Legalization, Libations, and Location: Transaction Costs as a Determinant in the Relationship between Marijuana and Alcohol

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Abstract

We examine how legalization and retail availability of recreational marijuana affects alcohol sales. We first develop a theoretical framework that supports the mixed findings of prior and current research, highlighting that substitution or complementarity may not be fixed; rather, the relationship may be endogenous to the transaction costs faced by consumers. Using disaggregated retailer spirit alcohol sales data from the state of Washington (2013-2017) and incorporating geographic proximity to marijuana dispensaries as a proxy for transaction costs, our estimates reveal spatial heterogeneity: spirit sales increase by 2-6% for all retailers within 0.5 miles of a recreational marijuana dispensary and decrease by 7-10% for those isolated from recreational marijuana dispensaries. These findings help reconcile conflicting evidence from previous studies and highlight the importance of spatial access in the empirical identification of substitution and complementarity. More broadly, the results underscore the role of transaction costs in mediating consumer responses to changes in the availability of related goods.

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1 Introduction

Since California's legalization of medical marijuana in 1996, 37 states and the District of Columbia have legalized marijuana for medical purposes (MML). Twenty-three of these states and the District of Columbia have also legalized marijuana for recreational consumption (RML) since 2012. The recent wave of marijuana legalization for recreational consumption provides researchers new opportunities to examine how this policy shifts affect consumer behavior.

One particular behavior that has attracted renewed interest is the relationship between marijuana and alcohol. With the non-uniform adoption of recreational marijuana policies across the US, researchers have examined whether marijuana and alcohol are economic *substitutes* or *complements*. Theory supports both possibilities. Alcohol and marijuana may be substitutes if they serve similar purposes (e.g., relaxation, intoxication, or as a coping mechanism). The two goods could also function as complements if consumed together. Some of the potential reasons for a complementary relationship include: social context where co-use may be normalized or expected (e.g., parties), experience enhancement with the combination intensifying the overall experience from each good, or a joint consumption as further coping or escapism.

Empirical findings examining the relationship between alcohol and marijuana mirror the theoretical ambiguity. Focusing on RML in the US, early studies concluded the two goods were substitutes as alcohol use declined after legalization (Dragone et al. 2019; Alley et al. 2020; and Miller and Seo 2021). Recent research reports mixed findings, highlighting context is an important consideration to understand this relationship (Calvert and Erickson 2021; Zhu et al. 2025). For example, Zhu et al. (2025) conclude that *recreational* marijuana users engage in complementary use patterns, while *medical* marijuana users exhibit substitution use patterns. Overall, the inconsistencies often reflect differences in data sources, methods, policy environments, and the populations studied.

We contribute to this ongoing debate by utilizing a new dataset and empirical approach. We combine retailer-level spirit alcohol sales data across the state of Washington with the staggered rollout of recreational marijuana dispensaries to examine the relationship between alcohol and marijuana. In addition, we offer a new theoretical framework to measure the relationship and to understand the mixed findings of previous research. We believe transaction costs play a pivotal role in driving this relationship and we incorporate geographic proximity between alcohol retailers

and marijuana dispensaries as a proxy for transaction costs. Using a staggered treatment design with disaggregated retailer-level sales data, our results reveal spatial heterogeneity: spirit alcohol sales increase by 2–6% for retailers located within 0.5 miles of a recreational marijuana dispensary, while sales decrease by 7–10% for retailers that are geographically isolated from recreational marijuana dispensaries. In addition, we recognize this relationship may be shaped by the retail opportunities available or known by consumers when these purchases are made. The visibility or familiarity of well-known retailers selling alcohol co-located with marijuana dispensaries may amplify complementarity. Our findings confirm these expectations: spirit alcohol sales increase by 10% for "chain" alcohol retailers within 0.5 miles of a recreational marijuana dispensary, with effects primarily driven by supermarkets and pharmacies.

Our approach and findings contribute to the existing empirical literature in two key ways. First, we use administrative sales data from the state of Washington to examine the relationship between marijuana and alcohol. Unlike prior studies that rely on surveys or highly aggregated measures, our retailer–quarter data allow for nearly 20,000 observations, mitigating aggregation bias and providing a clearer view of underlying consumer behavior. The granularity of the data also permits the use of updated econometric methods; we exploit the staggered variation in marijuana market entry across space and time to improve upon conventional difference-in-differences designs that identify all retailers as simultaneously treated.

Second, we provide a theoretical framework that highlights the role of transaction costs in shaping the relationship between these two goods, and we extend this rationale into our empirical model by incorporating transaction costs directly into our analysis. That is, we propose the relationship between alcohol and marijuana depends on the state of transaction costs varying across space, time, and population. We measure spatial distances between marijuana dispensaries and alcohol retailers, treating accessibility between the two goods as a consumer friction affecting the "full price" of use. Our approach provides a unified framework for understanding why marijuana and alcohol sometimes appear as substitutes and other times as complements. Together, our findings help reconcile conflicting evidence from previous studies and highlight the importance of spatial access in the empirical identification of substitution and complementarity.

Understanding the interaction between marijuana and alcohol consumption is vital for effective health and fiscal policy. If alcohol and marijuana are substitutes, increased marijuana availability

could reduce negative outcomes associated with alcohol use, such as traffic fatalities (Anderson et al. 2013) and chronic alcohol-related health conditions and use disorders. However, states may face offsetting declines in alcohol-related tax revenues (Miller and Seo 2021). If they act as complements, co-use and co-location could amplify public safety and health risks. Policymakers may view legalization as fiscally attractive, but need to weigh this against added healthcare costs. Given our observation of a non-uniform relationship, we suggest policy design that accounts for the relationship between alcohol and marijuana across space.

The remainder of the paper proceeds as follows. The next section summarizes the literature examining the relationship between marijuana and alcohol, and highlights our contribution to this literature. Section 3 presents our theoretical framework emphasizing the role of transaction costs, measured through co-location distance and familiarity with alcohol retailers in shaping the relationship. Sections 4 and 5 describe our data sources and empirical approaches, respectively. Section 6 discusses our main results and the consistency of our findings in a series of robustness checks. Finally, we provide a brief discussion of our findings and some concluding remarks in sections 7 and 8.

2 Literature Review

2.1 Mixed Findings in the Alcohol-Marijuana Debate

The debate whether alcohol and marijuana are substitutes or complements has been ongoing for more than three decades. We provide a comprehensive list of the literature in Appendix A.[1] The literature to date presents mixed findings regarding the economic relationship between alcohol and marijuana. The divergence in empirical results may reflect several underlying factors, including: 1) measurement - some studies measure price using taxes or consumer indices, while others rely on enforcement proxies or decriminalization status; 2) population - results often differ between adolescents, college students, and adults, suggesting age- or context-specific patterns; policy context - studies exploit different sources of variation (e.g., beer taxes, minimum legal drinking ages, campus bans, decriminalization); and 3) empirical strategy. In short and as noted by Guttmannova et al.

[1]The literature highlighted in Appendix A focuses on studies across the US. This literature is of primary interest due to the recent and rapid adoption of recreational marijuana legalization across the country and the relevance for our own sample. We recommend to those interested in additional reviews of the literature read Guttmannova et al. (2016) and Subbaraman (2016) for an early examination on the alcohol-marijuana literature, Risso et al. (2020) for a systematic review of the literature across all subject types, and Anderson and Rees (2023) for an updated review regarding the use of all substances related to marijuana legalization.

(2016), the relationship is complex. In general, differences across findings underscore the need for continued empirical work that integrates modern data sources, methods, and policy changes to understand this relationship.

Focusing attention to a brief overview of the literature, Table 1 highlights three different policies, each with mixed outcomes. Furthermore, Table 1 shows these differences occur despite the use of similar data or analyzing the same population. Narrowing focus to RML, initial research appeared consistent in finding a substitution relationship between marijuana and alcohol (Dragone et al. 2019; Alley et al. 2020; Miller and Seo 2021). Leveraging survey data of the entire US population, assessment data of US college students, and retailer-level data across the State of Washington, legalization was associated with a reduction in alcohol use and demand. However, these findings are less clear in recent research since the classification of the product (i.e., recreational or medical) or location may be important (Calvert and Erickson 2021; Zhu et al. 2025).

Given the mixed findings, there remains additional value in examining the relationship within the small set of literature. Some of the reasons to build upon this research include: Dragone et al. (2019) do not leverage retail sales, only opportunity after legalization; Alley et al. (2020) examine the specific population of college students; and Miller and Seo (2021) utilize retail-level data, rather than survey results, but aggregate their data at the county-level for a brief period after legalization (through 2016). We update this literature through 1) the inclusion of a larger timeframe (2013-2017), 2) utilizing more granular data, examining spirit sales at the retailer level and generating nearly 20,000 retailer-quarter observations, 3) implementing updated methods of staggered treatment design, and 4) including additional controls common in health studies looking at the effects of consumption (e.g., control for population).

2.2 Transaction Costs as an Explanation

An additional contribution to the literature is our hypothesis explaining why previous results differ: transaction costs matter. We propose that alcohol and marijuana can be both complements and substitutes depending on the *state of transaction costs*. That is, the mixed evidence on whether alcohol and marijuana are economic complements or substitutes can be reasoned through differences in *transaction costs* (i.e., search, access, legal risk, and non-monetary frictions) that vary across space, time, and populations. Policies analyzed (e.g., minimum legal drinking age,

decriminalization, MML, and RML), the structure of markets (i.e., co-location of marijuana dispensaries and alcohol retailers), and measurements used can affect the *full price* of each good and the observed relationships. For example, when a policy or market shock primarily raises *joint* frictions (e.g., increased enforcement, etc.), both uses can fall. This would suggest the goods act as complements. When a shock *differentially* lowers frictions for one good (e.g., nearby recreational marijuana dispensary openings), consumption of the other may fall and suggest a substitution effect. Our spatially-staggered design at the retailer level isolate these and our results provide a natural explanation for why the post-legalization empirical literature is mixed.

There is a small strand of literature recognizing the importance of transaction costs through legal risks, access restrictions such as minimum legal drinking ages (MLDA), campus alcohol bans, and enforcement intensity. These studies argue that changes to each of these affect the "full price" (i.e., monetary plus non-monetary costs) of substance use.[2] For instance, Pacula (1998) proxies marijuana's "full price" using police-to-crime ratios (risk of arrest) and MLDA as non-monetary costs of alcohol use. Crost and Guerrero (2012), Chaloupka and Laixuthai (1997), Cameron and Williams (2001), Zhao and Harris (2004), and DiNardo and Lemieux (2001) similarly treat changes to MLDAs or alcohol bans as shifts in the "full price" of alcohol. While these studies acknowledge the importance of non-monetary costs, including proxies in their empirical models, they rarely cite transaction costs as a systematic reason for explaining mixed findings. DiNardo and Lemieux (2001) are the only authors to formally model transaction costs within their theoretical and empirical frameworks.[3]

While novel in the context of the marijuana-alcohol relationship, our incorporation of transaction cost is related to other strands of economic literature that observe how such costs can change consumer behavior. Consider first the Alchian–Allen Effect where an additional fixed per-unit cost to both high- and low-quality goods alters their relative prices and leads consumers toward consumption of the higher-quality option (Alchian 1983). The increased fixed cost (such as a transportation fee) reduces the relative price difference and makes the higher-quality good more

[2]Some also frame this as opportunity costs, though most studies summarized in Appendix A do not explicitly apply such a framework. For example, Hansen (2019) incorporates geographic distance to marijuana dispensaries as a proxy for the opportunity cost of access, aligning with transaction cost considerations.

[3]Pacula (1998), Farrelly et al. (1999b), Cameron and Williams (2001), and Chaloupka and Laixuthai (1997) employ the concept of full price in empirical models, but do not elevate their discussion of transaction costs to a formal theoretical construct.

attractive. This shifts consumption patterns, influencing the perceived relationship between goods. While our theoretical model incorporates transaction costs directly into the budget constraint and can influence the observed relationship between complementary goods, the Alchian–Allen effect emphasizes how fixed costs can alter relative prices and, consequently, consumption choices. Both frameworks highlight the significant role of transaction costs in shaping consumer behavior.

As another example, Chakraborty et al. (2010) introduce transaction costs by allowing the same good to have different effective prices for different buyers. The implication is that transaction costs can lead to price dispersion, affect market equilibrium, and lead to varied consumption patterns across different buyers. Substitution or complementarity depends on the effective price faced by a consumer and observed behavior may be misleading if one interprets the relationship without accounting for transaction costs.

Finally, we recognize the work of Allen and Rehbeck (2022) highlighting latent complementarity that looks like substitution due to unequal frictions. This may explain previous findings: if some consumers prefer to use marijuana and alcohol jointly but laws or social norms reduce their joint availability, we may only observe substitution in aggregate data and mask underlying complementarity. Building on the idea that latent complementarity may not be revealed in observed behavior due to frictions, we explicitly model transaction costs that normally suppress joint consumption when goods, legally and readily available, may be complements in utility. That is, we make transaction costs explicit and central to our theoretical and empirical framework.

Collectively, these studies, along with our approach, underscore the role of transaction costs can play on the substitutability and complementarity of goods: high transaction costs can discourage the joint consumption of complementary goods, making them appear as substitutes in practice. Conversely, reducing transaction costs through co-location, bundling, or improved visibility can enhance the complementary relationship between goods.

We now turn our attention to the theoretical framework that explains the literature's mixed finding when transaction costs are included for goods that are both legal and available for purchase.

3 Theoretical Model

We begin by examining the conditions necessary to define two goods as either complements or substitutes based on their relationship in consumer preferences. Let a consumer choose quantities

q_A and q_M of alcohol (A) and marijuana (M), respectively, by maximizing their utility function that allows for interaction between the two goods. We identify this as:

$$U(q_A, q_M) = \alpha \ln(q_A) + \beta \ln(q_M) + \gamma q_A q_M \tag{1}$$

The log-linear Cobb–Douglas utility function satisfies standard economic properties: it is strictly monotonic and exhibits diminishing marginal utility (for $0 < \alpha, \beta < 1$). The interaction term (γ) provides flexibility in modeling complementarity or substitutability between two goods; it breaks separability and allows for complement/substitution patterns to vary with joint consumption. To see these properties more directly, consider the marginal utilities:

$$\frac{\partial U}{\partial q_A} = \frac{\alpha}{q_A} + \gamma q_M \qquad \frac{\partial U}{\partial q_M} = \frac{\beta}{q_M} + \gamma q_A \tag{2}$$

From these, $\alpha, \beta > 0$ are preference weights that determine an individual's base utility from each good, and γ captures the interaction between these two goods: $\gamma > 0$ implies the goods are *complements* and $\gamma < 0$ implies *substitutes*. This is in line with the standard *utility*-driven approach for examining the relationship between alcohol and marijuana.

To begin understanding the literature's mixed results, we incorporate a transaction cost τ into the decision to purchase these goods together or separately. To maintain differentiability, we model this as a smooth cost proportional to the product $q_A q_M$, directly incorporated into the budget constraint as:

$$P_A q_A + P_M q_M + \tau q_A q_M \leq m \tag{3}$$

This implies that when $q_A = 0$ or $q_M = 0$, there is no additional transaction cost required to purchase the joint consumption of goods. In addition, there is no transaction cost effect on the relationship if τ is zero (i.e., no additional search costs to get both goods). However, the consumer pays $\tau q_A q_M$ when there are preferences to consume both goods, increasing the effective cost of joint consumption as transaction costs increase. With this consideration, the first-order conditions are reduced to:

$$\frac{\frac{\alpha}{q_A} + \gamma q_M}{\frac{\beta}{q_M} + \gamma q_A} = \frac{P_A + \tau q_M}{P_M + \tau q_A} \tag{4}$$

This illustrates how a transaction cost of $\tau > 0$ distort the effective marginal rate of substitution between goods; τ moderates the observed complementarity or substitutability of goods in empirical behavior through changes in those quantities. If $\gamma > 0$ (utility-based complements), an increase in τ raises the effective marginal cost of joint consumption. That is, an increase in the transaction costs when the goods are utility-based complements may either (i) reduce the interior levels of both goods or (ii) it may lead to a corner solution in which the consumer purchases only one good if τ is large enough. The latter result would lead to these utility-based complements to behave like substitutes in empirical estimations. Table 2 provides a summary of our outcomes based on our utility framework. The introduction of transaction costs imply there may be unexplored relationships in the literature examining the survey responses or aggregate purchase behavior of consumers.[4]

3.1 Retail Visibility as a Moderator

The effective substitutability or complementarity of goods is not only shaped by utility interactions (γ) and transaction costs (τ); they may also be shaped by the visibility and retail context in which the goods are purchased. If goods A and M are utility complements ($\gamma > 0$), but good A is only readily available at a small "mom and pop" store when the consumer is most interested in purchasing good M. Even if τ is modest, the logistical frictions of visiting two distinct locations can functionally raise τ if the nearby store is not visible or well-known by the consumer. In this instance, the consumer may choose to buy only good M, forgoing the complementarity benefit from A because the individual is unaware of the opportunity for A. The observed behavior would identify the goods as substitutes. Conversely, if both goods are co-located or sold nearby with a large, visible retailer (e.g., Walmart), the visibility and accessibility of joint consumption lowers the effective τ. As a result, the consumer is more likely to purchase both together, allowing the underlying complementarity ($\gamma > 0$) to be observed in estimations.

[4]For completeness, Appendix B shows how our framework relates to the quadratic specification of DiNardo and Lemieux (2001). While both approaches highlight the sign of the cross-partial that determine intrinsic complementarity or substitutability, transaction costs τ distort effective prices and thus observed behavior.

4 Data

4.1 Washington as an area of study

The State of Washington has recently been a leader in marijuana-liberatlization policies. Washington was one of first three states to approve medical marijuana in 1998, preceded only by California's approval of medical marijuana in 1996. Fourteen years later, Washington residents, along with Colorado residents, were the first to vote in favor of legalizing, taxing, and regulating recreational marijuana on November 6, 2012.

Washington's Initiative 502 (I-502) allows those 21 and older the ability to possess up to one ounce of usable marijuana, 16 ounces of marijuana-infused products in solid form, 72 ounces of marijuana-infused products in liquid form, or any combination of all three. Consumption must take place in Washington, and it remains unlawful to publicly open or consume marijuana. Finally, to prevent driving under the influence of marijuana, driving under intoxication (DUI) standards with maximum thresholds for THC (the psychoactive ingredient in marijuana) blood concentration are enforced.

Retail sales are permitted only if the marijuana is grown by licensed Washington farmers and sold in stand-alone, marijuana-only stores operated by licensed and regulated Washington businesses. Personal marijuana growth, excluding plants for medical purposes, remains illegal. I-502 initially imposed a 25 percent sales tax on all retail marijuana sales, but the rate increased to 37 percent beginning July 1, 2015 (HB 2136). Forty percent of the tax revenue is dedicated to the state general fund and local budgets and the remaining sixty percent is dedicated to substance-abuse prevention, research, education, and health care. In July 2014, the first month of recreational marijuana dispensary sales, 18 locations across the State of Washington opened totaling over $1.8 million in sales ($1.33 per capita) and generating over $460,000 in tax revenue. By the end of 2017, there were over 380 active retail locations across the state generating nearly $900 million in yearly sales ($3.18 per capita) and over $330 million in yearly tax revenue. Figure 1 presents the total number of dispensaries by quarter-year for Washington state.

Figure 2 presents the average per capita (by zip code) alcohol and marijuana sales by quarter-year for Washington state. We note there is seasonality in the alcohol sales data with a slight increase over our study timeframe. Concerning marijuana sales, we notice an increase from July

2014 through the end of 2016, with a plateau in per capita sales by 2017. Given the continuous increase in the number of dispensaries over this period, this leveling out of per capita sales could indicate a saturation point within the marijuana market as a whole but growth continuing into new geographic markets.

4.2 Recreational Marijuana

The primary data for this study include monthly marijuana sales at the retailer-level from the Washington State Liquor and Cannabis Board (LCB) from 2014 to 2017. We aggregate new retailers to the quarterly level and identify the exact location of each marijuana dispensary using a reverse address to geospatial lookup.[5] We use these locations to pair with our retailer alcohol sales data below.

4.3 Alcohol Sales

Washington State's LCB classifies alcoholic beverages into three categories: beer, wine, and spirits. For our purposes, we use alcohol sales interchangeably with "spirit" sales, defined by the LCB as beverages that are distilled from raw materials rather than brewed or fermented.[6] These include products such as whiskey, gin, brandy, rum, tequila, and vodka. The LCB regulates the sale of spirits in Washington by overseeing distribution, requiring retailers to report sales, and ensuring compliance with state tax and licensing laws. These data were provided for each retailer across the state at the monthly level, and we aggregate sales for each quarter from 2013 to 2017. Similar to the marijuana data, we identify the exact location of each alcohol retailer using a reverse address geospatial lookup. Once recreational marijuana retail starts, we are able to measure the distance between each spirit retailer and marijuana dispensary.

Beyond alcohol sales and location, the dataset provided by Washington LCB included retailer name. We use these names and the number of their locations to identify "chain" stores with which Washington consumers may have more familiarity due to their more ubiquitous nature. In particular, we identify all stores with at least five unique retail locations within Washington. These include national chains (e.g., Walmart or Whole Foods) as well as regional chains (e.g., Quality Food Centers or Bartell Drugs). The detailed names also allow us to examine our results by store type, which we discuss in our "Retailer Classification" section below.

[5] More information on this data can be found here: https://lcb.wa.gov/records/frequently-requested-lists
[6] One exception is wines exceeding twenty-four percent of alcohol by volume. These are also labeled as "spirits."

4.4 Covariates

Since our data do not include information about area or population characteristics, we supplement our analysis with zip code characteristics from the American Community Survey (ACS). In particular, we include annual statistics on population counts, percent population between 18-24 years of age, percent population male, percent population with high school education or less, and percent population married.

5 Econometric Model

Our primary goal is to empirically estimate the relationship between alcohol and marijuana, focusing on the potential role of transaction costs. The legalization of marijuana and the rollout of retailers provides an opportunity for a well-defined natural experiment. We exploit the staggered opening and geography of recreational marijuana dispensaries in Washington, and match them to retailer-level alcohol sales. The spatial proximity to marijuana dispensaries proxies the search and access components of the full price of purchasing alcohol, letting us estimate how *localized changes* in marijuana transaction costs change alcohol sales.

As mentioned, we calculate the distance from an alcohol retailer to the nearest marijuana dispensary (d_i^{AM}) to proxy for potential transaction costs. Given the staggered implementation of marijuana dispensaries over time, a conventional two-way fixed effect (TWFE) can bias the estimates when treatment effects vary over time (Goodman-Bacon 2021). To account for this, we use the Borusyak et al. (2024) imputation estimator (BJS), which uses not-yet-treated period observations to construct the never-treated counterfactuals for each retailer. We then identify "treated" alcohol retailers as stores within various distance thresholds of a newly opened marijuana dispensary.

Denoting $Y_{izt|d}$ as the per-capita natural log of alcohol sales for retailer i in zip code z at time t given treatment distance threshold d, we can write:

$$Y_{izt|d} = Y_{izt|d}(0) + RMD_{t|d} \cdot \beta_{t|d} \tag{5}$$

where $Y_{izt|d}(0)$ is the untreated potential outcome, $RMD_{t|d}$ is an indicator equal to 1 if retailer i is "treated" in period t given distance d, with "treatment" identified as being located within a specified distance of a newly opened recreational marijuana dispensary (i.e., treated $\leq (d_i^{AM})$), and

β_{dt} is the cohort- and time-specific causal effect. BJS estimates $\beta_{t|d}$ by first modeling the untreated outcome as:

$$Y_{izt|d}(0) = \delta_i + \mu_t + X_{izt}\gamma + \epsilon_{izt|d} \tag{6}$$

where X_{izt} is a vector of time-varying controls, δ_i and μ_t are retailer and quarter by year fixed effects, and $\epsilon_{izt|d}$ is an idiosyncratic error clustered at the county level. The key identifying assumption is:

$$\mathbb{E}[\epsilon_{izt|d} \mid X_{izt}, \delta_i, \mu_t] = 0. \tag{7}$$

BJS uses only not-yet-treated observations to estimate the parameters in (6), then impute $Y_{izt|d}(0)$ for treated units in post-treatment periods. Treatment effects are the differences between observed outcomes and these imputed counterfactuals, and are aggregated across groups to produce an average treatment effect on the treated (ATT).[7] Estimates reported in tabular form, static ATT, are aggregated using uniform weights, $w_{izt|d} = 1/N_1$, where N_1 is the total number of treated observations. Estimates represented in event-study figures, dynamic ATT, are a simple average treatment, τ_h at event time h, and are weighted using horizon-based weights $w_{izt|d} = 1/\Omega_{1,h}$, where:

$$\tau_h = E(Y_{izt+h|d}(1) - Y_{izt+h|d}(0)) \tag{8}$$

We define the treatment date as the quarter when a marijuana dispensary opens within the given distance, d of an alcohol spirit retailer.

6 Results

6.1 Summary Statistics

Table 3 presents a series of summary statistics. Panel A represents all alcohol retailers, with columns stratifying the statistics by all time periods, periods before marijuana dispensaries operated, post-marijuana openings, non-treated alcohol retailers, and treated alcohol retailers. For simplicity, pre and post periods center around the overall recreational marijuana dispensaries start

[7] We link the BJS approach to the standard Difference-in-Differences framework and the biased results from the naive two-way fixed effects specification in Appendix 9. To check the robustness of our estimates, we also provide results from a Gardner (2022) regression.

date of July, 2014, and treated alcohol retailers in this table are those within 0.20 miles of a marijuana dispensary. Panels B and C repeat the columns, but offer another view of alcohol retailers by separating these by "chain" and "non-chain" retailers. Collectively, these stratified samples allow us to observe whether patterns, relationships of interest, or other potential differences are present that may need to be considered or controlled in our analysis.

Average quarterly alcohol sales were approximately \$103k (\$5.27 per capita) for all retailers, with a slight increase in sales to \$5.35 for all retailers post marijuana dispensary opening and a larger increase to \$7.84 for treated retailers. These findings are most pronounced for non-chain retailers. The number of close alcohol retailers within a 5 mile radius was 14, with little variation in the pre/post or un/treated classifications. The average distance to the nearest marijuana dispensary was 2.35 miles for all alcohol retailers and that distance contracted to 1.78 miles for chain retailers. Approximately 65% of our sample is comprised of chain retailers. To minimize potentially biasing estimates in our main analysis, we dropped any retailer that toggled treatment on or off after July 2014 or exited prematurely.[8] Please refer to Table 3 for further information.

6.2 Main Results

6.2.1 Close Proximity

We first examine the impact of marijuana dispensaries located within 0.5 miles or less of an alcohol retailer to evaluate the possible relationship of alcohol and marijuana consumption when the transaction costs for consuming both goods are near zero. Using the log of per capita alcohol sales, we look at retailer quarterly data with varying distance thresholds between 0.1 to 0.5 miles.

Our results are presented in Table 4. The estimates in columns (1) and (2) indicate that the presence of marijuana dispensaries within one-tenth of a mile of alcohol dealers leads to a 6% increase in the quarterly per capita alcohol sales for all retailers in Panel (a). We note that including demographic controls does not significantly change our estimate. As we increase the treatment distance threshold to two-tenths of a mile (column (3)), we still report a 4.7% increase in quarterly per capita alcohol sales. As expected, our estimates continue to decrease in magnitude but are still positive and statistically significant as we increase the threshold to one-half mile.

We also stratify our main results in Table 4 by chain and non-chain alcohol retailers (Panels (b) and (c)). Estimates for chain retailers grew in magnitude with a 10% increase at the closest

[8] Toggling and early exiting occurred in less than 3% of our sample.

threshold. The positive effect on alcohol sales for those retailers diminishes quickly and is similar to the oeverall results of 2% at a half-mile. We can see that these estimates are offset by weaker and smaller negative estimates of 4% from non-chain alcohol retailers in Panel (b).

Figure 3 presents the event study results for alcohol retailers in close proximity to marijuana dispensaries. Mirroring Table 4, Panels (a) - (e) display the one-tenth to half-mile models with controls and analyze a six-quarter pre- and eight-quarter post-treatment window around the staggered timing of initial marijuana dispensary presence. These figures show that alcohol spending has some seasonal variation but it does not influence our pretrends. Furthermore, the panel event study results show a clear pattern in the closer models (0.10 to 0.20 miles) that there is a strong, statistically significant upward trend after three quarters of treatment.

Since the nature of the newly emerged marijuana retail dispensary market left some alcohol retailers switching in and out of treatment due to marijuana dispensary closures or relocating, we test the robustness of these estimates by assuming once an alcohol retailer is treated, it stays treated. Estimates are reported in Appendix D.2. We see similar, slightly attenuated estimates of 5.7% at closer ranges that are diminished but positive as we expand the distance threshold.

6.2.2 Retailer Isolation

Given the overall positive association with marijuana dispensary presence and per capita alcohol sales that diminishes over larger distance thresholds, it is also important to test the counterfactual argument: alcohol retailers isolated from marijuana dispensaries (i.e., large distances) should experience a negative sales effect.[9] We explore this argument in Table 5 and report our estimates that examine isolation rings of 12.5+ to 22.5+ miles. We find a statistically significant relationship of negative 7.1% to negative 10% for all alcohol retailers at distances greater than twenty miles in Panel (a). It should be noted that these estimates are attenuated by the sparsity, or absence, of chain retailers at greater distances. Looking solely at non-chain alcohol retailer estimates in Panel (c), we find a 7.5% to 12% decrease in per capita alcohol sales. When testing the robustness of these estimates by including early exits and toggling treatment as constantly treated in (Appendix D.2), we find similar results to these findings.

The event studies for non-chain retailers in the 20+ and 22.5+ mile isolation rings are presented

[9]Isolated treated retailers were determined by the minimum of all quarter distances exceeding the specified distance ring threshold.

in Figure 4.[10] Although the estimates are noisier than our close proximity figures, with more noticeable but statistically insignificant pretrends, we can see a decrease in per capita alcohol sales occurring roughly one year post-treatment.

6.2.3 Retailer Classification

As an additional view of the moderating role of store type, we stratify our findings by alcohol retailer type for close and isolated distances in Tables 6 and 7. We define a "supermarket" retailer as offering for sale food, pharmacy, and at least one other main category of consumer retail (e.g., toys, electronics, furniture, home goods, etc.). Grocery stores are defined by their primary sales being food items with or without an attached pharmacy. Pharmacy is defined as their primary function is operating as a pharmacy. Finally, liquor stores are defined as stores that have a primary function of selling alcoholic beverages.

In Table 6, we find close alcohol sales are driven mainly by supermarkets and pharmacies with a 5% to 15% increase in alcohol sales from each type. Similar to our main findings, effects are largest at one-tenth of a mile and diminish as distance increases. Grocery stores add to the effect, though much smaller, at greater distances. Table 7 reports that the main driving factor for the decrease in alcohol sales comes from grocery stores.

6.3 Robustness Checks

6.3.1 Alternative Estimator

Our main analyses show estimates of the BJS imputation method to account for the staggered implementation of the recreational marijuana dispensaries across Washington. We discuss the choice of staggered estimation methodology in Appendix C. Furthermore, we choose to test the sensitivity of our findings with a naive TWFE and Gardner (2022) estimator in Tables C.1 and C.2 for Close and Isolated retailers. Though in some cases the Gardner estimates lose statistical significance at further distances with Close retailer, the sign and magnitude of effects are similar to BJS, reinforcing our confidence in our main estimates and the use of a staggered-timing model.

6.3.2 Continuing Treatment Effect

To properly access causality in a difference-in-differences framework, we rely on the fact that the treatment is applied a single time and those retailers are continuously treated through our

[10] Non-chain retailers are only reported given the sparse occurrence of chain retailers being isolated from marijuana dispensaries at great distances.

study's time frame. A few alcohol retailers and marijuana dispensaries moved or shutdown in this period, effectively toggling treatment on and off or prematurely exiting before the last quarter of 2017. This could bias our estimates due to unobservable market conditions that leads firms to exit early or temporarily shutdown. In Appendix D, Tables D.1 and D.2 keep these exiting firms and assumes the treatment effect continues as treated. The estimates are similar to our main findings, signaling no concerning bias from treated firms exiting the market.

6.3.3 Urban and Rural

Next, we test the robustness of our estimates across geographic variation, stratifying close alcohol sales by urban and non-urban locations defined by the Census Bureau. Panel A of Table E.1 report an increase in alcohol sales of 2% to 8% for urban retailers. This effect is slightly magnified from our main specification. Non-urban retailers in Panel B report a weakly significant decrease in sales within the closest treated retailers. This could be due to structural differences in how less populated cities are designed and planned versus urban settings.

6.3.4 Cross-Border Spillovers

Cross-border analysis is relevant to our estimates as neighboring states and Canada adopted varying marijuana policies at different times. Idaho has not adopted any medical or recreational marijuana laws. Oregon legalized medical marijuana in March 2014 and recreational in October 2015. Canada legalized medical commercial production in 2013 and legalized recreational sale in October 2018, outside of our study's timeframe. First, we consider the effect of bordering zip codes which may be subject to some increased level of demand from residents of the neighboring states of Oregon and Idaho, as well as residents from Canada, traveling across borders to obtain and consume marijuana. Given the increased opportunity costs associated with cross state and border travel, consumers have further incentive to co-purchase alcohol and marijuana at the final destination. We examine the effect of marijuana dispensary proximity on bordering zip codes in Panels C and D of Table E.1. Panel C estimates the impact of marijuana proximity only on bordering zip codes. We show that inclusion of only bordering zip codes amplifies our estimates. This reinforces the idea of complementary purchasing for travelers that may have traveled a great distance and purchased legalized marijuana. Panel D shows a slightly attenuated effect, but it remains positive, statistically significant, and decreasing with distance from a marijuana dispensary.

6.3.5 Treatment Intensity

To measure the effects of multiple marijuana dispensaries within close proximity on alcohol spirit sales, we report the effects of treatment intensity in Panel E of Appendix E.1 . Depending on the distance of the treatment applied, there are one to eight marijuana dispensaries varying the effects stated in our main estimations. We use the Gardner (2022) method to include the treatment intensity within the second stage, which allows for a continuous treatment effect unlike our preferred BJS estimation method. We find that increasing the number of marijuana dispensaries within three-tenths of a mile increases the sales of alcohol spirits by 2% to 4%, similar to our main findings. This added effect from multiple marijuana dispensaries could explain the difference in estimates from urban and non-urban areas.

7 Conclusion

Consistent with the literature, we find alcohol and marijuana act as both substitutes and complements. However, we examine a potential source for this varied response: transaction costs. The framework we utilize, backed by our empirical findings, suggests transaction costs and co-location effects are important in properly identifying consumer preferences of potentially related goods. In particular, our estimates show alcohol spirit sales increase by 2-6% for all retailers within 0.5 miles of a recreational marijuana dispensary, with results as high as 10% for "chain" retailers, and spirit sales decrease by 7-10% for those isolated from recreational marijuana dispensaries. These suggest that consumers view alcohol spirits and marijuana as complementary goods when the transaction costs are quite low and display substitutes when faced with significant transaction costs.

As noted earlier, understanding the relationship between marijuana and alcohol has important policy implications for public health and public finance. The substitution relationship suggests there may be reduced negative outcomes associated with alcohol use, such as chronic alcohol related health conditions and use disorders (Anderson et al. 2013), as well as offesetting declines in alcohol related tax revenues (Miller and Seo 2021). Complementarity suggests an increase in health concerns and legalization with co-location may be fiscally attractive. While we do not intend to solve the cost versus benefits decision of local public officials, our findings of a non-uniform relationship suggest that policy design should incorporate local conditions and account for spatial variation in the relationship between the two goods. Specifically, policymakers might restrict co-location

of marijuana and alcohol retailers to mitigate risks corresponding with co-use if their concern for health outweights revenue generation. Alternatively, higher excise taxes on both products could be implemented based on co-location to offset the potentially increased health costs.

While our analysis is the first to consider transaction costs changing the relationship between alcohol and marijuana based on ease of accessibility in an environment where both goods are legal, there are a number of natural extensions to our framework. For example, different types of information could affect substitution and complementarity. While our recognition of retailer type as a moderator can be considered one form of information (i.e., consumers' awareness of product availability), additional considerations could consider health risks and the awareness of these risks. Another promising avenue is to continue examining the heterogeneity across user groups (e.g., younger versus older consumers; heavy versus occasional users). As noted, recreational and medicinal marijuana users have already been shown to exhibit systematically different relationships with alcohol (Zhu et al. 2025). Including user type could provide a more comprehensive understanding of how transaction costs and consumer characteristics jointly shape the alcohol–marijuana relationship.

Our approach of including transaction costs measured through distance or ease of access to a related market could also help shed light on other historically ambiguous market relationships. Either through unintended aggregation bias from limited data or oversight of its importance in empirical modeling, transaction costs are integral to better understanding consumer behavior. Application within the public and health policy fields could improve our understanding of overall policy impacts or patient behavior that could ultimately be lost through misreporting relationships or concluding a null effect that indeed switches across space and level of access.

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8 Figures

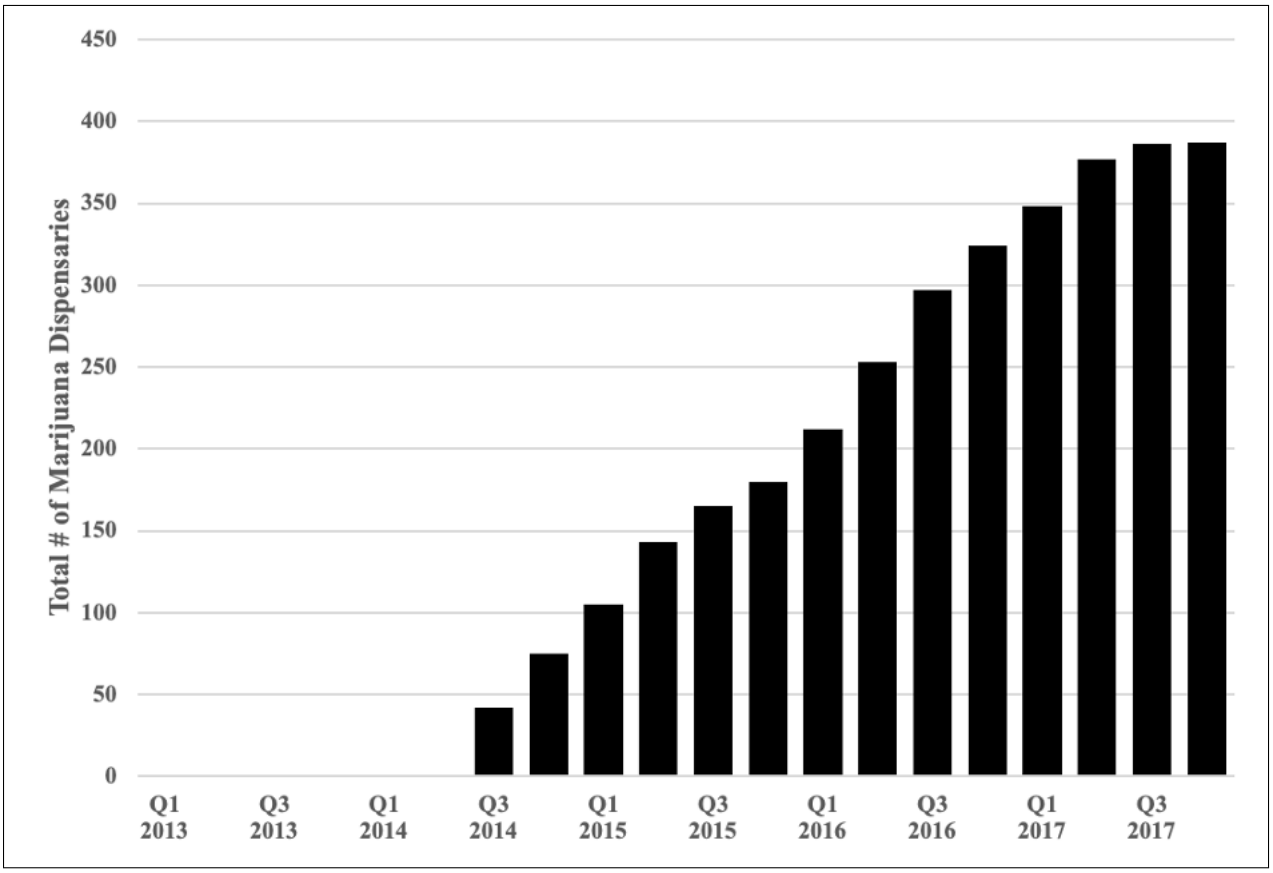


Figure 1: Total Number of Dispensaries in Washington, by quarter-year

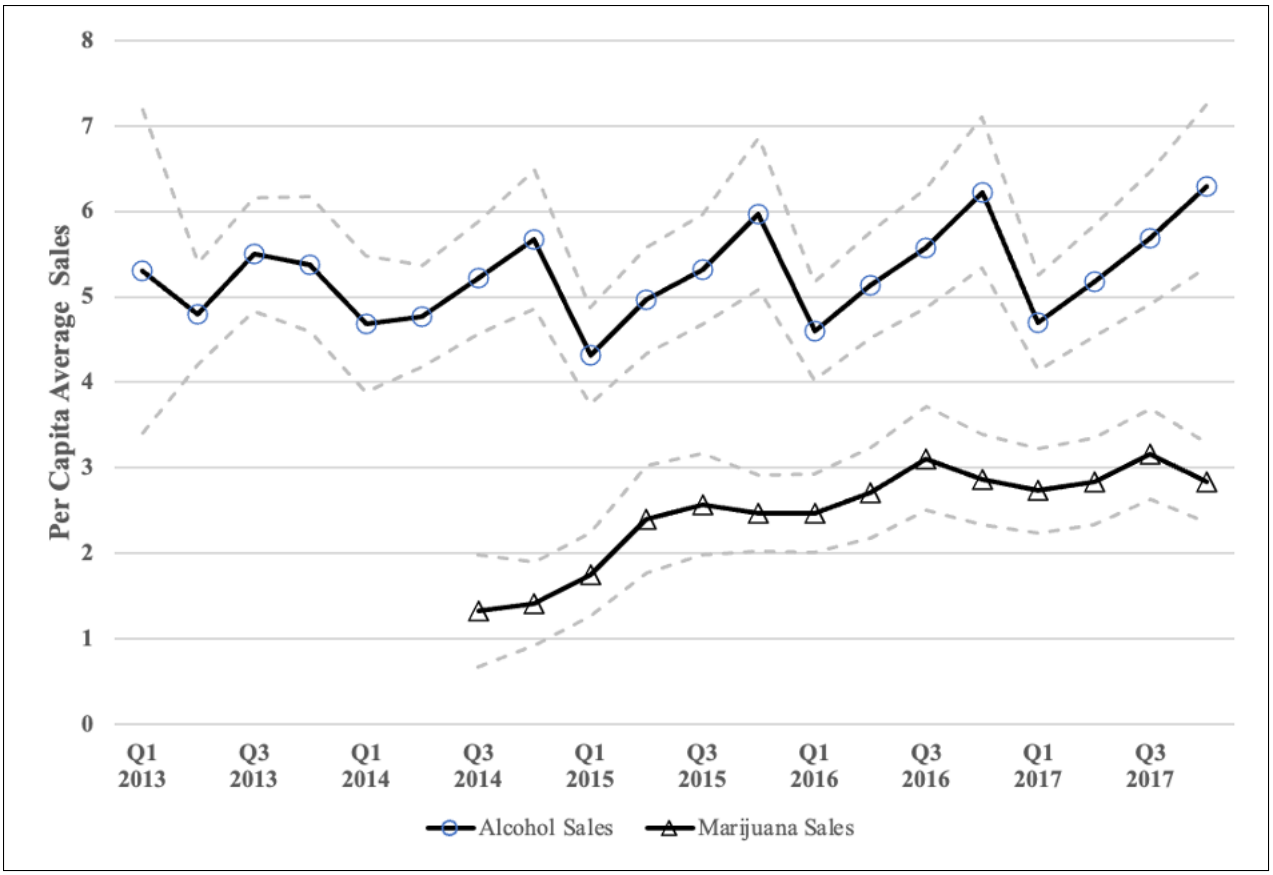
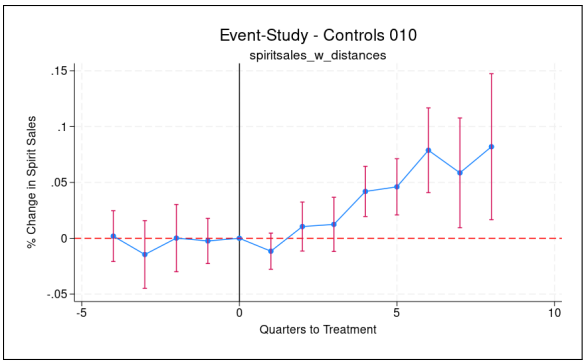
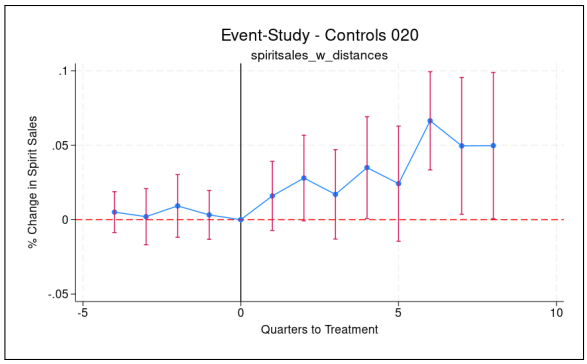


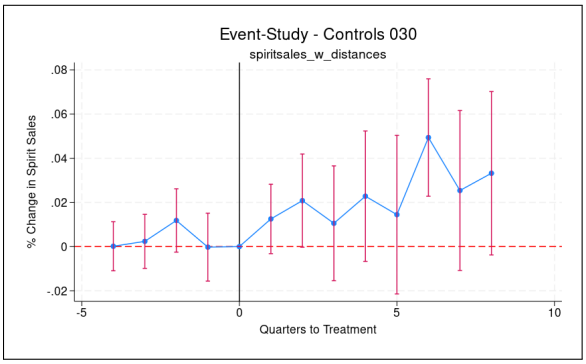
Figure 2: Average Spirit Alcohol and Marijuana Sales, per capita by quarter



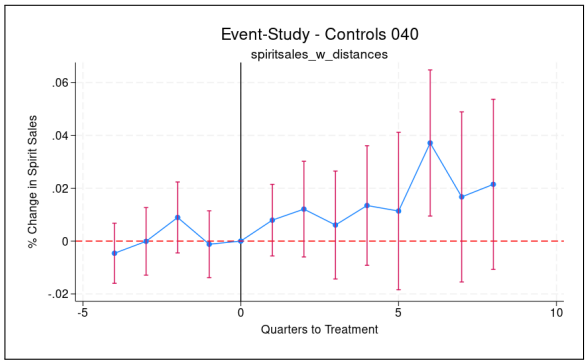
(a) "Treatment" $\leq$ 0.1-miles



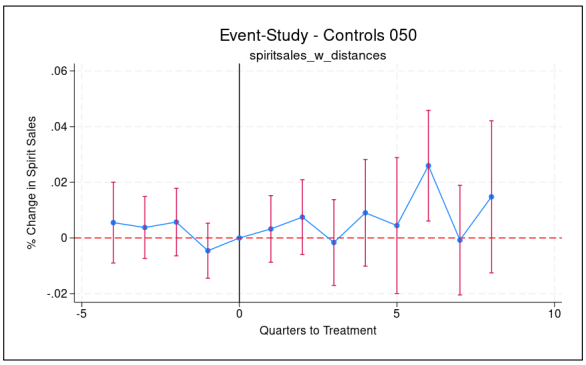
(b) "Treatment" $\leq$ 0.2-miles



(c) "Treatment" $\leq$ 0.3-miles



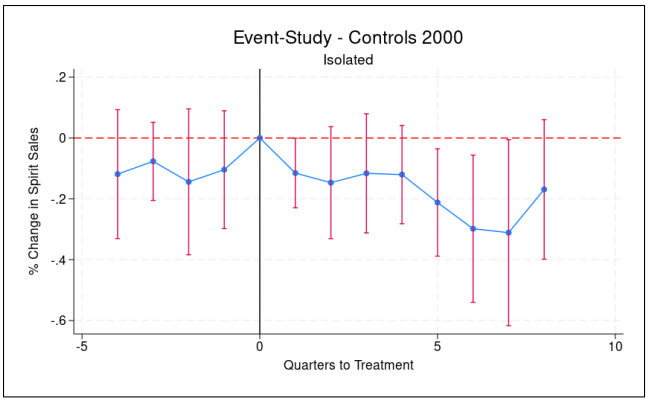
(d) "Treatment" $\leq$ 0.4-miles



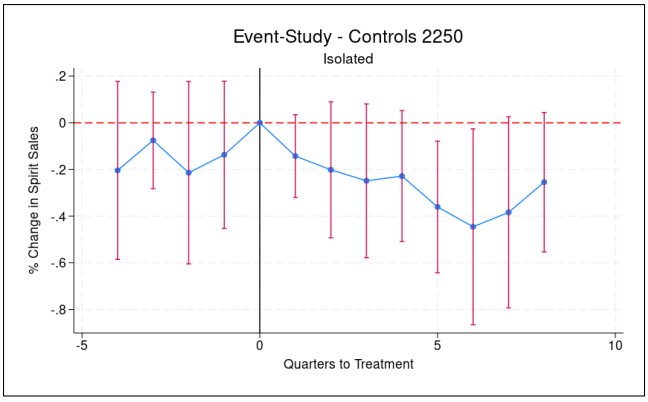
(e) "Treatment" $\leq$ 0.5-miles

Figure 3: Event Study for All Alcohol Retailers

Note: Each point in the figure represents the difference between "treated" alcohol retailers relative to "untreated" retailer, before and after treatment. Each specification includes the covariates of percent age 20-24, percent male population, percent with at least a high school education, and percent married at the zip code level, as well as fixed effects for retailer. Standard errors are clustered at the county level. 90% confidence intervals reported as band around point estimates.



(a) "Treatment" $\geq$ 20.0-miles



(b) "Treatment" $\geq$ 22.5-miles

Figure 4: Event Study for Non-Chain, Isolated Alcohol Retailers

Note: Each point in the figure represents the difference between "treated" alcohol retailers relative to "untreated" retailer, before and after treatment. Each specification includes the covariates of percent age 20-24, percent male population, percent with at least a high school education, and percent married at the zip code level, as well as fixed effects for retailer. Standard errors are clustered at the county level. 90% confidence intervals reported as band around point estimates.

9 Tables

Table 1: Abbreviated Literature Review

Study	**Policy**	**Data**	**Population**	**Conclusion**
Thies and Register (1993)	Decrim.	NLSY (1979, 1984, 1988)	U.S. males (ages 14–21)	Mixed
Yamada et al. (1996)	Decrim.	NLSY (1982)	High schoolers	Substitues
Pacula (1998)	Decrim.	NLSY(1979)	U.S. youth (ages 14–21)	Complements
Salomonsen-Sautel et al. (2014)	MML	FARS (1994-2011)	Colorado and 34 Non-MML States (21-24 years old)	No evidence
Anderson et al. (2013)	MML	FARS (1990-2010); BRFSS (1993-2010); Beer Institute's Brewers Almanac (1990-2010)	U.S. population	Substitutes
Sabia et al. (2017)	MML	BRFSS (1990-2012)	U.S. population	Substitutes
Andreyeva and Ukert (2019)	MML	BRFSS (1993-2013)	U.S. population	Mixed
Miller and Seo (2021)	RML	Nielsen retail scanner data (2013-2016)	Counties in Washington	Substitutes
Calvert and Erickson (2021)	RML	Nielsen Consumer Panel (2004-2017)	CO, OR, and WA compared to non-RML states	Mixed

Table 2: Hypotheses Under Varying Preferences and Transaction Costs

Preference (γ)	**Trans. Cost** (τ)	**Expected Behavior and Interpretation**
$\gamma > 0$ (complements)	Low	Complements: Joint consumption is valuable and low-cost.
$\gamma > 0$	High	Substitutes or weak complements: High cost of joint consumption offsets utility gains; consumers may buy only one.
$\gamma < 0$ (substitutes)	Low	Substitutes: Consumers prefer one over the other; low frictions allow free choice.
$\gamma < 0$	High	Strong substitutes: Frictions reinforce separation; single-good consumption is strictly optimal.
$\gamma = 0$ (independent)	Low	Independent: No interaction; choices depend on price and taste alone.
$\gamma = 0$	High	Substitutes (by constraint): Frictions prevent joint use; behavior mimics substitution even without utility interaction.

Table 3: Summary Statistics

	All		Pre		Post		Not Treated		Treated	
	Obs.	*Mean/SE*	*Obs.*	*Mean/SE*	*Obs.*	*Mean/SE*	*Obs.*	*Mean/SE*	*Obs.*	*Mean/SE*
Panel A: All Retailers										
Spirit Sales	19,271	102,569	5,598	95,852	13,673	105,319	16,844	102,267	2,427	104,669
		(103,463)		(98,851)		(105,173)		(101,441)		(116,540)
Per Capita Sales	19,271	5.27	5,598	5.07	13,673	5.35	16,844	4.90	2,427	7.84
		(12.716)		(15.373)		(11.452)		(9.209)		(26.232)
Nearest Marijuana Distance	19,191	2.33	–	–	13,617	2.35	16,764	2.65	2,427	0.10
		(3.782)		–		(3.818)		(3.943)		(0.066)
Number of Close Spirit Retailers	19,271	13.91	5,598	14.27	13,673	13.77	16,844	13.69	2,427	15.49
		(7.714)		(7.487)		(7.801)		(7.764)		(7.167)
Panel B: Chain Retailers										
Spirit Sales	13,920	113,607	4,114	104,351	9,806	117,491	12,118	114,261	1,802	109,211
		(104,782)		(97,418)		(107,490)		(103,200)		(114,796)
Per Capita Sales	13,920	4.66	4,114	4.49	9,806	4.73	12,118	4.53	1,802	5.56
		(9.321)		(14.215)		(6.209)		(9.273)		(9.594)
Nearest Marijuana Distance	13,900	1.77	–	–	9,792	1.78	12,098	2.02	1,802	0.10
		(2.516)		–		(2.519)		(2.607)		(0.067)
Number of Close Spirit Retailers	13,920	15.17	4,114	15.36	9,806	14.98	12,118	15.07	1,802	16.43
		(6.749)		(6.586)		(6.815)		(6.797)		(6.273)
Panel C: Non-Chain Retailers										
Spirit Sales	5,351	73,854	1,484	72,290	3,867	74,455	4,726	71,511	625	91,571
		(94,076)		(99,008)		(92,120)		(89,746)		(120,566)
Per Capita Sales	5,351	6.85	1,484	6.68	3,867	6.91	4,726	5.85	625	14.40
		(18.787)		(18.110)		(19.042)		(8.974)		(48.492)
Nearest Marijuana Distance	5,281	3.77	–	–	3,825	3.79	4,666	4.26	625	0.10
		(5.688)		–		(5.720)		(5.885)		(0.065)
Number of Close Spirit Retailers	5,351	10.65	1,484	11.25	3,867	10.41	4,726	10.36	625	12.77
		(9.003)		(8.877)		(9.041)		(9.002)		(8.728)

Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes:* Data is aggregated to the alcohol retailer and quarter by year level. For simplicity, a single pre/post date of July 2014 was chosen, representing the first opening of recreational marijuana dispensaries, and treated alcohol retailers are within 0.20 miles of a marijuana dispensary. Close alcohol retailers are determined if a neighboring alcohol retailer is within 5.0 miles of target alcohol retailer. Chain retailers are defined as having five or more locations within Washington state; non-chain fewer than five.

Table 4: Close Results

	Transaction cost treatment defined by distance (distances shown)					
	0.1 miles	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
	Coeff./SE	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*
Panel A: All Alcohol Retailers						
Treatment Start	0.059**	0.060**	0.047**	0.034**	0.028*	0.021*
	(0.022)	(0.022)	(0.017)	(0.013)	(0.011)	(0.009)
Observations	19,271	19,271	19,271	19,264	19,242	19,242
Dependent Mean			1.419			
Panel B: Chain Alcohol Retailers						
Treatment Start	0.099***	0.099***	0.055***	0.040***	0.029***	0.020**
	(0.031)	(0.031)	(0.017)	(0.009)	(0.010)	(0.007)
Observations	13,920	13,920	13,920	13,920	13,907	13,907
Dependent Mean			1.386			
Panel C: Non-Chain Alcohol Retailers						
Treatment Start	-0.040+	-0.042+	0.019	0.010	0.017	0.014
	(0.022)	(0.022)	(0.060)	(0.051)	(0.039)	(0.032)
Observations	5,351	5,351	5,351	5,344	5,335	5,335
Dependent Mean			1.504			
Covariates	No	Yes	Yes	Yes	Yes	Yes
R-Q FE	Yes	Yes	Yes	Yes	Yes	Yes

Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes*: R-Q FE represents retailer and quarter by year fixed effects. Chain retailers are defined as any licensed spirit alcohol retailer with five or more locations within the state of Washington; Non-Chain fewer than five. The outcome variable is the log of spirit sales per capita (by zip code). All models use Borusyak et al. (2024) estimation method. Covariates are zip-level variables, including: percent age 20-24, percent male population, percent with at least a high school education, and percent married. The number of alcohol retailers is included as our time-control variable. Standard errors are clustered at the county level. Symbols denote significance at the 0.5% (***), 1% (**), 5% (*), and 10% (+) levels.

Table 5: Isolated Results

	Transaction cost treatment defined by distance (distances shown)				
	12.5+ miles	15+ miles	17.5+ miles	20+ miles	22.5+ miles
	Coeff./SE	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*
Panel A: All Alcohol Retailers					
Treatment Start	-0.030	0.003	-0.072***	-0.071***	-0.100***
	(0.028)	(0.026)	(0.020)	(0.020)	(0.013)
Observations	19,245	19,245	19,245	19,259	19,271
Dependent Mean			1.417		
Panel B: Chain Alcohol Retailers					
Treatment Start	0.004	-0.030	0	0	0
	(0.019)	(0.034)	(.)	(.)	(.)
Observations	13,920	13,920	13,920	13,920	13,920
Dependent Mean			1.386		
Panel C: Non-Chain Alcohol Retailers					
Treatment Start	-0.034	0.021	-0.075***	-0.075***	-0.119***
	(0.034)	(0.030)	(0.023)	(0.022)	(0.022)
Observations	5,325	5,325	5,325	5,339	5,351
Dependent Mean			1.498		
Covariates	Yes	Yes	Yes	Yes	Yes
R-Q FE	Yes	Yes	Yes	Yes	Yes

Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes*: R-Q FE represents retailer and quarter by year fixed effects. Chain retailers are defined as any licensed spirit alcohol retailer with five or more locations within the state of Washington; Non-Chain fewer than five. The outcome variable is the log of spirit sales per capita (by zip code). All models use Borusyak et al. (2024) estimation method. Covariates are zip-level variables, including: percent age 20-24, percent male population, percent with at least a high school education, and percent married. The number of alcohol retailers is included as our time-control variable. Standard errors are clustered at the county level. Symbols denote significance at the 0.5% (***), 1% (**), 5% (*), and 10% (+) levels.

Table 6: Close Results by Retailer Type

	Transaction cost treatment defined by distance (distances shown)					
	0.1 miles	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
	Coeff./SE	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*
Panel A: Supermarket						
Treatment Start	0.062***	0.154***	0.076***	0.069***	0.063***	0.046***
	(0.012)	(0.036)	(0.010)	(0.011)	(0.012)	(0.015)
Observations	4,437	5,499	4,437	4,437	4,424	4,424
Dependent Mean	1.759					
Panel B: Grocery Store						
Treatment Start	-0.034	-0.038	-0.051*	0.037**	0.037**	0.037***
	(0.026)	(0.024)	(0.021)	(0.014)	(0.014)	(0.012)
Observations	7,218	7,218	7,218	7,218	7,209	7,209
Dependent Mean	1.657					
Panel C: Pharmacy						
Treatment Start	0.153***	0.154***	0.106***	0.070***	0.063***	0.045***
	(0.035)	(0.036)	(0.026)	(0.022)	(0.019)	(0.014)
Observations	5,499	5,499	5,499	5,499	5,499	5,499
Dependent Mean	0.686					
Panel D: Liquor						
Treatment Start	-0.027	-0.024	0.111	0.098	0.087	0.098
	(0.024)	(0.034)	(0.116)	(0.107)	(0.094)	(0.089)
Observations	1,341	1,341	1,341	1,341	1,341	1,341
Dependent Mean	1.928					
Covariates	No	Yes	Yes	Yes	Yes	Yes
R-Q FE	Yes	Yes	Yes	Yes	Yes	Yes

Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes*: R-Q FE represents retailer and quarter by year fixed effects. The outcome variable is the log of spirit sales per capita (by zip code). All models use Borusyak et al. (2024) estimation method. Covariates are zip-level variables, including: percent age 20-24, percent male population, percent with at least a high school education, and percent married. The number of alcohol retailers is included as our time-control variable. Standard errors are clustered at the county level. Symbols denote significance at the 0.5% (***), 1% (**), 5% (*), and 10% (+) levels.

Table 7: Isolated Results by Retailer Type

	Transaction cost treatment defined by distance (distances shown)				
	12.5+ miles	15+ miles	17.5+ miles	20+ miles	22.5+ miles
	Coeff./SE	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*
Panel A: Supermarket					
Treatment Start	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)
Observations	4,437	4,437	4,437	4,437	4,437
Dependent Mean			1.759		
Panel B: Grocery Store					
Treatment Start	-0.001	0.037	-0.048**	-0.048**	-0.114***
	(0.032)	(0.028)	(0.018)	(0.018)	(0.021)
Observations	7,204	7,204	7,204	7,218	7,218
Dependent Mean			1.655		
Panel C: Pharmacy					
Treatment Start	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)
Observations	5,499	5,499	5,499	5,499	5,499
Dependent Mean			0.686		
Panel D: Liquor					
Treatment Start	-0.126+	-0.012	-0.012	-0.012	0
	(0.065)	(0.041)	(0.041)	(0.041)	(.)
Observations	1,329	1,329	1,329	1,329	1,341
Dependent Mean			1.920		
Covariates	Yes	Yes	Yes	Yes	Yes
R-Q FE	Yes	Yes	Yes	Yes	Yes

Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes*: R-Q FE represents retailer and quarter by year fixed effects. The outcome variable is the log of spirit sales per capita (by zip code). All models use Borusyak et al. (2024) estimation method. Covariates are zip-level variables, including: percent age 20-24, percent male population, percent with at least a high school education, and percent married. The number of alcohol retailers is included as our time-control variable. Standard errors are clustered at the county level. Symbols denote significance at the 0.5% (***), 1% (**), 5% (*), and 10% (+) levels.

Appendix A: Overview of US Studies Examining Complement-Substitute Relationship of Marijuana and Alcohol

Study	MJ/Alcohol Policy Leveraged	Data	Population	Conclusion
Stephens et al. (1994)	No explicit policy change	12-month posttreatment data for men and women seeking help	Cannabis treatment seekers	**No evidence**: problems associated with alcohol did not appear to relate to reduced marijuana use
Farrelly et al. (1999a)	No explicit policy change	National Household Survey on Drug Abuse (1990–1996)	U.S. youth (12–20) & young adults (21–30)	**Complements** for youths; **weak complementarity** for adults.
Williams et al. (2004)	No explicit policy change, focus on campus alcohol bans	Harvard CAS surveys (1993, 1997, 1999)	U.S. college students	**Complements**: campus alcohol bans reduce both alcohol and marijuana use.
Aharonovich et al. (2005)	No explicit policy change	Psychiatric Research Interview for Substance and Mental Disorders	18 years old or older from an inpatient psychiatric/substance abuse setting	**Complements**: cannabis use increases the risk of returning to the use of alcohol
Alter et al. (2006)	No explicit policy change	Monitoring the Future	Midwest Sample	**Substitutes**: participants who reported no alcohol use were more likely to report cannabis use
Copersino et al. (2006)	No explicit policy change	Retrospective self-report data	adult, non-treatment-seeking cannabis users	**Substitutes**: some participants noted drinking alcohol to relieve withdrawal symptoms
Hughes et al. (2008)	No explicit policy change	Small sample of self-identified, daily cannabis users	18+ years old intending to quit marijuana use	**No evidence**: alcohol use on days of marijuana abstinence or reduction was similar to days marijuana use did not change
Kadden et al. (2009)	No explicit policy change	Study of treatments for marijuana dependence	Cannabis treatment seekers	**No evidence**: there was no relationship between marijuana and the use of alcohol

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Study	MJ/Alcohol Policy Leveraged	Data	Population	Conclusion
Reiman (2009)	No explicit policy change	Anonymous survey data were collected at the Berkeley Patient's Group (BPG)	Medical cannabis patients (18-81 years old)	**Substitutes**: Medical cannabis patients were found to be using cannabis as an alternative to alcohol.
Allsop et al. (2014)	No explicit policy change	prospective community based cohort	Adult, non-treatment seeking cannabis users	**Substitutes**: alcohol use increased when users decreased marijuana intake and decreased with using marijuana again
Weinberger et al. (2021)	No explicit policy change	National Survey on Drug Use and Health (2002-2018)	U.S. youth and young adults aged 12-25	**Complements**: Results were more consistent with a complementary relationship than substitution.
DiNardo and Lemieux (2001)	MLDA	Monitoring the Future (1980–1989)	U.S. high school seniors	**Substitutes**: raising MLDAs reduce alcohol use and increase marijuana use.
Yörük and Yörük (2011)	MLDA	NLSY (1997)	Young adults (19-22 years old)	**Complements**: marijuana use rises when alcohol consumption rises at 21
Crost and Guerrero (2012)	MLDA	National Survey of Drug Use & Health (2002–2007)	U.S. young adults (18–24)	**Substitutes**: Increased alcohol availability at 21 reduces marijuana use.
Keyes et al. (2015)	MLDA	Fatality Analysis Reporting System (1999-2011)	Young adults (16-25 years old)	**Substitutes**: While their findings suggest substitution, not always statistically significant.
Thies and Register (1993)	Decriminalization	National Longitudinal Survey of Youth (1979, 1984, 1988)	U.S. males (ages 14–21)	**Mixed**: decriminalization states featured fewer people taking "six or more" drinks of alcohol at a time, but positively associated with any alcohol use in 1984.
Model (1993)	Decriminalization	Drug Abuse Warning Network (1975-1977)	drug-related ER episodes in 24 major SMSA's	**Substitutes**: decriminalization was accompanied by a significant reduction in episodes involving drugs other than marijuana

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Study	MJ/Alcohol Policy Leveraged	Data	Population	Conclusion
Yamada et al. (1996)	Decriminalization and beer taxes	NLSY (1982)	High schoolers	**Substitues**: decriminalization is found to reduce the probability of becoming a frequent drinker.
Chaloupka and Laixuthai (1997)	Decriminalization	Monitoring the Future (1982 & 1989)	U.S. high school seniors	**Substitutes**: decriminalization linked to less alcohol consumption and less likely to engage in heavy drinking.
Pacula (1998)	Decriminalization	National Longitudinal Survey of Youth (1979)	U.S. youth (ages 14–21)	**Complements**: higher beer taxes reduce marijuana use. Decriminalization had no effect.
Saffer and Chaloupka (1999)	Decriminalization	NHSDA (1988, 1990, 1991)	U.S. adults 12-30	**Substitutes**: decriminalization is associated with decreased drinking.
Richmond et al. (2015)	MML	federally funded screening, brief intervention, and referral to treatment (SBIRT) initiative	patients with and without state legal access to medical marijuana (18-94 years old)	**Substitutes**: Patients with cards also had lower use of other substances than patients without cards.
Salomonsen-Sautel et al. (2014)	MML	Fatality Analysis Reporting System (1994-2011)	Colorado and 34 Non-MML States (21-24 years old)	**No evidence**: There were no significant changes in the proportion of alcohol-impaired drivers in fatal vehicle crashes
Anderson et al. (2013)	MML	Fatality Analysis Reporting System (1990-2010); BRFSS (1993-2010); Beer Institute's Brewers Almanac (1990-2010)	U.S. population	**Substitutes**: MML is associated with a decrease in traffic fatalities, a decrease in self-reported alcohol use, and a decrease in beer sales. There is no evidence that MML had an effect on wine or spirit sales.
Wen et al. (2015)	MML	NSDUH (2004-2012)	U.S. civilians, non-institutionalized, (12+)	**Complements**: MMLs are associated with an increase in binge drinking for those 21+, with no evidence in effects for 12-20 year olds.

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Study	MJ/Alcohol Policy Leveraged	Data	Population	Conclusion
Sabia et al. (2017)	MML	BRFSS (1990-2012)	U.S. population	**Substitutes**: MML is associated with reductions in past-month alcohol use and binge drinking
Johnson et al. (2018)	MML	Youth Risk Behavior Surveys (1991-2011)	U.S. high schoolers	**Substitutes**: MMLs are associated with a decrease in alcohol use.
Andreyeva and Ukert (2019)	MML	BRFSS (1993-2013)	U.S. population	**Mixed**: MMLs are associated with a decrease in heavy drinking and dispensary sales are associated with an increase in heavy drinking.
Baggio et al. (2020)	MML	Nielsen retail scanner data (2006-2015)	U.S. states	**Substitutes**: MMLs are associated with a reduction in alcohol sales.
Conyers and Ayres (2020)	MML	Hospital discharge data from Department of Health Services (2010-2016)	Arizona	**No evidence**: There is no association between alcohol-related hospital visits and newly opened medical dispensaries.
Veligati et al. (2020)	MML & RML	National Institute on Alcohol Abuse and Alcoholism's Alcohol Epidemiologic Data (1990-2016)	U.S. civilian, non-institutionalized adult population (18+)	**No evidence**: There is no association between MML or RML with changes in per capita alcohol sales
Dragone et al. (2019)	RML	National Survey on Drug Use and Health (NSDUH, 2010-2014)	U.S. population	**Substitutes**: Recreational marijuana legalization is associated with a reduction in binge drinking.
Alley et al. (2020)	RML	National College Health Assessment-II (2008-2018)	U.S. college students	**Substitutes**: legalization is associated with a reduction in alcohol use and binge drinking.
Miller and Seo (2021)	RML	Nielsen retail scanner data (2013-2016)	Counties in Washington	**Substitutes**: RML decreases alcohol demand.

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Study	MJ/Alcohol Policy Leveraged	Data	Population	Conclusion
Calvert and Erickson (2021)	RML	Nielsen Consumer Panel (2004-2017)	Colorado, Oregon, and Washington compared to non-RML states	**Mixed**: There was a measured decrease in alcohol consumption after legalization in Colorado and Oregon, but a measure increase in Washington.
Zhu et al. (2025)	RML	Washington Panel Survey (2014-4016)	Adult participants	**Mixed**: Recreational cannabis users engage in complementary use patterns, while medical cannabis users exhibit substitution use patterns.

Appendix B: Transaction Cost Addition to the DiNardo and Lemieux (2001) Framework

To directly compare our results with that of DiNardo and Lemieux (2001), we begin with their utility function and quadratic sub-utility function representing preferences for alcohol (q_a) and marijuana (q_m) as:

$$G(q) \;=\; u(q_0) + v(q_a, q_m), \tag{B.1}$$

and

$$v(q_a, q_m) = \gamma_0 \;+\; \gamma_a q_a \;+\; \gamma_m q_m \;+\; \gamma_{am} q_a q_m \;+\; \tfrac{1}{2}\gamma_{aa} q_a^2 \;+\; \tfrac{1}{2}\gamma_{mm} q_m^2. \tag{B.2}$$

The sign of the cross-partial $v_{am} = \gamma_{am}$ determines whether alcohol and marijuana are *utility-level* complements ($\gamma_{am} > 0$) or substitutes ($\gamma_{am} < 0$).

Rather than identify the budget constraint as an individual effect for the price of each good, including the "full" or shadow price of the purchases (e.g., enforcement risk or legality, including the availability of the product and age restrictions), we modify their budget constraint to include a potential joint transaction cost:

$$I \;=\; q_0 \;+\; p_a q_a \;+\; p_m q_m \;+\; \tau q_a q_m, \tag{B.3}$$

where $\tau \geq 0$ represents the per-unit cost of jointly consuming q_a and q_m.

The consumer's first-order conditions now reduce to:

$$\frac{v_a}{v_m} \;=\; \frac{p_a + \tau q_m}{p_m + \tau q_a}, \tag{B.4}$$

where

$$v_a \;=\; \frac{\partial v}{\partial q_a} = \gamma_a + \gamma_{am} q_m + \gamma_{aa} q_a, \qquad v_m \;=\; \frac{\partial v}{\partial q_m} = \gamma_m + \gamma_{am} q_a + \gamma_{mm} q_m. \tag{B.5}$$

The sign of the cross-partial continues to determine whether alcohol and marijuana are *utility-level* complements ($\gamma_{am} > 0$) or substitutes ($\gamma_{am} < 0$), but transaction costs τ may distort the effective price ratio and change the relationship. That is, high τ can discourage joint consumption or induce corner solutions even with $\gamma_{am} > 0$, making demand appear substitutable. Conversely, when τ is small, underlying complementarity is more easily revealed in behavior.

It is reasonable for early empirical attention to focus on a discrete shifts in the "full" prices of goods while not considering τ explicitly because these were not measurable (implicit to the individual). By contrast, the joint transaction-cost parameter τ captures frictions that arise when both goods are legally available and acquired/consumed together, such as additional search time. When $\tau > 0$, however, the cost of joint consumption rises with both, so even goods that are complements in preferences may behave as substitutes in observed demand. Similar arguments can be made for other papers. For example, Pacula's (1998) framework is another natural examination of a setting where both goods were (at least partly) illegal and the main margin of cost variation was legal risk. Our framework examines the issue when both goods are legal; our margin is whether people consume them jointly and what frictions might affect these decisions. This provides a natural explanation for the mixed empirical findings in the literature, and highlights the role of transaction costs in shaping observed substitution patterns.

Appendix C: BJS link to standard FE/DiD models

The BJS approach can be viewed as an extension of a Difference-in-Differences (DiD) framework with staggered treatment timing. In the standard DiD setup:

$$q_{it} = \alpha + \delta \cdot \text{Treatment}_{it} + \beta X_{it} + \mu_i + \lambda_t + \epsilon_{it}, \tag{C.1}$$

the treatment effect δ captures the average change in q_{it} for treated units after treatment, relative to never-treated units, controlling for fixed unit and time effects. This works well when treatment timing is uniform. With staggered rollouts, units become treated at different times and simple TWFE models effectively compare early-treated units to later-treated ones in periods before they are treated. If treatment effects evolve over time, these comparisons mix treatment and control observations in a way that biases δ.

BJS addresses this by, in effect, running (C.1) within the "not-yet-treated" subsample at each period to build a clean counterfactual, then imputing what treated units would have experienced had they never been treated. This preserves the DiD spirit—comparing treated and untreated outcomes—while avoiding the problematic contamination from staggered adoption.

For robustness, we also estimate: (1) a naive TWFE model of the form:

$$Y_{ist} = \alpha + RML_{st} \cdot \delta + X_{ist}\beta + \delta_s + \mu_t + \epsilon_{ist}, \tag{C.2}$$

and (2) the cohort-specific regression estimator of Gardner (2022). In all cases, the treatment date is defined as the effective date of RML implementation by distance proximity d.

Table 1: Close Results for Gardner and TWFE

	Transaction cost treatment defined by distance (distances shown)					
	0.1 miles	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
	Coeff./SE	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*
Panel A: BJS						
Treatment Start	0.059**	0.060**	0.047**	0.034**	0.028*	0.021*
	(0.022)	(0.022)	(0.017)	(0.013)	(0.011)	(0.009)
Observations	19,271	19,271	19,271	19,264	19,242	19,242
Dependent Mean			1.419			
Panel B: Gardner						
Treatment Start	0.051*	0.052*	0.044*	0.029+	0.012	0.007
	(0.021)	(0.022)	(0.021)	(0.016)	(0.016)	(0.014)
Observations	19,271	19,271	19,271	19,271	19,271	19,271
Dependent Mean			1.000			
Panel C: TWFE						
Treatment Start	-0.050	-0.044	0.008	-0.048	-0.048	-0.048
	(0.032)	(0.029)	(0.058)	(0.055)	(0.055)	(0.055)
Observations	19,269	19,269	19,269	19,269	19,269	19,269
Dependent Mean			1.419			
Covariates	No	Yes	Yes	Yes	Yes	Yes
R-Q FE	Yes	Yes	Yes	Yes	Yes	Yes

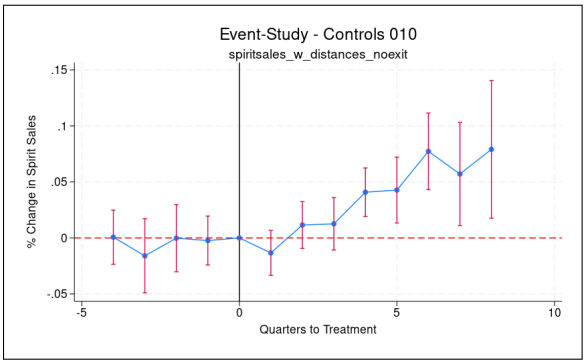
Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes*: R-Q FE represents retailer and quarter by year fixed effects. The outcome variable is the log of spirit sales per capita (by zip code). Covariates are zip-level variables, including: percent age 20-24, percent male population, percent with at least a high school education, and percent married. The number of alcohol retailers is included as our time-control variable. Standard errors are clustered at the county level. Symbols denote significance at the 0.5% (***), 1% (**), 5% (*), and 10% (+) levels.

Table 2: Isolated Results for Gardner and TWFE

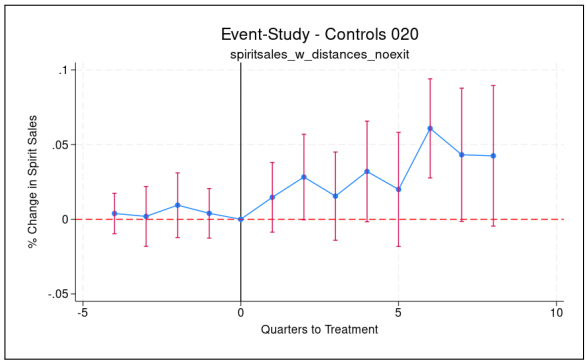
	Transaction cost treatment defined by distance (distances shown)				
	12.5+ miles	15+ miles	17.5+ miles	20+ miles	22.5+ miles
	Coeff./SE	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*
Panel A: BJS					
Treatment Start	-0.030	0.003	-0.072***	-0.071***	-0.100***
	(0.028)	(0.026)	(0.020)	(0.020)	(0.013)
Observations	19,245	19,245	19,245	19,259	19,271
Dependent Mean			1.417		
Panel B: Gardner					
Treatment Start	0.120	0.216	0.242	0.109	-0.083
	(0.108)	(0.157)	(0.221)	(0.174)	(0.069)
Observations	19,271	19,271	19,271	19,271	19,271
Dependent Mean			1.000		
Panel C: TWFE					
Treatment Start	0.048	0.048	0.048	0.048	0.048
	(0.055)	(0.055)	(0.055)	(0.055)	(0.055)
Observations	19,269	19,269	19,269	19,269	19,269
Dependent Mean			1.419		
Covariates	Yes	Yes	Yes	Yes	Yes
R-Q FE	Yes	Yes	Yes	Yes	Yes

Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes*: R-Q FE represents retailer and quarter by year fixed effects. The outcome variable is the log of spirit sales per capita (by zip code). Covariates are zip-level variables, including: percent age 20-24, percent male population, percent with at least a high school education, and percent married. The number of alcohol retailers is included as our time-control variable. Standard errors are clustered at the county level. Symbols denote significance at the 0.5% (***), 1% (**), 5% (*), and 10% (+) levels.

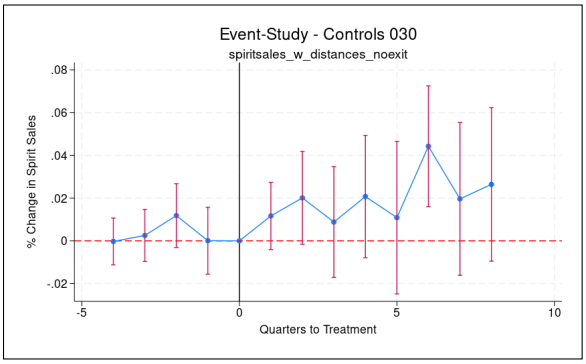
Appendix D: No Exit Results



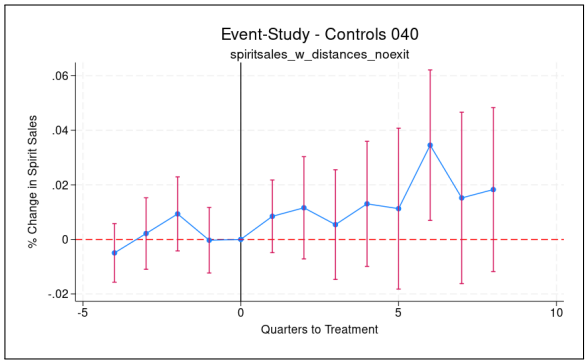
(D.a) "Treatment" $\leq$ 0.1-miles



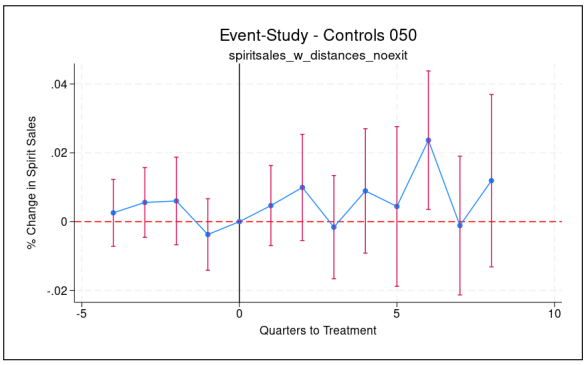
(D.b) "Treatment" $\leq$ 0.2-miles



(D.c) "Treatment" $\leq$ 0.3-miles



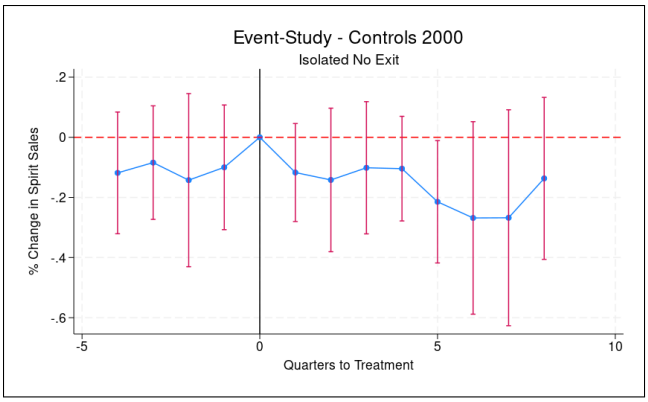
(D.d) "Treatment" $\leq$ 0.4-miles



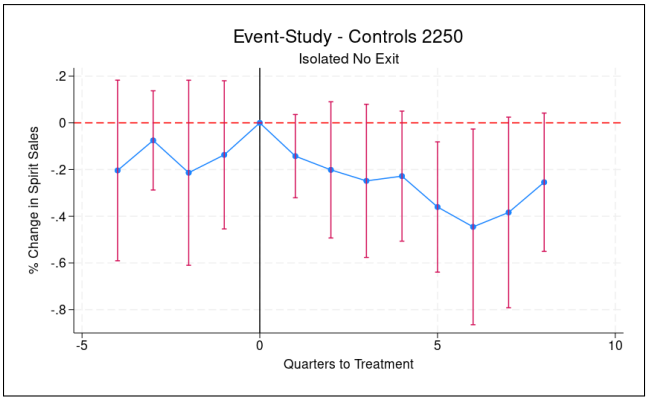
(D.e) "Treatment" $\leq$ 0.5-miles

Figure D.1: Event Study for All Alcohol Retailers, No Exit

Note: Each point in the figure represents the difference between "treated" alcohol retailers relative to "untreated" retailer, before and after treatment. Each specification includes the covariates of percent age 20-24, percent male population, percent with at least a high school education, and percent married at the zip code level, as well as fixed effects for retailer. Standard errors are clustered at the county level. 90% confidence intervals reported as band around point estimates.



(D.a) "Treatment" $\geq$ 20.0-miles



(D.b) "Treatment" $\geq$ 22.5-miles

Figure D.2: Event Study for Non-Chain, Isolated Alcohol Retailers, No Exit

Note: Each point in the figure represents the difference between "treated" alcohol retailers relative to "untreated" retailer, before and after treatment. Each specification includes the covariates of percent age 20-24, percent male population, percent with at least a high school education, and percent married at the zip code level, as well as fixed effects for retailer. Standard errors are clustered at the county level. 90% confidence intervals reported as band around point estimates.

Table D.1: Main Results, No Exit

	Transaction cost treatment defined by distance (distances shown)					
	0.1 miles	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
	Coeff./SE	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*
Panel A: All Spirit Retailers						
Treatment Start	0.057**	0.057**	0.043*	0.030*	0.026*	0.020*
	(0.021)	(0.021)	(0.018)	(0.013)	(0.012)	(0.009)
Observations	19,271	19,271	19,271	19,264	19,242	19,242
Dependent Mean			1.419			
Panel B: Chain Alcohol Retailers						
Treatment Start	0.095***	0.096***	0.056***	0.039***	0.030**	0.020**
	(0.028)	(0.029)	(0.016)	(0.010)	(0.011)	(0.008)
Observations	13,920	13,920	13,920	13,920	13,907	13,907
Dependent Mean			1.386			
Panel C: Non-Chain Alcohol Retailers						
Treatment Start	-0.040+	-0.042+	0.006	0.000	0.013	0.013
	(0.022)	(0.022)	(0.059)	(0.050)	(0.037)	(0.030)
Observations	5,351	5,351	5,351	5,344	5,335	5,335
Dependent Mean			1.504			
Covariates	No	Yes	Yes	Yes	Yes	Yes
R-Q FE	Yes	Yes	Yes	Yes	Yes	Yes

Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes*: R-Q FE represents retailer and quarter by year fixed effects. "Large" retailers are defined as any licensed spirit alcohol retailer with five or more locations within the state of Washington; "Small" fewer than five. The outcome variable is the log of spirit sales per capita (by zip code). All models use Borusyak et al. (2024) estimation method. Covariates are zip-level variables, including: percent age 20-24, percent male population, percent with at least a high school education, and percent married. The number of alcohol retailers is included as our time-control variable. Standard errors are clustered at the county level. Symbols denote significance at the 0.5% (***), 1% (**), 5% (*), and 10% (+) levels.

Table D.2: Isolated Retailer Results, No Exit

	Transaction cost treatment defined by distance (distances shown)				
	12.5+ miles	15+ miles	17.5+ miles	20+ miles	22.5+ miles
	Coeff./SE	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*
Panel A: All Spirit Retailers					
Treatment Start	-0.011	0.012	-0.053***	-0.053***	-0.100***
	(0.025)	(0.025)	(0.015)	(0.015)	(0.013)
Observations	19,245	19,245	19,245	19,259	19,271
Dependent Mean			1.417		
Panel B: Chain Alcohol Retailers					
Treatment Start	0.009	-0.013	0	0	0
	(0.017)	(0.016)	(.)	(.)	(.)
Observations	13,920	13,920	13,920	13,920	13,920
Dependent Mean			1.386		
Panel C: Non-Chain Alcohol Retailers					
Treatment Start	-0.011	0.027	-0.060*	-0.059*	-0.119***
	(0.030)	(0.028)	(0.026)	(0.026)	(0.022)
Observations	5,325	5,325	5,325	5,339	5,351
Dependent Mean			1.498		
Covariates	Yes	Yes	Yes	Yes	Yes
R-Q FE	Yes	Yes	Yes	Yes	Yes

Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes*: R-Q FE represents retailer and quarter by year fixed effects. "Large" retailers are defined as any licensed spirit alcohol retailer with five or more locations within the state of Washington; "Small" fewer than five. The outcome variable is the log of spirit sales per capita (by zip code). All models use Borusyak et al. (2024) estimation method. Covariates are zip-level variables, including: percent age 20-24, percent male population, percent with at least a high school education, and percent married. The number of alcohol retailers is included as our time-control variable. Standard errors are clustered at the county level. Symbols denote significance at the 0.5% (***), 1% (**), 5% (*), and 10% (+) levels.

Appendix E: Robustness by Location and Intensity

Table E.1: Robustness by Location and Intensity

	Transaction cost treatment defined by distance (distances shown)					
	0.1 miles	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
	Coeff./SE	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*
Panel A: Urban						
Treatment Start	0.073***	0.077***	0.038***	0.025***	0.020*	0.017**
	(0.024)	(0.024)	(0.010)	(0.007)	(0.008)	(0.006)
Observations	15,708	15,708	15,708	15,708	15,686	15,686
Dependent Mean	1.302					
Panel B: Non-Urban						
Treatment Start	-0.059+	-0.057+	0.115	0.097+	0.087	0.063
	(0.031)	(0.030)	(0.084)	(0.053)	(0.061)	(0.057)
Observations	3,563	3,563	3,563	3,556	3,556	3,556
Dependent Mean	1.937					
Panel C: Border						
Treatment Start	0.245***	0.268***	0.358*	0.276+	0.188	0.175
	(0.009)	(0.014)	(0.165)	(0.146)	(0.120)	(0.108)
Observations	1,518	1,518	1,518	1,518	1,518	1,518
Dependent Mean	1.416					
Panel D: Non-Border						
Treatment Start	0.044*	0.046*	0.022***	0.015**	0.014*	0.008
	(0.019)	(0.020)	(0.007)	(0.006)	(0.006)	(0.006)
Observations	17,753	17,753	17,753	17,746	17,724	17,724
Dependent Mean	1.419					
Panel E: Dispensary Intensity						
Treatment Start	0.040***	0.041***	0.033*	0.023*	0.010	0.008
	(0.014)	(0.014)	(0.014)	(0.011)	(0.010)	(0.008)
Observations	19,271	19,271	19,271	19,271	19,271	19,271
Dependent Mean	1.504					
Covariates	No	Yes	Yes	Yes	Yes	Yes
R-Q FE	Yes	Yes	Yes	Yes	Yes	Yes

Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes*: R-Q FE represents retailer and quarter by year fixed effects. The outcome variable is the log of spirit sales per capita (by zip code). Panels A - D models use Borusyak et al. (2024) estimation method and Panel E uses Gardner (2022) estimation method. Dispensary intensity is defined as the natural log of marijuana dispensaries within the stated distance. Covariates are zip-level variables, including: percent age 20-24, percent male population, percent with at least a high school education, and percent married. The number of alcohol retailers is included as our time-control variable. Standard errors are clustered at the county level. Symbols denote significance at the 0.5% (***), 1% (**), 5% (*), and 10% (+) levels.