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DISCUSSION PAPER SERIES

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Henry Saffer
NBER

Markus Gehrsitz
University of Strathclyde and IZA

Michael Grossman
City University of New York Graduate Center, NBER and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

The Effects of Alcohol Excise Tax Increases by Drinking Level and by Income Level*

The alcohol industry argues that alcohol excise taxes do not reduce heavy drinking because of substitutions to lower-cost products and that these taxes disproportionately burden low-income drinkers. Alternatively, some economists have argued that increases in alcohol excise taxes reduce heavy alcohol consumption. Using data from the Nielsen Homescan we investigate the effects of a large excise tax increase that raised alcohol prices. The results show that heavy drinkers reduce purchases, and this reduction is no different than the reductions by other drinkers. The results also show that only low-income drinkers pay more for ethanol after the tax increase.

JEL Classification: I18, H20
Keywords: alcohol, excise tax, heavy drinking, low income

Corresponding author:
Markus Gehrsitz
Department of Economics and Fraser of Allander Institute
University of Strathclyde
Glasgow
United Kingdom
E-mail: markus.gehrsitz@strath.ac.uk

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1. Introduction

Excessive alcohol consumption is costly to both the individual and the community. Sacks et al. (2015) report that most of the costs result from losses in workplace productivity. Other costs of excessive drinking include health care expenses, law enforcement and other criminal justice expenses, and damages and loss of life from motor vehicle crashes. Economists have argued that increases in alcohol excise taxes reduce heavy consumption and that taxation is justified by the cost to the community of alcohol abuse.[1] However, an alcohol industry review concludes that alcohol excise taxes do not reduce heavy drinking because of substitutions to lower cost products and that these taxes disproportionately burden low-income drinkers (International Alliance for Responsible Drinking, 2018). The effects of these excise taxes on heavy drinkers and low-income drinkers are the research questions addressed in this paper.

Alcohol is taxed at both the federal and state level. These taxes are generally excise taxes, which are based on volume rather than price. With some exceptions, federal tax rates are, for spirits $13.50 per gallon, for wine $1.07 per gallon and for beer $0.58 per gallon.[2] These rates were set in 1991 and their real value has eroded considerably since then. Most states also tax alcohol, although some maintain ownership of alcohol retail outlets and collect revenue directly from sales rather than taxes. These states are referred to as monopoly states. State excise tax rates are set by each state and vary for spirits, wine and beer.[3] Both at the federal and state level, the ethanol in beer and wine are taxed at a lower rate than the ethanol in spirits.[4] This differential somewhat offsets the lower costs of producing ethanol in the form of spirits. Between 2008 and 2018, eight states increased their alcohol excise taxes and two states changed their tax system. Four of these changes occurred in 2009, which is coincident with the decline in state revenue due to the 2008 recession.

[1] The sales tax is generally assumed not to be salient because it does not affect shelf prices and applies to most consumer goods (Chetty et al., 2009).

[2] Ethanol is the intoxicating ingredient in alcoholic beverages that distinguishes alcohol from other beverages. Alcohol refers to beer, wine and spirits while ethanol refers to the intoxicating ingredient in each of these beverage groups. Federal tax rates on spirits are pro-rated based on ethanol content, wines range up to $3.15 per gallon depending on the ethanol level, and sparkling wines are taxed at $3.40 per gallon.

[3] WA has the highest spirits tax rate at $14.25 and DC and MD have the lowest rate at $1.50 per gallon of 40% ethanol spirits. FL has the highest wine tax at $2.25 and TX has the lowest at $.20 per gallon of 12% ethanol. TN has the highest beer tax at $1.29 and WY has the lowest at $.02 per gallon of 5% ethanol.

[4] Alcohol refers to beer, wine, and spirits while ethanol refers to the active ingredient in each of these beverage groups.

We focus on the 2009 Illinois (IL) alcohol excise tax increases because they were large and were plausibly exogenous to the state's alcohol consumption level. This assertion of exogeneity is based on the state's financial situation consequent to the 2008 recession and the variety of other tax increases enacted at the same time. The changes in IL were a large percentage increase in the tax on spirits and wine and a trivial increase in the tax on beer. The tax changes in IL were substantial and introduced substitutions within and across beverage groups. However, the general effect of these tax increases was an increase in the price of alcohol. In reaction to tax increases, households can reduce alcohol purchases but can also make a variety of substitutions to mitigate the impact of the tax increase. The specific substitutions made can vary by drinking level and by income level. The goal of this paper is to provide new evidence on the effect of higher alcohol taxes on alcohol purchases and prices paid by heavy drinkers relative to other drinkers and how the tax affects purchases and prices paid by low-income households relative to higher-income households.

We rely on the Nielsen Homescan data set and directly estimate the change in purchases resulting from the large and exogenous increase in alcohol taxes in IL. The Nielsen data bypass problems with price data employed in prior studies. Our outcome measures are in units of ethanol for purchases and prices paid in dollars. Ethanol is the ingredient in alcoholic beverages that distinguishes them from other beverages. By using the ethanol content, we can add together purchases of spirits, wine, and beer. The research question regarding drinking level is the responsiveness of heavy drinkers relative to other drinkers. For this question, the primary outcome is purchases. The second question is the effect of the excise tax increase on low-income consumers. An excise tax is set independent of income and is therefore regressive. However, the effect of the tax by income level is a more complex question. We know that because of substitution, prices paid after an excise tax increase may not be higher than prices paid prior to the excise tax increase. We investigate the effect of the excise tax increase on prices paid by drinkers at different income levels.

2. Prior Studies

Many older studies of alcohol taxes or prices have serious data and methodological issues. A problem with taxes occurs because, as noted above, some states use state monopolies for the retail sale of wine or spirits. In these cases, state alcohol taxes are meaningless. Meaningful beer taxes are available for all but one state, but the dependent variable must be limited to beer. Previous price studies typically relied on problematic price data from a survey by the American Chamber of Commerce Research Association (ACCRA).[5] These data are reported by city and quarter but are based on only two or three observations in each city-quarter aggregate, which is likely to result in considerable measurement error (Ruhm et al. 2012). Previous studies also do not typically explain how the tax or price effects are identified.

More recent studies have shown that a given increase in an alcohol excise tax can result in a range of price changes for different products. Ally et al. (2014) examine the alcohol excise tax pass-through in the UK.[6] They find considerable heterogeneity in the pass-throughs with under shifting for products at the low end of the price distribution and over shifting for expensive products. Another study of pass-throughs is Conlon and Rao (2020) who examine the pass-through for excise tax increases on spirits in several states including IL. They find that when excise taxes are increased, some products have no price increase at all, but when prices are increased, they are increased by large increments such as whole dollars. This preserves the 99-cent component of the price. The Cournot oligopoly model (Scherer and Ross, 1990) shows that the pass-through is affected by market concentration and demand elasticity. This model shows that the pass-through increases with market concentration and increases for less elastic goods.

Studies by drinking level must classify individuals by their drinking level before any tax or price change. This is required to avoid endogenous selection, which is a problem when a sample is divided by values of the dependent variable. The same is true to a lesser extent for income. Thus, a longitudinal survey, which observes individuals or households both before and after the tax change, is needed. Barnett et al. (2020) studied the Washington state initiative called I-1183, which privatized spirits retail sales and distribution in June 2012. This initiative increased the number of retail outlets but imposed the highest state tax on spirits in the nation. They relied on data from the Nielsen Homescan from January 2010 to December 2014 and a differences-in-difference approach. The Nielsen Homescan data allowed them to define drinking levels prior to the policy change. The comparison group was limited to only 10 states. They found that low and moderate alcohol purchasers increased monthly purchases of ethanol and heavy

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[5] A number of these older studies are reviewed in Wagenaar, Salois, and Komro (2009).
[6] The pass-through is the ratio of the increment in price that results from an increment in taxes.

alcohol purchasers decreased purchases of ethanol. These results reflect both an increase in the money price of spirits due to the imposition of an extremely large tax and the reduction in the travel and shopping costs required to obtain ethanol due to an increase in the number of outlets.

Griffith, O'Connell, and Smith (2019) investigate whether ethanol price elasticities differ by drinking level by dividing households in the United Kingdom, in 2010, into five quintiles with each quintile accounting for 20 percent of weekly drinks purchased. They then estimate these elasticities in 2011 for households divided into the five quintiles based on their 2010 consumption. They find large elasticities in each quintile. These results are based on a system of structural demand functions. While their estimates are highly novel, they rely on data for a single country in a single year. Inclusion of region and month fixed effects limits price variation to that occurring within region in a short period of time. While a large set of instruments is employed to account for the possibility of price endogeneity, the source of price variation is less striking than one that can be tied to a large and plausibly exogenous tax hike.

There are a few studies of the effect of an alcohol excise tax increase by income level. Miravete, Seim, and Thurk (2018) argue that low priced spirits have less elastic demand functions. There is evidence that heavy drinkers and low-income drinkers buy low-priced alcohol products. If low-income drinkers also have less elastic demand functions, this group could experience larger price increases than other groups, which could create a greater effect at low-income levels. Another relevant study of differentials by income level was done by O'Donnell et al. (2019). They examined the effect of minimum unit pricing in Scotland on household alcohol purchases. Minimum unit pricing is not a tax but it established a floor price for ethanol, which increases prices at the low end of the price distribution. They relied on an interrupted time series analysis and Kantar data on Scottish households with English households as controls. They found that the increase in the price paid for alcohol was higher in lower-income households.

Overall, these prior studies show that tax-induced price changes can vary for products at different points in the price distribution. Also, drinkers at different drinking levels or at different income levels buy different products and can have different price elasticities. Thus, there is a reasonable theoretical basis for differential effects of a tax increase by drinking level and by income level. The Nielsen Homescan data allows us to define a household's drinking level and income level before the tax increase and then observe the same household's alcohol purchases and price paid after the tax increase.

There are also several prior studies of the IL tax increase. One of these studies is our own (Gehrsitz, Saffer, and Grossman, 2021). In this prior study we investigated the pass-through and substitutions that were a result of the IL tax increase. We constructed measures of alcohol prices and sales from Nielsen Retail Scanner data collected for hundreds of products in several thousand stores across the US. The results show that alcohol excise taxes in IL were over-shifted by a factor of up to 1.5. Consumers react by decreasing spirits and wine purchases but increased beer purchases. In one set of regressions the increase in ethanol purchased completely offsets the decline in ethanol purchased in the form of spirits and wine. The Nielsen Retail Scanner data is a comprehensive point of sale data covering most grocery and drug store sales in the represented states. The Nielsen Homescan data, which is used in this paper, samples and follows a nationally representative set of individuals who are at least 25 years old. Because beer is the most popular form of ethanol for those under 25, some of the increased beer sales in the Retail Scanner data are not included in the Nielsen Homescan data. In the current paper we find that ethanol purchases decline for specific groups defined by drinking level and income level.

Two additional papers with inconsistent results study the effect of the IL tax increase on highway fatalities. McClelland and Iselin (2019) use the synthetic control approach and they find no evidence that the tax increases led to a reduction in highway fatalities. These results suggest that the IL tax increases did not reduce heavy drinking. Another study of the effect of the tax increase on highway fatalities is Wagenaar, Livingston, and Staras (2015). They find that highway fatalities declined 26% after the tax increase. This suggests that the tax increase reduced heavy drinking.

The results we present in this paper show that heavy drinkers and other drinkers reduce purchases of ethanol as a result of higher alcohol excise taxes and that there is no significant difference between the responsiveness of heavy drinkers and other drinkers to tax increases. We also find that these two groups of drinkers are typically able to substitute to cheaper products and do not pay higher money prices more for ethanol after the tax increase. At face value, these two findings might appear contradictory. However, substitution to lower-priced products and reducing consumption are both potential responses to the tax increase. Search costs and individual preferences can influence the mix between these two choices. However, because of substitution, money prices paid after a tax increase may not be higher than those paid prior to the tax increase. The empirical approach to estimating causal effects of the tax increase relies on outcomes from IL before and after the tax increase in comparison to outcomes in a control group over the same period. The results show that heavy drinkers reduce purchases, and this reduction is not statistically different than the reductions in drinking by other drinkers. The results also show that there is no evidence of a change in prices paid by the different drinking level groups after the tax increase. Thus, the industry claim that ethanol taxes do not reduce consumption by heavy drinkers is not supported. At face value, these two findings might appear contradictory. However, both substitution to lower-priced products and reducing consumption are potential responses to the tax increase. Search costs and individual preferences can influence the mix between these two choices. Moreover, it may be that search costs and increases in shopping and other inconvenience costs required to obtain cheaper money prices resulted in higher total prices, if these costs are included, and therefore lower purchases. The results for low-income drinkers show that they reduce purchase but pay more for ethanol after the tax increase. This outcome may be due to greater resource constraints in low-income households. Compared to the literature just reviewed, our study is unique because it deals with the effects of exogenous tax increases rather than policies that influence outlet availability or minimum prices; avoids the necessity of addressing price endogeneity; and employs a difference-in-differences methodology that is not limited to a few comparison groups.

3. Data

Our working data set was derived from the Nielsen Homescan, which is a rotating panel of approximately 60,000 U.S. households who provide purchase information to Nielsen. The information on which products were purchased is collected with in-home scanners. The unit of observation in the working dataset is a household month. We use a 48-month window from September 2007 to August 2011. This interval was defined to place the IL tax increases at the center. The Homescan has data for DC but no data for Alaska and Hawaii. The five states, other than IL, listed in table 1 had alcohol excise tax changes at about the same time as IL and were excluded from the data set. IL had the largest tax increases of any state during this time period. NC is a control state for spirits and relies on a mark-up rather than excise taxes. NC is dropped because it increased the spirits mark-up and increased excise taxes on beer and wine during the sample period. There are a total of 44 states in the data set including IL.

The working dataset includes 22,262 households observed for at least 36 contiguous months between September 2007 and August 2011. Every household appears for at least 12 contiguous months in the year prior to the tax increase and for at least 12 contiguous months in the year after the tax increase. This makes the data set a balanced panel for the second and third years. Because some of these households also appear in the first and fourth years, the sample is not balanced for the entire four-year period. Nielsen also tries to maintain a demographically representative sample over time by selecting which households can participate. There is a total of 985,828 household-month observations in the dataset. A total of 5,728 households never bought any ethanol during their time in the sample. This generated 257,786 observations from households that never purchased ethanol. Among households that did purchase ethanol, there were 344,571 household-month observations with no ethanol purchased.[7] The Homescan does not include ethanol purchased and consumed at an onsite location such as a restaurant.

Drinking level and income level groups were defined based on the annual average for the year prior to the tax change. To define heavy, moderate, and light purchases we first calculated the average monthly alcohol purchases observed for at 12 contiguous months prior to the tax increase. This was divided by the number of adults in the household. Next, we calculated the distribution of ethanol purchases, per adult in the pre-period, excluding households that never purchased any ethanol. Heavy-drinking households were defined as households that were at or above the 90th percentile of this distribution with purchases greater than or equal to 38.87 ounces of ethanol per month. This is in line with and following findings from the 2010 National Survey of Drug Use and Health (NSDUH), which reported that 7% of the adult population were heavy drinkers and that 66% of the adult population were drinkers. Moderate-drinking households were defined those below the 90th percentile but above or at the 50th percentile. The lower cut off value for moderate drinkers is greater than or equal to 5.47 ounces of ethanol per month. Light drinkers were defined as those below the 50th percentile but with positive purchases. To facilitate comparisons between

[7] Months with no purchases were coded as zero.

heavy drinkers and all other drinkers we defined another category that is the sum of moderate and light drinkers. It consists of non-heavy drinkers and is referred to as other drinkers from now on. Households with no ethanol purchases in the pre-period were defined as non-drinking households. About 2% of these households had small purchases in the post period. We defined income groups in the same fashion as the drinking level groups. Average monthly income for households, per adult, was calculated. Low-income drinkers are defined as households with positive alcohol purchases in the 12 contiguous months before the tax increase and with income per adult less than or equal to $19,073 on average in the pre-period. Non-low-income households (referred to as other-income households from now on) are defined as households with positive alcohol purchases in the pre-period and with income per adult greater than $19,073 on average in the pre-period. The number of households in IL by group are: heavy = 90, moderate = 379, light = 526, low Income =266, and other income = 972.

Table 2 shows the weighted means for the pre-period in IL and in all other states, excluding those in table 1. The all other states sample includes 43 states. The weights are provided in the Nielsen data set and are designed to create means close to the census estimates of the demographic composition of the US. The demographic variables used by Nielsen to construct weights are household size, household income, age, race, Hispanic origin, education, occupation, presence of children and county size. The data reported in table 2 are for adults 25 and over and are means rather than medians. Note that, heavy drinkers, as defined, comprise only about 7% of the population but this group purchases about 50% of the ethanol. Table 2 shows that the demographics for IL are not significantly different from the all-other states group. However, this alone does not constitute sufficient evidence to conclude that the all-other states group is a good control for IL.

Tables 3a and 3b provide descriptive data on the relationship between drinking levels and income levels. These tables rely on data from the pre-period, from the all other states sample plus IL. Table 3a shows that the distribution of purchases by low-income drinkers is shifted more towards heavy than the distribution for higher-income drinkers. The correlation between ethanol purchases and income is nearly zero. Table 3b shows that the prices paid by low-income drinkers are lower than those paid by higher-income drinkers. Prices paid are not shelf prices but rather a weighted average of prices of products actually purchased. The data on prices paid are exclusive of sales taxes. Low-income heavy drinkers pay the least for ethanol of any group. However, the correlation between prices paid and income, while positive, is relatively low.

4. Empirical Approach

Several empirical issues that impact the estimation approaches need to be addressed. As noted above, endogenous selection is an issue because we wish to estimate separate effects of the tax increase on heavy drinkers and other drinkers. Recall that every household in the sample is observed for at least 12 months prior to the tax increase. We avoid the endogenous selection problem by defining the household's drinking status based only on the average monthly alcohol purchased per adult in the12 month period prior to the tax increase (Abadie, Chingos, and West, 2018).

The second issue is that while there are thousands of households in our sample, we effectively only have one treatment group (households in Illinois) and one control group (households in other states). Therefore, we adopt the procedure by Donald and Lang's (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 that we go from individual-level household observations in each of 48 months to 48 monthly observations for the treatment group and 48 monthly observations for the control group or 96 observations in total. By aggregating by group and month, we eliminate clustering of disturbance regression disturbance terms within state over time.[8] 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 the post-period differences, and zero for pre-treatment differences. The estimated regression is given by

$$\Delta Y_t = \alpha + \beta_{DD} Post_t + \varepsilon_t. \quad (1)$$

Here ΔY_t is either the difference in ethanol purchases per capita or price paid for ethanol between Illinois and the control group in month t, α is an intercept that reflects the average difference between purchase or

[8] With household-level data or data aggregated to the state-month level, this issue would be addressed by obtaining Huber standard errors clustered by state. But those standard errors would not recognize that there are only two groups.

price paid between Illinois and the control group in the pre-period, the variable $Post_t$ is an indicator that equal 1 in each of the 24 months in the post-period and is referred to as the DD variable, and ε_t is the random error term. The coefficient β_{DD}, measures the effect of the excise tax increase on purchases and prices paid in IL relative to the control group on average in the post-period net of the pre-period difference. That coefficient is exactly the same as one estimated with household-level data in which the dependent variable is purchases or price paid by each household in a given month and the regressors are an intercept, a dummy variable for the treatment group, a dummy variable for the post-period, and an interaction between the treatment dummy and the post-period.

The coefficient of the interaction just mentioned is exactly the same as β_{DD} in equation (1). The standard errors in equation (1), however, simply reflect the difference between the treatment group outcome and the control group outcome in each of the 48 months. Since equation (1) is a pure time series regression, we correct for serial correlation in the random disturbance term using a procedure developed by Newey and West (1987). In applying that procedure, we assume a lag length of two months.

The last issue we address is the key assumption in DD models that pre-treatment trends in the treatment and control groups are the same. This "parallel trends" assumption implies that post-period trends also would be the same absent the intervention in the treatment group (the tax hike in Illinois in our case). In other words, β_{DD} would be zero if Illinois had not raised alcohol excise taxes in September 2009. However, the assumption of parallel trends may not be valid.

To account for this possibility, we employ the "synthetic control" method developed by Abadie, Diamond, and Hainmuller (2010) to obtain an alternative estimate of the DD parameter. 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. The time series means for IL and the synthetic control group are used with the Donald-Lang procedure to estimate an alternative β_{DD}. That procedure together with the Newey-West algorithm procedure produces the correct standard error for the alternative β_{DD}.

The two analytical approaches just described, standard DD models and synthetic control, and the results obtained with each one have strengths and weaknesses. In the DD models, the evidence of parallel trends from the associated time series figures described in the next section is not always clear. These trends are much clearer in the synthetic control models, and the DDs are more precisely estimated using conventional tests. But in a few important cases, alternative inference tests used in these models and described in the next section are not consistent with Donald-Lang inference parameters. However, the primary strength of the analysis presented is that the two analytical approaches, the corresponding figures, and a set of robustness tests all produce the same conclusions regarding heavy drinkers and low-income drinkers. Our conclusions are based on this clear consensus of results.

5. Results for Purchase

Table 4 presents descriptive data on purchases in IL and in all states in the pre-period and the post-period. The columns labeled difference are the post-period minus the pre-period. The column labeled DD is the difference in difference (DD) between IL and the control states in the change in purchases between the pre-period and the post-period. The last column presents the DD as a percent of the pre-period value for IL. The data show that all groups reduce ethanol purchases after the tax increase although the reduction for low-Income drinkers is trivial. Heavy drinkers (based on pre-period purchases) reduce purchases by about 5.4%, other drinkers (also based on pre-period purchases) reduce purchases by about 3.9%, and other-income drinkers (based on pre-period income) reduce purchases by about 4.8%.[9]

Figures 1 through 4 present the time series of IL and the all-states control group for heavy drinkers, other drinkers, low-income drinkers, and other-income drinkers. These figures provide visual approach to assessing whether there is evidence of parallel trends in the pre-period and effect size in the post-period. There is some evidence of parallel trends in the pre-period, but it is not strong evidence. In the post-period,

[9] Since heavy drinkers purchase much more ethanol than other drinkers and other-income drinkers purchase more ethanol than low-income drinkers, we summarize magnitudes of purchase reductions by percentage changes.

heavy drinkers in IL appear to drink less than the control group. For the other three groups, there appears to be small effects in the post-period.

Although the evidence of parallel trends is limited, we estimate Donald and Lang DD models, which provide quantitative estimates of the post-period effect seen in the figures. These models are presented in table 5 and table 6. Table 5 uses the level form of the dependent variable while table 6 uses the log form. The tables present the results for the DD variable, the Donald and Lang models with Newey-West standard errors with two lags, and the R-squared from Donald and Lang regression models without the Newey-West adjustment. The results in both tables are similar, with mostly negative coefficients. However, only the DD coefficients for heavy drinkers and other-Income drinkers are statistically significant at the 5% level. The estimates in table 6 suggest heavy drinkers reduce purchases by about 6.6%, and other-income drinkers reduce them by about 5.1% after the tax increase.[10] They also suggest that other drinkers reduce purchases by 4.4%, which is significant at the 10% level, but not at the 5% level.

Next, we present results from synthetic control models of purchases. The synthetic control figures 5 through 8 rely on levels data and provide visual evidence of parallel trends in the pre-period between IL and the synthetic control group. Some of the variation between IL and the control group is due to noise in the IL data, which maybe a result of the data coming from a smaller sample than is used for the control group. Each drinking level group and income group has its own synthetic control group. The figures show reasonable pre-period matches between the IL and the control groups. These figures also show a gap between IL and the control group in the post-period.

Tables 7 and 8 present Donald and Lang synthetic control models for purchases. The dependent variables in table 7 are in level form and in table 8 are in log form. The regressions are based on the Donald and Lang approach with Newey-West standard errors using two lags. The R-squared values are from Donald and Lang regression models without the Newey-West adjustment. Also presented are placebo p-values based on t-tests, which are described by Roth et al. (2022). They argue that that a placebo test can be created by using a statistic from the data such as the t-statistic of the DD estimator,

[10]These estimates are slightly different from those in table 4 because they are based on a log model rather than on percentage changes computed from a linear specification.

and then computing this statistic for each control state as if it were assigned the treatment. This results in 43 signed placebo t-values. Abadie (2021) explains that a one-sided inference can result in a substantial gain of power, which is an important consideration with small samples. The one-sided tests account for the sign of the treatment effect.[11] A number of recent papers, such as Thomas et al. (2021) and Long (2019), also rely on one sided p-values. The placebo p-value is typically interpreted as the probability that the observed effect in the treatment state occurred by random chance.

The results in tables 7 and 8 are similar. All the DD coefficients are negative, and all are significant except for light drinkers. The one-sided placebo p-values for heavy drinkers and other drinkers suggest that there is only about a 10% chance that the results occurred at random. Heavy drinkers reduce purchases by about 5.5%, and other drinkers reduce purchases by about 4.8%. These values are approximately the same as in the DD models in Table 6. The coefficients by income are negative and highly significant. The two large coefficients for low-income households, however, have rather high placebo p-values. Hence, much less confidence should be placed in them than in the coefficients for other-income households.

The notable results from tables 5 through 8 and the figures 1 through 8 are that both heavy drinkers and other drinkers reduce their purchases. In percentage terms these reductions are similar. A test for equality between the coefficients for heavy drinkers and other drinkers fails to reject the null hypothesis.[12]

[11] If the treatment t-value is positive, then the number of placebo t-values, which are larger than the treatment state t-value is calculated. This number is divided by the total number of placebo t-values. For example, if two placebo t-values were larger than the t-value for IL, and there are 43 placebos, then the p value would be 2/43 = 0.0465. If the treatment t-value is negative, then the number of placebo t-values, which are smaller negative numbers than the treatment state t-value, is calculated (e.g., -5 is a smaller negative number than -4). This number is divided by the total number of placebo t-values.

[12] We perform the test for significant differences between the coefficients for Heavy Drinkers and Other Drinkers by first pooling the synthetic control data for the two-drinking group to create a dataset of 96 observations. Let h denote observations for heavy drinkers, o denote observations for other drinkers, and let the subscript g assume values of h or o. We then estimated the following equation with the pooled data:

$$\Delta Y_{gt} = \alpha_h + (\alpha_o - \alpha_h)d_o + \beta_h Post_t + (\beta_o - \beta_h)d_o Post_t,$$

where ΔY_{gt} is the difference in the logs between the treatment group and the control group for each drinking level in each time period t, d_O is a binary variable that equals 1 for each monthly difference for other drinkers, and $Post_t$ is a binary indicator for each month in the post period. The equation is obtained by first specifying separate equations for o and h and then multiplying the equation for o by d_o and the equation for h by $d_h = (1 - d_o)$. If $(\beta_o - \beta_H)$ is not statistically significant at the 5% level, then we fail to reject the null hypothesis of no difference between the coefficients. We do not distinguish between moderate and light drinkers in these tests because the light drinker coefficient is not significant in Table 8, and the moderate drinker coefficient has a high placebo p-value.

However, the coefficients for heavy, moderate, light drinkers decline in absolute value and in significance levels, suggesting that heavy drinkers are more responsive to the tax increase than moderate and light drinkers. This is predicted by rational addiction theory (Becker and Murphy, 1988).

6. Results for Price Paid

Table 9 presents descriptive data on prices paid per ounce of ethanol and follows the same approach as table 4. Substitutions by consumers as a result of the tax increase will affect prices paid. If consumers substitute to relatively cheaper products, then the change in prices paid will be smaller than the change in shelf prices. Table 9 shows that the percentage changes in prices paid are extremely small, except for the low-income group, which experiences a 6.1% increase. For the other groups, price changes range from -0.4% to +0.8%.

Figures 9 through 12 present the time series for IL and the all-states control group for heavy drinkers, other drinkers, low-income drinkers and other-income drinkers. These figures provide visual approach to assessing whether there is evidence of parallel trends in the pre-period and effect size in the post-period. These four figures show only weak evidence of parallel trends in the pre-period. In addition, there does not appear to be much change in the trends for IL in the post-period with the exception of the low-income group.

Next, we estimate DD models for each group. These models are presented in table 10 and table 11. Table 10 uses the level form of the dependent variable while table 11 uses the log form. The tables present the results for the DD variable, the Donald and Lang models with Newey-West standard errors with two lags, and the R-squared from Donald and Lang regression models without the Newey-West adjustment. The results in both tables are similar. The coefficients are mostly positive but insignificant. However, the DD coefficients for low-income drinkers are statistically significant at the 1% level. Table 11 suggests that the 6.0% increase in prices paid low-income drinkers is very precisely estimated.

The synthetic control results are presented next. The synthetic control figures 13 through 16, for prices paid, rely on levels data and provide visual evidence of parallel trends in the pre-period between IL and the synthetic control group. Each drinking and income group has its own synthetic control group. These figures also show the post-period time trends in the differential between the treatment and control groups. The figures show reasonable matches in the pre-period between the IL and the control group. Only the low-income group shows visual evidence of an effect in the post-period. Again, there is more noise in the IL data possibly because it is based on a smaller sample than the control groups.

Tables 12 and 13 present synthetic control models for prices paid. The dependent variables in table 12 are in level form and in table 13 are in log form. The regressions are based on the Donald and Lang approach with Newey-West standard errors using two lags. The R-squared values are from Donald and Lang regression models without the Newey-West adjustment. Also presented are placebo p-values based on a t-test as described above for tables 7 and 8.

The results in tables 12 and 13 are similar. All the coefficients are insignificant except for low-income drinkers, which are positive and significant. The placebo p-values for low-income drinkers in both tables are zero. This result suggests that the 5.3% increase in price, which is significant at the 1% level with a conventional t-test, is extremely precisely estimated based on the more demanding placebo test. As a whole, the sets of results in Tables 12 and 13 are consistent with those in tables10 and 11.

The reason that most households are able to avoid paying more for ethanol after the tax increase is substitution to cheaper products. It appears that low-income households make less of these substitutions. One possible reason for this is that the benefit-cost ratio of search for low-income households may be less favorable than it is for other households. Substitution requires searching for cheaper stores and searching for acceptable cheaper products. It could be that for low-income drinkers the benefits from search are smaller because these households generally do not buy that much ethanol and the benefits from search are proportional to the quantity purchased. Prior to the tax change, low-income consumers were already consuming comparatively inexpensive products, so that cheaper substitutes may be less easily obtainable thus further increasing search costs. Also, for these households, the costs of search maybe greater because of more rigid time constraints to engage in search than for higher income households.

The results for prices paid explain, at least in part, why the percentage reduction in purchases of ethanol by low-income households of 12.6% is larger than the percentage reduction of 8.6% by other-income households. The former group experiences a substantial increase in prices paid after the tax is raised while the latter group does not.

7. Results from Robustness Tests

We present three robustness tests. The first robustness test uses an alternative definition of heavy drinking. The second test uses a fixed synthetic control group for both heavy drinkers and for other drinkers. This fixed synthetic control group is based on a synthetic control model for a combined sample of heavy and other drinkers. The third robustness test relies on an alternative construction of the working data set which includes more households.

To define heavy drinking, we relied on the 2010 NSDUH, which estimates that the top 10% of drinkers are heavy drinkers. Alternatively, the U.S. Department of Health and Human Services (2021) recommends that adults limit intake to 2 drinks or less in a day for men and 1 drink or less in a day for women. Using these recommendations, and making some assumptions, we calculated an alternative definition for heavy drinking. A household with one man and one woman is the most common configuration in the data set and men are known to drink more than women. We assume that 70% of the purchased ethanol is consumed by men and 30% by women (White, 2020). This results in a limit of 1.7 drinks per day for a household with one man and one woman. A drink is .6 ounces of ethanol and the average number of days per month is 30.31. This result is 30.9 ounces of ethanol per month. As a robustness test, we estimate the purchase models defining heavy drinking as anything greater than 30.9 ounces per month. This change shifts some drinkers who were in the moderate drinking category to the heavy drinking category. The results are presented in table 14, which is comparable to table 8. The results for heavy drinkers change very little. The coefficient for the other drinker category gets smaller and becomes statistically insignificant. The coefficient for the moderate category gets smaller but remains significant at 10%. That is, shifting drinkers from the moderate category to the heavy category makes the moderate category less responsive to the tax increase. This again suggests that that price effects increase with the level of ethanol consumption, which is predicted by the rational addiction theory. However, in table 14, the heavy and moderate coefficients are not significantly different from one another at 5%.[13] The definition of the light category is not changed, and the results are repeated from table 8 for convenience. The conclusion that heavy drinkers are at least as price responsive as other drinkers is not changed by this robustness test.

The models for heavy drinkers and other drinkers presented in table 8 rely on their own unique synthetic control groups. The composition of the control group can have a major impact on the conclusions. As a robustness test, we estimate additional models, which use the same control group for both heavy drinkers and other drinkers. To do this first we estimated a synthetic control model by combining heavy drinkers and other drinkers. The resulting sample is referred to as the combined sample. The weights resulting from this estimation are presented in table 15. Next, we used the weights from table 15 first with the data restricted to heavy drinkers and, second with the data restricted to other drinkers to create two new control groups. Figure 17 provides the time series for heavy drinkers in IL and the control group based on the combined sample. Figure 18 provides the same data for other drinkers. These figures are similar to figure 5 and figure 6 but present the time series based on the fixed control group. Figures 17 and 18 show a good match in the pre-period although not as good as figures 5 and 6. The combined sample model in table 16 shows the results from a Donald and Lang estimation with these data.

Table 16 presents the Donald and Lang regression models for heavy drinkers and other drinkers using the combined control group. The results in table 16 show that heavy drinkers reduce purchases by about 7.5% and other drinkers reduce purchases by about 6.5%. There is no significant difference between these estimates. These estimates are also not significantly different than the corresponding results in table 8. These results from table 16 suggest that the conclusions from table 8 are robust with respect to a fixed control group based on all drinkers, in place of a drinking level specific control groups.[14]

A third robustness test concerns the selection of households for inclusion in the working data set. All the regressions by drinking level and income level include only households observed for at least 36

[13] The result of this test should be interpreted with caution because the moderate drinker coefficient has a large placebo p-value.

[14] There are no placebo tests in this table because these are not synthetic control models, per se. The control group was determined by the weights in table 15 and are fixed for both heavy and moderate drinkers.

contiguous months. This includes the 12 months prior to the tax increase and the 12 months after the tax increase and 12 additional contiguous months. These added 12 months are in year one or year four or split between both years. This approach allows for classification of drinking and income levels based only on pre-period data and ensures that only the households observed in the pre-period are observed in the post-period. The pre-period purchase level must be based on at least 12 months in the pre-period because households do not necessarily purchase alcohol each month and there are seasonal variations in alcohol purchases over the year.

To test the effects of these restrictions we defined an alternative data set, which includes at least 12 contiguous months in the pre-period but only two or more contiguous months in the post-period. This allows for classification of households by drinking and income level and also allows for a larger sample size. The sample size increases from 22,262 households to 29,926 households, which is about a 34% increase. To test the effects of this expansion we estimated two new sets of Synthetic Control models, which are presented in tables 17 and 18. Table 17 presents results for the log of purchases and table 18 presents results for the log of price paid.

A comparison of table 17 with table 8 shows no significant difference for heavy drinkers, other drinkers and both income groups. In table 17, heavy drinkers reduce consumption by about 7.9% while other drinkers reduce consumption by 5.0%. The reduction for heavy drinkers is somewhat larger than the 5.5% reduction in Table 8. On the other hand, the decline for other drinkers of 4.8 percent in Table 8 is almost identical to the one in Table 17. Moreover, the coefficients at issue in Table 17 are not significantly different from each other. One difference between the results in the two tables is that more confidence can be placed in the income coefficients based on the placebo-p values. This strengthens the finding that low-income households reduce their purchases of ethanol by a larger percentage than other-income households.

A comparison of table 18 with table 13 shows some differences. The key outcomes in these tables are for the two income groups. Table 18 shows that low-income drinkers increase price paid by about 8.6% while other-income drinkers reduce price paid by about 2.9%. The comparable values from table 11 are

5.3% and no change. The main finding from table 11 is that low-income drinkers pay more after the tax increase while others do not. This finding is confirmed with the expanded data set used for table 18. Moreover, the result for other-income drinkers is estimated imprecisely based on the placebo test. We conclude that the overall findings for both purchases and price paid are not affected by the expanded data set described above.

8. Conclusions

The results presented in this paper are an advance over prior empirical studies of the effects of higher alcohol prices by drinking level and by income level because, unlike past studies, these results: 1) are based on all forms of ethanol, 2) rely on Nielsen price data, which is superior to the price data used in past studies, 3) rely on an exogenous tax increase, 4) bypass endogenous selection by defining drinking status before the tax increase, 5) rely on a quasi-balanced sample which allows for observation of the same households before and after the tax increase and 6) are based on a sample of almost one million transactions over four years from both license states and control states.

The first research question relates to the effect of an increase in the alcohol excise tax on heavy drinkers. Based on the results from tables 5 through 8, the corresponding figures, and the robustness tests, we conclude that heavy drinkers reduce purchases in response to excise tax increases. This reduction is not statistically different from the reductions in total ethanol consumption by other drinkers. The results from tables 9 through 13, the corresponding figures, and the robustness tests, show that there is no evidence of a change in money prices paid by the different drinking level groups after the tax increase. Again, heavy drinkers react in the same fashion as other drinkers. Thus, the alcohol industry claim that ethanol taxes do not reduce consumption by heavy drinkers is not supported by our results. At face value, these two findings might appear contradictory. However, both substitution to lower priced products and reducing consumption are potential responses to the tax increase. Search costs and individual preferences can influence the mix between these two choices. Moreover, it may be that search costs and increases in shopping and other inconvenience costs required to obtain cheaper money prices resulted in higher total prices if these costs are included and therefore lower purchases.

The second research question relates to the effect of ethanol taxes on low-income individuals. Because excise taxes are not based on income, they are regressive by definition. However, the variety of ethanol prices available provide options for drinkers to substitute to relatively cheaper products in response to an increase of excise taxes. The results for prices paid in tables 9 through 13, the corresponding tables, and the robustness tests, show that low-income drinkers, unlike other drinkers, pay more for ethanol after the tax increase. The results for low-income drinkers show that they reduce purchases but pay more for ethanol after the tax increase. The latter outcome may be due to greater resource constraints in low-income households and may be the underlying cause, in part, of the reduction in purchases. We conclude that the industry claim that low-income drinkers are disproportionately hurt by alcohol taxes is supported by the results. However, heavy drinkers purchase almost 9 times more ethanol than low-income drinkers and low-income heavy drinkers account for only 22% of all heavy drinkers. Hence, the harm done to the low-income group by tax hikes may be more than offset by the benefits of reductions in heavy drinking.

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Table 1
Changes in State Level Alcohol Excise Taxes Per Gallon from 2008 to 2011 (Dollar Changes in Parentheses)

State	Date	Change in Excise Tax Beer	Change in Excise Tax Wine	Change in Excise Tax Spirits
NY	5/1/2009	27% (.11 to .14)	111% (.19 to .30)	-
NJ	8/1/2009	-	26% (.70 to .88)	25% (4.40 to 5.50)
*IL	9/1/2009	21% (.19 to .23)	90% (.73 to 1.39)	90% (4.50 to 8.55)
NC	9/1/2009	17% (.53 to .62)	27% (.79 to 1.00)	-
CT	5/4/2011	20% (.20 to .24)	20% (.60 to .72)	20% (4.50 to 5.40)
WA	12/8/2011	-	-	Monopoly to $14.25

*IL is the treatment state

Table 2
Weighted Pre-Period Means

	IL	All Other States*
Variable	Mean	Mean
Adults per Household	1.76	1.75
Ounces Ethanol Purchased per Month per Adult	12.03	11.45
Percent Heavy Drinkers	8%	8%
Percent Moderate Drinkers	33%	30%
Percent Light Drinkers	37%	35%
Percent Non-Drinkers	22%	27%
Household Income	$59,796.	$56,224.
Low Income	23%	28%
Other Income	77%	72%
Years of Male Education	13.99	13.82
Years of Female Education	13.77	13.62
Male Age	50.86	51.56
Female Age	51.70	51.43
Percent Married	44.0%	46.0%
Percent Households with Children	26.0%	26.0%
Percent White	81.0%	80.0%
Percent Black	11.0%	10.5%
Percent Asian	2.0%	2.0%
Percent Hispanic (regardless of race)	9.0%	11.0%

*All other states exclude the five listed in table 1.

Table 3a Population Weighted Average Monthly Ounces Ethanol Purchased per Adult in the Pre-Period in All States			
	Low Income	Other Income	All Income Levels
Heavy	82.6323	75.2220	76.8448
Other Drinker	7.334	8.4991	8.2437
Moderate	15.97381	15.3514	15.4666
Light	1.8600	2.1880	2.1066
All Drinking Households	9.0802	12.3874	11.4819
Correlation between ethanol purchases and income is 0.0172.			

Table 3b Population Weighted Average Monthly Prices Paid per Ounce of Ethanol in the Pre-Period in All States			
	Low Income	Other Income	All Income Levels
Heavy	0.9104	1.1231	1.0770
Other Drinker	1.2091	1.4846	1.4280
Moderate	1.1390	1.4267	1.3724
Light	1.3215	1.6117	1.5416
All Drinking Households	1.1507	1.4175	1.3620
Correlation between prices paid and income is 0.181			

Table 4 Descriptive Data for Changes by Drinking Level and Income Level Purchases of Ethanol per Adult in Ounces								
	IL			Control Group				
	Pre-Period	Post-Period	Difference	Pre-Period	Post-Period	Difference	DD	DD/ IL Pre-Period
Heavy	70.3482	57.4491	-12.8991	73.7530	64.6845	-9.0685	-3.8306	-5.45%
Other Drinker	8.1416	7.6444	-0.4972	8.4042	8.2216	-0.1826	-0.3146	-3.86%
Moderate	15.3034	13.7938	-1.5096	15.6252	14.7119	-0.9132	-0.5964	-3.90%
Light	2.4121	2.7936	0.3815	2.3814	2.9348	0.5534	-0.1719	-7.13%
Low Income	8.1988	7.2667	-0.9321	8.8302	8.0130	-0.8171	-0.1150	-1.40%
Other Income	11.9747	10.5907	-1.3840	11.6975	10.8848	-0.8127	-0.5713	-4.77%

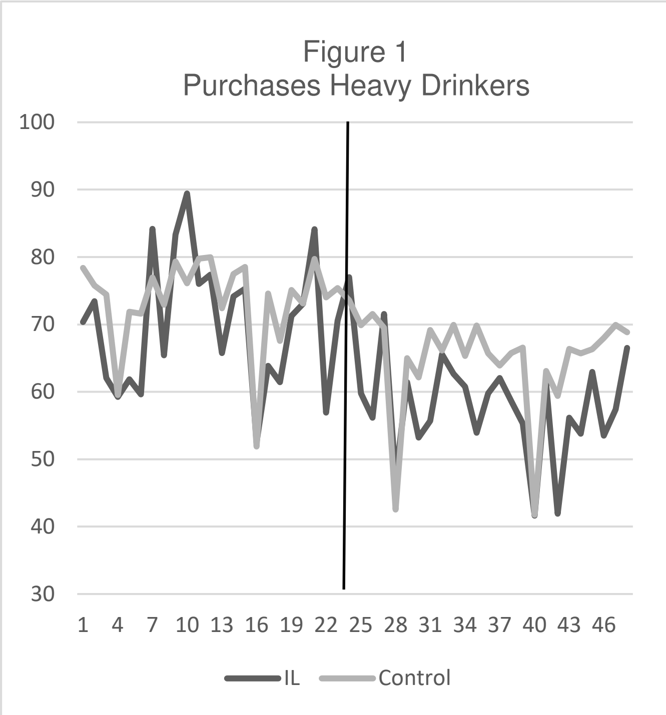
Figure 1
Purchases Heavy Drinkers
100
90
80
70
60
50
40
30
1 4 7 10 13 16 19 22 25 28 31 34 37 40 43 46
IL
Control

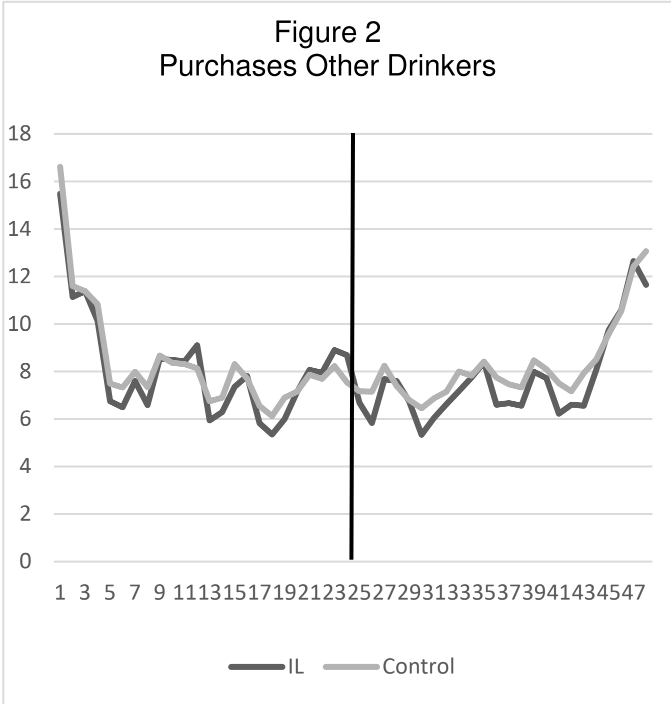
Figure 2
Purchases Other Drinkers
18
16
14
12
10
8
6
4
2
0
1 3 5 7 9 11131517192123252729313335373941434547
IL
Control

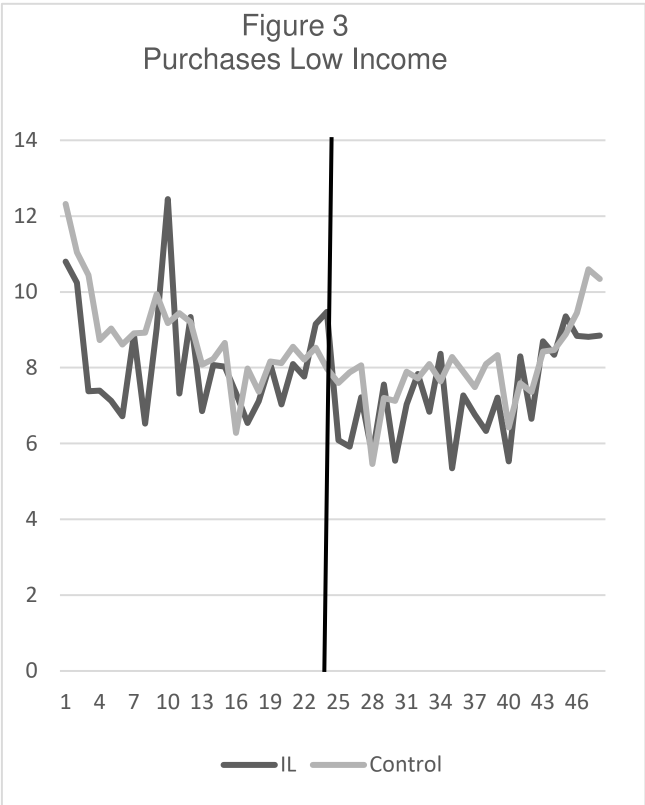
Figure 3
Purchases Low Income
14
12
10
8
6
4
2
0
1 4 7 10 13 16 19 22 25 28 31 34 37 40 43 46
IL
Control

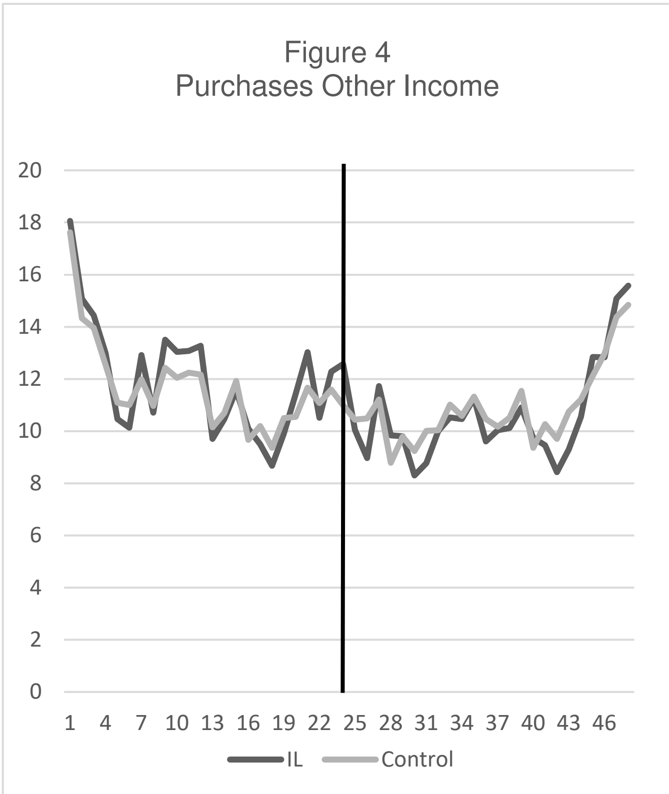
Figure 4
Purchases Other Income
20
18
16
14
12
10
8
6
4
2
0
1 4 7 10 13 16 19 22 25 28 31 34 37 40 43 46
IL
Control

Table 5
Difference in Difference Models
Donald and Lang SE
Ethanol Purchases

	Heavy Drinker	Other Drinker	Moderate Drinker	Light Drinker	Low Income	Other Income
DD	-3.8306**	-0.3146	-0.5963	-0.1719	-0.1150	-0.5713**
NWSE	(1.6901)	(0.1924)	(0.3838)	(0.1289)	(0.3461)	(0.2381)
R-Squared	0.083	0.072	0.067	0.036	0.003	0.133

All regressions are Donald and Lang models are based on a sample size of 48. NWSE = Newey West Standard Error with 2 lags. R-Squares are from regressions without the NW correction. * = p < 0.10 ** = p < 0.05 *** = p< 0.01.

Table 6
Difference in Difference Models
Donald and Lang SE
Log Ethanol Purchases

	Heavy Drinker	Other Drinker	Moderate Drinker	Light Drinker	Low Income	Other Income
DD	-0.0662**	-0.0437*	-0.0440	-0.0541	0.0233	-0.0510**
NWSE	(0.0265)	(0.0260)	(0.0264)	(0.0495)	(0.0422)	(0.0218)
R-Squared	0.100	0.0780	0.073	0.031	0.007	0.124

All regressions are Donald and Lang models are based on a sample size of 48. NWSE = Newey West Standard Error with 2 lags. R-Squares are from regressions without the NW correction. * = p < 0.10 ** = p < 0.05 *** = p< 0.01.

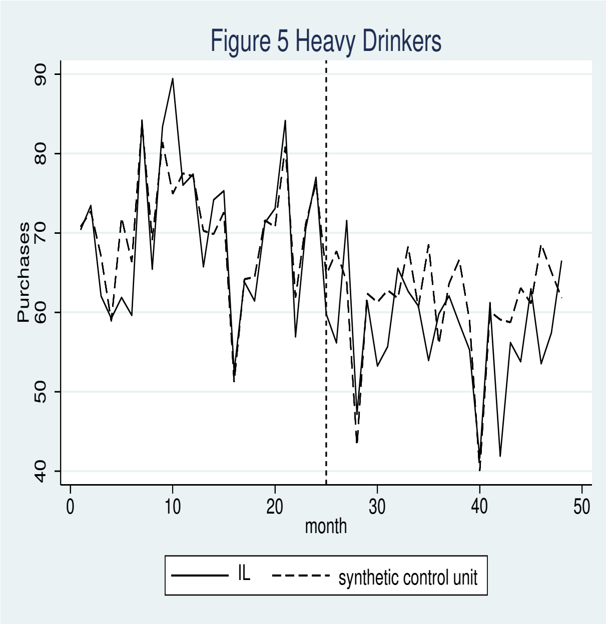
Figure 5 Heavy Drinkers
Purchases
month
IL
synthetic control unit

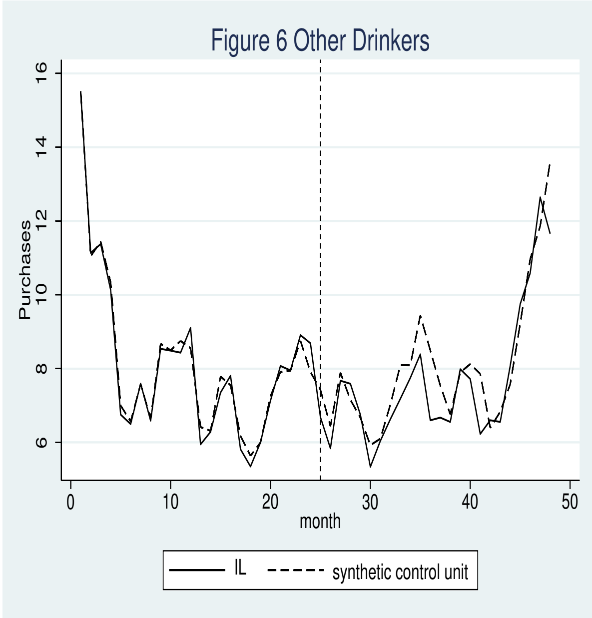
Figure 6 Other Drinkers
Purchases
month
IL
synthetic control unit

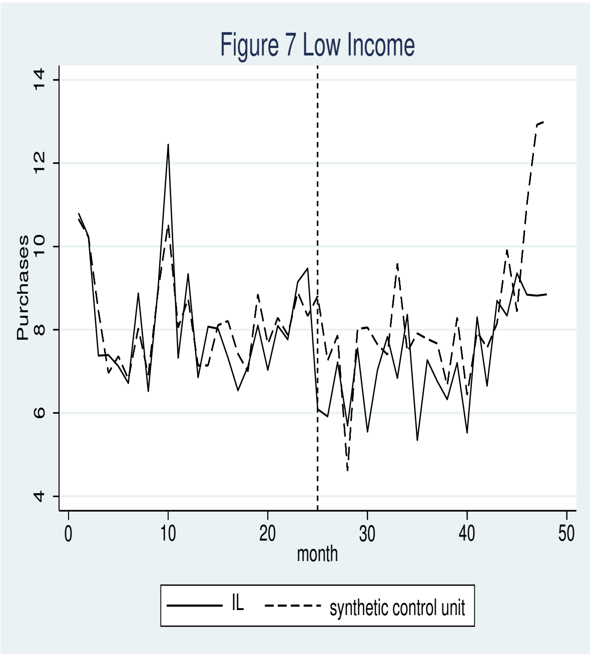
Figure 7 Low Income
Purchases
month
IL
synthetic control unit

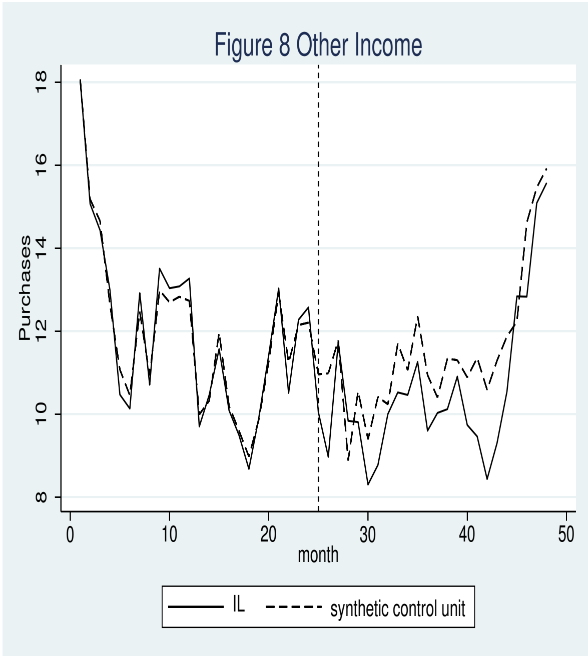
Figure 8 Other Income
Purchases
month
IL
synthetic control unit

Table 7
Synthetic Control Models
Ethanol Purchases

	Heavy Drinker	Other Drinker	Moderate Drinker	Light Drinker	Low Income	Other Income
DD	-3.3831**	-0.3775**	-0.6409*	-0.1378	-1.0911***	-0.9308***
NWSE	(1.6546)	.167576	(0.3062)	(0.1338)	(0.3537)	(0.1795)
R-Squared	0.083	0.090	0.080	0.238	0.185	0.368
Placebo p-Value	0.0930	0.1163	0.2093	0.3488	0.1628	0.0233

All regressions are Donald and Lang models are based on a sample size of 48. Other Drinker is the sum of Moderate and Light drinkers. The control group is based on Synthetic Control using all 43 states as the donner states. NWSE = Newey West Standard Error with 2 lags. R-Squares are from regressions without the NW correction. Placebo p-values are one sided and based on t-values using NWSEs and use all 43 placebo states. The p values for the DD coefficients are: * = p < 0.10 ** = p < 0.05 *** = p< 0.01.

Table 8
Synthetic Control Models
Log Ethanol Purchases

	Heavy Drinker	Other Drinker	Moderate Drinker	Light Drinker	Low Income	Other Income
DD	-0.0547**	-0.0475**	-0.0434**	-0.0312	-0.1255***	-0.0862***
NWSE	(0.0236)	(0.0211)	(0.0213)	(0.0391)	(0.0377)	(0.0183)
R-Squared	0.079	0.096	0.074	0.014	0.177	0.346
Placebo p-Value	0.0930	0.1163	0.2093	0.3953	0.1395	0.0233

All regressions are Donald and Lang models are based on a sample size of 48. Other Drinker is the sum of Moderate and Light drinkers. The control group is based on Synthetic Control using all 43 states as the donner states. NWSE = Newey West Standard Error with 2 lags. R-Squares are from regressions without the NW correction. Placebo p-values are one sided and based on t-values using NWSEs and use all 43 placebo states. The p values for the DD coefficients are: * = p < 0.10 ** = p < 0.05 *** = p< 0.01.

Table 9
Descriptive Data for Changes by Drinking Level and Income Level
Price Paid per Ounce Ethanol

	IL			Control Group				
	Pre-Period	Post-Period	Difference	Pre-Period	Post-Period	Difference	DD	DD/ Pre-Period IL
Heavy	0.9927	1.0492	0.0565	1.0483	1.0969	0.0486	0.0079	0.80%
Other Drinker	1.3282	1.3815	0.0533	1.4233	1.4737	0.0504	0.0029	0.22%
Moderate	1.2518	1.3109	0.0592	1.3545	1.4077	0.0533	0.0059	0.47%
Light	1.4746	1.5005	0.0260	1.5621	1.5896	0.0275	-0.0015	-0.10%
Low Income	1.0566	1.1793	0.1227	1.1362	1.1947	0.0585	0.0642	6.08%
Other Income	1.3129	1.3566	0.0436	1.4135	1.4620	0.0485	-0.0049	-0.37%

Figure 9
Price Paid by Heavy Drinkers

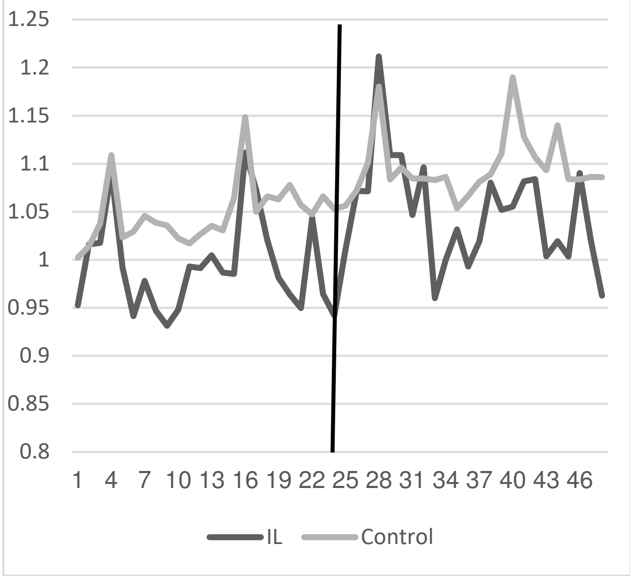

Figure 10
Price Paid Other Drinker

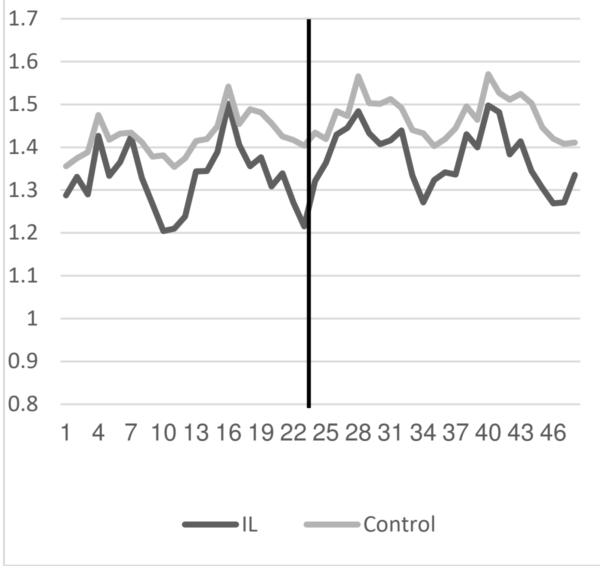

Figure 11
Prices Paid Low Income

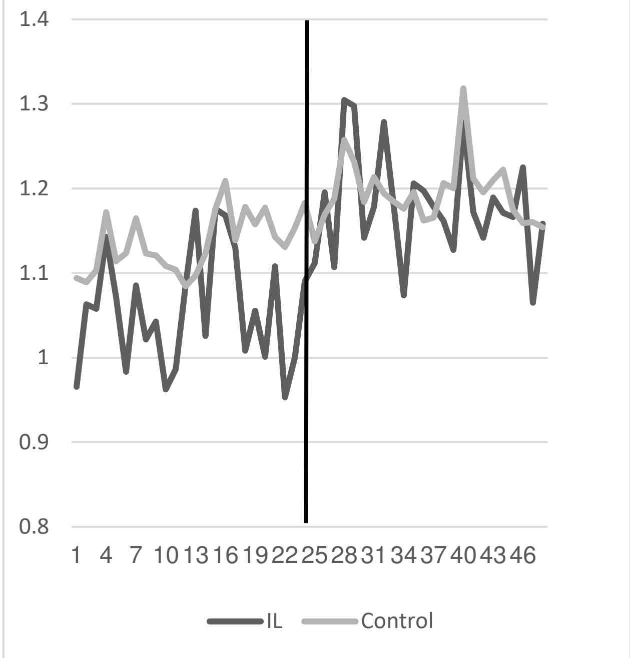

Figure 12
Prices Paid Other Income

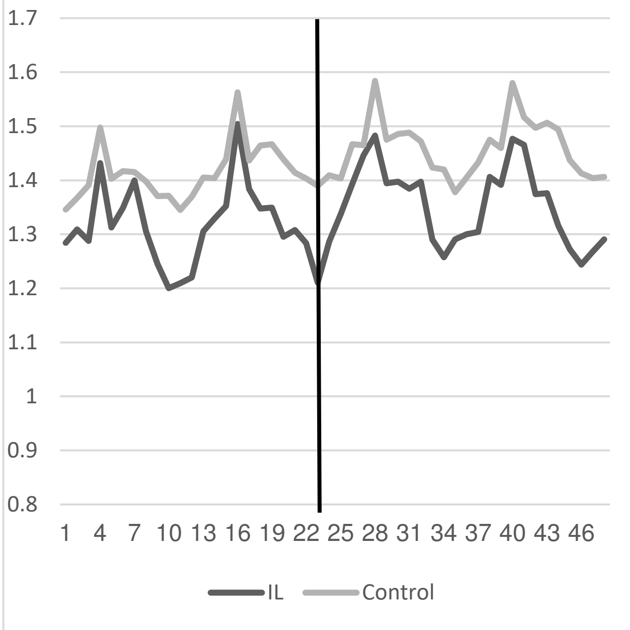

Table 10
Difference in Difference Models
Donald and Lang SE
Price Paid per Ounce Ethanol

	Heavy Drinker	Other Drinker	Moderate Drinker	Light Drinker	Low Income	Other Income
DD	0.0079	0.0029	0.0059	-0.0015	0.0642***	-0.0049
NWSE	(0.0149)	(0.0121)	(0.0161)	(0.0307)	(0.0174)	(0.0158)
R-Squared	0.008	0.001	0.005	0.000	0.236	0.004

All regressions are Donald and Lang models are based on a sample size of 48. NWSE = Newey West Standard Error with 2 lags. R-Squares are from regressions without the NW correction. * = p < 0.10 ** = p < 0.05 *** = p< 0.01.

Table 11
Difference in Difference Models
Donald and Lang SE
Log Price Paid per Ounce Ethanol

	Heavy Drinker	Other Drinker	Moderate Drinker	Light Drinker	Low Income	Other Income
DD	0.0078	-0.0005	0.0080	0.0012	0.0603***	-0.0009
NWSE	(0.0144)	(0.0125)	(0.0128)	(0.0208)	(0.0155)	(0.0124)
R-Squared	0.013	0.006	0.015	0.000	0.249	0.000

All regressions are Donald and Lang models are based on a sample size of 48. NWSE = Newey West Standard Error with 2 lags. R-Squares are from regressions without the NW correction. * = p < 0.10 ** = p < 0.05 *** = p< 0.01.

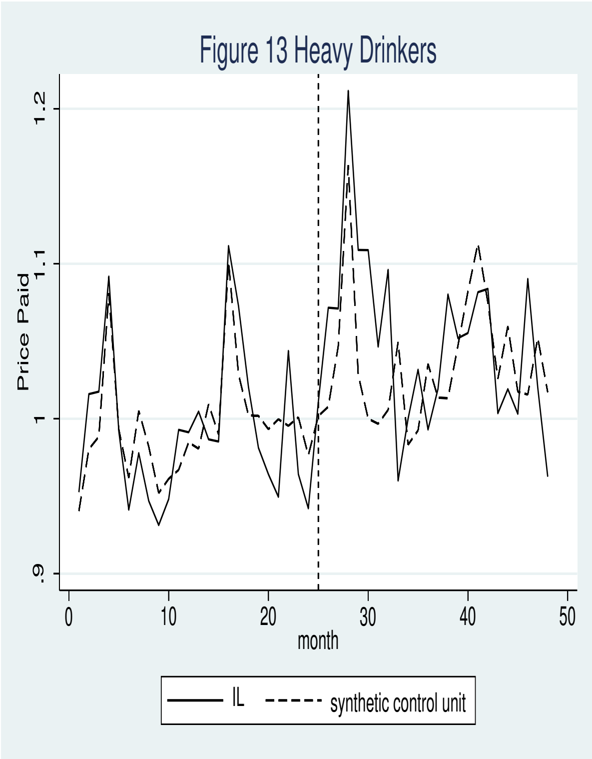
Figure 13 Heavy Drinkers
Price Paid
.9
1
1.1
1.2
0
10
20
30
40
50
month
IL
synthetic control unit

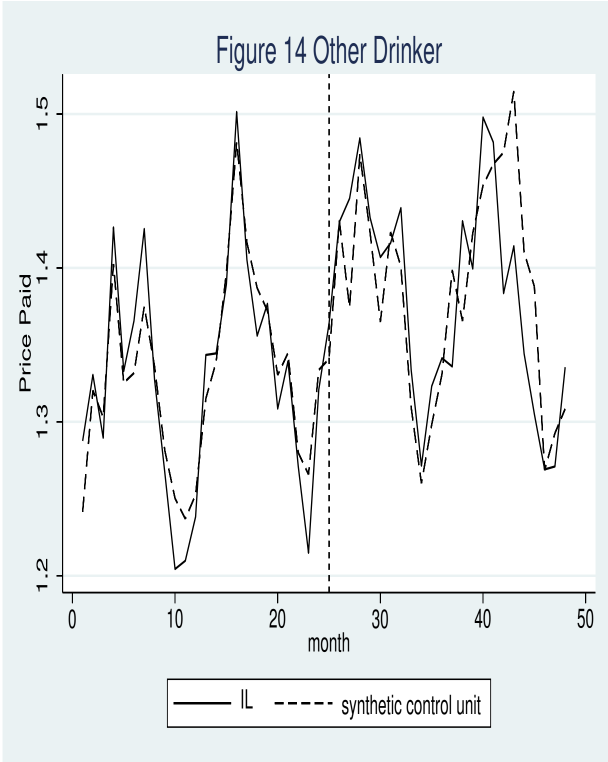
Figure 14 Other Drinker
Price Paid
1.2
1.3
1.4
1.5
0
10
20
30
40
50
month
IL
synthetic control unit

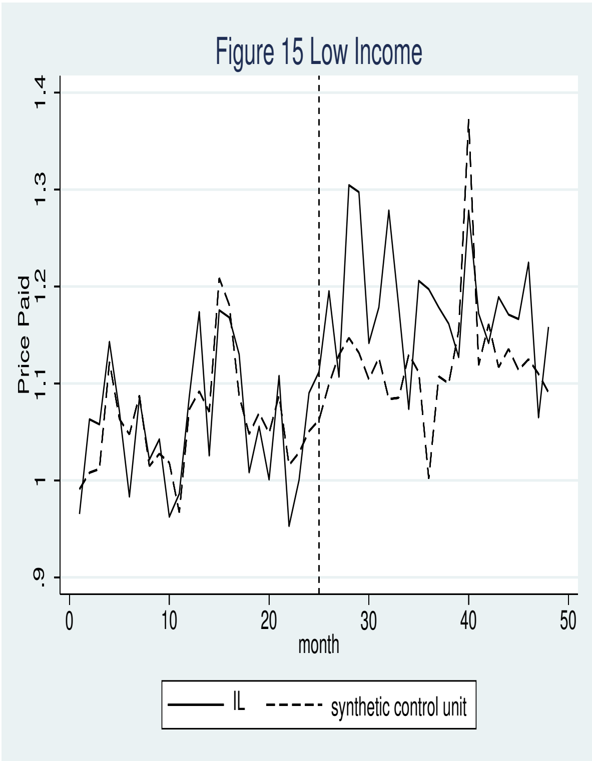
Figure 15 Low Income
Price Paid
.9
1
1.1
1.2
1.3
1.4
0
10
20
30
40
50
month
IL
synthetic control unit

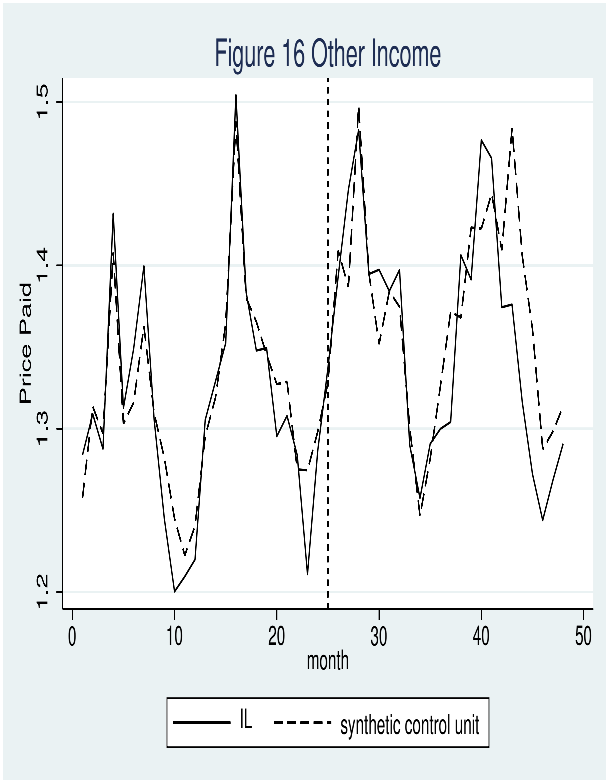
Figure 16 Other Income
Price Paid
1.2
1.3
1.4
1.5
0
10
20
30
40
50
month
IL
synthetic control unit

Table 12
Synthetic Control Models
Price Paid per Ounce Ethanol

	Heavy Drinker	Other Drinker	Moderate Drinker	Light Drinker	Low Income	Other Income
DD	-0.0162	0.0001	-0.0460*	-0.0066	0.0604***	-0.0085
NWSE	(0.0116)	(0.0140)	(0.0272)	(0.0262)	(0.0144)	(0.0134)
R-Squared	0.038	0.000	0.088	0.002	0.208	0.014
Placebo p-Value	0.2791	0.3953	0.2326	0.4651	0.0000	0.3953

All regressions are Donald and Lang models are based on a sample size of 48. Other Drinker is the sum of Moderate and Light drinkers. The control group is based on Synthetic Control using all 43 states as the donner states. NWSE = Newey West Standard Error with 2 lags. R-Squares are from regressions without the NW correction. Placebo p-values are one sided and based on t-values using NWSEs and use all 43 placebo states. The p values for the DD coefficients are: * = $p < 0.10$ ** = $p < 0.05$ *** = $p < 0.01$.

Table 13
Synthetic Control Models
Log Price Paid per Ounce Ethanol

	Heavy Drinker	Other Drinker	Moderate Drinker	Light Drinker	Low Income	Other Income
DD	-0.0156	0.0007	-0.0322	-0.0035	0.0533***	-0.0060
NWSE	(0.0129)	(0.0102)	(0.0195)	(0.0176)	(0.0144)	(0.0100)
R-Squared	0.038	0.000	0.087	0.001	0.208	0.013
Placebo p-Value	0.3023	0.3953	0.2326	0.4884	0.0000	0.4186

All regressions are Donald and Lang models are based on a sample size of 48. Other Drinker is the sum of Moderate and Light drinkers. The control group is based on Synthetic Control using all 43 states as the donner states. NWSE = Newey West Standard Error with 2 lags. R-Squares are from regressions without the NW correction. Placebo p-values are one sided and based on t-values using NWSEs and use all 43 placebo states. The p values for the DD coefficients are: * = $p < 0.10$ ** = $p < 0.05$ *** = $p < 0.01$.

Table 14 Synthetic Control Models Alternative Definitions of Heavy, Other and Moderate Log Ethanol Ounces Purchases				
	Heavy Drinker	Other Drinker	Moderate Drinker	Light Drinker[1]
DD	-0.0509***	-0.0282	-0.0382*	-0.0312
NWSE	(0.0187)	(0.0208)	(0.0215)	(0.0391)
R-Squared	0.0955	0.048	0.078	0.014
Placebo p-Value	0.0930	0.3488	0.3721	0.3953

[1]Taken from table 8.
All regressions are Donald and Lang models are based on a sample size of 48. Other Drinker is the sum of Moderate and Light drinkers. The control group is based on Synthetic Control using all 43 states as the donner states. NWSE = Newey West Standard Error with 2 lags. R-Squares are from regressions without the NW correction. Placebo p-values are one sided and based on t-values using NWSEs and use all 43 placebo states. The p values for the DD coefficients are: * = p < 0.10 ** = p < 0.05 *** = p< 0.01.

Table 15 Synthetic Control Weights from Purchases by Combined Drinkers		
Fips Code	State Name	Synthetic Control Weight
10	Delaware	0.126
11	DC	0.059
19	Iowa	0.017
29	Missouri	0.350
30	Montana	0.001
31	Nebraska	0.077
35	New Mexico	0.076
38	North Dakota	0.052
40	Oklahoma	0.063
45	South Carolina	0.009
50	Vermont	0.006
55	Wisconsin	0.158
56	Wyoming	0.006

Figure 17 Purchases Heavy Drinkers

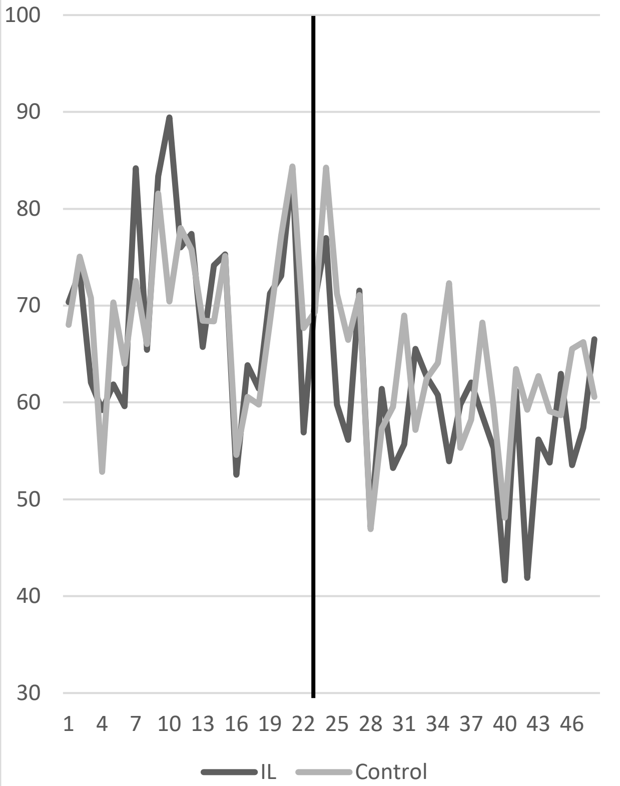

Figure 18 Purchases Other Drinkers

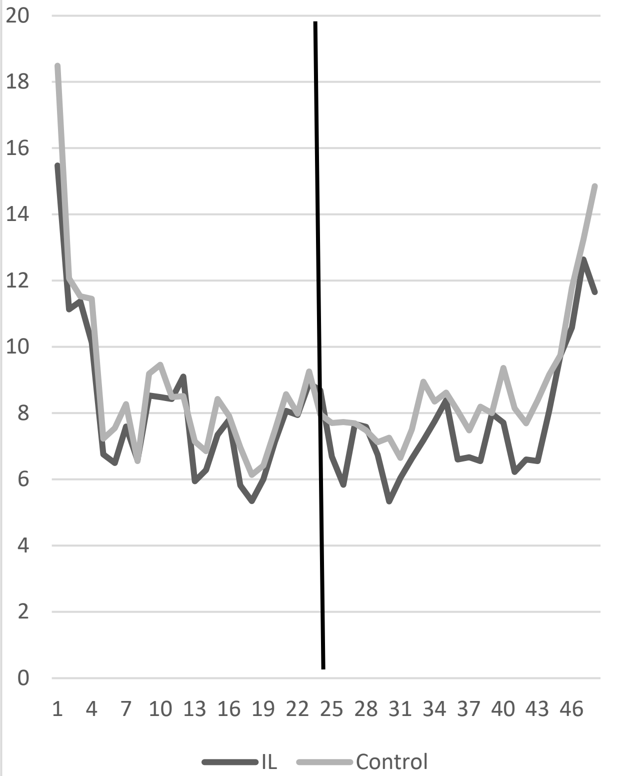

Table 16 Control Group based on Weights from Table 15 Log Purchases Ounces Ethanol			
	Combined Sample	Heavy Drinker	Other Drinker
DD	-0.0638***	-0.0752***	-0.0647***
NWSE	(0.0183)	(0.0259)	(0.0230)
R-Squared	0.200	0.108	0.127

The control group is defined with the synthetic control weights from the model with the combined sample. The combined sample includes both Heavy Drinkers and Other Drinkers. Regressions are Donald and Lang models and are based on a sample size of 48. NWSE = Newey West Standard Error with 2 lags. R-Squares are from regressions without the NW correction. * = p < 0.10 ** = p < 0.05 *** = p< 0.01.

Table 17 Expanded Sample Synthetic Control Models Log Ethanol Purchases				
	Heavy Drinker	Other Drinker	Low Income	Other Income
DD	-0.0791**	-0.0498**	-0.1030***	-0.0509***
NWSE	(0.0321)	(0.0185)	(0.0281)	(0.0162)
R-Squared	0.108	0.153	0.140	0.188
Placebo p-Value	0.1163	0.1163	0.0465	0.1163

Expanded sample includes at least 12 contiguous months in the pre-period and at least two contiguous months in the post-period. All regressions are Donald and Lang models are based on a sample size of 48. Other Drinker is the sum of Moderate and Light drinkers. The control group is based on Synthetic Control using all 43 states as the donner states. NWSE = Newey West Standard Error with 2 lags. R-Squares are from regressions without the NW correction. Placebo p-values are one sided and based on t-values using NWSEs and use all 43 placebo states. The p values for the DD coefficients are: * = p < 0.10 ** = p < 0.05 *** = p< 0.01.

Table 18 Expanded Sample Synthetic Control Models Log Price Paid per Ounce Ethanol						
	Heavy Drinker	Other Drinker	Moderate Drinker	Light Drinker	Low Income	Other Income
DD	-0.0427**	0.0159	-0.0361**	-0.0165	0.0860***	-0.0292**
NWSE	(0.0197)	(0.0124)	(0.0155)	(0.0203)	(0.0238)	(0.0114)
R-Squared	0.145	0.045	0.124	0.019	0.165	0.203
Placebo p-Value	0.2093	0.2791	0.1395	0.3256	0.0000	0.1628

Expanded sample includes at least 12 contiguous months in the pre-period and at least two contiguous months in the post-period. All regressions are Donald and Lang models are based on a sample size of 48. Other Drinker is the sum of Moderate and Light drinkers. The control group is based on Synthetic Control using all 43 states as the donner states. NWSE = Newey West Standard Error with 2 lags. R-Squares are from regressions without the NW correction. Placebo p-values are one sided and based on t-values using NWSEs and use all 43 placebo states. The p values for the DD coefficients are: * = p < 0.10 ** = p < 0.05 *** = p< 0.01.