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, contributing to a growing literature in which the relationship between these two goods remains mixed. We also highlight the potential role transaction costs play in determining this relationship. Using disaggregated retailer spirit sales data from the state of Washington (2013-2017) and incorporating geographic proximity to marijuana retailers 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 retailer, with results as high as 10% for "large" retailers, and spirit sales decrease by 6-10% for those isolated from recreational marijuana retailers. 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 Overview/Goals

Since California's legalization of medical marijuana in 1996 and the first recreational legalizations in Colorado and Washington in 2012, 37 states and the District of Columbia have legalized marijuana for medical use (MML). Twenty-three of these states and the District of Columbia have also legalized marijuana for recreational use (RML). The recent wave of states legalizing marijuana for recreational use provides researchers new opportunities to examine how this policy shift affects 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. They may be substitutes if they serve similar purposes (e.g., relaxation, intoxication, coping mechanism, etc.), or they may function as complements if they are consumed together. Empirical findings mirror this 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 populations studied.

We contribute to this ongoing debate utilizing a new dataset and approach. We combine retailer spirit alcohol sales data across the state of Washington with the staggered rollout of recreational marijuana retailers to examine the relationship between alcohol and marijuana. Using a staggered treatment design with disaggregated retailer-level sales data, we incorporate geographic proximity between alcohol and marijuana retailers as a proxy for transaction costs. This approach reveals spatial heterogeneity: spirit alcohol sales increase by 2–6% for retailers located within 0.5 miles of a recreational marijuana retailer, while sales decrease by 6–10% for retailers that are geographically isolated from recreational marijuana retailers. In addition, we recognize the relationship between these goods may be further shaped by the retail context in which the goods are purchased. The

visibility or familiarity of "large" alcohol retailers co-located with marijuana retailers may amplify complementarity. Our findings confirm these expectations: spirit alcohol sales increase by 10% for "large" alcohol retailers within 0.5 miles of a recreational marijuana retailer.

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 staggered variation in marijuana market entry across space and time and improve upon conventional difference-in-differences designs that identify all retailers as simultaneously treated.

Second, we provide a theoretical rationale 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. Specifically, we measure spatial distances between marijuana and alcohol retailers, treating the potential 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 relationship between marijuana and alcohol is important for policy. There are implications for public health, tax revenues, and regulatory design. If they are substitutes, increased marijuana availability could reduce negative outcomes associated with alcohol use (e.g., traffic fatalities as evidenced by Anderson et al. 2013 and chronic health conditions). However, states may face offsetting declines in alcohol-related tax revenues (Miller and Seo 2021). If they are complements, however, co-use 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 local conditions and relationships between these two goods across space.

The remainder of the paper proceeds as follows. The next section summarizes the literature

examining the relationship between marijuana and alcohol. Section 3 presents our theoretical framework emphasizing the transactions costs, measured through co-location, and the moderating role of alcohol retailer size and familiarity. 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 section 7.

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. While some studies suggest complementarity - particularly among youth or in social contexts - others find substitution effects, especially when policy changes alter access costs. The divergence in empirical results may reflect several underlying factors, including: measurement - some studies measure price using taxes or consumer indices, others rely on enforcement proxies or decriminalization status; 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 empirical strategy. In short and as noted by Guttmannova et al. (2016), the relationship is complex. In general, the differences across findings underscore the need for continued empirical work that integrates modern data sources, methods, and policy changes to understand this relationship.

Instead of surveying the entire body of research, we focus our attention to a brief overview of the literature in Table 1. Here we highlight the different policies that have been examined with similar data and the mixed results regardless of the policy and population analyzed. Narrowing focus to RML, initial research appeared consistent in finding these goods as substitutes (Dragone et al. 2019; Alley et al. 2020; Miller and Seo 2021). Leveraging survey data of the entire US

[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.

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 appear less clear in recent research (Calvert and Erickson 2021; Zhu et al. 2025). Given this mixed evidence, there remains additional value in examining the relationship within this small set of literature. Some of the reasons to build upon this research include: Dragone et al. (2019) does not leverage retail sales, only opportunity after legalization and Alley et al. (2020) examines a specific population. Finally, 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. We update this literature through the inclusion of 1) a larger timeframe (2013-2017), 2) utilizing more granular data where we examine spirit sales by retailer, generating nearly 20,000 retailer-quarter observations, 3) we implement updated methods utilizing staggered treatment design, and 4) we include additional controls common in health studies looking at the effects of consumption (e.g., control for population).

2.2 Transaction Costs as an Explanation

Perhaps a more important addition to the literature is our hypothesis explaining why there are differing results, rather than a simple recognition that results differ. Our hypothesis: transaction costs matter. There is a strand of literature that recognizes the importance of transaction costs through legal risks, enforcement, access restrictions such as minimum legal drinking ages, campus bans, and enforcement intensity. These studies argue these changes affect the "full price" (monetary plus non-monetary costs) of substance use.[2] For instance, Pacula (1998) proxies marijuana's "price" using police-to-crime ratios (risk of arrest) and incorporates MLDA as a non-monetary cost 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 MLDAs or bans as shifts in the shadow price of alcohol. While these studies acknowledge the importance of non-monetary costs and include proxies in their empirical models, they rarely deploy direct transaction costs as a systematic method for explaining mixed findings.

DiNardo and Lemieux (2001) are the only authors to formally model transaction costs within

[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 retailers as a proxy for the opportunity cost of access, aligning with transaction cost considerations.

a utility framework.[3] We propose that alcohol and marijuana can be both complements and substitutes depending on the *state of transaction costs*, measured through market access. The mixed evidence on whether alcohol and marijuana are economic complements or substitutes can also be reasoned through differences in *transaction costs* (search, access, legal risk, and non-monetary frictions) that vary across space, time, and populations. Policies (MLDA, decriminalization), market structure (store proximity and opening waves), and measurement (prices vs. proxies) shift the *full price* of each good and hence their observed cross-price relationships. When a policy or market shock primarily raises *joint* frictions (e.g., on-campus alcohol bans, broad enforcement), both uses can fall (complements). When a shock *differentially* lowers frictions for one good (e.g., MLDA at 21, nearby outlet openings), the other often falls (substitutes). As we discuss in our estimation, our spatial-staggered design isolates these directly. Our model and time frame provides a natural explanation for why the post-legalization empirical literature finds mixed results.

Finally, we recognize that size or familiarity with a retailer may act as a moderator in the observed consumer behavior and resulting market frictions. The appendix from Miller and Seo (2021) assumes there is no change in consumer behavior concerning alcohol store selection as a result of the introduction of recreational marijuana. While this may be true at the aggregate - consumers who complement these goods may not change to a new store just for convenience - there may be changes related to stores from substitution; those farther from marijuana may see a greater reduction in sales since the search costs increase for alcohol increases due to travel. This is in line with the findings of Seo (2017) showing consumers change their behavior towards convenience (i.e., "one-stop shopping").[4]

While novel in the context of the marijuana-alcohol relationship, our idea is related to other findings in the literature. Consider the Alchian–Allen Effect where an additional fixed per-unit cost (such as a transportation fee) to both high- and low-quality goods alters their relative prices and leads consumers toward consumption of the higher-quality option (Alchian 1983. That is, the increased fixed cost reduces the relative price difference and makes the higher-quality good

[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.

[4] Although not our primary focus, we also add to Miller and Seo (2021) findings by examining all retailers selling spirits documented by the state of Washington rather than four major grocery store chains, two major discount store chains, and two drug store chains in the State. This is important to measure the full effect, as well as for our consideration of visibility acting as a moderator in our transaction cost framework.

more attractive. This shifts consumption patterns, influencing the perceived relationship between goods. While our 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) view market equilibrium when transaction costs are present. Their study extends the classical Fisher market model by introducing transaction costs, 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 influence how goods are consumed. Substitution or complementarity is not fixed, it is endogenous to the transaction cost structure. Observed behavior may mislead if one interprets the relationship without accounting for transaction costs. While our model focuses on how transaction costs affect consumption choices between two goods and empirically measures this using space as a key defining cost, this study examines the broader market implications: transaction costs can lead to varied consumption patterns across different buyers.

Finally, we recognize the work of Allen and Rehbeck (2022) highlighting latent complementarity that looks like substitution due to unequal frictions. This helps 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 multifaceted impact of transaction costs 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_B of goods A and B by maximizing their utility function that allows for interaction between goods A and B, identified as:

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

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:

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

From these, $\alpha, \beta > 0$ are preference weights that govern an individual's base utility of 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 $q_A q_B$, directly incorporated into the budget constraint as:

$$P_A q_A + P_B q_B + \tau q_A q_B \leq m \tag{3}$$

This implies that when $q_A = 0$ or $q_B = 0$, 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 $\tau q_A q_B$ 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:

$$\frac{\frac{\alpha}{q_A} + \gamma q_B}{\frac{\beta}{q_B} + \gamma q_A} = \frac{P_A + \tau q_B}{P_B + \tau q_A} \tag{4}$$

This illustrates how transaction costs τ 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; the equilibrium response is typically (i) a reduction in the interior levels of both goods and, if τ is large enough, (ii) a corner solution in which the consumer purchases only one good. This mechanism can make utility-based complements appear to behave like substitutes in empirical estimations. Thus, low transaction costs tend to reveal complementarity in observed purchases, whereas high transaction costs can suppress the marginal benefit of joint consumption and produce substitute-like behavior. Table 2 provides a summary of our expectations. The introduction of transaction costs imply there may be unexplored relationships in the literature examining the survey responses or aggregate purchase behavior of consumers.[5]

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 B 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 B. 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 B, 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) as one of the locations, the visibility and accessibility of joint consumption lowers the effective τ. As a result, the consumer is more likely to purchase both

[5] 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.

together, allowing the underlying complementarity ($\gamma > 0$) to manifest in actual consumption.

4 Data

4.1 Washington as an area of study

The State of Washington has recently been a leader in pro-marijuana policies. Washington was one of 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, including marijuana infused products. Finally, to prevent driving under the influence of marijuana, DUI standards with maximum thresholds for THC 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 retail sales, 18 locations across the State of Washington opened totaling over $1.8 million in sales and generating over $460,000 in tax revenue. By the end of 2016, there were over 330 active retail locations across the state generating nearly $700 million in yearly sales and over $250 million in yearly tax revenue. Figure 1 presents the total sales and the number of retailers by quarter for Washington state.

Figure 2 illustrates both cross-county and within-county variation over time by showing the average number of retailers per county each year. Nearly every county experienced growth in the number of retailers. The largest increase occurred in King County, home to Seattle, where the

average number of retailers rose from 5.3 in 2014 to 63 in 2016 (starting 2016 with 41 retailers and ending with 75). Some counties never received any retailers (Columbia, Garfield, Lincoln, and Wahkiakum), while Klickitat and San Juan counties remained stagnant between 2015 and 2016. Franklin County was the only jurisdiction to see a decline, dropping from one retailer in 2015 to zero in 2016.

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 retailer using a reverse address to geospatial lookup.[6] 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.[7] These include products such as whiskey, gin, brandy, rum, tequila, and vodka. The Washington Department of Revenue, which administers the state's spirits tax, provides an identical definition. 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 are aggregated at the retailer level for each quarter from 2013 to 2017.

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, percent population male, percent population with high school education or less, and percent population married.

[6] More information on this data can be found here: https://lcb.wa.gov/records/frequently-requested-lists

[7] One exception is wines exceeding twenty-four percent of alcohol by volume. These are also labeled as "spirits."

5 Econometric Model

Beyond a theoretical construct our goal is to empirically estimate the relationship between alcohol and marijuana, and to examine the role of transaction costs and their effect on consumer choice. 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 retailers in Washington and match them to retailer-level alcohol sales. Spatial proximity to marijuana outlets 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 demand.

We first calculate the distance from an alcohol retailer to the nearest marijuana retailer (d_i^{AM}) which we use to identify transaction costs. Given the staggered implementation of marijuana retailers 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 retailer.

Denoting Y_{izt} 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:

$$Y_{izdt} = Y_{izdt}(0) + RML_{dt} \cdot \beta_{dt} \tag{5}$$

where $Y_{izdt}(0)$ is the untreated potential outcome, RML_{dt} 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 retailer (i.e., treated $\leq (d_i^{AM})$), and β_{dt} is the cohort- and time-specific causal effect. BJS estimates β_{dt} by first modeling the untreated outcome as:

$$Y_{izdt}(0) = \delta_z + \mu_t + X_{izt}\gamma + \epsilon_{izt} \tag{6}$$

where X_{izt} is a vector of time-varying controls, δ_z and μ_t are zip code and time fixed effects, and ϵ_{izt} is an idiosyncratic error clustered at the zip code level. The key identifying assumption is:

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

BJS uses only not-yet-treated observations to estimate the parameters in (6), then impute $Y_{izdt}(0)$ for treated units in post-treatment periods. Treatment effects are the differences between observed outcomes and these imputed counterfactuals, and are aggregated to produce single coefficient representing the average treatment effect on the treated (ATT).[8]

6 Results

6.1 Summary Statistics

Average quarterly alcohol sales were \$103k (\$5.27 per capita), with an increase in sales to \$7.84 for treated 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 retailer was 2.33 miles for all alcohol retailers and that distance contracted to 1.73 miles for large retailers. Approximately 65% of our sample is comprised of large 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.[9] Please refer to Table 3 for further information.

6.2 Main Results

6.2.1 Close Proximity

We first examine the impact of marijuana retailers 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.

We present the results in Table 4. In columns (1) and (2) estimates indicate that the presence of marijuana retailers 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

[8] We link the BJS approach to the standard Difference-in-Differences framework and the biased results from the simple TWFE models in Appendix C .

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

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 examine the event study for alcohol retailers in Figure 3 for those in close proximity to marijuana retailers. In Panels (a) - (e) we display the one-tenth to half-mile models with controls. We note that alcohol spending has some seasonal variation but it does not influence our pretrends. We analyze a six-quarter pre- and eight-quarter post-treatment window around the staggered timing of marijuana retailer presence. It is clear in the closer models of 0.10 to 0.20 mile, there is a strong, statistically significant upward trend after three quarters of treatment.

Since the nature of the newly emerged marijuana retail market left some alcohol retailers switching in and out of treatment due to marijuana retailer closures and relocations, we test the robustness of these estimates by assuming once an alcohol retailer is treated, it stays treated. Estimates are reported in Appendix D.1. We see similar, slightly attenuated estimates of 5.7% at closer ranges that are diminished but positive as we expand the distance threshold.

We also stratify our main results in Table 4 by large- and small-sized alcohol retailers (Panels (b) and (c)).[10] We report that estimates for large retailers grew in magnitude with a 10.5% increase at the closest threshold. The positive effect on alcohol sales diminishes and with similar 2.2% results at a half-mile. We can see that these estimates are offset by weaker and smaller negative estimates of 3% from small alcohol retailers in Panel (b).

6.2.2 Retailer Isolation

Since we notice an overall positive association with marijuana retailer 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 retailers (i.e., large distances) should experience a negative sales effect.[11] We explore this argument in Table 5 and report our estimates examining isolation rings of 12.5+ to 22.5+ miles. We find a statistically significant negative relationship of negative 6.4% to 9.8% for all 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 large retailers at greater distances. Looking solely at small alcohol retailer estimates in Panel (c),

[10] Alcohol retailer size was determined and identified through name filtering of large national and local chain stores, such as Walmart, Target, and CVS.

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

we find a 6.6% 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 E.1), we find similar results to these main estimates.

The event studies for small retailers in the 20+ and 22.5+ mile isolation rings are presented in Figure 4.[12] Although the estimates are noisier than our close proximity figures, with more noticeable but insignificant pretrends, we can see a decrease in per capita alcohol sales occurring roughly one year post-treatment.

7 Conclusion

Consistent with the literature, we find alcohol and cannabis 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 retailer, with results as high as 10% for "large" retailers, and spirit sales decrease by 6-10% for those isolated from recreational marijuana retailers. We find that consumers view alcohol spirits and marijuana as complementary goods when the transaction costs are quite low and display qualities of substitutes when faced with significant transaction costs.

As noted earlier, understanding the relationship between marijuana and alcohol has important policy implications for public health, tax revenues, and regulatory design. Furthermore, 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. 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 size

[12] Small retailers are reported given the sparse occurrence of large retailers being isolated from marijuana retailers at great distances.

as a moderator may be considered one type (i.e., consumers' awareness of product availability), additional frictions such as health risks, and the awareness of these risks, could also be examined. 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 cannabis 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–cannabis relationship.

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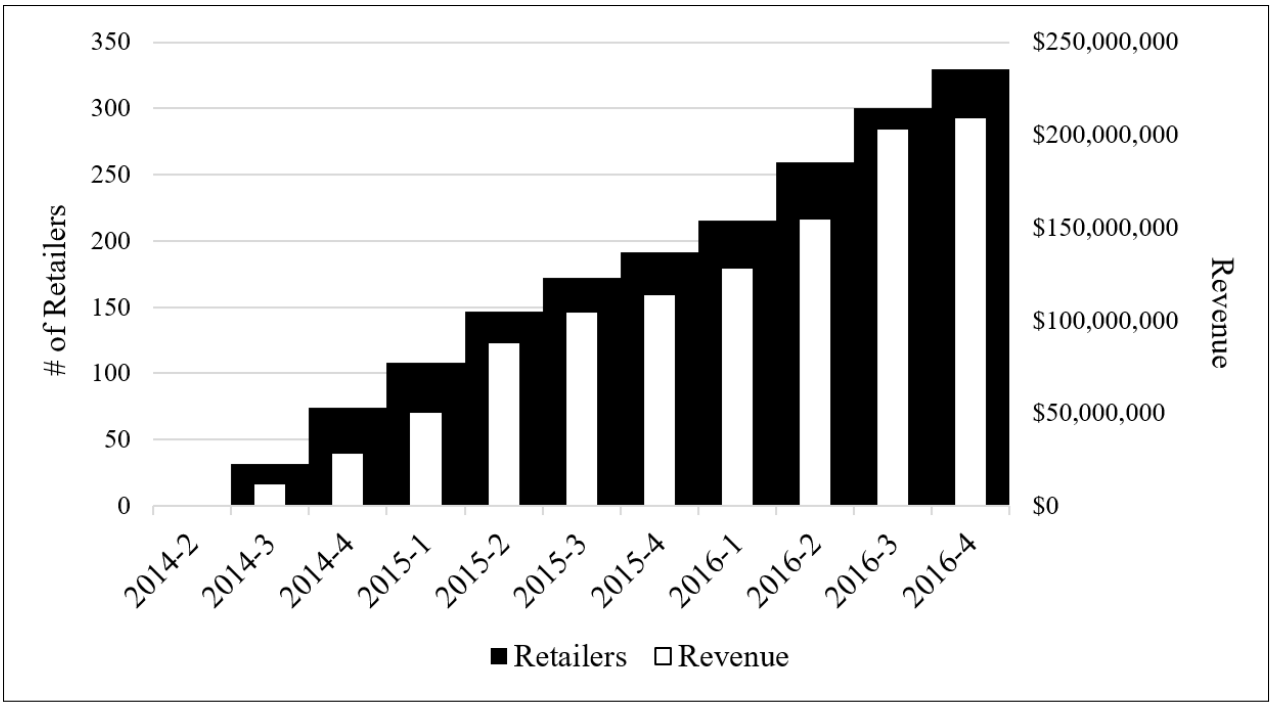


Figure 1: Total Sales and Number of Retailers in Washington, by quarter

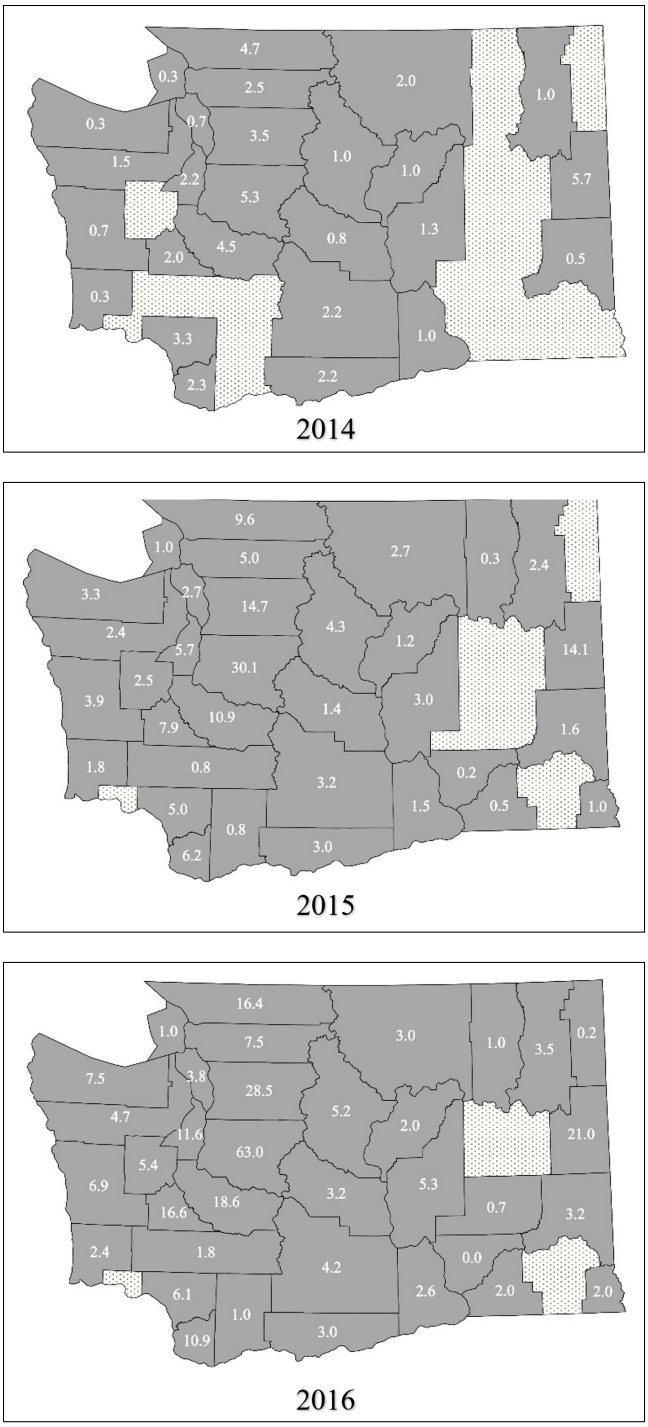
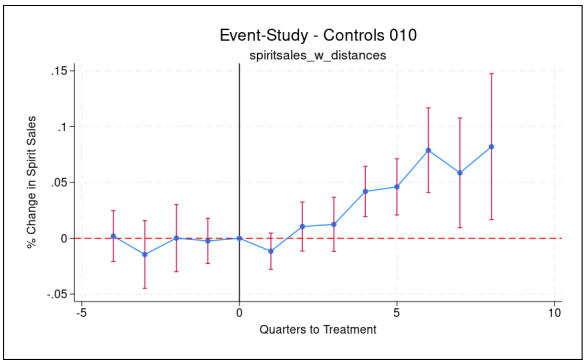
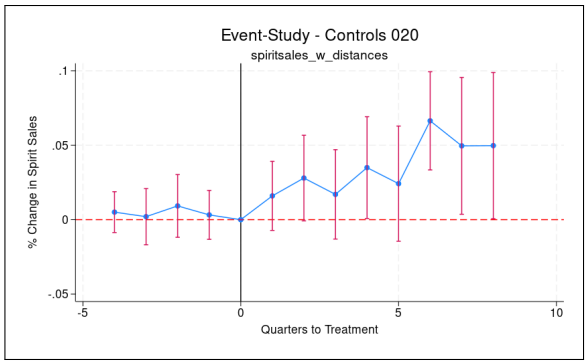


Figure 2: Average Number of Retailers, by county-year

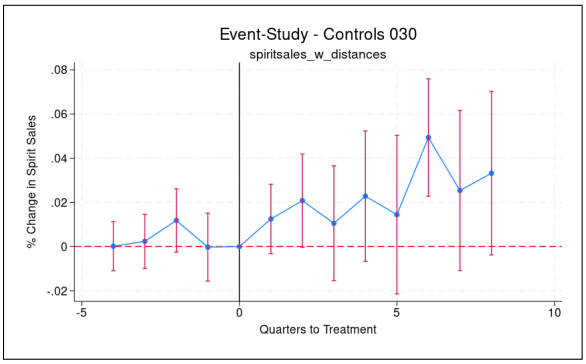
Note: Averages are calculated using the monthly number of retailers in each county, with 2014 only including the second half of the year since marijuana retailers were not licensed to sell prior to this period.



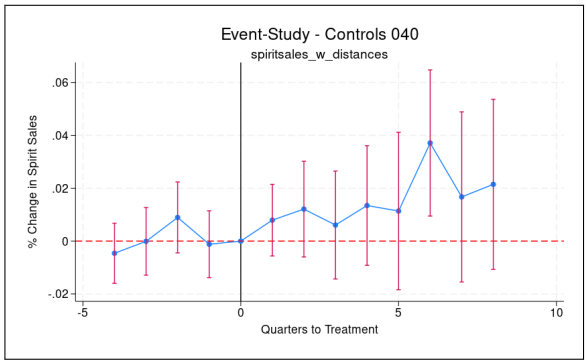
(a) "Treatment" $\leq$ 0.1-miles



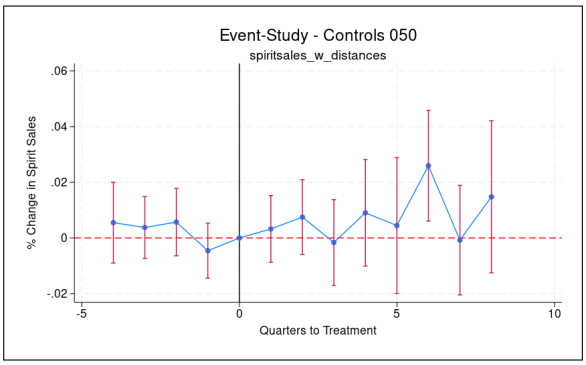
(b) "Treatment" $\leq$ 0.2-miles



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



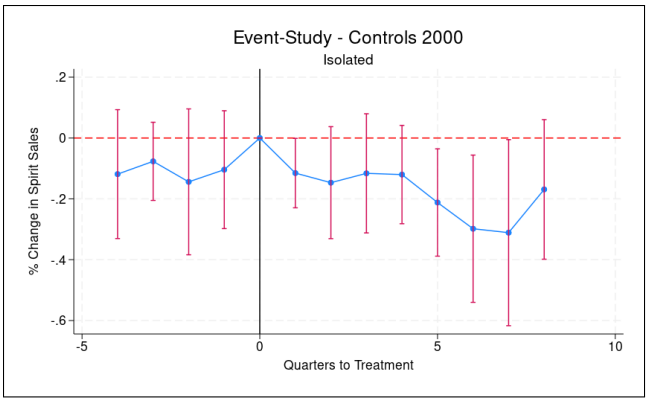
(d) "Treatment" $\leq$ 0.4-miles



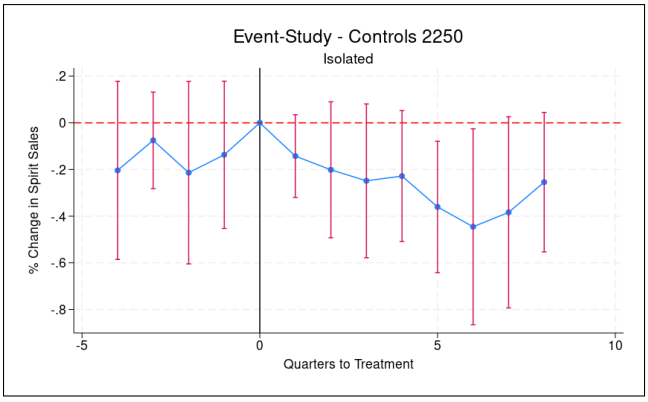
(e) "Treatment" $\leq$ 0.5-miles

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 identified in our paper, as well as fixed effects for quarter and zip code. Standard errors are clustered at the zip code 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 Small, 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 identified in our paper, as well as fixed effects for quarter and zip code. Standard errors are clustered at the zip code level. 90% confidence intervals reported as band around point estimates.

Table 1: Abbreviated Literature Review

Study	**Policy**	**Data**	**Population**	**Conclusion**
Thies & 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, Hansen, & Rees (2013)	MML	FARS (1990-2010); BRFSS (1993-2010); Beer Institute's Brewers Almanac (1990-2010)	U.S. population	Substitutes
Sabia, Swigert, & Young (2017)	MML	BRFSS (1990-2012)	U.S. population	Substitutes
Andreyeva & Ukert (2019)	MML	BRFSS (1993-2013)	U.S. population	Mixed
Miller & Seo (2021)	RML	Nielsen retail scanner data (2013-2016)	Counties in Washington	Substitutes
Calvert & 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	
Panel A: All Retailers	*Obs.*	*Mean / S.E.*	*Obs.*	*Mean / S.E.*	*Obs.*	*Mean / S.E.*	*Obs.*	*Mean / S.E.*	*Obs.*	*Mean / S.E.*
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 Cannabis 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: Large Retailers										
Spirit Sales	13,763	111,576	4,051	102,714	9,712	115,273	12,061	112,032	1,702	108350
		(106,457)		(99,259)		(109,112)		(104,625)		(118,619)
Per Capita Sales	13,763	4.57	4,051	4.41	9,712	4.64	12,061	4.42	1,702	5.60
		(9.387)		(14.330)		(6.262)		(9.307)		(9.876)
Nearest Cannabis Distance	13,743	1.73	–	–	9,698	1.74	12,041	1.96	1,702	0.10
		(2.411)		–		(2.414)		(2.491)		(0.066)
Number of Close Spirit Retailers	13,763	15.20	4,051	15.46	9,712	15.10	12,061	15.07	1,702	16.15
		(6.759)		(6.535)		(6.848)		(6.800)		(6.384)
Panel C: Small Retailers										
Spirit Sales	5,508	80,063	1,547	77,883	3,961	80,914	4,783	77,643	725	96,026
		(91,797)		(95,498)		(90,309)		(88,265)		(111,109)
Per Capita Sales	5,508	7.01	1,547	6.80	3,961	7.10	4,783	6.09	725	13.07
		(18.476)		(17.706)		(18.770)		(8.846)		(45.138)
Nearest Cannabis Distance	5,448	3.83	–	–	3,919	3.86	4,723	4.40	725	0.10
		(5.707)		–		(5.746)		(5.925)		(0.065)
Number of Close Spirit Retailers	5,508	10.70	1,547	11.17	3,961	10.52	4,783	10.21	725	13.93
		(8.922)		(8.819)		(8.956)		(8.877)		(8.540)

Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes:* Data is aggregated to the alcohol retailer by quarter level. For simplicity, treated alcohol retailers are within 0.20 miles of a marijuana retailer and a single pre/post date of July, 2014. Close alcohol retailers are determined if a neighboring alcohol retailer is within 5.0 miles of target alcohol retailer.

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 Spirit 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: "Large" Spirit Retailers						
Treatment Start	0.104***	0.106***	0.061***	0.042***	0.029*	0.022**
	(0.034)	(0.035)	(0.019)	(0.011)	(0.012)	(0.008)
Observations	13,763	13,763	13,763	13,763	13,750	13,750
Dependent Mean			1.356			
Panel C: "Small" Spirit Retailers						
Treatment Start	-0.029+	-0.031+	0.008	0.003	0.013	0.006
	(0.018)	(0.018)	(0.052)	(0.043)	(0.035)	(0.030)
Observations	5,508	5,508	5,508	5,501	5,492	5,492
Dependent Mean			1.578			
Covariates	No			Yes		
Z-Q FE	Yes			Yes		

Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes*: Z-Q FE represents zip code-by-quarter fixed effects. The outcome variable is 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 zip code level. Symbols denote significance at the 0.5% (***), 1% (**), 5% (*), and 10% (+) levels.

Table 5: Isolated Retailer 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 Spirit Retailers					
Treatment Start	0.075	0.169	0.185	-0.064***	-0.098***
	(0.099)	(0.137)	(0.172)	(0.019)	(0.012)
Observations	19,649	19,649	19,649	19,649	19,663
Dependent Mean			1.432		
Panel B: "Large" Spirit Retailers					
Treatment Start	0.007	0	0	0	0
(0.026)	(.)	(.)	(.)	(.)	
Observations	13,883	13,883	13,883	13,883	13,883
Dependent Mean			1.364		
Panel C: "Small" Spirit Retailers					
Treatment Start	0.092	0.198	0.223	-0.066***	-0.120***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Observations	5,766	5,766	5,766	5,766	5,780
Dependent Mean			1.595		
Covariates			Yes		
Z-Q FE			Yes		

Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes*: Z-Q FE represents zip code-by-quarter fixed effects. The outcome variable is 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 zip code 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, Lohrmann, and Greene (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 & Lemieux (2001)	MLDA	Monitoring the Future (1980–1989)	U.S. high school seniors	**Substitutes**: raising MLDAs reduce alcohol use and increase marijuana use.
Yoruk & Yoruk (2011)	MLDA	NLSY (1997)	Young adults (19-22 years old)	**Complements**: marijuana use rises when alcohol consumption rises at 21
Crost & 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, Brady, & Li (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 & 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

Appendix A continued on next page

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 & 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.

Appendix A continued on next page

Study	MJ/Alcohol Policy Leveraged	Data	Population	Conclusion
Sabia, Swigert, & Young (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 & 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, Chong, & Kwon (2020)	MML	Nielsen retail scanner data (2006-2015)	U.S. states	**Substitutes**: MMLs are associated with a reduction in alcohol sales.
Conyers & 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.
Velegati 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:

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

and

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

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 of 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 the budget constraint to include a potential joint transaction cost:

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

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:

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

where

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

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_J explicitly because these were not measurable (implicit to the individual). By contrast, the joint transaction-cost parameter τ_J captures frictions that arise when both goods are legally available and acquired/consumed together, such as additional search time. When $\tau_J > 0$, 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:

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

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.

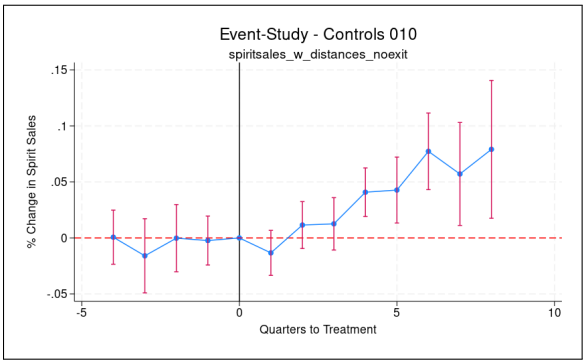
BJS addresses this by, in effect, running (A.1) within the "not-yet-treated" subsample at each period to build a clean counterfactual, then imputing what treated units would have experienced had they never been treated. This preserves the DiD spirit—comparing treated and untreated outcomes—while avoiding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

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

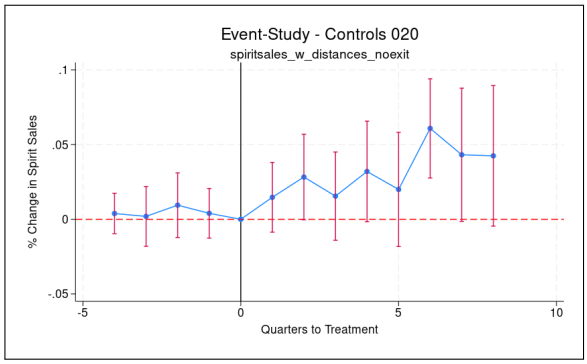
$$Y_{ist} = \alpha + RML_{st} \cdot \delta + X_{ist}\beta + \delta_s + \mu_t + \epsilon_{ist}, \tag{A.2}$$

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.

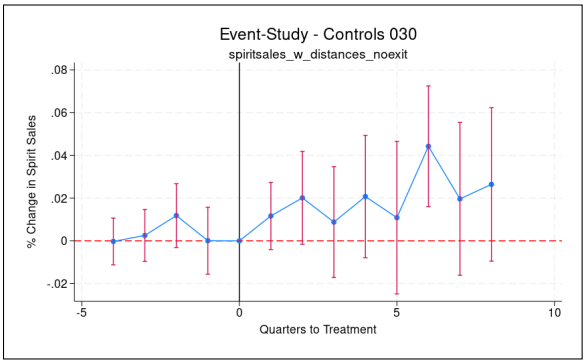
Appendix D: No Exit Results and Event Study for Close Proximity



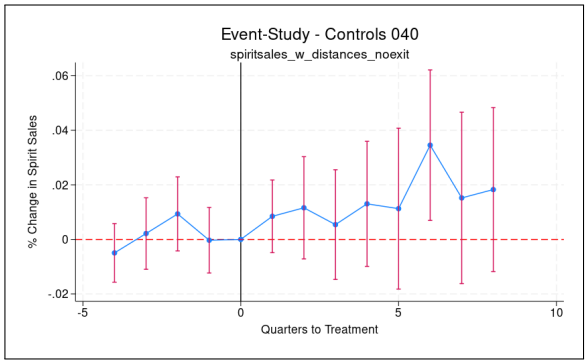
(D.a) "Treatment" $\leq$ 0.1-miles



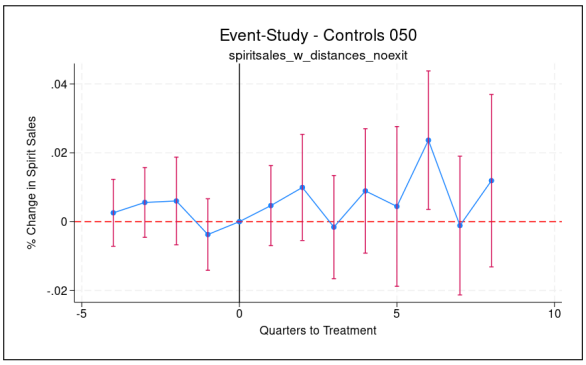
(D.b) "Treatment" $\leq$ 0.2-miles



(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

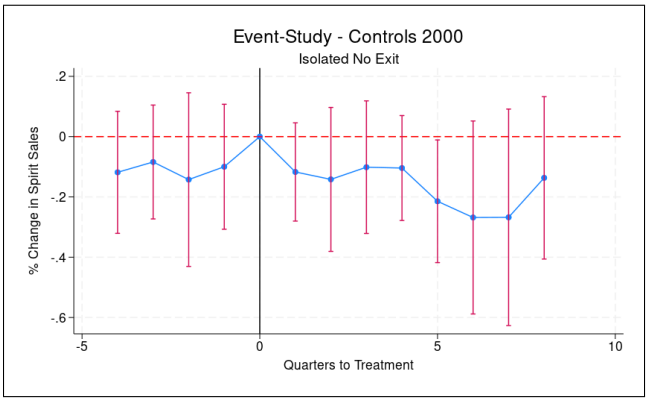
Note: Each point in the figure represents the difference between "treated" alcohol retailers relative to "untreated" retailer, before and after treatment. Each specification includes the covariates identified in our paper, as well as fixed effects for quarter and zip code. Standard errors are clustered at the zip code level. 90% confidence intervals reported as band around point estimates.

Table D.1: Main Results, No Exit

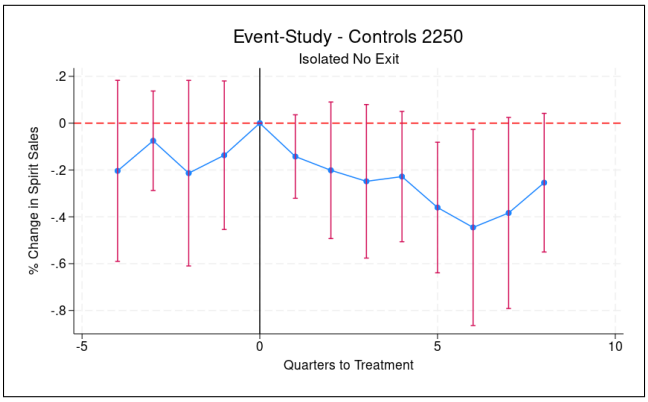
	Transaction cost treatment defined by distance (distances shown)					
	0.1 miles	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
	Coeff./SE	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*
Panel A: All Spirit Retailers						
Treatment Start	0.057**	0.057**	0.043*	0.030*	0.026*	0.020*
	(0.021)	(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: "Large" Spirit Retailers						
Treatment Start	0.100***	0.102***	0.061***	0.040***	0.029*	0.021**
	(0.032)	(0.032)	(0.019)	(0.012)	(0.012)	(0.008)
Observations	13,763	13,763	13,763	13,763	13,750	13,750
Dependent Mean			1.356			
Panel C: "Small" Spirit Retailers						
Treatment Start	-0.029+	-0.031+	-0.002	-0.004	0.011	0.007
	(0.018)	(0.018)	(0.050)	(0.042)	(0.033)	(0.029)
Observations	5,508	5,508	5,508	5,501	5,492	5,492
Dependent Mean			1.578			
Covariates	No			Yes		
Z-Q FE	Yes			Yes		

Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes*: Z-Q FE represents zip code-by-quarter fixed effects. The outcome variable is 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 zip code level. Symbols denote significance at the 0.5% (***), 1% (**), 5% (*), and 10% (+) levels.

Appendix E: No Exit Results and Event Study for Isolated Retailers



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



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

Figure E.1: Event Study for Small, 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 identified in our paper, as well as fixed effects for quarter and zip code. Standard errors are clustered at the zip code level. 90% confidence intervals reported as band around point estimates.

Table E.1: 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	0.093	0.168	0.184	-0.046***	-0.098***
	(0.098)	(0.142)	(0.202)	(0.015)	(0.012)
Observations	19,649	19,649	19,649	19,649	19,663
Dependent Mean			1.432		
Panel B: "Small" Spirit Retailers					
Treatment Start	0.114	0.197	0.225	-0.051*	-0.120***
	(0.115)	(0.152)	(0.214)	(0.025)	(0.021)
Observations	5,766	5,766	5,766	5,766	5,780
Dependent Mean			1.595		
Covariates			Yes		
Z-Q FE			Yes		

Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes*: Z-Q FE represents zip code-by-quarter fixed effects. The outcome variable is 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 zip code level. Symbols denote significance at the 0.5% (***), 1% (**), 5% (*), and 10% (+) levels.