Beyond the Shelf Price: Alcohol Sales Taxes and Alcohol Consumption*

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Abstract

While most research on alcohol taxation examines volume-based excise taxes, much less is known about the effects of sales taxes, which are applied as a percentage of retail prices and may differ in salience and incidence. This paper evaluates the impact of Maryland's 2011 three percent alcohol sales tax on household alcohol purchases using NielsenIQ Consumer Panel data and a synthetic difference-in-differences approach. I find that the tax led to a roughly ten percent decline in monthly ethanol ounces purchased per adult per household, equivalent to about two fewer standard drinks per adult per month. Purchases declined across beer, wine, and spirits, providing no evidence of substitution across beverage types. Heavy drinkers also reduced their purchases, while low-income households exhibit more constrained substitution patterns. These results provide new evidence on the behavioral effects of sales taxes and highlight the potential of price-based alcohol policies to reduce consumption and related harms.

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1 Introduction

Excessive alcohol consumption is one of the leading preventable causes of death in the United States (U.S.) and is associated with over $249 billion in medical costs and lost productivity (Sacks et al., 2015). It accounts for 385 deaths per day and more than 3.59 million years of potential life lost annually (National Center for Drug Abuse Statistics, 2024). A substantial body of evidence shows that alcohol taxation can effectively reduce consumption and mitigate the health and social harms of excessive drinking (Wagenaar et al., 2009).

Alcohol can be taxed either by volume or by price. Volume-based excise taxes are levied per unit of alcohol (e.g., per gallon or liter), while price-based sales taxes are applied as a percentage of the beverage's retail price.[1] Although both taxation methods aim to regulate alcohol consumption, they operate through distinct mechanisms. Excise taxes are fixed per unit of alcohol and typically embedded in shelf prices, whereas sales taxes are applied at the point of sale and automatically adjust with inflation. These differences have implications for both the economic incidence of the tax and its behavioral effects.

There is strong evidence that alcohol excise taxes reduce excessive alcohol consumption (see Wagenaar et al. (2009) for a review of the literature), but relatively little empirical research on the impact of alcohol sales taxes on consumption, as most states do not impose additional sales taxes on alcoholic beverages. Research suggests that excise taxes may be more effective than sales taxes, as the latter are less salient to consumers, being visible only at the point of sale (Chetty et al., 2009). In theory, both types of taxes increase alcohol prices and *should* reduce consumption, but their relative effectiveness remains an open empirical question.

To examine this, I focus on Maryland's 2011 increase in the alcohol sales tax from 6% to 9%, the state's first such hike in four decades. Anecdotal evidence suggests the change was adopted primarily to align Maryland's tax rate with those of neighboring states, which supports treating the increase as plausibly exogenous to underlying alcohol consumption trends. The policy, implemented on July 1, 2011, applied to both on-premise purchases (e.g., bars and restaurants) and off-premise purchases (e.g., liquor stores and supermarkets).[2]

Using the NielsenIQ Consumer Panel data, I construct a balanced household-level panel

[1]Sales taxes are also known as ad valorem consumer taxes.

[2]On-premise sales refer to alcohol purchased and consumed at the point of sale, such as in bars, restaurants, or clubs, while off-premise sales refer to alcohol purchased for consumption elsewhere, such as from liquor stores, supermarkets, or wholesalers.

from June 2009 to July 2011. To estimate the tax's impact, I compare changes in ethanol ounces purchased per adult per household in Maryland relative to a weighted set of control households from other states, using a synthetic difference-in-differences approach.[3] I therefore estimate the effect of the tax on alcohol purchases rather than consumption.[4]

My preferred specification estimates that the tax increase led to a 10% reduction in ethanol ounces purchased per household per month, equivalent to about two fewer standard drinks per month.[5] There is no evidence of substitution across beverage categories, including beer, wine, and spirits. The detail of the household panel also allows me to examine heterogeneous effects across drinking categories, income levels, and proximity to potential border purchasing using pre-treatment information (Saffer et al., 2024).

The tax significantly reduced ethanol purchases even among heavy-drinking households, who on average purchased the equivalent of one fewer 750 ml bottle of spirits per month. In contrast, low-income households show a trivial but statistically significant increase in ethanol purchases, equivalent to less than a shot per adult per month.

To interpret this pattern, I examine how the tax affected the price paid per ounce of ethanol, a measure that indicates whether households respond by shifting toward cheaper sources of ethanol (Saffer et al., 2024).[6] Most drinking groups experience a clear decline in this measure, consistent with substitution toward lower-priced alcohol following the tax. Low-income households do not, which suggests that they already purchase inexpensive products and have limited ability to substitute further. Their statistically significant but economically trivial increase in ethanol purchased, combined with stable prices paid per ounce, implies that they shifted toward products that provide slightly more ethanol at similar prices. Finally, households in Maryland's border counties reduced their alcohol purchases by amounts similar to those elsewhere in the state.

Overall, the findings suggest that, like excise taxes, alcohol sales taxes effectively reduce alcohol purchases. This reduction in purchases is a first-order impact that may lead to declines in alcohol-related externalities, including adverse health outcomes, crime, and traffic fatalities. At the same time, the results show that low-income households have limited scope

[3] I focus on ethanol ounces rather than beverage volume because ethanol is the component of alcohol responsible for its harmful effects.

[4] The data do not include individual-level alcohol consumption.

[5] The number of drinks is calculated assuming 0.6 ounces of ethanol per standard drink.

[6] The price per ounce of ethanol is constructed by dividing total alcohol expenditures by total ethanol content purchased. A decline in this measure reflects substitution toward products that deliver more ethanol per dollar, such as larger bottles or lower-priced brands (Saffer et al., 2024).

to substitute toward lower-priced products, a pattern that raises distributional concerns Policymakers should take this into account when evaluating the use of sales taxes, which also differ from excise taxes in that they automatically adjust with inflation. The ability of sales taxes to retain their real value without legislative updates may offer an advantage for long-run policy design, but their distributional effects warrant careful consideration.

2 Background

2.1 Alcohol taxation in the U.S.

Alcohol taxation is a common economic policy tool aimed at reducing excessive alcohol consumption and mitigating alcohol-related harms. In the U.S., alcohol taxes are imposed at both the federal and state levels. At the federal level, taxes are primarily excise taxes, levied based on alcohol volume rather than price. Excise tax rates vary by beverage type, with spirits taxed at the highest rate per gallon, followed by wine and beer. Federal excise rates have remained unchanged since 1991, though certain producers receive tax reductions based on annual production volume, leading to a decline in the real value of these taxes over time. Although excise taxes are usually levied on producers or retailers, they are generally passed on to consumers in the form of higher prices, with many estimates suggesting a pass-through rate greater than one (Kenkel, 1993, 2005; Nelson and Moran, 2019; Shrestha and Markowitz, 2016; Young and Bielińska-Kwapisz, 2002).

At the state level, alcohol taxation varies depending on the type of beverage, the point of sale (on-premise vs. off-premise), and the distribution system. In some states, known as "monopoly states," alcohol retail sales are directly controlled by the government, generating revenue through state-operated outlets rather than through traditional tax mechanisms. However, most states impose their own excise taxes, and some also apply additional sales taxes on alcohol. According to the Alcohol Policy Information System (APIS), alcohol sales taxes are defined as the difference between a state's alcohol retail sales tax and its general sales tax, reflecting the extra tax burden placed on alcohol compared to other goods (National Institute on Alcohol Abuse and Alcoholism, 2024). Because sales taxes are calculated as a percentage of the product's price, they automatically increase with inflation, and result in higher tax burdens on more expensive alcoholic beverages.

Alcohol sales taxes are far less common than excise taxes, with only a few states having implemented them. In the past two decades, only Washington, D.C., Kansas, Maryland, Minnesota, Oklahoma, Texas, and Vermont have applied additional sales taxes on alcohol. During the study period, from June 2009 to July 2013, five states (Connecticut, Illinois, New Jersey, New York, and North Carolina) changed their excise tax rates. Washington became a monopoly state, and both D.C. and Kansas adjusted their existing alcohol sales taxes. Maryland is the only state that introduced a new, standalone alcohol sales tax during this time.[7]

2.1.1 The 2011 Maryland Alcohol Sales Tax Increase

Maryland presents a unique case as one of the few states to ever implement an additional sales tax for alcohol. Prior to 2011, the state's alcohol tax structure had remained unchanged for decades. Maryland had not adjusted its excise tax on distilled spirits since 1955 or on beer and wine since 1972. As a result, the real value of these taxes declined substantially over time, making alcohol increasingly affordable. By 2011, Maryland's excise tax per drink was estimated to be less than two cents, among the lowest in the country. Compared to its five bordering jurisdictions, Maryland had the lowest distilled spirits excise tax and the second-lowest beer and wine excise tax (Esser et al., 2016).

On July 1, 2011, Maryland increased its state alcohol sales tax from 6% to 9%, a 50% increase (MD.§11–104(g)). Unlike excise taxes, which had remained unchanged for decades (MD.§5–105), this policy linked the tax burden directly to alcohol prices and applied to both on-premise and off-premise sales. This tax increase was the result of nearly a decade of advocacy rather than a response to short-term shifts in alcohol consumption. As early as 2002, policymakers introduced proposals to increase alcohol and tobacco taxes as revenue sources for state healthcare expansion plans, but these efforts initially failed (Ramirez and Jernigan, n.d.). Unlike tax changes aimed at reducing excessive consumption, Maryland's policy was primarily framed as a fiscal measure to align with neighboring states and support healthcare funding. This historical context suggests that the tax increase was not directly driven by fluctuations in the state's alcohol consumption levels, reinforcing its plausibly exogenous nature.

[7] Tables A2 and A1 in the Appendix summarize state-level changes in alcohol excise and sales taxes, respectively.

2.2 Alcohol taxes and consumption

There is a broad empirical literature examining the impact of alcohol taxes on consumption. Economic theory suggests that taxation increases the price of a good, which in turn reduces its consumption (Maldonado-Molina and Wagenaar, 2010; Xuan et al., 2015). Alcohol is generally considered a normal good (Hanson and Sullivan, 2016; Meng et al., 2014; Wagenaar et al., 2009), though its demand is inelastic, meaning consumption does not decrease in proportion to price increases (Ayyagari et al., 2013; Gehrsitz et al., 2021; Pryce et al., 2019). Price responsiveness varies across both beverage types (Clements et al., 2022; Gehrsitz et al., 2021; Son and Topyan, 2011) and consumer characteristics (Pryce et al., 2019; Wagenaar et al., 2009).

Among beverage types, beer tends to be less elastic than wine and spirits (Clements et al., 2022), suggesting that demand for spirits and wine is more sensitive to price changes (Gehrsitz et al., 2021; Son and Topyan, 2011). One possible explanation for these differences is substitution effects. For example, Gehrsitz et al. (2021) find that after the 2009 Illinois alcohol excise tax increase, consumers shifted to lower-priced alcohol, increasing their beer purchases and partially offsetting the overall decline in ethanol consumption. This suggests that when the price of one type of alcohol rises, consumers may switch to cheaper alternatives within the alcohol category to maintain their consumption levels.

Consumer responses to alcohol price changes also vary with income and drinking patterns. Heavy and binge drinkers tend to exhibit relatively inelastic demand, often substituting toward lower-cost options rather than reducing overall consumption (Pryce et al., 2019; Wagenaar et al., 2009). In contrast, consumers who have lower incomes, are more price-sensitive (Shrestha, 2015). As a result, alcohol taxes can have disproportionate effects across income groups, with lower-income individuals bearing a larger financial burden (Saffer et al., 2024).

Taste preferences and perceptions of quality further contribute to heterogeneous price responses. Consumers with preferences for premium alcohol, typically higher-priced products, may be less responsive to price increases. Ally et al. (2014) show that prices for the cheapest alcoholic products tend to rise less than tax increases, while prices for products above the median are often raised more than the tax. This pricing pattern suggests that consumers of higher-end alcohol may absorb price increases more readily, further reducing the overall elasticity of their demand. Taken together, these heterogeneous responses complicate the ability of taxes to reduce alcohol consumption universally, as individuals' responsiveness to price changes is shaped by factors such as income, drinking behavior, and taste preferences (Griffith et al., 2019; O'Donnell et al., 2019; Ally et al., 2014).

2.2.1 Contributions to the Literature

Most empirical studies looking at the impact of alcohol taxes and consumption have focused on excise taxes. While both excise and sales taxes increase alcohol prices and can reduce consumption, their behavioral effects may differ due to tax salience (Chetty et al., 2009). Moreover, sales taxes disproportionately affect higher-priced alcohol, which generally has higher ethanol content, and they adjust with inflation, potentially making them more enduring than excise taxes. Despite these distinct features, relatively little research has examined the effects of alcohol sales taxes on consumption.

This paper contributes to the literature by offering the first empirical analysis of how a sales tax affects household-level alcohol purchases, using the NielsenIQ Consumer Panel and Maryland's 2011 alcohol sales tax increase as a case study. While previous research has evaluated the Maryland reform, earlier studies have largely focused on downstream health or behavioral outcomes, rather than consumption. For instance, Lavoie et al. (2016) document a decline in alcohol-positive drivers; Staras et al. (2016) find reductions in gonorrhea rates; and Smart et al. (2018) report fewer alcohol-related disciplinary actions among college students. These studies suggest a reduction in consumption and imply that the tax was at least partially passed through to prices, but they do not directly observe alcohol purchases.

Only one study, Esser et al. (2016), directly analyzes sales data following the tax. They find modest declines in per capita alcohol sales across all categories but estimate post-tax effects using a mixed-effects model without a control group. Their reliance on county-level data also precludes any assessment of household-level heterogeneity. By contrast, my analysis uses a synthetic difference-in-differences design with a flexible control group and household-level panel data, allowing for a causal interpretation of the tax's impact on purchasing behavior and rich heterogeneity analysis.

This study also builds on recent research using Nielsen IQ Consumer Panel data. Saffer et al. (2024) study the 2009 Illinois alcohol excise tax increase using a difference-in-differences model, where the control group is constructed via a synthetic control method (SCM). Their estimation approach, following Donald and Lang (2007), aggregates monthly differences between Illinois (the treatment state) and the control aggregate from the SCM, reducing the effective sample size to 43. They find that both heavy and moderate drinkers, as well as low-income drinkers, reduce ethanol purchases in response to the tax. Their findings also indicate substitution effects, as heavy and moderate drinkers, along with middle- and high-income individuals, shift their purchases to avoid higher ethanol prices, while low-income drinkers end up paying more per unit of ethanol.

My research builds on this study but differs in two key ways. First, I examine an alcohol sales tax rather than an excise tax, which can generate different behavioral responses. Second, my identification strategy differs. I use a synthetic difference-in-differences model, which leverages a balanced panel of households from June 2009 to July 2013 and includes household and year-month fixed effects.

Unlike Saffer et al. (2024), who aggregate data to the state level, I exploit within-household variation in alcohol purchases, allowing for a more granular analysis. My results show that the tax reduces overall ethanol purchases. Similar to their findings, I observe that heavy-drinking households exhibit large reductions, despite prior literature suggesting that their consumption is more inelastic. Low-income households respond differently: they face tighter substitution constraints and show limited ability to switch to cheaper products, which alters both the magnitude and composition of their response. I also compare Maryland border and non-border households and find no meaningful differences in their responses to the tax. By addressing these gaps, this study provides new empirical evidence on how sales taxes affect alcohol purchasing behavior.

3 Data

3.1 NielsenIQ Consumer Panel

To estimate the impact of Maryland's 2011 alcohol sales tax on alcohol purchases, I use household-level data from the NielsenIQ Consumer Panel. This dataset is particularly well suited for this analysis because it captures both overall changes in alcohol purchases and variation in responses across different types of consumers. The panel tracks a rotating sample of 40,000 to 60,000 households annually, with participating households recording their purchases using in-home scanners. Households are included in the sample only after consistently reporting purchases for at least 12 months. The dataset covers a wide range of consumer purchases across all retail channels and includes demographic information on household size, income, presence of children, and select characteristics of household heads, such as age and education.[8] [9]

My analysis focuses specifically on alcoholic beverage purchases. Because the data only capture off-premise alcohol sales, this study effectively examines the tax's impact on alcohol bought for home consumption rather than on-premise sales (e.g., restaurants and bars). Alcohol purchases are aggregated to the household-month level over 48 months, from June 2009 to July 2013. Since the tax was implemented in July 2011, this allows for two years of pre- and post-tax observations. Data from earlier years are excluded to mitigate potential biases from the sharp decline in alcohol purchases during the 2007–2008 U.S. financial crisis, which may have had differential effects across states.[10] [11] To construct the analytic sample, I apply further restrictions at both the state and household levels.

At the state level, states that experienced any changes in alcohol sales or excise taxes during the study period are excluded from the sample, as these policy changes could affect alcohol purchases independently of Maryland's 2011 tax, making those states unsuitable comparison units. Specifically, Kansas and D.C. are excluded due to sales tax changes, while New York, Connecticut, North Carolina, Illinois, New Jersey, and Washington are excluded because they implemented significant alcohol excise tax increases.[12]

At the household level, I restrict the sample to households that participated for the full 48-month period surrounding the sales tax implementation to exploit household-level heterogeneity and control for time-invariant unobserved characteristics. Households that changed their state of residence while in the sample are excluded to maintain consistent exposure to the policy, as their alcohol purchasing behavior could be influenced by differences in state-level policies or economic conditions. In addition, to define the relevant population for this analysis, I limit the sample to households that made at least one alcohol purchase in the 12 months preceding the tax increase, so that the analysis focuses on the tax's impact on active alcohol consumers rather than non-drinking households.[13] The final sample includes

[8]Retail channels include grocery stores, liquor stores, drug stores, mass merchandise retailers, superstores, club stores, and convenience stores. Purchases are recorded as long as the consumer scans them.

[9]Demographic characteristics are aggregated to the household level. For households with both a male and female head, I use the higher level of education, age, and other characteristics reported. For households with a single head, I use that individual's information. Household heads are 25 years and older.

[10]Maryland raised its general sales tax from 5% to 6% in 2007; while this change was not specific to alcohol, it could still have influenced alcohol purchases and introduced additional bias into the estimates.

[11]Appendix Table [add number] reports robustness checks using data aggregated to the state level and covering additional years beyond the main household-level analysis.

[12]The specific tax changes are presented in Tables A2 and A1 in the appendix.

[13]Figure 6 includes models with the full sample which include non-drinking households.

households from 41 states, including Maryland, and consists of 10,653 households, with 205 in the treated group and 10,448 in the control group.[14]

3.1.1 Alcohol Measures

I evaluate both the quantity and the price of alcohol purchased for home consumption. Rather than using total ounces of alcohol purchased, I focus on ethanol ounces, which directly measure the alcohol content, the component with direct health implications. I calculate ethanol purchases by converting beverage ounces into ethanol equivalents, assuming standard alcohol contents of 5% for beer, 12% for wine, and 40% for spirits. By multiplying these percentages by the respective ounces purchased, I obtain a measure of monthly ethanol purchases per household. In addition to total ethanol purchases, I also look at ounces purchased by beverage type: beer, wine, and spirits

To capture variation in prices paid across households, I construct a price-per-unit measure of ethanol, following Saffer et al. (2024). This variable is calculated as total monthly alcohol spending in a household divided by the total ethanol ounces purchased in a household. It reflects the average price households pay per ounce of ethanol, inclusive of Maryland's 3% sales tax, providing a complementary perspective on purchasing behavior. All price measures are presented in $2013.

3.1.2 Household Classification

Since a key objective of this study is to examine behavioral responses across different drinking levels and income groups, it is important to first establish pre-tax consumption patterns and income distributions. Households are categorized by drinking level according to their average monthly alcohol purchases in the 12 months prior to the tax increase, adjusted for the number of adults in each household. Heavy drinkers are defined as those in the 90th percentile or above, consuming on average 47.88 ounces of ethanol per adult per month.[15] Moderate drinkers fall between the 50th and 90th percentiles, consuming an average of 4.98 ounces per adult per month, while light drinkers fall below the 50th percentile but report positive alcohol purchases, averaging roughly half an ounce of ethanol per adult per month. I define low-income households based on the 2009 and 2010 federal low-income guidelines

[14] NielsenIQ does not include data on Hawaii and Alaska.

[15] This definition aligns with estimates from the 2010 National Survey on Drug Use and Health (NSDUH), which reports that approximately 7% of the adult population are heavy drinkers.

(Department of Health and Human Services, 2009).[16] Under these guidelines, a household is classified as low-income if its annual earnings fall below \$ 18,310 for a family of three or \$ 22,050 for a family of four.[17]

4 Methods

4.1 Identification strategy

My identification strategy exploits variation in household alcohol purchases before and after Maryland's 2011 alcohol sales tax increase. Specifically, I compare monthly ethanol ounces purchased by households in Maryland with those purchased by households in other states. Although the dataset contains many households, there is only one treated state, which makes conventional difference-in-differences (DiD) methods unsuitable as standard errors can be severely underestimated when there is a single treated unit or few aggregate treatment clusters. In such settings, a weighted combination of untreated units provides a more credible counterfactual than any single comparison unit (Abadie and Gardeazabal, 2003; Abadie et al., 2010; Abadie, 2021).

To address this limitation, I use the synthetic difference-in-differences (SDiD) estimator, which combines the strengths of DiD and synthetic control method (SCM) approaches (Arkhangelsky et al., 2021). Whereas SCM is designed for cases with a single treated unit, constructing an optimally weighted combination of untreated units that closely reproduces the pre-treatment trajectory of the treated unit, SDiD extends this framework by incorporating unit and time fixed effects, as in the canonical DiD. This allows for persistent level differences between treated and control units while still matching on pre-treatment trends. By including these fixed effects, SDiD absorbs unobserved heterogeneity across households and over time, an important feature in this context, as households differ systematically in baseline alcohol consumption, purchasing frequency, and responsiveness to price changes.

Consistent with this intuition, I estimate an SDiD model that minimizes the following error (using terminology from Arkhangelsky et al. (2021)):

[16] The 2010 guidelines were nearly identical to those of 2009 due to legislation that delayed their publication.

[17] Table A3 in the appendix details the specific income thresholds for low-income classification.

$$\arg\min_{\lambda,\gamma,\mu,\beta}\Big\{\sum_{h=1}^{N}\sum_{t=1}^{T}\left(Y_{ht}-\mu-\gamma_h-\lambda_t-W_{ht}\tau\right)^2\hat{\omega}_h^{sdid}\hat{\lambda}_t^{sdid}\Big\} \tag{1}$$

where Y_{ht} is the inverse hyperbolic sine (IHS) of monthly ethanol purchases per adult (in ounces) of household h at month-year t.[18] Because ounces of ethanol purchased per adult is non-negative, highly right-skewed, and contains many zeros, I use the IHS transformation, which approximates the natural log for large values but remains defined at zero.[19] Although nonlinear models such as Poisson regressions are often appropriate for skewed non-negative outcomes (Chen and Roth, 2024), the SDiD estimator is defined for linear models and does not extend naturally to nonlinear specifications. Following best practice in the SDiD literature, I therefore use a linear model with an IHS-transformed dependent variable to approximate percentage changes while preserving the identification properties of the estimator.

W_{ht} is an indicator equal to one for households in Maryland during the post-tax period (July 2011 onward) and zero otherwise. γ_h and λ_t denote household and month-year fixed effects. τ is the average treatment effect on the treated and measures the change in household ethanol purchases after the tax relative to before, comparing Maryland to the synthetic control group. Household weights $\hat{\omega}_h^{SDiD}$ and time weights $\hat{\lambda}_t^{SDiD}$ are optimally chosen to minimize Maryland's pre-treatment prediction error.[20] Standard errors are clustered at the state level. Some specifications also include household demographic covariates reported in Table 1.[21]

The SDiD estimator is doubly robust, meaning it remains consistent if either the parallel-trends assumption holds or if the synthetic control weights correctly approximate the treated unit's counterfactual path. This makes it particularly appealing in policy settings with a single treated state or a limited number of treated units. Simulation and empirical evidence show that SDiD generally outperforms both DiD and SCM (Arkhangelsky et al., 2021). For completeness, Table 2 reports DiD and SCM estimates for comparison.[22]

[18] Appendix Figure A1 presents results estimated using $\log(Y_{ht}+1)$ as an alternative transformation of the outcome variable.

[19] Inverse hyperbolic sine transformation: $\text{asinh}(Y_{ht}) = \ln(Y_{ht}+\sqrt{Y_{ht}^2+1})$.

[20] The derivation of unit ($\hat{\omega}_h^{SDiD}$) and time ($\hat{\lambda}_t^{SDiD}$) weights is presented in Appendix B.1.1.

[21] To incorporate covariates, Arkhangelsky et al. (2021) recommend a residualization approach: first regress Y_{ht} on X_{ht} to obtain $\hat{\beta}$, then apply the SDiD algorithm to the residualized outcome $Y_{ht}^{res} = Y_{ht} - X_{ht}\hat{\beta}$.

[22] The DiD model uses a comparison group matched to Maryland via propensity score matching based on pre-tax purchase trends and demographics.

To assess the dynamics of the policy effect, I complement the main SDiD estimates with an event-study–style analysis based on the SDiD framework, following the implementation in Clarke et al. (2024). The model is estimated at the monthly level, consistent with the main specification, to fully capture the timing of the 2011 tax and short-run fluctuations in alcohol purchases. For presentation, however, I aggregate the resulting monthly coefficients to the quarterly level to facilitate interpretation and improve visual clarity, as quarterly effects provide a smoother depiction of post-treatment dynamics. This event-study analysis serves two purposes: first, to confirm that Maryland's pre-tax purchasing trends were closely aligned with its synthetic control; and second, to illustrate the temporal persistence of the treatment effect following the 2011 tax. Having outlined the SDiD estimator and its dynamic extension, I next turn to the inference procedures used to obtain valid standard errors for these estimates.

4.2 Inference

Following Arkhangelsky et al. (2021), I compute standard errors using a clustered bootstrap procedure, which provides valid inference when there are many observed units but a single treated cluster. The bootstrap proceeds as follows:

1. Draw B bootstrap resamples of households (resampled with replacement). Each resample must include both treated and control units; otherwise, it is discarded and redrawn.

2. For each resample b, re-estimate the SDiD model and obtain $\hat{\tau}_{sdid}^{(b)}$.

3. Define the bootstrap variance as:

$$\hat{V}_{\tau}^{(boot)} = \frac{1}{B}\sum_{b=1}^{B}\left(\hat{\tau}_{sdid}^{(b)} - \bar{\tau}^{boot}\right)^2 \tag{2}$$

   where $\tau^{boot} = \frac{1}{B}\sum_{b=1}^{B}\hat{\tau}_{(b)}^{sdid}$ is the mean of the bootstrap estimates.

4. The standard error is then estimated as the square root of the variance:

$$\hat{SE}_{\tau}^{boot} = \sqrt{\hat{V}_{\tau}^{(boot)}}. \tag{3}$$

I perform $B = 1{,}000$ bootstrap replications for each model. Conceptually, this procedure repeatedly re-estimates the treatment effect on resampled data to approximate the empirical sampling distribution of $\hat{\tau}^{(b)}_{SDiD}$, thus producing consistent standard errors and confidence intervals under weak distributional assumptions. The same bootstrap procedure is used for the event-study analysis. While computationally intensive, this approach is suitable here because the data include thousands of households, giving adequate resampling variation even though Maryland is the only treated state. As an additional robustness check, I report estimates obtained using the jackknife inference procedure proposed by Arkhangelsky et al. (2021).[23]

5 Results

Table 1 reports weighted pre-tax means for outcomes and household characteristics for Maryland and the comparison states, using the NielsenIQ Consumer Panel projection factor as a sample weight to ensure national representativeness.[24] The weighted means indicate that Maryland and the comparison states are broadly similar across most pre-treatment characteristics. Maryland households have somewhat higher income and education levels, a larger share of Black households, and a smaller share of Hispanic households. These differences are not consequential for the analysis, as the SDiD procedure subsequently constructs a weighted synthetic comparison that matches Maryland households' alcohol purchasing pre-treatment trends.

[Table 1 here]

Figure 1 displays raw ethanol purchasing trends for Maryland (blue circles) and the control states (black squares). The figure shows similar pre-tax trajectories in ethanol ounces purchased per adult across both groups, followed by a decline in Maryland relative to the comparison states after the tax increase. These raw trends provide descriptive evidence of comparable pre-treatment patterns and a decrease in ethanol purchases in Maryland relative to the control group.

[Figure 1 here]

[23] Results using jackknife inference are shown in Figure 6.

[24] The NielsenIQ dataset provides sample weights designed to approximate the demographic composition of the U.S. Census.

To formally assess the trends shown in Figure 1, I estimate the effect of the tax using equation 1. Figure 2 presents the dynamic event-study coefficients from the SDiD specification. Blue dots denote point estimates, and gray capped lines denote 95% confidence intervals based on standard errors from equation 3. Although the SDiD model is estimated at the monthly level, the event-study coefficients are aggregated to the quarter level for ease of display. Pre-treatment coefficients remain close to zero, and the post-treatment coefficients show a sustained decline in ethanol ounces purchased per adult, consistent with the raw trends.

[Figure 2 here]

Table 2 reports the overall treatment effect estimates, with and without covariates. Columns (1) and (2) present the preferred estimates using SDiD, while the remaining columns provide robustness checks: columns (3) and (4) report SCM estimates, and columns (5) and (6) report traditional DiD estimates with a 1:1 propensity score–matched control group.[25] All models use the IHS of monthly ethanol ounces purchased per adult as the outcome. The SDiD and DiD specifications include household and year-month fixed effects, with standard errors clustered at the state level. By construction, SCM does not include household fixed effects. Specifications (1), (3), and (5) additionally include the household demographic covariates listed in Table 1.

[Table 2 here]

Across all specifications, the estimated tax effect is negative and statistically significant, with the exception of the SCM estimate without covariates in column (3). Because the outcome is an IHS transformation, coefficients can be interpreted as semi-elasticities. For example, column (1) reports a coefficient of -0.10, corresponding to an approximate 10% reduction in monthly ethanol ounces purchased per adult in Maryland households relative to the pre-tax period. Across models, the estimated reductions range from 10% to 14%.

The lack of statistical significance in column (3) likely reflects the fact that SCM does not account for household-level heterogeneity, whereas SDiD does through the inclusion of household fixed effects. When covariates are added to the SCM specification in column (4), the estimate becomes more precise. By incorporating household fixed effects, SDiD

[25] Appendix B.2 describes in detail the SCM and DiD methods used here.

adjusts for time-invariant household characteristics, such as long-term drinking patterns or socio-economic factors, which helps avoid attenuation of the treatment effect and increases precision relative to models that do not account for such heterogeneity.

The SDiD estimates with and without covariates (columns (1) and (2)) are nearly identical in magnitude and precision, with both specifications producing a coefficient estimate of -0.10. Because household fixed effects absorb all time-invariant demographic characteristics, and the remaining covariates vary little over time within households, including covariates does not materially change the estimates. Given that the specification without covariates yields equivalent results with lower computational burden, particularly for bootstrap-based inference, column (1) is used as the preferred specification for the subsequent analyses.

5.1 Beverage Types

Having established that the tax increase reduced overall ethanol purchases, I now examine which beverage types drive this reduction and whether any substitution effects emerge. To do so, I estimate a version of the event study derived from equation 1 where the outcome variable is the inverse hyperbolic sine of ounces purchased for each beverage type: beer, wine, and spirits. Figure 3 presents the corresponding estimates, with panel 3a showing results for beer, panel 3b for wine, and panel 3c for spirits.

[Figure 3 here]

Across all beverage categories, there do not appear to be differential pre-treatment trends, and purchases decline following the tax increase, although there is some volatility in the quarter immediately after treatment. Table 3 presents the average treatment effect estimates for households in Maryland after the tax increase compared with synthetic Maryland. Column (1) reports the estimate for beer, column (2) for wine, and column (3) for spirits. The average reductions are approximately 6% for beer, 10% for wine, and 7% for spirits, translating to monthly declines of roughly 4.5 ounces of beer, 3 ounces of wine, and 0.66 ounces of spirits per adult per household. Using the ethanol content assumptions applied throughout the paper (5% for beer, 12% for wine, and 40% for spirits), these reductions correspond to monthly decreases of approximately 0.22 ounces of ethanol from beer, 0.36 ounces from wine, and 0.26 ounces from spirits per adult per household. In volume terms, beer accounts for the largest decline, whereas in ethanol terms the reductions are similar across beverage types, with no evidence of substitution across alcohol categories.

[Table 3 here]

5.2 Heterogeneous behavioral responses

The results so far indicate an overall decrease in ethanol purchases with no evidence of substitution across beverage types. However, these aggregate effects may mask different response patterns across consumer groups. In particular, both drinking behavior and economic resources may shape the extent to which households adjust their purchasing patterns following a price change. I therefore examine heterogeneity along two dimensions: household drinking levels and household income. After that, I analyze the price paid per unit of ethanol to assess potential substitution toward lower-priced products. Finally, I consider households residing in border counties, who may have the option to engage in cross-border shopping, and assess how their purchase levels respond to the tax increase.

5.2.1 Drinking levels and income groups

I first evaluate how monthly ethanol ounces purchased per adult change across household drinking levels and income groups. Figure 4 presents the event study estimates for heavy drinkers (panel 4a), moderate drinkers (panel 4b), light drinkers (panel 4c), and low-income households (panel 4d). Heavy drinkers exhibit the largest post-treatment reductions in ethanol purchases. Moderate drinkers also reduce their purchases, although the first three post-treatment quarters are not statistically different from zero. Light drinkers show a smaller decline in the post-treatment period, with an increase emerging near the two-year mark after the tax change. In contrast, low-income households display an increase in ethanol purchases following the tax increase.

[Figure 4 here]

Table 4 reports the corresponding average treatment effects estimated from equation 1 using SDiD, with the outcome measured as the inverse hyperbolic sine of monthly ethanol ounces purchased per adult. Columns (1)–(3) report estimates for heavy, moderate, and light drinkers, respectively, while column (4) presents results for low-income households. Confirming event study estimates, overall ethanol purchases decline in response to the tax increase across drinking levels. The estimated reductions are approximately 36% for heavy drinkers, 7% for moderate drinkers, and 3% for light drinkers. These correspond to decreases of roughly 17 ounces of ethanol per adult per month for heavy drinkers, 0.4 ounces for moderate drinkers, and 0.02 ounces for light drinkers.

For low-income households, the estimated effect differs from the drinking-level patterns, as the coefficient implies an 11% increase in ethanol purchases in the post-treatment period, corresponding to an increase of approximately 0.5 ounces per adult per month (less than a shot). These results suggest that low-income households exhibit higher post-treatment ethanol purchases relative to their pre-treatment levels. One possibility is that low-income households adjust the mix of products they purchase, such as opting for lower-priced or larger-volume products, which can increase total ethanol purchased even when overall spending does not rise.

[Table 4 here]

5.2.2 Price paid per unit of ethanol

I next examine how the tax affected the price households pay per unit of ethanol, which mostly reflect consumer substitution within alcohol categories. Following Saffer et al. (2024), I construct a measure of the average dollar amount paid per ounce of ethanol in each household-month. This measure captures potential shifts toward lower-priced alcohol products in response to the tax. To explore whether these adjustments vary across consumer groups, I estimate equation 1 using the inverse hyperbolic sine of the price per unit of ethanol as the outcome and report results in Table 5. Column (1) presents estimates for the full sample, column (2) for heavy drinkers, column (3) for moderate drinkers, column (4) for light drinkers, and column (5) for low-income households.

Across all groups except low-income households, there is a statistically significant reduction in the price paid per unit of ethanol following the tax increase. These declines should not be interpreted as evidence that the tax reduces prices; rather, they are consistent with consumers substituting toward lower-priced products. For the full sample, households pay approximately 3% less per ounce of ethanol after the tax. Heavy drinkers exhibit the largest change, paying about 8% less per ounce of ethanol, while moderate drinkers pay roughly 2% less and light drinkers about 3% less. For low-income households, the estimates indicate no meaningful change in the price paid per unit of ethanol. In dollar terms, these reductions correspond to decreases of approximately \$0.02 for the full sample, \$0.10 for heavy drinkers,

$0.01 for moderate drinkers, and less than a cent for light drinkers per ounce of ethanol. Relative to average pre-tax ethanol purchases, these dollar changes represent very small adjustments in monthly alcohol expenditures. For low-income households, the combination of a trivial increase in ethanol purchases and no detectable change in the price paid per unit suggests that any post-treatment adjustment likely reflects changes in product mix rather than changes in prices, consistent with households maintaining consumption levels using similarly priced alcohol products.

[Table 5 here]

5.2.3 Border counties

Another potential response to alcohol taxes is cross-border shopping, particularly for households located near lower-tax jurisdictions. Maryland borders several states, creating opportunities for consumers to avoid the tax by purchasing alcohol elsewhere. To assess whether proximity to other states affects purchasing patterns, I compare ethanol ounces purchased by households residing in Maryland border counties with those in non-border counties.

Because this analysis focuses exclusively on Maryland households, the SDiD framework is not applicable. Instead, I estimate a traditional two-way fixed effects (TWFE) model with household and time fixed effects, clustering standard errors at the county level. By interacting the event-time indicators with an indicator for border-county residence, this specification identifies whether the post-tax changes in ethanol purchases differ for border households relative to non-border households. Figure 5 presents the event study estimates from this model.

[Figure 5 here]

Although this approach does not directly observe cross-border shopping, it captures whether households in border counties reduce their ethanol purchases differently from those in non-border counties. If cross-border shopping were substantial, Maryland households living in border counties would be expected to reduce their alcohol purchases less than households in non-border counties, because they could shift some of their shopping to nearby lower-tax states. In the event-study framework, this would appear as positive post-treatment coefficients for border counties, since their overall alcohol purchases would fall by a smaller amount relative to non-border households. However, the estimates show no such pattern. Border and non-border households reduce their ethanol purchases by similar amounts, and in several post-treatment quarters border households reduce slightly more. These results indicate no meaningful differences in purchasing behavior between border and non-border households.

5.3 Robustness checks

In addition to the alternative model specifications already presented in Table 2, I assess the robustness of the main SDiD estimates using two further sets of checks: an alternative inference procedure and alternative sample definitions. These checks evaluate whether the estimated average treatment effect of the Maryland sales tax is sensitive to the choice of standard errors or to the composition of the household sample.

Figure 6 summarizes these robustness exercises using a coefficient plot. Each point represents an SDiD estimate of the average treatment effect of the Maryland sales tax on the inverse hyperbolic sine of monthly ethanol ounces purchased per adult under a different robustness specification. Blue circles reproduce the main result from Table 2, column (1), and serve as the reference point. Red diamonds correspond to jackknife inference, green squares represent estimates using a sample that includes non-drinking households, and teal crosses show estimates based on a donor pool restricted to states geographically close to Maryland.

[Figure 6 here]

Because the preferred specification relies on household and month-year fixed effects with standard errors clustered at the state level, I examine whether inference is sensitive to alternative approaches. First, I compute jackknife standard errors (red diamonds in Figure 6). The jackknife evaluates how the estimated effect varies when individual households are iteratively left out of the donor pool.[26] The resulting standard errors are larger but the estimate remains statistically significant.

The main specification throughout the paper restricts the sample to households that purchase alcohol at least once in the pre-treatment period. To assess whether the estimated

[26] Placebo inference proposed by Arkhangelsky et al. (2021) is not appropriate in this setting because it requires equal-sized clusters and is designed for settings where treatment assignment occurs at the cluster level rather than at the household level.

effect depends on this restriction, I re-estimate the model using a broader sample that includes households regardless of whether they make any alcohol purchases (green squares in Figure 6). This check evaluates whether infrequent or non-drinking households influence the results. In addition, I estimate the SDiD model using a donor pool limited to states geographically close to Maryland (teal cross): Delaware, Pennsylvania, Virginia, and West Virginia. Across all robustness specifications, the estimated effects remain negative. When including non-drinking households, the magnitude of the estimated effect decreases slightly but remains statistically significant and in the same direction, indicating that the main findings are not driven by the sample restriction to drinkers. The estimate based on nearby donor states is also negative, though somewhat smaller in magnitude, consistent with the reduced donor pool size.

Together, these robustness checks show that the estimated effect of the tax is stable across inference procedures and alternative sample definitions.

6 Discussion

The literature suggests that increases in the price of alcohol generally lead to reductions in alcohol consumption (Wagenaar et al., 2009; Maldonado-Molina and Wagenaar, 2010; Saffer et al., 2024; Xuan et al., 2015); however, the extent and consistency of these effects are complicated by the inelastic nature of alcohol demand. In particular, the impact of price changes on consumption is not uniform across different types of consumers, as price sensitivity varies depending on factors such as drinking habits and income levels (Ayyagari et al., 2013; Gehrsitz et al., 2021; Pryce et al., 2019). While much of the existing research has focused on the effects of excise taxes on alcohol, less attention has been given to the role of alcohol sales taxes, which are implemented in only a few states. This paper addresses this gap by examining the impact of Maryland's 2011 alcohol sales tax on total ethanol purchases, beverage-type purchasing patterns, and key behavioral responses across consumer groups, including differences by drinking intensity, price paid per unit of ethanol, and proximity to state borders.

The primary takeaway from the main results is the significant reduction in ethanol purchases following the tax increase. My preferred specification, SDiD, estimates a 10% reduction in ethanol purchased per adult per household, which is both economically meaningful and statistically significant. This reduction corresponds to approximately 1.2 fewer ounces of ethanol purchased per month per adult, equivalent to a reduction of two standard drinks.[27] This finding is consistent with the broader literature on the effectiveness of price increases, such as excise taxes, in reducing harmful consumption behaviors (Wagenaar et al., 2009), where higher prices typically lead to lower alcohol purchases. This result is robust across different model specifications.

To understand how these reductions manifest across products, I next examine the impact by beverage type. I find no evidence of substitution among categories. Although beer accounts for the largest decline in raw volume terms, the reductions are similar across beverages when expressed in ethanol ounces. This indicates that the tax reduced overall alcohol purchases rather than shifting consumption toward particular products. While beer is often found to be more inelastic (Gehrsitz et al., 2021), I still observe a decrease of approximately 4.5 ounces of purchased beer per adult per month, equivalent to about one-third of a standard 12-ounce can. Although small in magnitude, this reduction reinforces the conclusion that the tax affected all beverage types rather than prompting substitution among them. This pattern is broadly consistent with Esser et al. (2016), who also document declines in beer purchases following the Maryland tax increase.

A critical aspect of this analysis is the ability to explore heterogeneous effects based on drinking levels, made possible by the panel structure of the data and the long pre-treatment observation window for each household. The SDiD estimates indicate a 36% reduction in monthly ethanol ounces purchased per adult among heavy-drinking households. Although heavy drinkers are generally thought to have relatively inelastic demand, the magnitude of the effect is consistent with their higher baseline consumption. In absolute terms, this translates to a reduction of roughly one 750 ml bottle of spirits per month per adult in the household. This result is particularly salient given the common policy concern that heavy drinkers are unresponsive to price changes. Notably, while this finding contrasts with earlier literature, it is consistent with recent evidence from Saffer et al. (2024), who also document a significant decline in alcohol purchases among heavy drinkers following a tax increase. Taken together, these findings suggest that sales taxes can meaningfully reduce excessive alcohol purchases among the group most associated with alcohol-related externalities, such as impaired driving.

The estimated effects for moderate and light drinkers are notably smaller. Moderate drinkers reduce their ethanol consumption by the equivalent of over half a shot per month, and the effects for light drinkers are economically negligible. These patterns are intuitive:

[27] A standard drink contains 0.6 ounces of ethanol.

heavy drinkers adjust in absolute terms because they consume large quantities; moderate drinkers adjust modestly; and light drinkers, who consume very little to begin with, show negligible behavioral change. The minimal response among light drinkers also indicates that the tax does not meaningfully burden individuals who drink infrequently. In contrast, low-income drinkers appear to increase their ethanol ounces purchased. To further explore this pattern, I estimate changes in the price paid per unit of ethanol. This measure captures how much a household spends per ounce of ethanol and can reveal substitution toward different alcohol products. For example, households may purchase alcohol with higher ethanol content or larger container sizes, which would increase total ethanol purchased without necessarily increasing the price per ounce if they are constrained by pre-tax expenditure levels.

For all drinking groups except low-income households, there is a statistically significant decline in the price paid per ounce of ethanol following the tax. This should not be interpreted as evidence that the tax lowered prices; rather, it reflects substitution toward cheaper products within beverage categories. The fact that low-income households do not exhibit a decline in the price paid per unit but do show an increase in ethanol ounces purchased is informative. This group likely already purchases the lowest-priced products available, leaving little room for further downward price substitution. Combined with the observed increase in ethanol purchases, this pattern suggests that low-income households may have responded to the tax by shifting toward products that deliver more ethanol at similar prices, such as larger container sizes or higher-ethanol products, in order to maintain or increase ethanol intake despite higher tax-inclusive prices. This indicates tighter constraints on their substitution margins. It is important to note that these substitution patterns have welfare implications, many of which are not observable (Saffer et al., 2024).

Finally, as a last behavioral response, I examine what happened to households living in Maryland border counties, where cross-border shopping is more accessible. I find no differential purchasing patterns in border counties compared with non-border counties. This implies that Maryland border households, despite having the option to shop in nearby states, reduced their ethanol purchases similarly to households in non-border areas. These differential patterns highlight how sales taxes can affect consumer behavior in distinct ways across market segments, with implications for both tax effectiveness and equity.

7 Conclusion

Overall, the 2011 sales tax increase in Maryland appears to have been an effective tool for reducing ethanol purchases. Across specifications, the tax is associated with a decline in monthly ethanol ounces purchased per adult per household, equivalent to roughly two fewer standard alcoholic drinks per month. There is no evidence of substitution across beverage types; purchases of beer, wine, and spirits all decrease following the tax. These reductions are particularly pronounced among heavy drinkers, who purchase the equivalent of one fewer 750 ml bottle of spirits per month. Low-income households show a different pattern, displaying signs of tighter substitution in their responses to the tax. Taken together, the findings show that the sales tax effectively reduced alcohol purchases overall, while also highlighting potential distributional considerations for low-income households with more limited substitution options.

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Figures

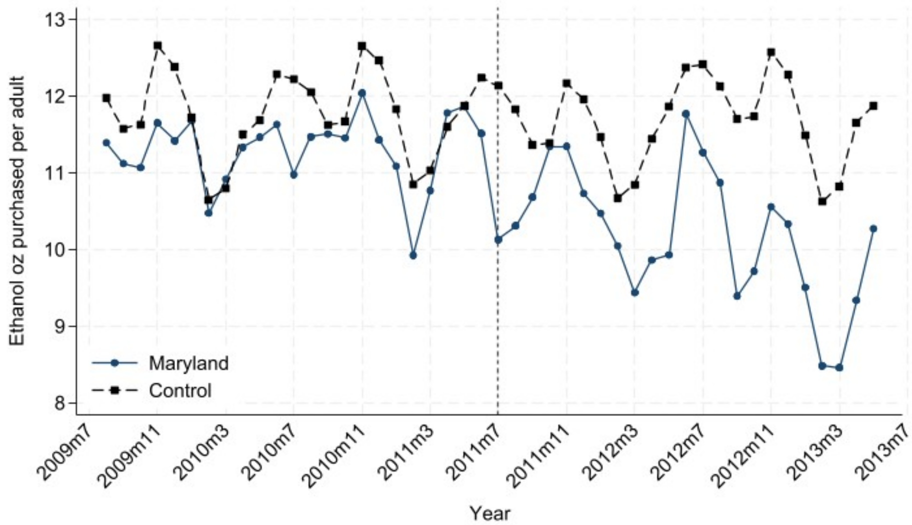


Figure 1. Trends in Ethanol Ounces Purchased per Adult in Maryland and Control States: Pre- and Post-Tax Increase

Note. The Y-axis shows the three-month moving average of ethanol ounces purchased per adult per household, and the X-axis represents month–year. The figure presents two series: average alcohol consumption in Maryland (blue circles, solid line) and in the comparison states (black squares, dashed line). The vertical dashed line indicates the month of Maryland's sales tax increase (July 2011). Comparison states exclude households in Connecticut, Illinois, New Jersey, New York, North Carolina, Washington, the District of Columbia, and Kansas, as these states experienced alcohol tax changes during the sample period. Data come from the NielsenIQ Consumer Panel.

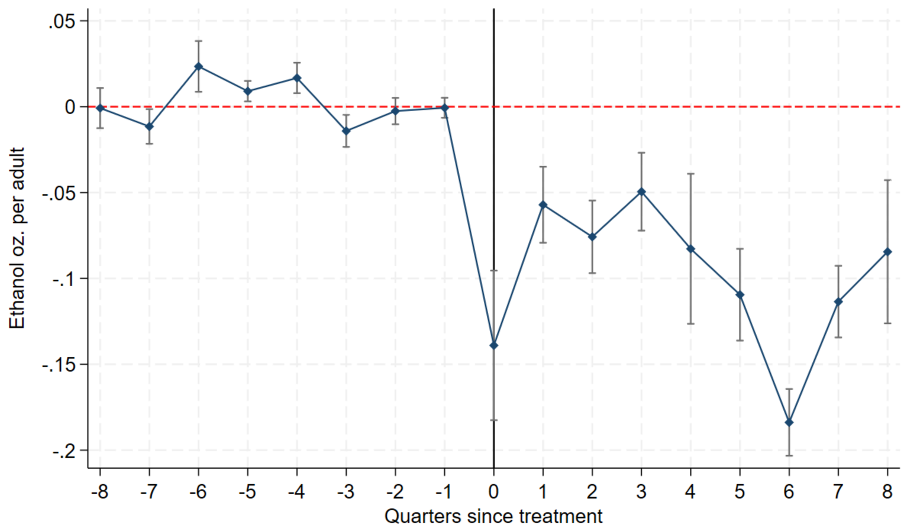
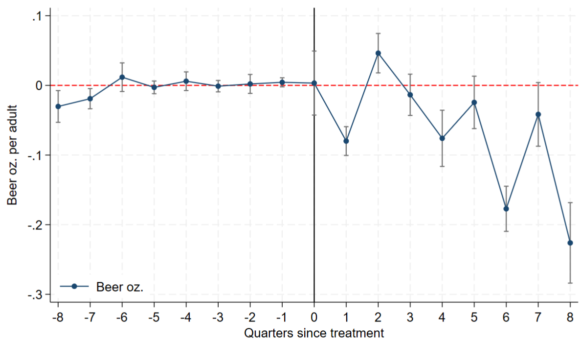
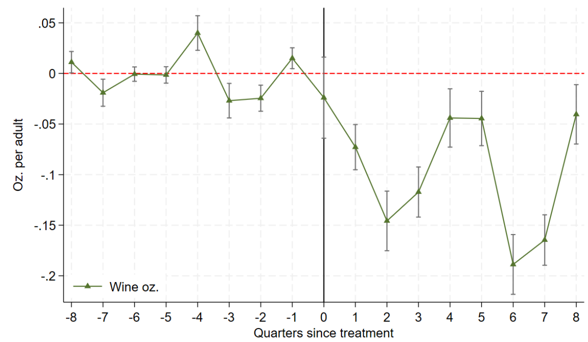


Figure 2. Dynamic Event study estimates for ethanol ounces purchased

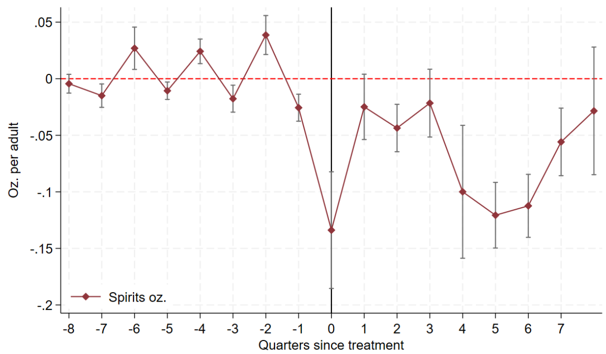
Note. The figure shows dynamic treatment effects estimated using synthetic difference-in-differences (SDiD). The vertical line at event time zero marks the month of the tax change. The dependent variable is the inverse hyperbolic sine of ethanol ounces purchased per adult per household per month. Monthly event-time coefficients were averaged to the quarter level. Blue diamonds indicate point estimates, and gray capped lines represent 95% confidence intervals computed from 1,000 bootstrap replications, standard errors were clustered at the state level. Data on alcohol purchases come from the NielsenIQ Consumer panel for the period from June 2009 to July 2013.



(a) Beer



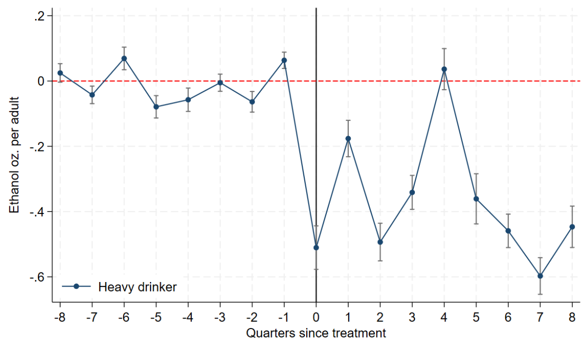
(b) Wine



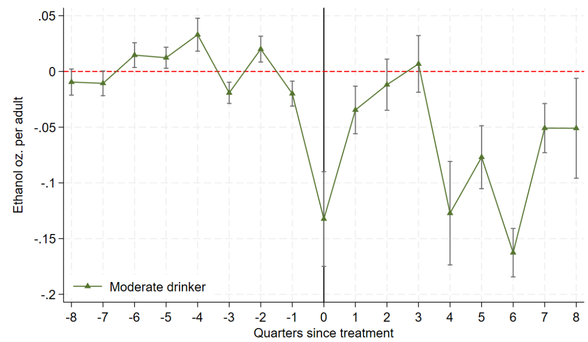
(c) Spirtis

Figure 3. Event study estimates by alcohol type: beer, wine, and spirits

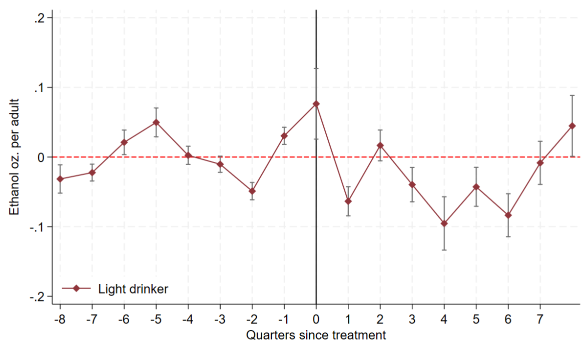
Note. The figure presents event study estimates of the inverse hyperbolic sine of ethanol ounces purchased per adult per household per month by alcohol type, estimated using the Synthetic Difference-in-Differences (SDID) method. Monthly event-time coefficients were averaged to the quarter level for presentation. The vertical line at event time zero marks the month of the tax change. Panel (a) displays results for beer (blue circles), panel (b) for wine (green triangles), and panel (c) for spirits (maroon diamonds). Gray capped lines represent 95% confidence intervals estimated from 1,000 bootstrap replications. Data on alcohol purchases are from the NielsenIQ Consumer Panel for the period June 2009–July 2013.



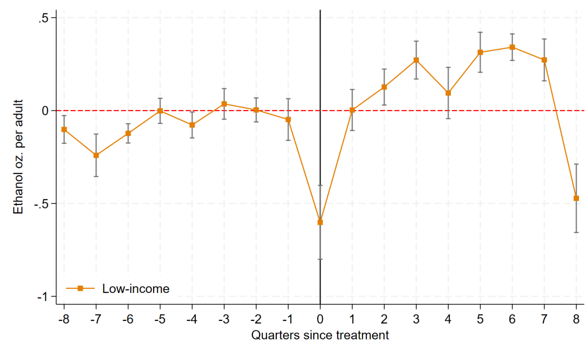
(a) Heavy drinker



(b) Moderate drinker



(c) Light drinker



(d) Low-income drinker

Figure 4. Event study estimates by drinker type: light, moderate, and heavy

Note. The figure presents event study estimates of the inverse hyperbolic sine of ethanol ounces purchased per adult per household per month by drinker type, estimated using the Synthetic Difference-in-Differences (SDID) method. Monthly event-time coefficients were averaged to the quarter level for presentation. The vertical line at event time zero marks the month of the tax change. Panel (a) displays results for heavy drinkers (blue circles), panel (b) for moderate drinkers (green triangles), panel (c) for light drinkers (maroon diamonds), and panel (d) for low-income drinkers (yellow squares). Gray capped lines represent 95% confidence intervals estimated from 1,000 bootstrap replications. Data on alcohol purchases are from the NielsenIQ Consumer Panel for the period June 2009–July 2013.

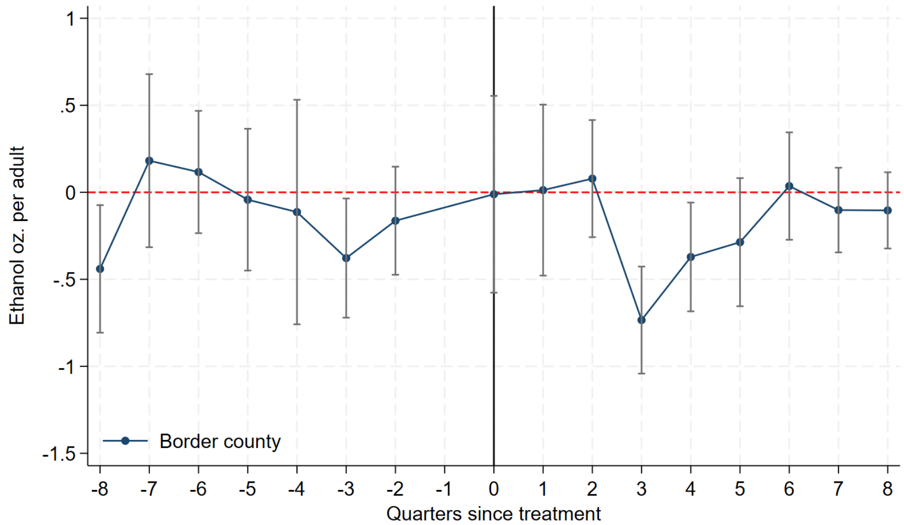


Figure 5. Event study estimates for ethanol ounces purchased for Maryland border counties

Note. The figure shows treatment effects estimated using a TWFE model restricted to Maryland households. The dependent variable is the inverse hyperbolic sine of monthly ethanol ounces purchased per adult per household. Monthly event-time coefficients are averaged to the quarter level for display. The vertical line at event time zero marks the month of the tax change. Blue circles indicate point estimates, and gray capped lines represent 95% confidence intervals. Standard errors are clustered at the county level.

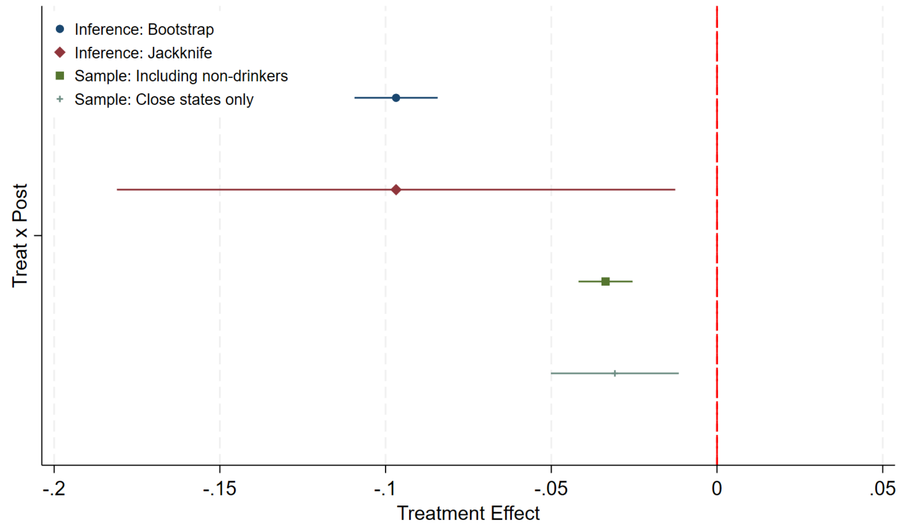


Figure 6. SDiD estimates under alternative inference procedures and sample restrictions

Note. The figure shows synthetic difference-in-differences (SDiD) estimates across alternative inference procedures and sample restrictions. The dependent variable is the inverse hyperbolic sine of monthly ethanol ounces purchased per adult per household. Blue circles represent the main SDiD estimates using the clustered bootstrap for inference, and red diamonds represent estimates obtained using the jackknife. Green squares show estimates from a sample that includes non-drinkers, and teal crosses show estimates using only nearby donor states (Alabama, Virginia, New Jersey, and Delaware). Lines represent 95% confidence intervals. Standard errors are clustered at the state level for all specifications except the jackknife which is clustered at the household level.

Tables

Table 1. Weighted Pre-Treatment Means for Maryland and Comparison States

	Comparison States	Maryland
Ethanol ounces per adult	12.43	11.96
Total quantity of alcohol (oz) per adult	125.30	105.20
Quantity of beer (oz) per adult	90.46	62.43
Quantity of wine (oz) per adult	22.34	30.63
Quantity of spirits (oz) per adult	12.49	12.14
Heavy drinker	0.18	0.20
Moderate drinker	0.64	0.57
Light drinker	0.18	0.23
Low income	0.09	0.06
Premium drinker	0.11	0.22
Household size	2.35	2.19
Income: < $25,000	0.18	0.11
Income: $25,000-$34,999	0.10	0.10
Income: $35,000-$49,999	0.15	0.10
Income: $50,000–$69,999	0.17	0.14
Income: $70,000-$99,999	0.16	0.26
Income: $100,000+	0.24	0.30
Head age: 25-44 Years	0.19	0.17
Head age: 45-64 Years	0.56	0.59
Head age: 65+ Years	0.25	0.25
Head education: < High school graduate	0.02	0.00
Head education: High school graduate	0.28	0.22
Head education: Some college	0.31	0.28
Head education: College graduate+	0.38	0.50
Married	0.46	0.45
With child	0.24	0.26
White	0.81	0.58
Asian	0.03	0.05
Black	0.10	0.32
Hispanic	0.11	0.03

Notes. Each observation is at the household-month level. This table presents pre-treatment means for households in Maryland and comparison states. Comparison states exclude households in Connecticut, Illinois, New Jersey, New York, North Carolina, Washington, the District of Columbia, and Kansas, as these states experienced alcohol tax changes during the sample period. Values for demographic and household characteristics (e.g., drinking levels) are expressed as proportions. For age and education, the reported values correspond to the highest level among male and female household heads when both are present. Data come from the NielsenIQ Consumer Panel, and means are weighted using the panel's projection factor.

Table 2. Impact of the Maryland 2011 sales tax on ethanol ounces purchased.

	SDiD		SCM		DiD	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat x Post	-0.10***	-0.10***	-0.12	-0.14***	-0.13***	-0.13***
	(0.01)	(0.01)	(0.08)	(0.05)	(0.01)	(0.01)
MD Pre-treatment mean	11.29	11.29	11.29	11.29	11.29	11.29
Household FE	YES	YES	NO	NO	YES	YES
Month-year FE	YES	YES	YES	YES	YES	YES
Household demographic covariates	NO	YES	NO	YES	NO	YES
Observations	511344	511344	511344	511344	20256	20256

Note: *** p<0.01, ** p<0.05, * p<0.10. The table reports estimates from six model specifications using the same outcome variable: the inverse hyperbolic sine of monthly ethanol ounces purchased per adult per household. Columns (1) and (2) present estimates from the baseline Synthetic Difference-in-Differences (SDiD) model; columns (3) and (4) report estimates from the Synthetic Control Method (SCM); and columns (5) and (6) report estimates from a Difference-in-Differences (DiD) model in which the control group is identified using propensity score matching. Columns (2), (4), and (6) include household-level covariates. Column (1) represents the preferred specification. Models (1), (2), (5), and (6) include household and year-month fixed effects, with standard errors clustered at the state level. The SCM specifications include only month-year fixed effects by construction. Standard errors for the SDiD estimates are obtained via bootstrapping with 1,000 replications. Alcohol purchase data come from the NielsenIQ Consumer Panel for the period June 2009–July 2013.

Table 3. Impact of the Maryland 2011 sales tax on ounces of beer, wine and spirits purchased.

	Beer	Wine	Spirits
	(1)	(2)	(3)
Treat x Post	-0.06***	-0.10***	-0.07***
	(0.01)	(0.01)	(0.01)
MD Pre-treatment mean	75.17	28.85	9.44
Household FE	YES	YES	YES
Month-year FE	YES	YES	YES
Household demographic covariates	NO	NO	NO
Observations	511344	511344	511344

Note: *** p<0.01, ** p<0.05, * p<0.10. The table presents synthetic difference-in-differences (SDID) estimates for three outcome variables: the inverse hyperbolic sine of ethanol ounces purchased per adult per household per month for (1) beer, (2) wine, and (3) spirits. All specifications include household and year-month fixed effects. Standard errors, reported in parentheses, are obtained via bootstrapping with 1,000 replications and clustered at the state level. Data on alcohol purchases are from the NielsenIQ Consumer Panel for the period June 2009–July 2013.

Table 4. Impact of the Maryland 2011 sales tax by drinking and income level.

	Heavy drinker	Moderate drinker	Light drinker	Low-income drinker
	(1)	(2)	(3)	(4)
Treat x Post	-0.36***	-0.07***	-0.03***	0.11***
	(0.02)	(0.01)	(0.01)	(0.04)
MD Pre-treatment mean	47.60	5.22	0.53	4.52
Household FE	YES	YES	YES	YES
Month-year FE	YES	YES	YES	YES
Household demographic covariates	NO	NO	NO	NO
Observations	90816	328080	92448	29520

Note: *** p<0.01, ** p<0.05, * p<0.10. This table presents synthetic difference-in-differences estimates for the natural log plus one of the ounces of ethanol purchased per adult across three distinct drinking groups: (1) heavy drinkers, defined as those in the 90th percentile or above; (2) moderate drinkers, defined as those between the 90th and 50th percentiles; and (3) light drinkers, defined as those below the 50th percentile of the drinking distribution 12 months before the tax increase. All specifications include household and year-month fixed effects. Standard errors, reported in parentheses, are clustered at the state level. Confidence intervals estimated from 1,000 bootstrap replications. Data on alcohol purchases are from the NielsenIQ Consumer Panel for the period June 2009–July 2013. Data on alcohol purchases and household drinking distribution come from the NielsenIQ Consumer panel for the period from June 2009 to July 2013.

Table 5. Impact of the Maryland 2011 sales on price paid per unit of ethanol by drinking and income level.

	All	Heavy drinkers	Moderate drinkers	Light drinkers	Low-income
	(1)	(2)	(3)	(4)	(5)
Price per unit of ethanol	-0.03***	-0.08***	-0.02***	-0.03***	-0.01
	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)
Pre-treatment mean	0.69	1.27	0.68	0.22	0.44

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.10$. The table presents synthetic difference-in-differences (SDID) estimates of the effects of Maryland's 2011 alcohol sales tax increase on the average price paid per unit of ethanol. Columns (1)–(5) correspond to all households, heavy drinkers, moderate drinkers, light drinkers, and low-income households, respectively. All specifications include household and year–month fixed effects. Standard errors, reported in parentheses, are obtained via bootstrapping with 1,000 replications and clustered at the state level. Pre-treatment means correspond to the average value of each outcome in the period before the tax change for that group. Data on alcohol purchases are from the NielsenIQ Consumer Panel for the period June 2009–July 2013.

A Appendix

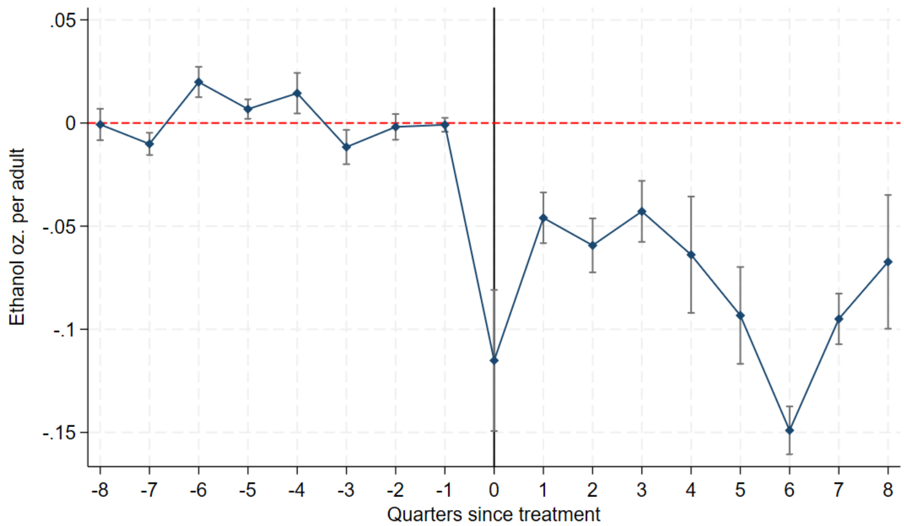
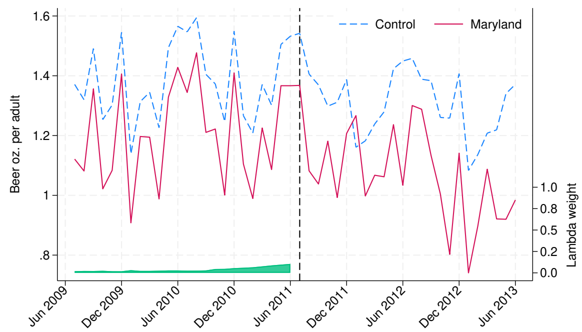
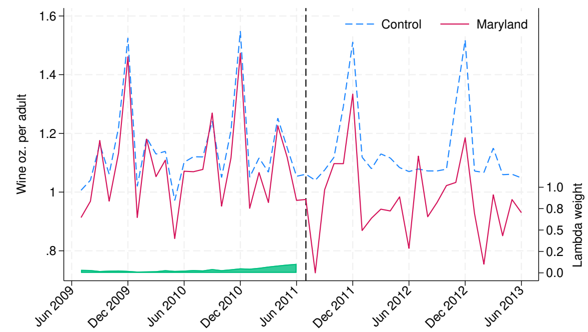


Figure A1. Dynamic Event study estimates for ethanol ounces purchased

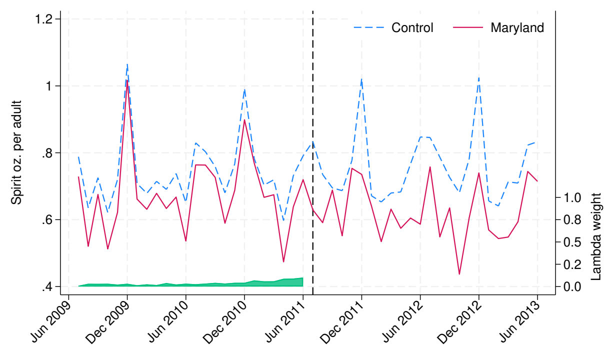
Note. The figure shows dynamic treatment effects estimated using synthetic difference-in-differences (SDiD). The vertical line at event time zero marks the month of the tax change. The dependent variable is the natural log plus one of ethanol ounces purchased per adult per household per month. Monthly event-time coefficients were averaged to the quarter level. Blue diamonds indicate point estimates, and gray capped lines represent 95% confidence intervals computed from 1,000 bootstrap replications, standard errors were clustered at the state level. Data on alcohol purchases come from the NielsenIQ Consumer panel for the period from June 2009 to July 2013.



(a) Beer



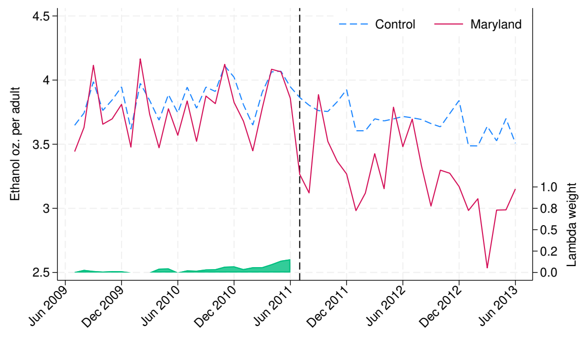
(b) Wine



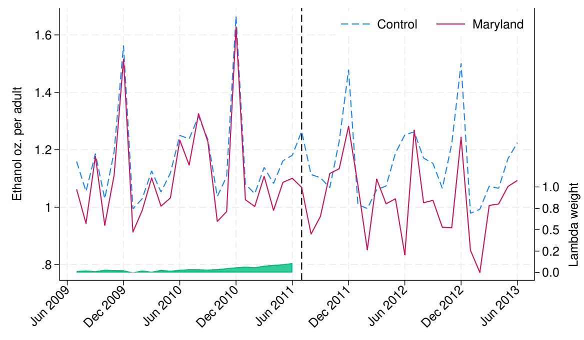
(c) Spirits

Figure A2. Trends in alcohol purchases by beverage for Maryland and synthetic control

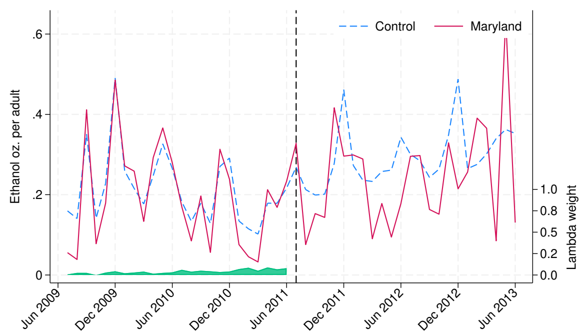
Note. Trends in ounces of ethanol purchased per adult for Maryland households using Synthetic Difference-in-Differences (SDiD) weights, disaggregated by alcohol type: beer, wine, and spirits. Panel (a) reports trends for beer, panel (b) for wine, and panel (c) for spirits. The time weights (λ_t) are estimated by minimizing the error term in Equation 1. The solid line shows the trend for Maryland, and the dashed line shows the corresponding trend for the synthetic control.



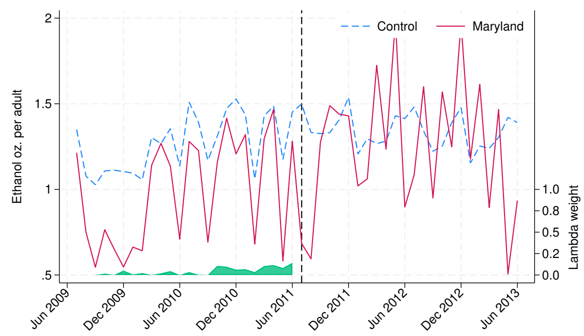
(a) Heavy drinker



(b) Moderate drinker



(c) Light drinker



(d) Low-income drinker

Figure A3. Trends in ethanol purchases by drinking and income levels for Maryland and synthetic control

Note. Trends in ounces of ethanol purchased per adult for Maryland households using Synthetic Difference-in-Differences (SDiD) weights, disaggregated by heavy, moderate, light, and low-income drinkers. Panel (a) reports trends for heavy drinkers, panel (b) for moderate drinkers, panel (c) for light drinkers, and panel (d) for low-income drinkers. The time weights (λ_t) are estimated by minimizing the error term in Equation 1. The solid line shows the trend for Maryland, and the dashed line shows the corresponding trend for the synthetic control.

Table A1. States with Alcohol Sales Tax Changes from June 2009 to July 2013

State	FIPS	Date	On-Premise	Off-Premise
District of Columbia	11	9/30/2009	↓ (4.25% to 4%)	↓ (3.25% to 3%)
Kansas	20	6/30/2010	↓ (4.70% to 3.70%)	↓ (2.70% to 1.70%)
Kansas	20	6/30/2013	↑ (3.70% to 3.85%)	↑ (1.70% to 1.85%)
Maryland	24	6/30/2011	↑ (3%)	↑ (3%)

Note: This table lists states that implemented changes in alcohol sales tax rates between 2009 and 2013, based on data from the Alcohol Policy Information System (APIS) (National Institute on Alcohol Abuse and Alcoholism, 2024). The table details the direction and magnitude of tax changes for on-premise (e.g., bars, restaurants) and off-premise (e.g., retail stores) alcohol purchases.

Table A2. States with Alcohol Excise Tax Increases from June 2009 to July 2013

State	FIPS	Date	Beer tax	Wine tax	Spirit tax
Connecticut	9	5/4/2011	↑ \$0.04	↑ \$0.12	↑ \$1.00
Illinois	17	9/1/2009	↑ \$0.04	↑ \$0.66	↑ \$4.05
New Jersey	9	8/1/2011	↑ \$0.04	↑ \$0.18	↑ \$1.10
New York	36	5/1/2009	↑ \$0.03	↑ \$0.11	-
North Carolina	37	9/1/2009	↑ \$0.09	↑ \$0.21	-
Washington (FIPS 53) became a monopoly state on 12/8/2011.					

Note: This table lists states that implemented alcohol tax increases between 2009 and 2013, based on data from the Alcohol Policy Information System (APIS) (National Institute on Alcohol Abuse and Alcoholism, 2024). The table details the tax increase amounts for beer, wine, and spirits, where applicable.

Table A3. 2009 Federal Poverty Guidelines for the 48 Contiguous States and the District of Columbia

Persons in Family	Poverty Guideline ($)
1	10,830
2	14,570
3	18,310
4	22,050
5	25,790
6	29,530
7	33,270
8	37,010

Note: This table describes the federal poverty guidelines by family size (Department of Health and Human Services, 2009). For families with more than 8 persons, add $3,740 per additional person.

B Methods Appendix

B.1 Synthetic difference-in-difference

B.1.1 Unit and time weights

Equation 1 describes the error that SDiD minimizes. Part of this objective are the unit and time weights, $\hat{\omega}_h^{SDiD}$ and $\hat{\lambda}_t^{SDiD}$. The weights are chosen to minimize the pre-treatment prediction error. This appendix provides additional detail on the construction of these weights, following Arkhangelsky et al. (2021).

Unit weights. Unit weights are obtained by solving:

$$(\hat{\omega}_0, \hat{\omega}^{sdid}) = \arg \min_{\omega_0 \in \mathbb{R},\, \omega \in \Omega} \sum_{t=1}^{T_{pre}} \left(\omega_0 + \sum_{h=1}^{N_{co}} \omega_h Y_{ht} - \bar{Y}_t^{tr} \right)^2 + \zeta_{T_{pre}}^2 \|\omega\|_2^2, \tag{4}$$

where

$$\bar{Y}_t^{tr} = \frac{1}{N_{tr}} \sum_{h=N_{co}+1}^{N} Y_{ht}. \tag{5}$$

Here, $\|\omega\|_2$ is the Euclidean norm and ζ is a regularization described in Arkhangelsky et al. (2021). This procedure yields a vector of non-negative unit weights, $\hat{\omega}^{sdid}$, and an intercept ω_0. The weights ω_h ensure that the synthetic control created from untreated households matches Maryland's pre-treatment trajectory, while ω_0 allows for level differences between Maryland and its synthetic counterpart. Thus, SDiD matches trends, not necessarily looking for a perfect pre-treatment fit, unlike the synthetic control method, which requires exact pre-treatment fit.

Time weights. Time weights are obtained through an analogous optimization:

$$(\hat{\lambda}_0, \hat{\lambda}^{sdid}) = \arg \min_{\lambda_0 \in \mathbb{R},\, \lambda \in \Lambda} \sum_{h=1}^{N_{co}} \left(\lambda_0 + \sum_{t \leq T_{pre}} \lambda_t Y_{ht} - \bar{Y}_{post}^{tr} \right)^2 + \zeta_{N_{co}}^2 \|\lambda\|_2^2, \tag{6}$$

where

$$\bar{Y}_{post}^{tr} = \frac{1}{T_{post}} \sum_{t > T_{pre}} \bar{Y}_t^{tr}. \tag{7}$$

Intuitively, the time weights $\hat{\lambda}^{sdid}$ assign more weight to pre-treatment periods whose outcomes most closely resemble the post-treatment period.** This allows SDiD to create a synthetic "time path" that places greater emphasis on pre-tax months that are most predictive of post-tax behavior. In other words, while unit weights make the control group resemble Maryland, time weights make the pre-treatment periods used for comparison resemble the dynamics of the treated period. The resulting estimator therefore balances treated vs. control and early vs. late periods in a way that minimizes pre-treatment prediction error.

B.1.2 Jackknife inference

As an alternative to the bootstrap, I implement the jackknife variance estimator proposed by Arkhangelsky et al. (2021). This approach is computationally less intensive and provides conservative standard errors when the number of observed units is large.

Let $\hat{\tau}^{sdid}$ denote the treatment effect estimated from the full sample. The jackknife procedure proceeds as follows:

1. Sequentially remove one household i from the sample and re-estimate the SDID model, keeping the optimal unit and time weights $\hat{\omega}^{sdid}$ and $\hat{\lambda}^{sdid}$ fixed from the original estimation.
2. Denote the resulting estimate as $\hat{\tau}^{sdid}_{(-i)}$.
3. Compute the jackknife variance as

$$\hat{V}_{\tau}^{(jack)} = \frac{N-1}{N} \sum_{i=1}^{N} \left(\hat{\tau}^{sdid}_{(-i)} - \bar{\tau}^{sdid} \right)^2 ,$$

   where $\bar{\tau}^{sdid}$ is the mean of the leave-one-out estimates.

The jackknife is advantageous because it avoids recomputing the optimal weights in each iteration, substantially reducing computation time relative to the bootstrap. It tends to yield slightly larger (more conservative) confidence intervals, particularly when the number of treated units is small. In this study, where Maryland is the only treated cluster but there are many treated households, the jackknife provides a useful robustness check on the bootstrap-based inference.

B.2 Alternate Methods

My primary specification estimates the effect of the alcohol sales tax using an estimator designed to minimize the error structure described in equation 1. To assess the robustness of these results, I also re-estimate the model using alternative procedures for constructing the control group. Each approach generates a different set of comparison weights and carries its own strengths and limitations. The following sections describe these weighting strategies and the assumptions underlying each.

B.2.1 Synthetic Control Method

Firstm I use the synthetic control method (SCM) suggested by Abadie and Gardeazabal (2003), where I estimate a DiD model that minimizes the following error (using terminology from Arkhangelsky et al. (2021)):

$$\arg\min_{\lambda,\mu,\beta}\left\{\sum_{s=1}^{S}\sum_{t=1}^{T}\left(Y_{ht}-\mu-\lambda_t-W_{ht}\tau\right)^2\hat{\omega}_h^{sc}\right\} \tag{8}$$

where $\hat{\omega}_h^{sc}$ is a vector of household control weights statistically-derived according to Abadie et al. (2010). Equation 8 has similar parameters to equation 1 but it omits the unit (household) weights.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 based on pre-treatment characteristics.

The SCM is used for comparative case study cases where there is a single treated unit. In such cases, a combination of untreated units might be a better suited comparison than a single treated unit (Abadie and Gardeazabal, 2003; Abadie, 2021; Abadie et al., 2010). Although Maryland is the only treated state, the unit of observation is at the household-month level, therefore there are several treated households. Since the SCM assumes only one treated unit, it omits the inclusion of household level intercepts which help account for time-invariant differences in alcohol consumption across households.

B.2.2 Difference-in-difference + Matching

This comparison group consists of a matched set of households drawn from states that did not experience an alcohol tax change during the sample period. Households are matched on pre-treatment ethanol purchases, household income, race/ethnicity, household size, and education. For this specification, valid inference requires that the matched control households provide a credible counterfactual for how Maryland households would have behaved in the absence of the tax increase. A key advantage of household-level data is the ability to verify whether pre-tax trends in purchases evolve similarly across treated and matched control households. Figure B1 shows that the trends in ethanol ounces purchased per adult were parallel in the months leading up to the tax increase, supporting the plausibility of this identifying assumption.

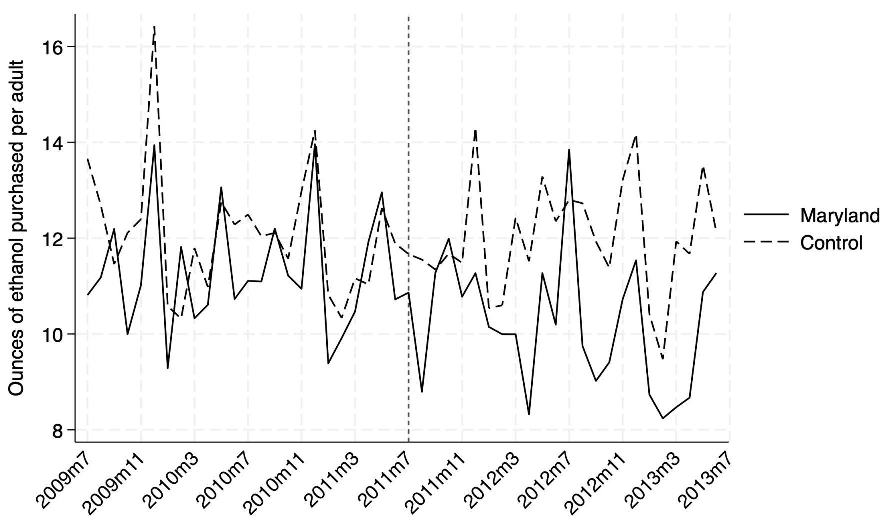


Figure B1. Trends in Ethanol Ounces Purchased per Adult in Maryland and Matched Control States

Note. The Y-axis represents the average ounces of ethanol per adult per household. The solid line shows the average monthly alcohol purchases in Maryland; the dashed line shows the average among matched control households selected via 1:1 nearest-neighbor matching.

The matching procedure proceeds in two steps. First, I estimate propensity scores using the logistic regression model:

$$P(Treatment_h = 1 \mid X_h) = \frac{e^{X_h'\beta}}{1 + e^{X_h'\beta}}, \tag{9}$$

where X_h includes household income, race/ethnicity, household size, education, and pre-treatment ethanol purchases. Matching is performed using only pre-treatment observations to prevent treatment-induced differences from influencing the algorithm. For each Maryland household, I select one nearest neighbor from the donor pool based on the closest propensity score (1:1 matching without replacement), imposing a common-support restriction to ensure comparability.

This process yields a matched sample that is balanced on observed characteristics. Using the matched sample, I then estimate the DiD specification that minimizes the following error:

$$\arg \min_{\lambda,\gamma,\mu,\beta} \left\{ \sum_{h=1}^{N} \sum_{t=1}^{T} (Y_{ht} - \mu - \gamma_h - \lambda_t - W_{ht}\tau)^2 \right\} \tag{10}$$

Equation 10 has the same parameters as equation 1 but it omits the unit and time weights. The credibility of this approach depends on the quality of the matches. Following Imbens and Rubin (2015), standardized differences below 0.25 indicate adequate balance. Table B1 reports pre-treatment covariate balance, showing that treated and matched control households are similar across all included characteristics.

Balance diagnostics are presented visually in Figure B2, which plots standardized differences (in percent terms) and the variance ratio of residuals orthogonal to the propensity-score index. A variance ratio near one indicates strong balance. Both diagnostics confirm that the matched control group provides a credible comparison for Maryland households.

Note. Scatter plot of standardized percentage bias versus the residual variance ratio of covariates orthogonal to the linear index of the propensity score. The standardized difference is presented as percentage bias, while the variance ratio reflects Rubin (2001) ratio of the variance of the covariates orthogonal to the propensity score.

Table B1. Covariate Balance on pre-treatment characteristics, and its standardized difference

	Mean		
Variable	Treated	Control	Standardized Difference
Household Size	2.14	2.13	0.007
Household Income	22.81	23.11	-0.054
Male Head Age	5.80	5.95	-0.045
Male Head Education	3.48	3.58	-0.050
Female Head Age	6.28	6.35	-0.023
Female Head Education	3.89	3.95	-0.036
% Married	0.62	0.65	-0.068
% With Child	0.16	0.15	0.040
% White	0.69	0.71	-0.056
% Asian	0.04	0.04	-0.000
% Black	0.24	0.23	0.038
% Hispanic	0.02	0.02	0.013

Note: Each observation is at the household-month level. This table presents pre-treatment means for households in Maryland and comparison states. Comparison states exclude households in Connecticut, Illinois, New Jersey, New York, North Carolina, Washington, the District of Columbia, and Kansas, as these states experienced alcohol tax changes during the sample period. The standardized difference measures the difference in means between the treatment and control groups, scaled by their pooled standard deviation, with values below 0.25 indicating good covariate balance (Imbens and Rubin, 2015). Data comes from the NielsenIQ HomeScan panel for the period from June 2009 to July 2013.

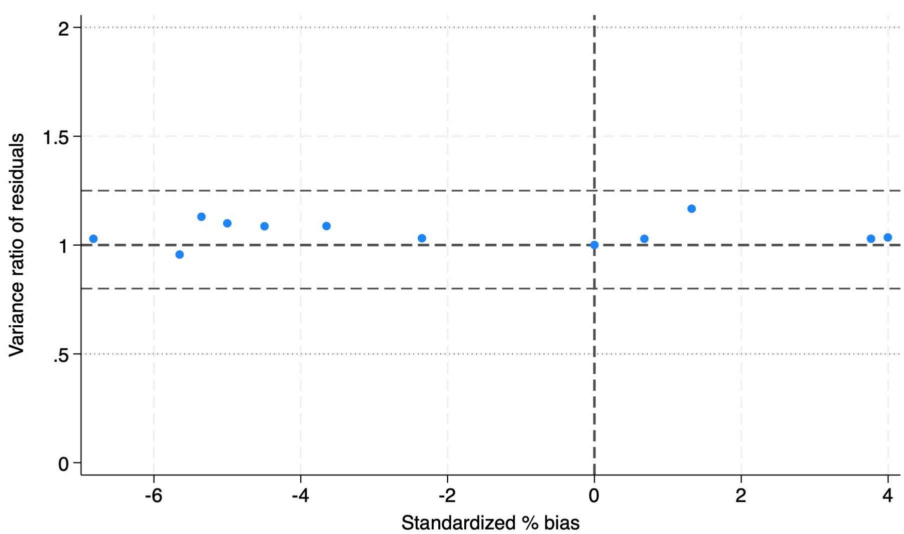


Figure B2. Standardized Bias and Variance ratio of the residuals