of beer tax increases at the state and federal levels in the United States. This result is also confirmed by Hindriks and Serse (2019) who show that the degree of over-shifting of a spirits tax increase in Belgium depended on stores' proximity to low-tax Luxembourg.

In contrast, there is an extensive literature on empirical estimates of alcohol price elasticities. Reviews by Gallet (2007) and Wagenaar et al. (2009) examined 132 and 112 past studies, respectively. Many of these studies struggle to find good measures of alcohol prices or sales. Most of the US literature (e.g. Manning et al., 1995; Grossman et al., 1998) has used alcohol prices provided by the American Chamber of Commerce Research Association (ACCRA). ACCRA collects these data in order to calculate a general Cost of Living Index for major US cities. Alcohol is only a small component of overall cost of living, which is why ACCRA only obtains the prices of a single brand of beer, wine, and whiskey

[1] This holds if the market price elasticity of demand, the Herfindahl index, and average cost are constant (do not depend on industry output) (for example, Saffer et al., Forthcoming).

in each city.[2] Ruhm et al. (2012) show that these data are likely to suffer from measurement error, are unlikely to reflect the purchases of typical drinkers, and can severely and unpredictably bias estimates of alcohol demand elasticities. These factors suggest that it might be misleading to use existing price elasticities to predict the effects of recent tax hikes.

There are also difficulties in measuring alcohol consumption. Researchers have usually turned to survey data, such as the Behavioral Risk Factor Surveillance Survey (BRFSS) (Ruhm and Black, 2002), the National Epidemiologic Survey on Alcohol and Related Conditions (NESARC) (Nelson, 2013), or the Panel Study of Income Dynamics (PSID) and the Health and Retirement Study (HRS) (Dave and Saffer, 2008). Survey data, of course, may suffer from social desirability bias. For adolescents in particular, the direction of this bias is hard to determine. Moreover, these data usually only offer a crude consumption measure that also relies on respondents' ability to retrospectively recall their drinking behavior. For instance, the number of drinks consumed in the past month is a popular measure of alcohol consumption in most surveys.

Finally, previous research typically relied on non-experimental research designs, such as interrupted time-series approaches or panel methods. For example, An and Sturm (2011) merge BRFSS data from different waves with state-level information on alcohol taxes in a given wave, and use the resulting panel to correlate taxes with consumption. Indeed, using state tax rates as a proxy for alcohol prices – and thus avoiding the potential pitfalls of using ACCRA information – is a widespread practice, even though Young and Bielińska-Kwapisz (2002) have warned that alcohol taxes are not a good proxy for alcohol prices. Regardless of the price measure, the identifying variation in these setups often comes from inflation rather than changes in nominal price or taxes. In addition, prices and taxes may be endogenously determined. Observational studies, therefore, might reflect spurious correlations between alcohol prices and consumption, even after including state and/or time fixed effects.

2.2 The Illinois Tax Increase

In this study, we exploit an unexpected, very substantial, and rapidly introduced excise tax increase in Illinois in 2009, in order to obtain causal estimates of alcohol tax pass-throughs and various changes in sales. The tax increase was included in the legisla-

[2] As of 2017, the beer product was a 6-pack of Heineken (12-oz. containers), and the wine product was a 1.5-liter bottle Chablis or Chenin Blanc, or any white table wine. ACCRA stopped collecting spirits prices in 2004; prior to 2004, they collected information on the price of a 750ml bottle of J&B Scotch.

tion of the "Illinois Jobs Now Act," which is a $31 billion state-construction plan focused primarily on construction of transportation infrastructure, but also had funding earmarked for school construction and community developments and went into effect on September 1, 2009. The bill was passed rapidly within 10 weeks of a new governor taking office.

While most of the state-construction plan was funded by issuing bonds, the bill also included a few measures designed to raise tax revenue, among them tax increases that were thought to raise $162 million from sales of candy, coffee, hygiene products, and alcohol (Center on Budget and Policy Priorities, 2010). Crucially, these alcohol tax increases were not instituted because of public health concerns or changing anti-alcohol sentiment in Illinois. The spirits excise tax was almost doubled from $4.50 to $8.55 per gallon, the wine tax also was almost doubled from $0.73 to $1.39 per gallon, and the beer tax had a negligible increase of 4 cents per gallon. All taxes are assessed at the manufacturer level and paid by the distributor. Sales taxes were not affected.

The magnitude of the spirits excise tax increase, in particular, is unprecedented. By way of comparison, the second largest spirits tax increase since 2006 (when our scanner data first become available) took place in Rhode Island in 2013 where the spirits excise tax increased from $3.75 to $5.40.[3] The size of the Illinois tax increases, thus, allows for price and sales responses that are large enough to be detectable with our data, which we describe in the next section.

3. Data

A key innovation of this study is that price and sales measures are constructed from a wide range of stores and products. These data are derived from the Nielsen Retail Scanner data. Nielsen collects weekly point-of-sale data on prices and sales volumes of about 2.6 million distinct products in over 35,000 stores. Products are identified by uniform product codes (UPC). There are about 6,000 distinct spirits products and 12,500 distinct wine products. Most stores in the sample are grocery or drug stores, but Nielsen also obtains data from large mass merchandise stores and smaller liquor and convenience stores. The data are gathered in 52 markets and cover about 50% of the total sales volume in US grocery and drug stores.

In order to construct a price measure that is representative for its product category but also consistent across time and different stores, we calculate our spirits price mea-

[3] However, the number of stores sampled in Rhode Island is too small to conduct a separate analysis. The state of Washington also recently introduced high spirits sales taxes, but these were part of a transition from a system of state monopoly to allowing grocery store sales.

sure from a subset of around 6,000 distinct spirits products that are on sale across the US. First, we exclude any transactions that relate to the "cocktails and coolers" product category and any products that are less than 60 proof (<30% alcohol). Second, only products in the most common bottle sizes (375ml, 750ml, 1000ml, and 1750ml) are used. These account for 89% of transactions with the vast majority of excluded transactions coming from 50ml mini-sampler bottles. We also treat products that are essentially identical but have different UPCs as the same product. For example, holiday versions of a particular liquor type are not treated as distinct from the regular version. This also includes collating products that come in plastic and glass bottles. However, products of the same brand but with different container size are treated as distinct products. Third, very niche products are eliminated by including only products in our price index that had at least 10 transactions per day in at least one store. We also limit ourselves to products that were present in at least 18 of the 21 states that allow grocery store sales of spirits in the week of the tax change, and were being sold 2 years prior to the tax change and were still on the shelves two years after the tax change.

These exclusion criteria result in a product mix that consists of 204 products. By definition, these are all top-selling products that have been available across the United States both before and after the Illinois tax reform. This consistency over time and location is crucial as selective changes in the availability of products might induce bias into our difference-in-differences estimator. Of course, there is a trade-off between consistency and representativeness. After all, one motivation for constructing this price index in the first place was the narrow focus of ACCRA price data on just a single product. Our final product basket of 204 spirits products, on the other hand, accounts for 52 percent of all spirits sales in the AC Nielsen data. For each product, we calculate the price per gallon, so that we can construct a price for each store in each week by dividing the total revenue created by a product by the number of gallons sold.[4] A single store-week spirits price is then constructed as the average of all 204 products.

Wine prices are constructed in the same manner: we exclude wine-flavored refreshments and alcohol-free wine; focus on bottle sizes of 187ml, 750ml, 1500ml, 3000ml, 4000ml, and 5000ml; collate virtually identical products; and only keep products that were present throughout our period of observation in at least 32 of the 35 states that allow wine sales in grocery stores, and that sold at least 80,000 units between September 2007 and August 2011. This results in a store-week wine price that is based on 252 distinct wine products, which account for over 50 percent of overall sales volume.

[4]All prices used here are pre-sales-tax. A larger literature on tax salience (e.g. Chetty et al., 2009) suggests that it is this posted price that grocery shoppers pay most attention to.

The main unit of observation in these data is the store-week. Stores in states that ban sales in grocery stores are dropped. In order to avoid issues related to selective attrition, we only use stores that are observed in every single one of the 210 weeks between the beginning of Sept. 2007 and the end of August 2011. For spirits, that leaves us with a balanced panel of 4,304 stores spread across the US. Three hundred twenty-seven of these stores are located in our treatment state, Illinois. Appendix Figure A1 shows the geographic distribution of these stores.[5] Appendix Figure A2 illustrates the geographic distribution of the 9,356 stores in our balanced sample that sell wine. Both maps indicate that Nielsen stores are primarily sampled in urban areas. This is confirmed in Appendix Figures A3 and A4, which show that the majority of stores in Illinois is concentrated in the Chicago metropolitan area. It is important to note that – unlike other retail scanner datasets – our data are based on individual stores rather than retail market area aggregates.

Panel A of Table 1 provides an overview of our price measures. The average price per gallon of spirits is around $80 in both Illinois and our control states in the two years prior to the tax change. This is equivalent to about $16 per 750ml bottle. In the post-period, the spirits price is markedly higher in Illinois. In fact, the raw data suggest that the tax increase of $4.05 was over shifted. For each store-week in our sample, we also calculate aggregate spirits, wine, and beer sales in gallons based on all products sold in a given store during a given week rather than just those products in our basket that are used to calculate prices. In an average week, around 145 gallons of spirits are sold in a typical store in Illinois (162 in our control states), but the raw data suggest that sales barely changed after the tax increase. We also split our product basket into 51 low-priced products, 102 mid-tier products, and 51 high-priced products and recorded both prices and sales for each category. Section 5.3 provides more details on how this split is used to investigate substitution within product category. Panel B presents total beer sales in gallons and total ethanol sales for each store-week, in order to investigate substitution patterns.

Panel C of Table 1 provides additional information on the composition of our product basket that is used to calculate our spirits price. The basket is dominated by 750ml bottles and contains all kinds of different spirits with different varieties of whiskey (bourbon, scotch, etc.) and vodka making up the largest shares. Sixteen percent of products

[5] Note that we include some states that notionally do not allow the sale of spirits in grocery stores, but where the data indicate that loopholes exist and are being used. For instance, the state of Florida has a ban on grocery store sales of spirits, but allows sales from liquor stores that are located within a grocery store as long as there is a separate entrance. These stores sell a large range of spirits and AC Nielsen data treat these stores as grocery stores.

cost less than \$45 per gallon (or \$9 per 750ml) and 37 percent of products cost more than \$95 per gallon. Overall, our product mix covers a diverse range of different spirit types and several price segments. Panel D shows that the vast majority of stores in our sample are grocery and drug stores, which reflects the fact that Nielsen cooperates with 90 major chains to collect the data.

Similar to spirits, the raw data of Panel A in Table 2 suggest that the wine excise tax increase was over-shifted. The tax increase was \$0.66 per gallon, but prices in Illinois increased by \$0.86 relative to prices in our control states. The raw data are also indicative of a moderate, negative sales response. Panel B shows that our sample of wine products is dominated by domestic wine in 750ml or 1,500ml bottles. Almost 50% of products are sold at under \$6 per 750ml (or \$30 per gallon). Our representative wine price is thus, not constructed from fine wines but from widely available inexpensive products that are primarily sold in grocery and drug stores (see Panel C).

4. Methodology

4.1 Main Approach: Difference-in-Differences

The 4,304 stores across the US that sell spirits in all 210 weeks under consideration, give us a sample with 903,840 observations to analyze the effect of the tax change on spirits prices and sales; our balanced sample of 9,356 wine-selling stores provides 1,964,760 observations. Our unit of observation is the store-week, i.e. we have a representative price and sales for every store in every week. We combine these data with the quasi-experimental exogenous variation created by the Illinois tax increase through a difference-in-differences model, estimated using OLS, of the following form:

$$y_{sjt} = \alpha + \beta_{DD} Treat_{jt} + \gamma_s + \theta_t + u_{sjt}, \tag{1}$$

where y_{sjt} is price or sales outcome in store s, located in state j, in week t, γ_s denotes a full set of store fixed effects. These time-invariant fixed effects subsume any state fixed effects. The vector θ_t is a set of week fixed effects common across stores, although in most specifications we use year-month fixed effects, which does not affect the results, but allows us to cleanly plot the results of an event-study-specification. Time fixed effects also subsume the post-treatment-period indicator of a classic difference-in-differences setup. $Treat_{jt}$ is an interaction between a dummy indicator for whether a store is located in Illinois (our treatment state) and a dummy indicator for whether an observation pertains to a week after September 1, 2009 (our post-period), and u_{sjt} is the disturbance term.

Our main coefficient of interest is β_{DD} which yields the treatment effect. That is the effect of the excise tax increase on prices and sales in Illinois relative to those in our control states where taxes were not raised.[6] We also run several event-study specifications in which the single treatment indicator of equation (1) is replaced by a set of Year-Month dummies, interacted with a treatment state indicator:

$$y_{sjt} = \alpha + \sum_{\tau=9/2007}^{8/2011} \delta_\tau \times IL_j + \gamma_s + \delta_\tau + \epsilon_{sjt} \tag{2}$$

The lags in the specification allow us to evaluate whether any effect of the tax increase grows over time or fades out. The leads provide an additional test of the main identifying assumption, which is that trends in our outcomes of interest would have continued to be the same in Illinois and the control states in the absence of the tax increase.

Panel A of Figure 1 provides a first piece of evidence for this "common time-trends assumption". The thin solid line plots the mean per-gallon price in Illinois stores for the four-year period under evaluation. The figure plots raw, unweighted means. That is, every store carries the same weight. The figure reveals substantial seasonality, especially around the Christmas holidays. In order to not have the data obscured by this seasonality, we overlay the raw means with a thick solid Locally Weighted Scatterplot Smoothing (LOWESS) line. We repeat this procedure for stores in control states, which are shown in Figure 1 with dashed lines.

The data show that spirits prices in Illinois and the control states are on strikingly similar trajectories prior to the tax increase. In fact, even the seasonal jump in prices around the holiday season is all but identical in Illinois and the control states. Second, spirits prices diverge sharply during the week in which the excise tax went up, indicated by the black dotted vertical line. This jump corresponds to about a \$6 increase in price and the gap between Illinois and the control states persists until mid-2011. Not surprisingly, the size of this one-off shift in prices is also consistent with the over shifting result implied by our descriptive statistics in Table 1.

Panel B of Figure 1 plots raw and smoothed data for spirits sales. Similar to spirits prices, gallon-sales per store appear to follow similar pre-treatment time trends in Illinois

[6] The estimate of the coefficient of interest is not sensitive to the inclusion of the store-specific characteristics listed in panel C of Tables 1 and 2 because these characteristics have almost no time variation. Thus, they are almost perfectly collinear with store fixed effects.

and our control states. Both sets of stores are subject to the same seasonality, although the sales spike around Thanksgiving and Christmas may be slightly more pronounced in Illinois. We account for this pattern by seasonally adjusting our spirits sales data. Specifically, we regress gallon-sales on a set of calendar month dummies interacted with Illinois dummies to account for potentially differential seasonality. We then use the residuals in our main analysis. In practice, seasonal adjustment makes little difference and our main results using unadjusted data are reported in the appendix.

In contrast to spirits prices, sales appear to barely budge after the tax increase. If anything, there is a small increase in Illinois sales in the week prior to the tax change. We address such anticipatory effects using a "donut" approach outlined below, although Figure 1 suggests that any bias induced by anticipatory behavior should be negligible.

Figure 2 shows trends in the raw data for wine prices and wine sales. Panel A documents that wine prices are slightly higher in Illinois than in our control states, but they follow very similar trends. There is a clear break at the time of the tax increase, such that the price gap widens by a magnitude that suggests over-shifting, albeit to a slightly smaller extent than observed for the spirits tax increase. Wine sales show slightly more extreme seasonal patterns around the Christmas and Thanksgiving periods.[7] Again, we seasonally adjust the sales data and report the unadjusted results in the appendix. The raw data also show no obvious divergence for wine sales, if anything the gap between sales in Illinois stores and control stores appears to shrink marginally.

4.2 Alternative Standard Errors

The group structure in our data creates an issue for the calculation of valid standard errors. All of the stores in the same state might be subject to common influences creating a clustering problem. As is standard in the literature (Bertrand et al., 2004), we therefore cluster the standard errors at the state-level in our baseline specification. A related issue is that while there are thousands of stores in our sample, we effectively only have one treatment group (stores in Illinois) and one control group (stores in other states). Therefore, we adopt the procedure by Donald and Lang (2007) to obtain appropriate standard errors in difference-in-differences (DD) models with only one large treatment group and one large control group observed over multiple before- and after- treatment periods. In the first step of their two-step procedure, both treatment and control stores are collapsed into treatment and control averages. In the context of our study, this means

[7]The smaller spikes in sales tend to correspond to other holidays (e.g. Independence Day, Memorial Day) or events (e.g. Super Bowl Sunday, St. Patrick's Day).

that we go from 4,304 (stores) $\times$ 210 (weeks) to 2 $\times$ 210 observations. We can then calculate the difference in outcomes between our treatment state aggregate and the control state aggregate. In a second step, we regress this difference on an indicator for the post-treatment period that is equal to one for half of our 210 weekly observations, and zero for pre-treatment differences:

$$\Delta y_t = \alpha + \beta_{DL} Post_t + u_t. \tag{3}$$

Because each store receives the same weight in the construction of our treatment and control aggregates, the Donald and Lang (2007) method ensures that β_{DL} in equation (3) will be identical to β_{DD} of equation (1), but calculates standard errors that reflect the difference in y_t between the treatment and control groups in each of 210 periods. Throughout our results section we report both the regular cluster-adjusted standard errors as well as standard errors obtained using the above procedure, which we refer to as "Donald-Lang standard errors."[8]

Bedard and Kuhn (2015) and Stearns (2015), who adopted the Donald and Lang approach just specified, point out that serial correlation in the disturbance term in equation (3) poses another threat to the validity of our standard errors. Following their procedures, we re-run the pure time series regression given by equation (3) and calculate standard errors using the method of Newey and West (1987). These standard errors are auto-correlation consistent in that they allow for correlated shocks to the disturbance term (u_t) to have persistence over 3 weeks.[9]

4.3 Anticipatory Effects and Adjusted Control Group

Figures 1 and 2 support our main identifying assumption. So does the event-study specification, which will be shown in Section 5. Nonetheless, one may be concerned that anticipatory effects may bias our estimates. In order to address any such concerns, we rerun all analyses excluding observations that pertain to either the "treatment week" (i.e. the week ending on Saturday, 5 September 2009), or the three weeks following the treat-

[8] Rodicki et al. (2018) show in a simulation study that the Donald-Lang aggregation method performs very well relative to other techniques to address statistical issues when there is only one treatment group.

[9] We experimented with several shorter and longer lag-specifications, all of which yielded very similar standard errors. Donald and Lang (2007) suggest taking first differences of equation (3) to correct for serial correlation. That assumes that the disturbance term in the differenced equation follows a random walk. We do not take that approach because it measures the treatment effect in the first post-treatment period only rather than an average effect over the whole post-treatment period.

ment week, or the four weeks preceding the treatment week. In other words, we reduce the number of observations per store from 210 to 202. The idea here is that any stocking up by consumers is likely to have occurred in the weeks leading up to the tax increase, and that this may have depressed sales in the weeks following the tax increase. Such behavior would bias our estimates away from zero, i.e. we might overestimate the extent to which the tax reduced sales. A "donut"-specification that cuts out the middle part of the time period we analyze, addresses this issue and at the very least provides a useful robustness check.

One might still be concerned about whether all the stores in states outside of Illinois are a valid comparison group to those in Illinois. As an additional robustness check, we therefore use a method developed by Abadie et al. (2010). This technique constructs a "synthetic" control group that approximates the outcome in the treatment group as closely as possible by selecting a weighted combination of untreated units. The control units are usually selected based on pre-treatment characteristics and pre-intervention outcome data, in our case only the latter. Weights for the control units are selected such that the pre-period mean-squared prediction error (MSPE) is minimized. In other words, a control unit is constructed as a weighted average of all potential control units such that during the pre-intervention period this synthetic control unit matches the outcomes of interest of the treatment unit as closely as possible. The weights add up to one with some states receiving a weight of zero.

To implement the synthetic control model we aggregate our data to the state level. That is, we calculate state-averages for every week by giving every store located in a given state the same weight. We then construct our synthetic control group and calculate Δy_t, i.e. the difference in outcome between our treatment and our synthetic control group. Note that we construct a different control group for each outcome, so the composition of our comparison group is not fixed. We then, re-run equation (3) and calculate both regular Donald-Lang standard errors and auto-correlation robust Newey-West standard errors.[10]

5. Results

Tables 3-6 follow the structure outlined above. Column (1) shows the coefficient for the conventional difference-in-differences regression outlined in equation (1). In this specifi-

[10]Following Bedard and Kuhn (2015) and Stearns (2015), we also assessed statistical significance with a variant of the permutation test employed by Abadie et al. (2010). Our results are not affected by this alternative approach.

cation all control states serve as a comparison group. Column (2) repeats the analysis but discards data that refer to time periods either 4 weeks prior or after the tax change. In column (3) the control group is constructed using the synthetic control approach; column (4) combines the synthetic control method with our donut estimation. We report three sets of standard errors: State-cluster adjusted standard errors, standard errors calculated using Donald and Lang's (2007) two step procedure (DL-standard errors), and DL models with Newey-West standard errors.

5.1 Results for Spirits

Table 3 summarizes our results for the pass-through of the excise spirits tax increase. It is striking that all four specifications yield point estimates that are of very similar size and statistically significant at any reasonable level of significance. Table 3 suggests that the tax increase led to a spirits price increase of \$6.62 per gallon (column (1)). Since the excise tax increase was \$4.05 per gallon, this indicates a pass-through of about 1.5. In other words, the tax is over-shifted. In percentage terms, the almost 100 percent increase in the tax rate increased the price of spirits by approximately 8.2%.[11]

Panel A of Figure 3 illustrates the results of the event-study specification outlined by equation (2). The reference period here is the first week in our sample, i.e. the week ending on September 9, 2007. Two features stand out. First, the pre-period coefficients and 95% confidence intervals all hover around zero. This further bolsters the common-time trend assumption and thus adds credibility to a causal interpretation of our results. Second, prices appear to adjust almost immediately and remain at a higher level for the full 2-year post-period. This suggests that the tax increase led to over-shifting that resulted in a permanent price increase.

Table 4 shows the results for (log) spirits sales.[12] Column (1) suggests that the tax increase resulted in about a 3.5 percent reduction in gallon sales. The effect is statistically significant at the 5% level regardless of how the standard errors are obtained. Picking

[11]Let p be price and r be the tax rate. For small changes

$$\partial ln(p)/\partial ln(r) = (\text{pass-through}) \times (\text{tax share}),$$

where tax share is the share of the tax in the price of spirits. Since that share is small, the percentage change in the tax is much smaller than the percentage change in the price. For large changes, the small tax share still accounts for the much smaller percentage change in price.

[12]For ease of interpretation we report the effect of log-sales. Appendix Tables A1 through A4 show the results for specifications for levels, and also for log prices. They also show the results for both prices and sales without seasonal adjustment. All results are very similar in magnitude to the main results.

the control group using the synthetic control method slightly increases the point estimate although there is no statistically significant difference across specifications.

Panel B of Figure 3 shows the results of our event-study specification. Despite our seasonal adjustment, sales are much noisier than prices. Nonetheless, the lagged interaction coefficients hover around zero for the vast majority of the period leading up to September 2009. The graph also indicates that sales spiked in Illinois, relative to other states, in the month prior to the tax increase. We can see about a 10 percent increase in sales in the month prior to the tax increase. However, the donut-specification of columns (2) and (4) in Table 4 produces point estimates that are virtually identical to those that use the full time period. In other words, there is evidence for anticipation behavior but it does not appear to play a large enough role to significantly affect our headline estimate. It is notable that from 2010 on, sales settle at a level that is markedly lower than prior to the tax increase. The event study estimates suggest a reduction of about 3 percent, which – not surprisingly – corresponds to the point estimates of Table 4.

5.2 Results for Wine

We also evaluate the pass-through and sales-response for the 2009 increase in the Illinois wine tax, using the same difference-in-differences setup. Column (1) of Table 5 suggests that the wine-tax increase of \$0.66 per gallon resulted in a price increase of \$0.84 per gallon. The shift is statistically significant at any reasonable level of significance and stable across donut (column 2) and synthetic control specifications (columns 3 and 4) as well as alternative ways of calculating the standard error. The suggested pass-through of about 1.3 is slightly smaller than for spirits, but clearly, the wine tax was also over-shifted to the consumer.[13] In percentage terms, the almost 100 percent increase in the tax rate increased the price of spirits by approximately 2.4%.[14] The results of our event-study specification, shown in Panel A of Figure 4, are slightly noisier than the equivalent for spirits. They nonetheless support the common time-trend assumption and show a clear break at the time of the tax increase when wine prices shifted up abruptly.

Similarly, Panel B of Figure 4 shows a clear break for wine sales at the time of the tax increase. All pre-treatment interaction terms hover around zero, but wine sales in Illinois decreased by 3-4 percent after the tax increase kicked in. This is consistent with the

[13] Appendix Tables A7 to A10 further show that our results are not sensitive to seasonal adjustment or the type of specification.

[14] The percentage increase in the wine price is smaller than the percentage increase in the spirits price because the pass-through is smaller and, more importantly, because the tax share is smaller for wine than for beer.

aggregate point estimate of Table 6. Columns (1) and (2) indicate that the tax increase led to a drop in wine sales of about 3%. Our synthetic control estimation, shown in column (3) indicates a slightly larger drop of 4.2%. All effects are statistically significant at the 5% level regardless of the how the standard errors are calculated.

5.3 Substitution across Price Segments

Our wine and spirits price was calculated using a heterogeneous product basket. This allows us to investigate whether the taxes were passed on uniformly across price segments. For instance, it is conceivable that price increases could be concentrated at the top end if consumers of cheap spirits were more price-conscious and retailers, therefore, raise prices less in this segment. In addition, consumers might react to price increases by substituting towards cheaper products.

In order to analyze a differential pass-through by price segment, we calculate the average price for all products in our basket during the first week of our data and rank them accordingly. We identify the 51 least expensive products (Bottom), 102 middle range products (Middle) and the 51 most expensive products (Top). Likewise, we split our wine product basket into 63 bottom products, 126 middle range products and 63 most expensive products. Table 7 shows that the spirits tax was passed on almost uniformly across spirits price segments. The point estimates suggest that prices of Bottom 51 products increased by \$6.32 per gallon and those of Top 51 products increased by \$6.53. The difference is not statistically significant and the middle of the price distribution saw similar pass-throughs. However, sales responded differentially. Table 7 shows that gallon sales of bottom priced spirits increased by about 4.1%. Sales of mid-tier products dropped by 1.9% and expensive spirits products were unaffected.[15] This suggests that consumers substituted toward cheaper spirits products, which reflects a substitution of quantity for quality. If quality of has a larger income elasticity than its quantity, income effects associated with the tax increase may cause quality to fall relative to quantity.

To pursue the last point in more detail, since alcohol taxes are specific, rather than ad valorem excise taxes, they take the form of fixed costs on quantity. A tax hike raises the price of quantity relative to quality. The net (utility-constant) effect of the hike is to lower the optimal amount of ethanol and to raise quality (reflected shifts to more expen-

[15]Note that for this analysis we restrict our analysis to our selected 204/252 products and firmly keep each product in a specific category. The alternative would be to consider all sales, select price cut-offs and categorize products depending on their location relative to these cut-offs. The issue with this approach is that the tax increase moves products across cut-offs and induces a mechanical reduction in the sales of inexpensive products because post-tax they are no longer in the inexpensive-category.

sive brands) if quality and quantity are net substitutes (for example, Becker and Lewis, 1973; Calcott, 2019). But this result can be mitigated if quality is more income-elastic than quantity. Moreover, taken by itself, a reduction in quality lowers the price of quantity (Becker and Lewis, 1973).

For wine, the pass-throughs are concentrated at the bottom and the top of the price distribution with prices in the middle not responding very much. Sales in all wine price categories declined with the largest decreases in the bottom and top price categories.

5.4 Substitution towards Beer and Total Ethanol Consumption

While the Illinois spirits and wine taxes increased sharply in 2009, beer taxes experienced only a trivial increase. As a result, beer became relatively cheaper and assuming positive cross-price elasticities while discounting any income effects, consumers should have increased their beer consumption. This substitution would offset some of the decrease in spirits and wine consumption. Table 8 suggests that the spirits and wine tax increases were indeed associated with an increase in beer sales of 5.5% (2.6% for the synthetic control specification).[16] This corresponds to an additional 50 gallons of beer sold per store-week in Illinois relative to the control states.

Assuming an average alcohol content of 4.5% in beer, our results suggests that the tax led to an increase in weekly sales of about 2.8 gallons of pure ethanol per store in the form of beer. By way of comparison, our estimated sales responses suggest a reduction of about 2.2 gallons of pure ethanol from spirits (assuming 40% alcohol content) and 0.9 gallons of ethanol from wine (assuming 12% alcohol content). In other words, most of the presumed reduction in alcohol consumption through spirits and wine was offset by increased beer consumption. Table 9 confirms this pattern of avoidance behavior by explicitly using ethanol sales per store-week as the left-hand side variable. Columns (1) and (2) indicate that the tax increases did not significantly affect total ethanol consumption. The synthetic control model, on the other hand, suggests a very small and precisely estimated reduction in total ethanol sales of about 1.7%. However, much of this reduction appears to be driven by reductions in the final months of our analysis. Either way, our results strongly suggest that cross-product substitution might have severely limited the health benefits of the alcohol tax hike.

McClelland and Iselin (2019) report that the Illinois tax hike had no impact on alcohol-related fatal motor vehicle crashes, which are the second leading cause of death of

[16] We only used stores in this analysis that sell all three types of alcohol (spirits, wine, beer), but the result is robust to using all stores in our sample that sell beer.

persons between the ages of 15 and 34 and the leading cause of death of persons between the ages of 1 and 34 (Centers for Disease Control and Prevention, National Center for Injury Prevention and Control, 2020). Our results on the effects of the tax increases on ethanol consumption provide an explanation of that finding.[17] Beer is the alcoholic beverage of choice of young adults, and beer sales actually increased as a result of the Illinois tax hikes on spirits and wine.

5.5 Robustness: Cross-Border Shopping and the Role of Cook County

So far, we have only considered substitution across product segments and categories. We now explore the effect of the Illinois alcohol tax increase on cross-border shopping. Illinois stores at the state border are likely to compete with stores across the border, which were not directly affected by the alcohol tax increase. However, Appendix Figures A1 to A4 show that the number of border stores in our sample is small and - more importantly - in many cases (e.g. the Wisconsin border) we also have even fewer observations from stores just across the border.

We, therefore, investigate the importance of cross-border shopping by splitting our sample of Illinois stores into stores that are located in state-border counties and those in non-border counties located further inside the state. Columns (1) and (2) of Table 10 show that both sets of stores increased spirits prices by about the same amount, and columns (4) and (5) indicate that the sales response was also identical in both sets of stores.

There is, thus, little evidence for cross-border shopping, which may be partly due to the fact that, despite the tax increase, alcohol prices do not differ massively across Illinois and its neighboring states. For instance, the average gallon price in Kentucky, Illinois, and Wisconsin is about \$80, in Iowa and Missouri it is about \$86 while the tax increase took Illinois from about \$80 to \$86.

Similarly, there is no statistically significantly different increase in wine prices following the tax increase, although it appears as if sales reductions were more pronounced in border counties. Finally, Appendix Figures A3 and A4 show that about half the Illinois stores are located in Cook County, which contains most of the Chicago metropolitan area and where about 40% of the Illinois population resides. While it is conceivable that most of our effects could have been driven by stores in this county, columns (3) and (6) of Table 10 show that our results do not change when we exclude Cook County from our sample.

[17] See McClelland and Iselin (2019) for a critique of an earlier one by Wagenaar et al. (2015) that did find an impact of the tax increase on fatal crashes.

5.6 Elasticities

We estimated that the effect of the tax increase on spirits sales was about -3.5% and on spirits prices was about 8.2%. This ratio is about -0.4 but is not the own price elasticity (the elasticity of spirits with respect to the spirits price) because it includes the effect of higher wine prices on spirits sales. Along the same lines, we estimated the effect of the tax hike on wine sales was approximately -3.0% and on wine prices was 2.4%, which yield a price elasticity of -1.3. As in the case of spirits, this is not an own price elasticity because both the price of wine and the price of spirits are varying. Finally, the tax hikes increased beer sales by about 4.0%, which is an average of our main and synthetic estimates in columns (1) and (3) of Table 8. The ratio of this increase to the increase in the price of spirits equals 0.5, but that figure does not correspond to the cross elasticity of beer with respect to the price of spirits because the price of wine is not held constant.

To compare the elasticities just computed to conventional own and cross price elasticities, assume that the utility function depends on consumption of spirits (s), wine (w), beer (b), and a fourth good (x). Let p_i ($i =$ s, w, b, x) denote the prices of s, w, b, and x. Assume that money income and the prices of b and x are held constant. Then demand functions for s, w, and b in logarithmic differential form (E $\equiv$ dln) can be written

$$\mathrm{E}s = k_s(\sigma_{ss} - \eta_s)\mathrm{E}p_s + k_w(\sigma_{sw} - \eta_s)\mathrm{E}p_w \tag{4}$$

$$\mathrm{E}w = k_s(\sigma_{sw} - \eta_w)\mathrm{E}p_s + k_w(\sigma_{ww} - \eta_w)\mathrm{E}p_w \tag{5}$$

$$\mathrm{E}b = k_s(\sigma_{sb} - \eta_b)\mathrm{E}p_s + k_w(\sigma_{sw} - \eta_b)\mathrm{E}p_w \tag{6}$$

Here k_i is the share of income spent on good i; σ_{ii} is the own partial elasticity of substitution in consumption between good i and itself; σ_{ij} ($j \neq i$) is the cross partial elasticity of substitution in consumption between goods i and j; $\sigma_{ij} = \sigma_{ji}$; and η_i is the income elasticity of demand for good i.

It is well known that

$$k_s\sigma_{ss} = -k_w\sigma_{sw} - k_b\sigma_{sb} - k_x\sigma_{sx} < 0 \tag{7}$$

$$k_w\sigma_{ww} = -k_s\sigma_{sw} - k_b\sigma_{wb} - k_x\sigma_{wx} < 0 \tag{8}$$

Let $\mathrm{E}p_s = r\mathrm{E}p_w$ ($r = 3.42 > 1$ in our case) and let $q \equiv r^{-1}$ ($q = 0.30 < 1$). Using these definitions and equations (7) and (8), one can rewrite equations (4), (5), and (6) as follows:

$$\frac{\mathrm{E}s}{\mathrm{E}p_s} \equiv e_s = \varepsilon_s + qk_w(\sigma_{sw} - \eta_s) \tag{9}$$

$$\frac{\mathrm{E}w}{\mathrm{E}p_w} \equiv e_w = \varepsilon_w + rk_s(\sigma_{sw} - \eta_w) \tag{10}$$

$$\frac{\mathrm{E}b}{\mathrm{E}p_s} \equiv e_b = k_s(\sigma_{sb} - \eta_b) + qk_w(\sigma_{wb} - \eta_b) \tag{11}$$

In equation (9), ε_s is the own price elasticity of s. It holds money income and the price of w constant. In equation (10), ε_w is the own price elasticity of wine, with money income and the price of spirits constant.[18]

Our results pertain to $e_s = -0.43$, $e_w = -1.25$ and $e_b = 0.49$. If the cross elasticity of spirits with respect to the price of wine $[k_w(\sigma_{sw} - \eta_s)]$ is positive, we underestimate the own price elasticity of spirits (ε_s) in absolute value. That is, an increase in spirits prices reduces spirits sales, but that effect is partially offset by the increase in wine prices. The reverse holds if the cross price elasticity just mentioned is negative because σ_{sw} is negative or because the positive value of σ_{sw} is smaller than η_s. We also underestimate ε_w if the cross elasticity of wine with respect to the price of spirits $[k_s(\sigma_{sw} - \eta_w)]$ is positive and overestimate it if it is negative.[19]

Not surprisingly, our estimate of the cross elasticity of beer with respect to the price of spirits depends on the cross elasticity that holds the price of wine constant $[k_s(\sigma_{sb} - \eta_b)]$ and the one that holds the price of spirits constant $[k_w(\sigma_{sw} - \eta_b)]$. If both are positive,

[18] $\varepsilon_s = k_s(\sigma_{ss} - \eta_s)$ and $\varepsilon_w = k_w(\sigma_{ww} - \eta_w)$. Given that all goods are superior, which we assume from now on, all own price elasticities are negative.

[19] The net or utility-constant cross elasticity of s with respect to the price of w ($k_w\sigma_{sw}$) must have the same sign as the net cross elasticity of w with respect to the price of s ($k_s\sigma_{sw}$). But the cross elasticities referred to in the text have opposite signs if, for example, $\sigma_{sw} > \eta_s$ but $\sigma_{sw} < \eta_w$. Note also that it is well known that goods on average must be net substitutes ($\sigma_{ij} > 0$ on average), which means that net cross price elasticities ($k_i\sigma_{ij}$ and $k_j\sigma_{ij}$) must be positive on average. But that proposition does not hold for gross or money-income constant cross elasticities.

we overestimate the former. Clearly, at least one of the two elasticities must be positive. If one is negative, the other must have a large enough positive value to more than offset the impact of the beverage that is a complement for beer.

While we do not have enough information to compute conventional own and cross price elasticities and compare them to estimates in the literature, we can reach several suggestive conclusions about the impacts of a large tax hike in the price of spirits accompanied by a smaller but still substantial increase in the tax on wine. First, sales of both beverages are likely to fall. That is in contrast to a situation in which there were no substitutes for spirits other than wine and in which income effects were small enough to be ignored. In that case, sales of wine would rise since its relative price has fallen. Second, if the tax on beer remains the same, that beverage appears to be a good enough substitute for spirits and wine to offset a significant portion of the tax hike on total ethanol consumption.

Finally, the observed elasticities that we obtain are consistent with the large increase in tax revenue generated by the increase in the tax rates on spirits and wine and the increase in the sale of beer. Table 11 shows that average tax revenue per store increased in all three beverage categories. Tax revenue from spirits and wine nearly doubled and tax revenue from beer sales increased by about 23%. Overall, alcohol tax revenue increased by about 75%. Note that the increase in wine tax revenue that accompanied the increase in the tax on wine is not inconsistent with the observed price elasticity of wine that exceeds one in absolute value. That increase would occur as long as the price elasticity was smaller than the inverse of the pass-through multiplied by the share of the tax in the price of wine. Since the pass-through was 1.30 and the tax share was approximately 0.03, tax revenue from an increase in the wine tax rate would increase as long as the price elasticity was smaller than approximately 26 in absolute value.

6. Conclusion

This study relies on an exogenous increase in alcohol taxes in Illinois in 2009 and leverages a data set of alcohol transactions for hundreds of products across thousands of stores in the US to reveal that taxes are over-shifted; that the resulting changes in relative prices induce substitutions both within and across product categories; and that overall ethanol consumption was barely affected. The tax changes were about a 100% increase on spirits and wine taxes with virtually no change in beer taxes. We find that the Illinois excise tax increase on spirits resulted in price increases of about 150% of the tax increase with a somewhat smaller pass-through of 130% for wine. These values may be different in other states. Since the share of the spirits tax in the price of spirits exceeded the corresponding share for wine and since the pass-through is larger for the spirits than for wine, the price of spirits rose by about 8 percent, while the price of wine rose by about 2 percent. These price hikes resulted in sales reductions of each beverage, with implied price elasticities of -0.4 for spirits and -1.3 for wine. These are not own price elasticities because neither one holds the price of the other beverage constant. Hence, they are not directly comparable to previous estimates in the literature, but they do reveal that wine sales decline even when its price relative to that of spirits falls. Wine prices, however, relative to beer prices increase.

Our study also reveals several interesting substitution patterns. Consumers respond to higher alcohol taxes by shifting either to cheaper products within the same product category or to less heavily taxed alcohol in a different product category. Indeed, we find that cross price elasticities between spirits and beer and between spirits and wine suggest a high degree of substitutability between these products. The Illinois tax increases reduced overall spirits consumption by about 3.5%, wine consumption by about 3.0% and increased beer consumption by about 4.0%. The tax hike also increased low price spirits consumption. As a result, overall ethanol sales declined by at most 1.7%. This avoidance behavior, especially the increase in beer sales, likely accounts for the failure of alcohol-involved fatal motor vehicle crashes to fall following the tax hikes.

The results of our study are robust to several difference-in-differences specifications and we provide evidence supporting the main identifying assumption underpinning our research design. While the point estimates of our analytical and graphical analyses are striking, we are mindful of inference issues that may arise in a setting in which treatment units are clustered together. We, therefore, use additional non-standard techniques to calculate our standard errors. Neither inference method fundamentally changes the interpretation of our results.

Since the mid-1970s, the federal government of the U.S. and state governments have been involved in a campaign to curtail the consequences of the misuse and excessive use of alcohol (for example, Grossman, 2017). Excise tax increases have not been part of this campaign. Since 1951, federal taxes on alcohol have been increased twice: on distilled spirits in 1985 and on all three beverages in 1991. State taxes have also been raised sporadically during this period. As in the case of the Illinois 2009 legislation, the purpose of the federal and state tax increases has been to raise revenue.

Our results have suggestive implications for the design of a policy to use taxes as a policy tool in the campaign just mentioned, should that tool be considered in the future.

They suggest that legislation that raises the price of one beverage relative to the others may not have the intended effects on public health due to substitution among the beverages. Given that, what type of policy might be optimal? One option would be to raise the tax rate on each beverage by the same amount. That would keep relative prices constant only if the price of an ounce of each beverage were the same.[20] Another option would be to raise the beverage-specific tax by the same percentage. That would keep relative prices constant only if the share of the tax in the price of each product were the same. Neither condition holds. For example, after the Illinois tax increases the price of an ounce of spirits was \$0.67, the price of an ounce of wine was \$0.29, and the price of an ounce of beer was \$0.05. The corresponding tax shares expressed as percentages were 10% for spirits, 4% for wine, and 3% for beer. Hence, a policy to raise the price of each beverage by, for example, 1% would require raising the tax rates on spirits, wine, and beer by approximately 10%, 25%, and 33%, respectively.

Even the policy just mentioned might not be desirable because the price of each beverage would rise relative to that of other items demanded by consumers. If one beverage is a better substitute or complement for these items than others, the resulting shifts in alcohol consumption might defeat the aim of the policy. For example, suppose an important goal is to reduce alcohol-involved motor vehicle fatalities. For reasons discussed previously, it would be important to curtail misuse or overuse of beer. That would not be accomplished if beer were a poor substitute for non-alcoholic items.

Another policy is one in which the ethanol in each beverage is taxed at the same rate. As discussed by Grossman (2017) that policy has been considered in academic research, but not by policymakers, especially in the context of measures to reduce alcohol-involved fatal motor vehicle crashes.[21] Federal rates reflect significant disparities in the rates at issue. An ounce of ethanol in spirits is taxed at the rate of \$0.21, in wine at \$0.07, and in beer at \$0.10. Similar disparities are present in the Illinois rates of \$0.17 on an ounce of ethanol in spirits, \$0.09 in wine, and \$0.04 on beer. Partly as a result of these differences, the price of an ounce of ethanol in Illinois after the tax increase was \$1.18 in beer, was \$1.70 in spirits, and was \$2.17 in wine. Our results show that consumers reacted to price increases by shifting towards lower-priced ethanol in beer. They also substituted towards cheap spirits after the tax hikes, since the price of an ounce of ethanol in low

[20] $\partial ln(p_i)/\partial r_i = (\text{pass-through}_i)/p_i$. We assume that pass-through$_i$ is the same for each beverage. The conclusions with regard to this policy and one in which the tax is raised by the same percentage for each beverage would not be altered if the pass-through was approximately the same for each one.

[21] In a rare departure from the failure to consider tax policy to curtail alcohol abuse, Congresswoman Eleanor Holmes Norton introduced an alcohol tax equalization in the House of Representatives in 1997 (United States Congress, 1997).

price-tier spirits of \$0.99 was even less than the corresponding price in beer.[22] These substitutions result in a considerably smaller effect on ethanol consumption than would be anticipated based on the magnitude of the tax increases.

Even if the federal rates of beverage-specific ethanol taxation were the same, the total rates of taxation would not be equal unless each state adopted the same policy. Moreover, since these taxes are specific, as opposed to ad valorem excise taxes, the price of ethanol relative to other goods would fall over time absent adjustments for inflation. That happened during most of the last half of the twentieth century. As pointed out above, it can lead to perhaps undesirable shifts in the pattern of ethanol consumption if some beverages are better substitutes for non-alcoholic items than others.

One other approach, which is a departure from the existing specific taxation system, is for future tax increases to be a percentage of the wholesale price. With an ad valorem tax the relative prices of beer, wine, and spirits would not change and thus there would be no new tax induced substitution. An ad valorem tax would also increase revenue along with inflation but would put a lower tax on cheap alcohol because it would increase the price difference between more and less expensive alcohol, and that difference is what determines incentives to search and switch. Minimum pricing legislation could offset this problem with cheap alcohol but does not address effects due to differences in the rate at which an ounce of ethanol in each beverage is taxed.

A recent paper by Griffith et al. (2019) bears on the issue just mentioned. They conduct a penetrating analysis of whether uniform ethanol taxation is a second-best solution to correct for the external costs associated with the consumption of alcohol. The first-best solution of a tax on each consumer equal to the marginal external cost of his or her consumption clearly is not feasible. To determine the optimal second-best solution, they assume that external costs depend solely on ethanol consumption per adult and increase as this measure increases. They combine this with empirical evidence that a large percentage of external costs are accounted for by heavy drinkers. Hence, they divide households in the United Kingdom into five quintiles with each quintile accounting for 20% of weekly drinks purchased. Based on the UK definition of excessive consumption as of that greater than eight "standard drinks" a week, where a standard drink contains 8 grams (approximately 0.3 ounces) of ethanol (6 ounces of beer, 2.5 ounces of wine, or 0.8 ounces of

[22]This reflects the lower cost of producing an ounce of spirits relative to an ounce of beer.

spirits), the top three quintiles who consume 60% of the ethanol are heavy drinkers.[23]

If the externality function is linear in ethanol consumption, the marginal external cost of ethanol is constant. In that case, the 19% of households who account for 60% of consumption also account for 60% of external costs. In that case, uniform ethanol taxation is the optimal second-best solution. But if the externality function is convex (marginal cost rises as consumption rises or heavy drinkers cause more than 60% of the external costs), Griffith et al. (2019) conclude that it is optimal to impose different tax rates on them than the rates imposed on the consumption of other (light) drinkers. That follows if the types of beverages consumed by heavy drinkers and the ethanol content of those beverages vary by category of drinker and if the alcoholic content of those beverages vary. The rates also depend on own and cross price elasticities of demand for beverages that differ not only by type and ethanol content but also by quintile of the drinking distribution.

After obtaining the relevant elasticities Griffith et al. (2019), conclude that the optimal tax on strong spirits (ethanol content greater than 20%) is the highest. That is because heavy drinkers prefer strong spirits, are reasonably price sensitive, and substitute towards beverages that contain less alcohol when the price of strong spirits rises. Somewhat counterintuitively but again based on the behavior of heavy drinkers, low ethanol beer and wine are taxed at higher rates than high ethanol beer and wine, respectively. They also conclude that a uniform tax policy would improve welfare when compared to the current U.K. system.

In interpreting these results, one should keep in mind that they depend on obtaining price elasticities from data for a single year and on an extremely complicated estimation strategy that involves simulation methods and instrumental variables for a wide variety of prices. Not only do the elasticities vary by beverage type and ethanol content, but they also vary by quintiles of the drinking frequency distribution. They also depend on rough estimates of external costs and of the percentage of these costs attributed to heavy drinkers. Finally, they do not include such measures of misuse of alcohol as drinking after driving in ethanol consumption, although Griffith et al. (2019) present data that heavy use and misuse are positively correlated.

These comments are not intended as criticisms of the study just discussed. Instead, we presented them and the detailed discussion of the study to compare it to our own to highlight areas for future research. Unlike Griffith and colleagues, we exploit a

[23]The U.S. definition of heavy drinking (National Institute on Alcohol Abuse and Alcoholism, 2020b) is gender-specific: more than seven standard drinks a week for females and more than fourteen drinks a week for males. It also considers a standard drink as one that contains 0.6 ounces of ethanol (12 ounces of beer, 5 ounces of wine, or 1.5 ounces of distilled spirits). Based on that definition, there are many more heavy drinkers in the U.K. than in the U.S.

quasi-natural experiment that involved large excise tax hikes that altered relative alcoholic beverage prices to examine the types of substitutions made by drinkers. Our finding that sales of cheap spirits rose is related to the high prevalence of low prices of spirits among heavy drinkers in their data. We cannot investigate that in our data, which contains sales by stores as opposed to purchases by households. Along the same lines, they do not report what happens to observed prices of spirits or consumptions of cheap distilled spirits when prices rise. Their findings provide some support for the U.S. tax system that taxes the ethanol in an ounce of spirits at a higher rate than the ethanol in an ounce of wine or beer.

These comments point to important topics on an agenda for future research. Our results are based on a single quasi-natural experiment. They should be extended to situations in which other U.S. states raise alcohol taxes to increase revenue. Our methodology also should be applied to the kind of data used by Griffith and colleagues so that more detailed information can be obtained on the behavior of consumers. These studies will contribute to a more precise formulation of the optimal set of tax rates to address the external costs of ethanol consumption should that policy tool be added to the arsenal of those used to achieve this aim. And even if it is not, the studies will inform policymakers of the potential health impacts of potential revenue-increasing excise tax hikes.

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Tables and Figures

Table 1: *Table of Means: Spirits and Beer*

	Pre-treatment			Post-treatment		
Panel A: Spirits Outcome Measures						
Variables	Control	Illinois	Difference	Control	Illinois	Difference
Gallon-Price (in $)	80.22	79.98	0.23	79.21	85.65	-6.44
Gallon Sales (per Store)	161.58	144.94	16.64	165.70	146.51	19.18
Gallon-Price (Low-End)	43.46	43.71	-0.25	43.37	50.00	-6.63
Gallon-Price (Mid-Tier)	81.03	81.14	-0.11	79.62	85.36	-5.74
Gallon-Price (High-End)	151.94	148.10	3.84	150.37	153.18	-2.81
Gallon Sales (Low-End)	78.06	66.20	11.86	82.53	59.59	22.94
Gallon Sales (Mid-Tier)	62.71	58.00	4.71	63.00	65.03	-2.03
Gallon Sales (High End)	20.81	20.74	0.06	20.19	21.55	-1.36
Panel B: Beer and Total Ethanol Sales						
Gallon Sales (Beer)	1121.00	1630.01	-509.00	1091.43	1655.56	-564.14
Gallon Sales (Ethanol)	152.43	184.59	-32.16	153.48	186.13	-32.65

Panel C: Spirits Basket Characteristics			
Share 375ml	9.80	Share Canadian Whiskey	7.84
Share 750ml	64.22	Share Cordials	9.80
Share 1750ml	25.98	Share Gin	8.33
Share <$45 (Low-End)	15.69	Share Irish Whiskey	1.47
Share $45-$95 (Mid)	47.06	Share Rum	12.75
Share >$95 (High-End)	37.25	Share Scotch	7.84
Share Bourbon (Blended)	1.47	Share Tequila	8.33
Share Bourbon (Straight)	9.80	Share Vodka	25.98
Share Brandy/Cognac	6.37		

Panel D: Spirits Store Characteristics			
Share Grocery Stores	67.33	Share Liquor Stores	4.21
Share Drug Stores	24.65	Share Convenience Stores	1.49
Share Mass Market Stores	2.32		

Table 2: *Table of Means: Wine*

	Pre-treatment			Post-treatment		
Panel A: Outcome Measures						
Variables	Control	Illinois	Difference	Control	Illinois	Difference
Gallon-Price (in $)	34.44	35.85	-1.41	34.75	37.02	-2.27
Gallon Sales (per Store)	214.97	329.75	-114.78	223.74	328.97	-105.23
Gallon-Price (Low-End)	15.45	14.66	0.79	15.94	16.33	-0.39
Gallon-Price (Mid-Tier)	31.19	32.10	-0.91	30.77	31.81	-1.04
Gallon-Price (High-End)	65.70	66.98	-1.29	65.44	69.67	-4.23
Gallon Sales (Low-End)	103.71	154.46	-50.76	103.46	137.48	-34.02
Gallon Sales (Mid-Tier)	79.29	126.16	-46.86	86.38	139.84	-53.46
Gallon Sales (High End)	31.97	49.13	-17.15	34.01	51.43	-17.42

Panel B: Characteristics Top 252 Products:			
Share 187ml	0.04	Share Domestic	0.71
Share 750ml	0.58	Share Imported	0.21
Share 1500ml	0.29	Share Kosher	0.01
Share 3000ml	0.02	Share Sangria	0.01
Share 4000ml	0.02	Share Sparkling	0.06
Share 5000ml	0.05	Share <$30 (Low-End)	0.49
		Share >$50 (High-End)	0.15
Panel C: Store Characteristics			
Share Grocery Stores	64.33	Share Liquor Stores	4.21
Share Drug Stores	24.65	Share Convenience Stores	1.49
Share Mass Market Stores	2.32		

Table 3: *Regression Results –Spirits Prices*

	(1) **Main**	(2) **Donut**	(3) **Synth**	(4) **Synth-Donut**
$Treat_{jt}$	6.623***	6.717***	6.521***	6.635***
Cluster SEs	(0.438)	(0.451)	-	-
Donald-Lang SEs	(0.131)	(0.117)	(0.144)	(0.121)
DL - Newey - West SEs	(0.268)	(0.181)	(0.239)	(0.201)
Observations	903,840	869,408	210	202
R-squared	0.788	0.787	0.908	0.938
Store-Fixed-Effects	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No
Seasonal Adj.	Yes	Yes	Yes	Yes

Notes: Cluster Standard Errors: The dependent variable is the average sprits price in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table 4: *Regression Results – (Log) Spirit Sales*

	(1) **Main**	(2) **Donut**	(3) **Synth**	(4) **Synth-Donut**
$Treat_{jt}$	-0.035***	-0.033***	-0.043***	-0.042***
Cluster SEs	(0.008)	(0.009)	-	-
Donald-Lang SEs	(0.008)	(0.007)	(0.006)	(0.006)
DL - Newey - West SEs	(0.008)	(0.007)	(0.006)	(0.006)
Observations	903,840	869,408	210	202
R-squared	0.951	0.950	0.193	0.210
Store-Fixed-Effects	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No
Seasonal Adj.	Yes	Yes	Yes	Yes

Notes: Cluster Standard Errors: The dependent variable is the natural logarithm of total sprits gallon sales in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table 5: *Regression Results – Wine Prices*

	(1) **Main**	(2) **Donut**	(3) **Synth**	(4) **Synth-Donut**
$Treat_{jt}$	0.842***	0.856***	0.786***	0.799***
Cluster SEs	(0.145)	(0.149)	-	-
Donald-Lang SEs	(0.049)	(0.086)	(0.044)	(0.045)
DL - Newey - West SEs	(0.084)	(0.049)	(0.074)	(0.076)
Observations	1,964,760	1,889,912	210	202
R-squared	0.814	0.813	0.602	0.607
Store-Fixed-Effects	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No
Seasonal Adj.	Yes	Yes	Yes	Yes

Notes: Cluster Standard Errors: The dependent variable is the average wine price in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table 6: *Regression Results – (Log) Wine Sales*

	(1) **Main**	(2) **Donut**	(3) **Synth**	(4) **Synth-Donut**
$Treat_{jt}$	-0.030***	-0.029***	-0.042***	-0.042***
Cluster SEs	(0.009)	(0.009)	-	-
Donald-Lang SEs	(0.006)	(0.006)	(0.004)	(0.004)
DL - Newey - West SEs	(0.006)	(0.006)	(0.005)	(0.005)
Observations	1,964,760	1,889,912	210	202
R-squared	0.967	0.967	0.340	0.341
Store-Fixed-Effects	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No
Seasonal Adj.	Yes	Yes	Yes	Yes

Notes: Cluster Standard Errors: The dependent variable is the natural logarithm of total wine gallon sales in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table 7: *Regression Results – Substitution Across Price Segments*

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Spirits Results						
	Spirits Prices			**Log Spirits Sales**		
	Bottom	Middle	Top	Bottom	Middle	Top
$Treat_{jt}$	6.322***	5.558***	6.533***	0.041**	-0.019*	0.012
Cluster SEs	(0.161)	(0.502)	(0.811)	(0.019)	(0.010)	(0.010)
Donald-Lang SEs	(0.097)	(0.180)	(0.309)	(0.012)	(0.009)	(0.013)
DL - Newey - West SEs	(0.157)	(0.293)	(0.445)	(0.016)	(0.012)	(0.016)
Panel B: Wine Results						
	Wine Prices			**Log Wine Sales**		
	Bottom	Middle	Top	Bottom	Middle	Top
$Treat_{jt}$	1.163***	0.128**	2.921***	-0.072***	-0.029**	-0.049**
Cluster SEs	(0.110)	(0.055)	(0.560)	(0.025)	(0.013)	(0.018)
Donald-Lang SEs	(0.025)	(0.064)	(0.133)	(0.009)	(0.006)	(0.012)
DL - Newey - West SEs	(0.041)	(0.064)	(0.204)	(0.013)	(0.007)	(0.016)
Store-Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month-Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal Adj.	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Cluster Standard Errors: The dependent variable is the average spirits (Panel A) or wine price (Panel B) and the natural logarithm of total spirits/wine gallon sales in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table 8: *Regression Results – (Log) Beer Sales*

	(1) Main	(2) Donut	(3) Synth	(4) Synth-Donut
$Treat_{jt}$	0.055***	0.056***	0.026***	0.026***
Cluster SEs	(0.014)	(0.015)	-	-
Donald-Lang SEs	(0.016)	(0.017)	(0.009)	(0.009)
DL - Newey - West SEs	(0.027)	(0.028)	(0.012)	(0.012)
Observations	879,270	845,774	210	202
R-squared	0.917	0.917	0.038	0.040
Store-Fixed-Effects	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No
Seasonal Adj.	Yes	Yes	Yes	Yes

Notes: Cluster Standard Errors: The dependent variable is the natural logarithm of total beer gallon sales in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level

Table 9: *Regression Results – (Log) Ethanol Sales*

	(1) Main	(2) Donut	(3) Synth	(4) Synth-Donut
$Treat_{jt}$	0.003	0.004	-0.021***	-0.021***
Cluster SEs	(0.010)	(0.010)	-	-
Donald-Lang SEs	(0.010)	(0.010)	(0.007)	(0.007)
DL - Newey - West SEs	(0.013)	(0.013)	(0.009)	(0.009)
Observations	879,270	845,774	210	202
R-squared	0.934	0.933	0.044	0.048
Store-Fixed-Effects	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No
Seasonal Adj.	Yes	Yes	Yes	Yes

Notes: Cluster Standard Errors: The dependent variable is the natural logarithm of total ethanol gallon sales in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table 10: *Regression Results – Cross-Border Shopping and the Role of Cook County*

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Spirits Results						
		Spirits Prices			**Log Spirits Sales**	
$Treat_{jt}$	6.527***	6.662***	6.539***	-0.035**	-0.035***	-0.039***
Cluster SEs	(0.438)	(0.438)	(0.438)	(0.008)	(0.008)	(0.008)
Donald-Lang SEs	(0.141)	(0.136)	(0.128)	(0.007)	(0.008)	(0.008)
DL - Newey - West SEs	(0.220)	(0.216)	(0.202)	(0.008)	(0.008)	(0.008)
Panel B: Wine Results						
		Wine Prices			**Log Wine Sales**	
$Treat_{jt}$	0.766***	0.873***	0.837***	-0.008	-0.039***	-0.022**
Cluster SEs	(0.145)	(0.145)	(0.145)	(0.009)	(0.009)	(0.009)
Donald-Lang SEs	(0.055)	(0.050)	(0.049)	(0.006)	(0.007)	(0.006)
DL - Newey - West SEs	(0.090)	(0.085)	(0.081)	(0.006)	(0.006)	(0.006)
Observations	854,700	884,310	872,760	854,700	884,310	872,760
R-squared	0.791	0.789	0.789	0.951	0.950	0.951
Store-Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month-Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal Adj.	Yes	Yes	Yes	Yes	Yes	Yes
Counties	Inner	Border	No Cook	Inner	Border	No Cook

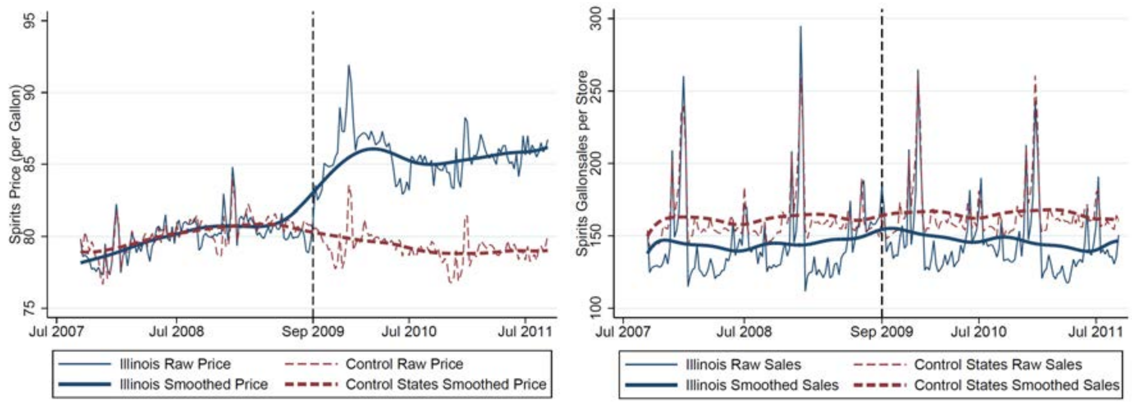
Notes: Cluster Standard Errors: The dependent variable is the average spirits (Panel A) or wine price (Panel B) and the natural logarithm of total spirits/wine gallon sales in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table 11: *Change in Alcohol Excise Tax Revenue per Store After the 2009 Tax Increases*

	Old Tax Rate	**Old Sales***	**Revenue**	**New Tax Rate**	**New Sales***	**Revenue**	**Difference**
Spirits Revenue	$4.50	144.94	$652.23	$8.55	146.51	$1252.66	$600.43
Wine Revenue	$0.73	329.75	$240.72	$1.39	328.97	$457.27	$216.55
Beer Revenue	$0.19	1,545.03	$293.56	$0.23	1,564.82	$359.91	$66.35
Total							$883.33

* Data from Tables 1 and 2.

Figure 1: Trends in Spirits Prices and Sales

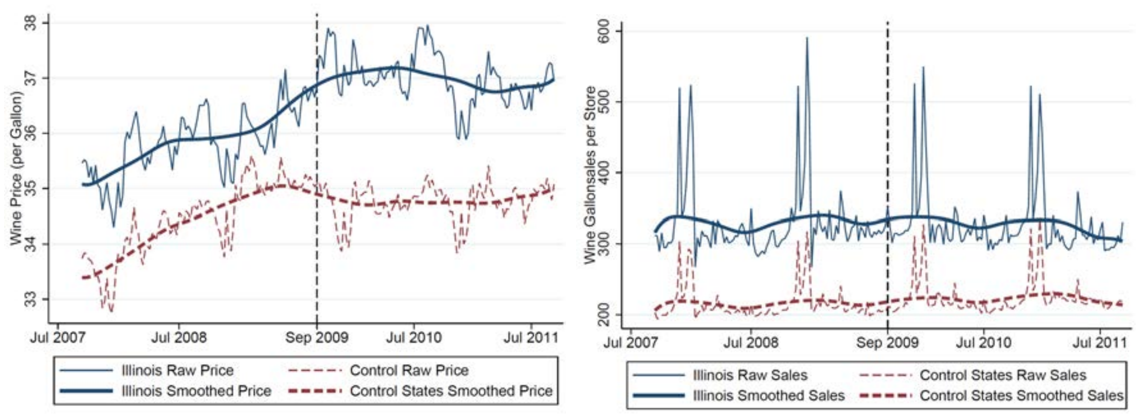


(a) Panel A

(b) Panel B

Notes: These graph displays store-averages in spirits prices (in \$) and gallon sales, aggregated to the treatment state (Illinois) and control state (all other states) levels. In these calculations, each store received the same weight. The thin lines display the raw data (no seasonal adjustments are made) and are overlaid with thicker Locally Weighted Scatterplot Smoothing (LOWESS) lines.

Figure 2: Trends in Wine Prices and Sales

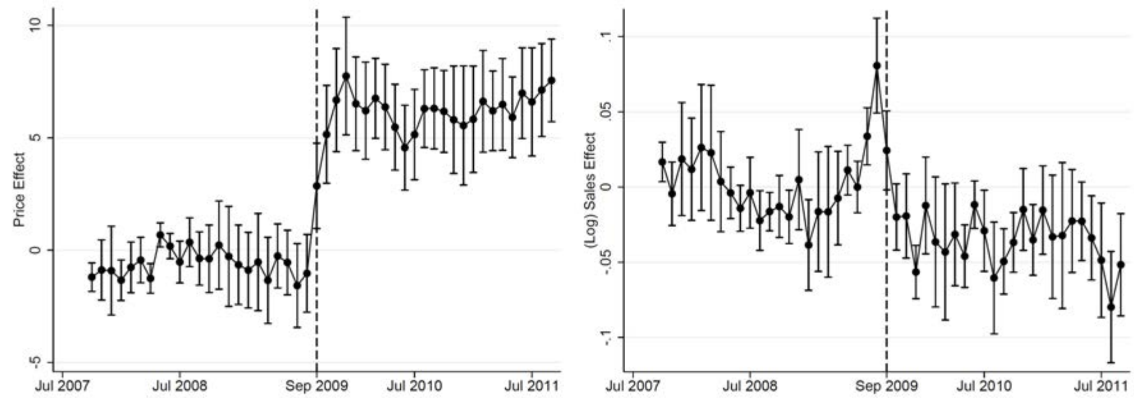


(a) Panel A

(b) Panel B

Notes: These graph displays store-averages in wine prices (in \$) and gallon sales, aggregated to the treatment state (Illinois) and control state (all other states) levels. In these calculations, each store received the same weight. The thin lines display the raw data (no seasonal adjustments are made) and are overlaid with thicker Locally Weighted Scatterplot Smoothing (LOWESS) lines.

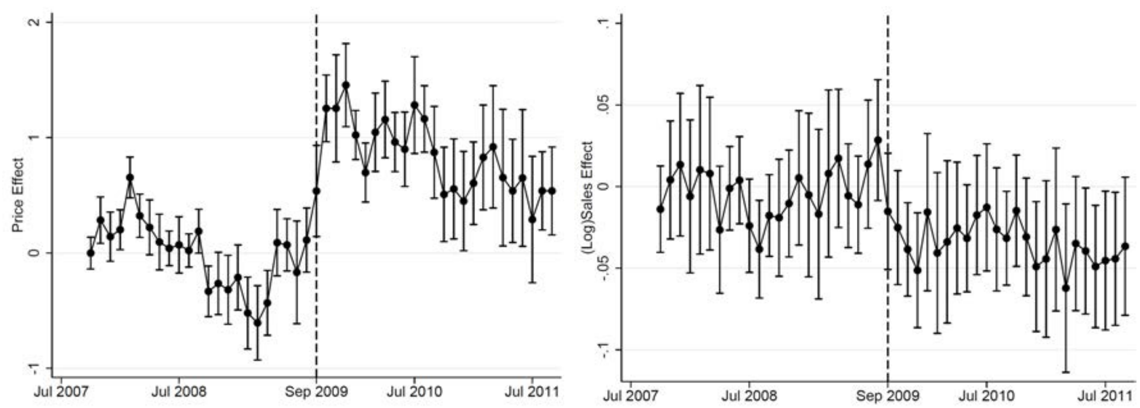
Figure 3: Event-Study Results - Spirits



(a) Panel A - Spirits Price (b) Panel B - Sprits (Log) Sales

Notes: These figures show the results of the event-study specification as specified in equation (2). That is, each dot is the point estimate of a month-year fixed effect interacted with our treatment state indicator. Confidence intervals are adjusted for clustering at the state level. The reference period is the first week of September 2007. The dependent variable are spirits price and log spirits sales at the store-week level.

Figure 4: Event-Study Results - Wine

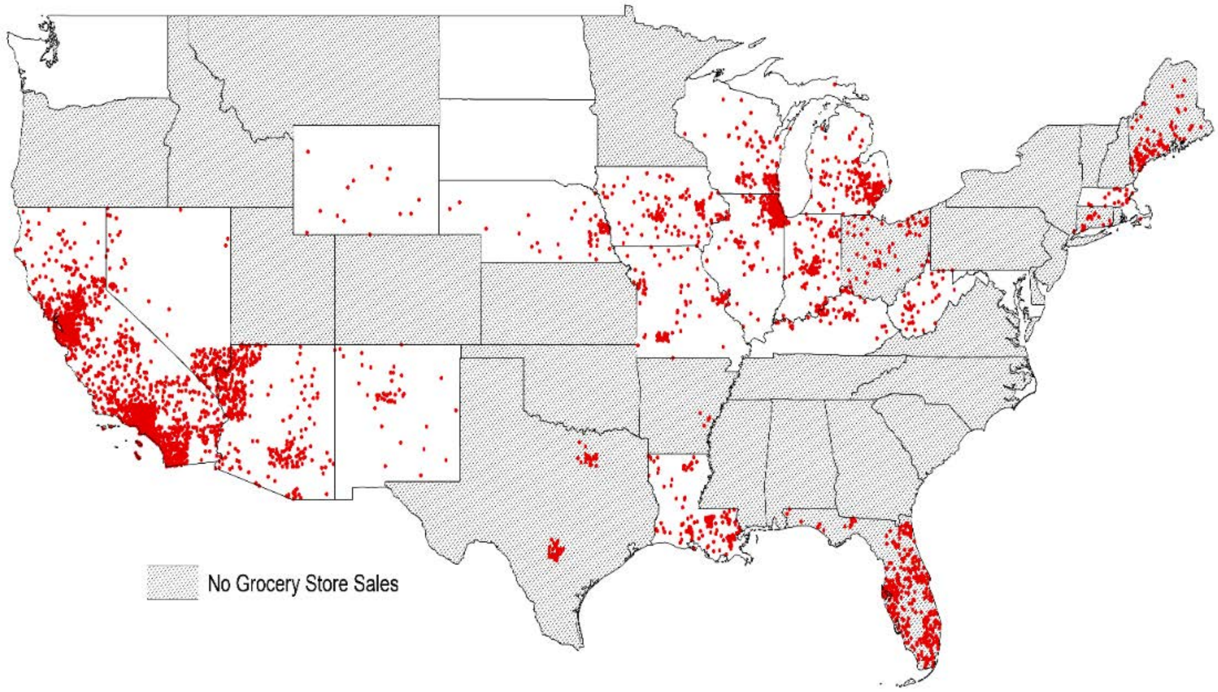


(a) Panel A - Wine Price (b) Panel B - Wine (Log) Sales

Notes: These figures show the results of the event-study specification as specified in equation (2). That is, each dot is the point estimate of a month-year fixed effect interacted with our treatment state indicator. Confidence intervals are adjusted for clustering at the state level. The reference period is the first week of September 2007. The dependent variable are wine price and log wine sales at the store-week level.

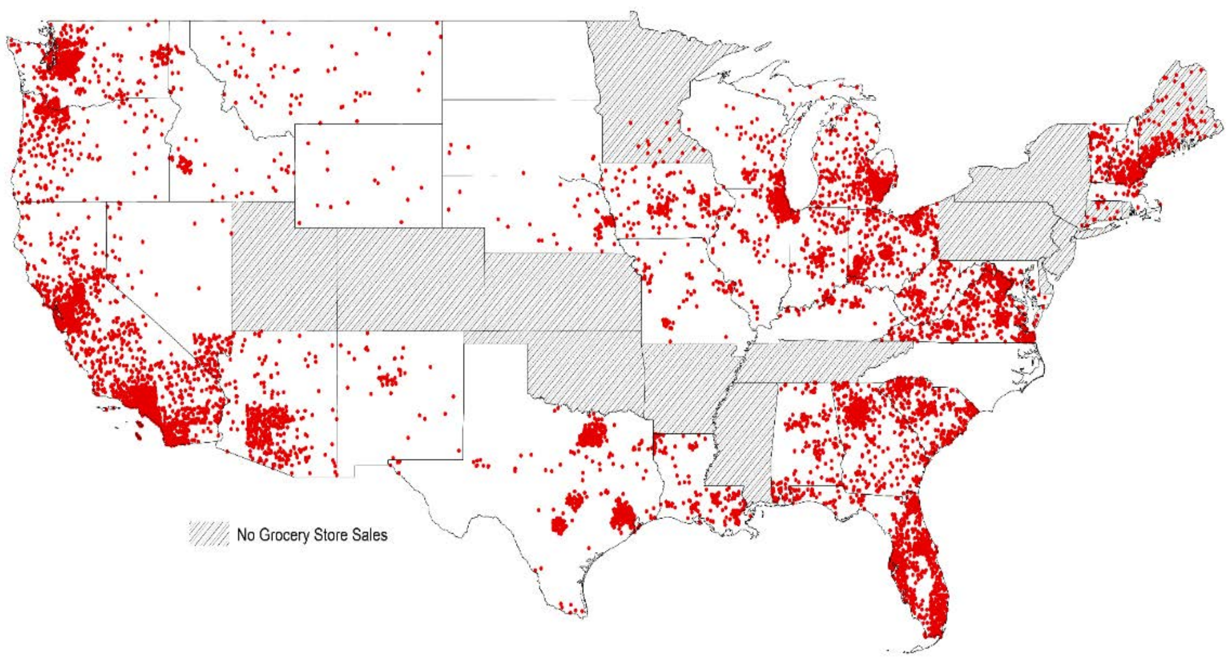
Appendix

Figure A1: Spirits Stores



Notes: This Figure illustrates the geographical distribution of the 4,304 stores in our balanced sample that sell spirits. Shaded states do not allow spirits sales in grocery stores, but in some cases loopholes exist and the data indicate that such sales occur. Each dot represents a store. Dots are not set at the exact geo-location of a store, but at a random point in the county of the store. State of Washington currently allows spirits sales in grocery stores but did not in 2009.

Figure A2: Wine Stores



Notes: This Figure illustrates the geographical distribution of the 9,356 stores in our balanced sample that sell wine. Shaded states do not allow wine sales in grocery stores, but in some cases loopholes exist and the data indicate that such sales occur. Each dot represents a store. Dots are not set at the exact geo-location of a store, but at a random point in the county of the store.

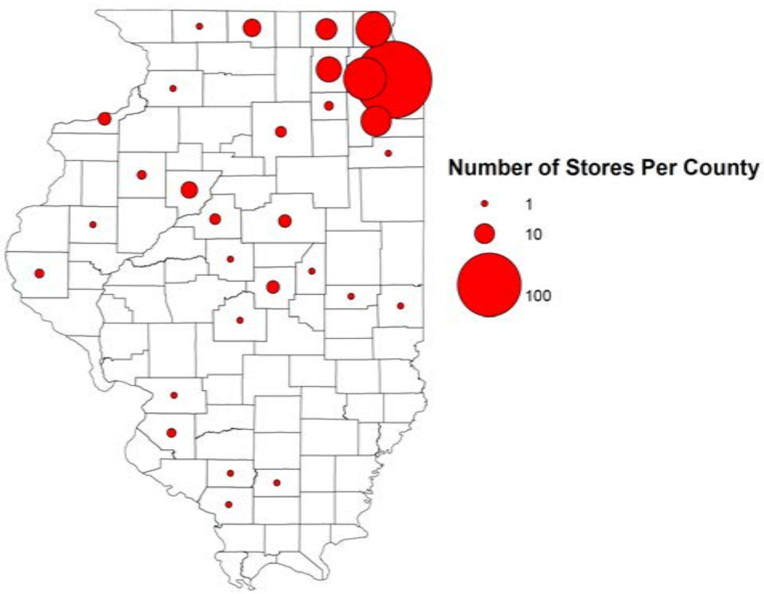


Figure A3: Spirits Stores in Illinois

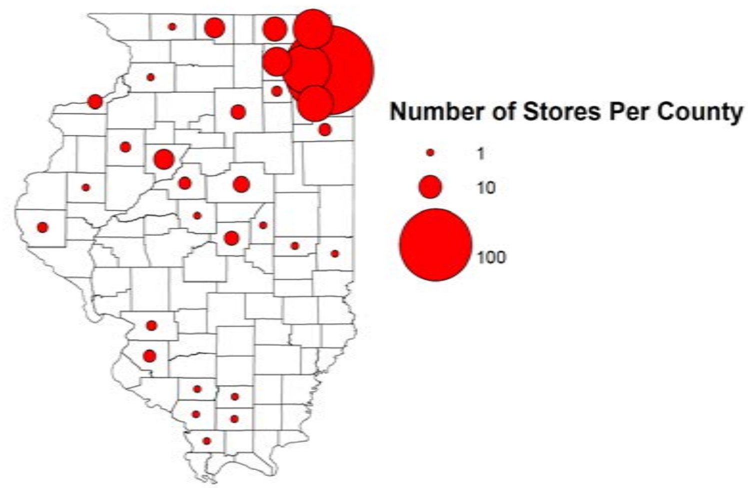


Figure A4: Wine Stores in Illinois

Notes: Figures A3 and A4 illustrate the geographical distribution of stores that sell spirits and wine, respectively across Illinois. Both maps indicate that Nielsen stores are primarily sampled in urban areas, in the case of Illinois they are concentrated in the Chicago metropolitan area.

Table A1: *Spirits Prices - Regression Results without seasonal adjustment*

	(1) **Main**	(2) **Donut**	(3) **Synth**	(4) **Synth-Donut**
$Treat_{jt}$	6.616***	6.737***	6.521***	6.635***
Cluster SEs	(0.438)	(0.451)	-	-
Donald-Lang SEs	(0.159)	(0.136)	(0.144)	(0.121)
DL - Newey - West SEs	(0.268)	(0.231)	(0.239)	(0.201)
Observations	903,840	869,408	210	202
R-squared	0.790	0.789	0.908	0.938
Store-Fixed-Effects	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No
Seasonal Adj.	No	No	No	No

Notes: Cluster Standard Errors: The dependent variable is the average sprits price in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table A2: *Regression Results for Log Spirits Prices*

	Seasonally Adjusted				Not Seasonally Adjusted			
	(1) **Main**	(2) **Donut**	(3) **Synth**	(4) **Synth-Donut**	(5) **Main**	(6) **Donut**	(7) **Synth**	(8) **Synth-Donut**
$Treat_{jt}$	0.082***	0.083***	0.078***	0.079***	0.082***	0.083***	0.081***	0.082***
Cluster SEs	(0.006)	(0.005)	-	-	(0.006)	(0.006)	-	-
Donald-Lang SEs	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
DL - Newey - West SEs	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Observations	903,840	869,408	210	202	903,840	869,408	210	202
R-squared	0.784	0.848	0.915	0.928	0.786	0.786	0.912	0.936
Store-Fixed-Effects	Yes	Yes	No	No	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No	Yes	Yes	No	No
Seasonal Adj.	Yes	Yes	Yes	Yes	No	No	No	No

Notes: Cluster Standard Errors: The dependent variable is the natural log of the average spirits price in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4) as well as (7) and (8), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.

***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table A3: *Log Spirits Sales (Not seasonally adjusted)*

	(1) **Main**	(2) **Donut**	(3) **Synth**	(4) **Synth-Donut**
$Treat_{jt}$	-0.035***	-0.033***	-0.032***	-0.032***
Cluster SEs	(0.008)	(0.009)	-	-
Donald-Lang SEs	(0.006)	(0.006)	(0.006)	(0.006)
DL - Newey - West SEs	(0.009)	(0.009)	(0.009)	(0.009)
Observations	903,840	869,408	210	202
R-squared	0.951	0.950	0.112	0.112
Store-Fixed-Effects	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No
Seasonal Adj.	No	No	No	No

Notes: Cluster Standard Errors: The dependent variable is the natural logarithm of total spirits gallon sales in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table A4: *Regression Results - Spirits Sales (Levels)*

	Seasonally Adjusted				Not Seasonally Adjusted			
	(1) **Main**	(2) **Donut**	(3) **Synth**	(4) **Synth-Donut**	(5) **Main**	(6) **Donut**	(7) **Synth**	(8) **Synth-Donut**
$Treat_{jt}$	-2.681***	-2.629**	-2.647**	-2.617**	-2.547**	-2.474*	-2.889**	-2.924***
Cluster SEs	(1.212)	(1.253)	-	-	(1.212)	(1.253)	-	-
Donald-Lang SEs	(1.506)	(1.482)	(1.172)	(1.127)	(1.923)	(1.871)	(1.148)	(1.094)
DL - Newey - West SEs	(1.584)	(1.492)	(1.671)	(1.634)	(2.814)	(2.718)	(1.680)	(1.640)
Observations	903,840	869,408	210	202	903,840	869,408	210	202
R-squared	0.920	0.919	0.024	0.026	0.921	0.920	0.030	0.034
Store-Fixed-Effects	Yes	Yes	No	No	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No	Yes	Yes	No	No
Seasonal Adj.	Yes	Yes	Yes	Yes	No	No	No	No

Notes: Cluster Standard Errors: The dependent variable are total sprits gallon sales in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4) as well as (7) and (8), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table A5: *Wine Prices (Not seasonally adjusted)*

	(1) **Main**	(2) **Donut**	(3) **Synth**	(4) **Synth-Donut**
$Treat_{jt}$	0.852***	0.862***	0.934***	0.943***
Cluster SEs	(0.145)	(0.149)	-	-
Donald-Lang SEs	(0.052)	(0.054)	(0.044)	(0.045)
DL - Newey - West SEs	(0.092)	(0.095)	(0.074)	(0.076)
Observations	1,964,760	1,889,912	210	202
R-squared	0.814	0.814	0.683	0.682
Store-Fixed-Effects	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No
Seasonal Adj.	No	No	No	No

Notes: Cluster Standard Errors: The dependent variable is the average wine price in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table A6: *Regression Results - Log Wine Prices*

	Seasonally Adjusted				Not Seasonally Adjusted			
	(1) **Main**	(2) **Donut**	(3) **Synth**	(4) **Synth-Donut**	(5) **Main**	(6) **Donut**	(7) **Synth**	(8) **Synth-Donut**
$Treat_{jt}$	0.022***	0.023**	0.019***	0.019***	0.023***	0.023***	0.026***	0.026***
Cluster SEs	(0.004)	(0.004)	-	-	(0.004)	(0.004)	-	-
Donald-Lang SEs	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)
DL - Newey - West SEs	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
Observations	1,964,760	1,889,912	210	202	1,964,760	1,889,912	210	202
R-squared	0.820	0.820	0.520	0.520	0.821	0.821	0.681	0.681
Store-Fixed-Effects	Yes	Yes	No	No	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No	Yes	Yes	No	No
Seasonal Adj.	Yes	Yes	Yes	Yes	No	No	No	No

Notes: Cluster Standard Errors: The dependent variable is the natural log of the average wine price in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4) as well as (7) and (8), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table A7: *Log Wine Sales (Not seasonally adjusted)*

	(1) **Main**	(2) **Donut**	(3) **Synth**	(4) **Synth-Donut**
$Treat_{jt}$	-0.030***	-0.030***	-0.002***	-0.001***
Cluster SEs	(0.009)	(0.009)	-	-
Donald-Lang SEs	(0.007)	(0.007)	(0.004)	(0.004)
DL - Newey - West SEs	(0.009)	(0.009)	(0.004)	(0.004)
Observations	1,964,760	1,889,912	210	202
R-squared	0.967	0.967	0.001	0.001
Store-Fixed-Effects	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No
Seasonal Adj.	No	No	No	No

Notes: Cluster Standard Errors: The dependent variable is the natural logarithm of total wine gallon sales in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table A8: *Regression Results - Wine Sales (Levels)*

	Seasonally Adjusted				Not Seasonally Adjusted			
	(1) **Main**	(2) **Donut**	(3) **Synth**	(4) **Synth-Donut**	(5) **Main**	(6) **Donut**	(7) **Synth**	(8) **Synth-Donut**
$Treat_{jt}$	-9.087***	-9.024**	-12.630***	-12.594***	-9.549***	-9.950***	-3.819**	-3.850***
Cluster SEs	(2.571)	(2.688)	-	-	(2.571)	(2.668)	-	-
Donald-Lang SEs	(3.417)	(3.519)	(2.140)	(2.179)	(4.019)	(4.162)	(1.562)	(1.571)
DL - Newey - West SEs	(3.083)	(3.154)	(2.363)	(2.408)	(5.414)	(5.618)	(1.443)	(1.447)
Observations	1,964,760	1,889,912	210	202	1,964,760	1,889,912	210	202
R-squared	0.935	0.934	0.143	0.143	0.936	0.935	0.028	0.029
Store-Fixed-Effects	Yes	Yes	No	No	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No	Yes	Yes	No	No
Seasonal Adj.	Yes	Yes	Yes	Yes	No	No	No	No

Notes: Cluster Standard Errors: The dependent variable are total wine gallon sales in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4) as well as (7) and (8), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table A9: *Log Beer Sales (Not seasonally adjusted)*

	(1) **Main**	(2) **Donut**	(3) **Synth**	(4) **Synth-Donut**
$Treat_{jt}$	0.057***	0.059***	0.032***	0.032***
Cluster SEs	(0.014)	(0.015)	-	-
Donald-Lang SEs	(0.014)	(0.014)	(0.008)	(0.007)
DL - Newey - West SEs	(0.023)	(0.023)	(0.011)	(0.009)
Observations	879,270	845,774	210	202
R-squared	0.920	0.920	0.076	0.084
Store-Fixed-Effects	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No
Seasonal Adj.	No	No	No	No

Notes: Cluster Standard Errors: The dependent variable is the natural logarithm of total beer gallon sales in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table A10: *Log Ethanol Sales (Not seasonally adjusted)*

	(1) **Main**	(2) **Donut**	(3) **Synth**	(4) **Synth-Donut**
$Treat_{jt}$	0.004	0.005	-0.017***	-0.018***
Cluster SEs	(0.010)	(0.010)	-	-
Donald-Lang SEs	(0.011)	(0.011)	(0.006)	(0.005)
DL - Newey - West SEs	(0.017)	(0.017)	(0.008)	(0.007)
Observations	879,270	845,774	210	202
R-squared	0.934	0.934	0.043	0.056
Store-Fixed-Effects	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No
Seasonal Adj.	No	No	No	No

Notes: Cluster Standard Errors: The dependent variable is the natural logarithm of total ethanol sales (in gallons) in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table A11: *Regression Results - Beer Sales (Levels)*

	Seasonally Adjusted				Not Seasonally Adjusted			
	(1) **Main**	(2) **Donut**	(3) **Synth**	(4) **Synth-Donut**	(5) **Main**	(6) **Donut**	(7) **Synth**	(8) **Synth-Donut**
$Treat_{jt}$	50.549***	51.250***	15.021	13.081	55.143***	56.343***	60.790***	59.732***
Cluster SEs	(11.253)	(11.350)	-	-	(11.253)	(11.350)	-	-
Donald-Lang SEs	(25.409)	(25.635)	(22.265)	(22.294)	(34.246)	(34.193)	(15.223)	(14.084)
DL - Newey - West SEs	(25.082)	(24.155)	(26.635)	(26.024)	(51.941)	(51.404)	(20.659)	(17.526)
Observations	879,270	845,774	210	202	879,270	845,774	210	202
R-squared	0.836	0.835	0.002	0.002	0.843	0.842	0.071	0.083
Store-Fixed-Effects	Yes	Yes	No	No	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No	Yes	Yes	No	No
Seasonal Adj.	Yes	Yes	Yes	Yes	No	No	No	No

Notes: Cluster Standard Errors: The dependent variable are total beer gallon sales in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4) as well as (7) and (8), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table A12: *Regression Results - Ethanol Sales (Levels)*

	(1) **Main**	(2) **Donut**	(3) **Synth**	(4) **Synth-Donut**	(5) **Main**	(6) **Donut**	(7) **Synth**	(8) **Synth-Donut**
$Treat_{jt}$	0.237	0.299	-0.587	-0.543	0.486	0.519	-1.022	-1.146
Cluster SEs	(1.537)	(1.594)	-	-	(1.537)	(1.594)	-	-
Donald-Lang SEs	(2.126)	(2.138)	(1.662)	(1.657)	(2.724)	(2.711)	(1.224)	(1.110)
DL - Newey - West SEs	(2.162)	(2.072)	(2.176)	(2.133)	(4.006)	(3.947)	(1.762)	(1.537)
Observations	879,270	845,774	210	202	879,270	845,774	210	202
R-squared	0.887	0.886	0.003	0.001	0.888	0.887	0.003	0.005
Store-Fixed-Effects	Yes	Yes	No	No	Yes	Yes	No	No
Year-Month-Fixed-Effects	Yes	Yes	No	No	Yes	Yes	No	No
Seasonal Adj.	Yes	Yes	Yes	Yes	No	No	No	No

Notes: Cluster Standard Errors: The dependent variable are total ethanol sales (in gallons) in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change. In columns (3) and (4) as well as (7) and (8), the dependent variable is the difference between Illinois state aggregates and a synthetic control state aggregate.
***/**/* indicate statistical significance at the 1%/5%/10%-level.

Table A13: *Regression Results – Cross border shopping and the role of Cook County (not seasonally adjusted)*

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Spirits Results						
		Spirits Prices			**Log Spirits Sales**	
$Treat_{jt}$	6.520***	6.655***	6.532***	-0.035**	-0.035***	-0.039***
Cluster SEs	(0.438)	(0.438)	(0.438)	(0.008)	(0.008)	(0.008)
Donald-Lang SEs	(0.175)	(0.161)	(0.162)	(0.010)	(0.011)	(0.011)
DL - Newey - West SEs	(0.296)	(0.268)	(0.277)	(0.016)	(0.017)	(0.018)
Panel B: Wine Results						
		Wine Prices			**Log Wine Sales**	
$Treat_{jt}$	0.775***	0.882***	0.846***	-0.008	-0.039***	-0.022**
Cluster SEs	(0.145)	(0.145)	(0.145)	(0.009)	(0.009)	(0.009)
Donald-Lang SEs	(0.057)	(0.054)	(0.051)	(0.007)	(0.007)	(0.007)
DL - Newey - West SEs	(0.096)	(0.094)	(0.088)	(0.009)	(0.009)	(0.010)
Observations	854,700	884,310	872,760	854,700	884,310	872,760
R-squared	0.791	0.791	0.790	0.952	0.951	0.951
Store-Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month-Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal Adj.	No	No	No	No	No	No
Counties	Inner	Border	No Cook	Inner	Border	No Cook

Notes: Cluster Standard Errors: The dependent variable is the average spirits (Panel A) or wine price (Panel B) and the natural logarithm of total spirits/wine gallon sales in the store-week cell. $Treat_{jt}$ equals one for Illinois (treatment) stores from Sep 2009 on, and zero otherwise. SEs account for clustering at the state-level. Donald-Lang and DL-Newey-West: The dependent variable is the difference in the Illinois and the control states' store-average, by week. Sample size is 210 weeks. Newey-West method adjusts for first-order autocorrelation with maximum lag set at 3 weeks. Donut-specifications leave out 8 weeks around policy change.
***/**/* indicate statistical significance at the 1%/5%/10%-level.