Joint Consumption? Transaction Costs as a Determinant in the Relationship between Marijuana and Alcohol

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Abstract

Economic models often assume goods are either complements or substitutes. We challenge this by incorporating transaction costs into a theoretical model and testing its implications. Our framework predicts that utility-based complements may appear as substitutes when frictions are high. Using Washington State spirit sales and proximity to marijuana dispensaries as a proxy for transaction costs in a staggered difference-in-differences design, results reveal heterogeneity: sales increase 2–6% near dispensaries and decrease 7–10% when in isolation. These findings support our theoretical framework and highlight the importance of spatial access in identifying complementarity or substitution.

Keywords: Alcohol, Marijuana Legalization, Marijuana-Alcohol Relationship, Transaction Costs, Related Goods
JEL Classification Numbers: I12, I18, K32

Acknowledgment: The authors report no financial research support tied specifically to this research project and report no conflicts of interest.

1 Introduction

Complementarity and substitutability are foundational concepts in economics. Traditional models often treat this relationship as fixed, classifying goods as either substitutes or complements. This oversimplifies consumer behavior and overlooks critical sources of heterogeneity; preferences and cultural trends shift, social norms or tastes can redefine usage, cross-price elasticities can change over time as relative prices or income constraints change, and purchase or consumption conditions differ among consumers. Market frictions can take many forms, though our work focuses on transaction costs faced by consumers.

Economists have increasingly recognized that relationships between goods may be context dependent rather than fixed. For example, Chakraborty, Devanur and Karande (2010) allow the same good to have different effective prices for different buyers. This can lead to price dispersion, affect market equilibrium, and lead to varied consumption patterns across different buyers; substitution or complementarity may depend on the effective price faced by a consumer. Observed behaviors may be misleading if one interprets the relationship without accounting for the different prices faced by consumers. Similarly, Gentzkow (2007) demonstrates that observed co-consumption can reflect correlated unobserved tastes or interactions with other goods in the choice set rather than true structural complementarity. Failing to account for unobserved forces can lead to misleading inferences about the relationship between goods. Together, these examples highlight that empirical patterns may be sensitive to frictions, heterogeneity, and broader market environments.

Beyond modeling, the identification or consistency in empirical findings determining the relationship between goods is often unclear or does not match expectation.[1] Formal explanations for research with mixed findings concerning the relationship between goods are often ignored. Rather than provide a unifying framework, empiricists approach the question with new data or an updated empirical approach to "finalize" the question at hand.

A prime example where there are mixed findings with continuously updated results is the relationship between alcohol and marijuana. The recent wave of marijuana legalization for recreational use has reignited this debate. With the non-uniform adoption of recreational marijuana policies

[1] Some empirical examples where the nature of goods may not match expectation include the complementary nature of cigarettes and e-cigarettes examined by Cotti, Nesson and Tefft (2018), or phone calls and text messages observed by Grzybowski and Pereira (2008).

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 joint consumption as further coping or escapism.

Empirical findings examining the relationship between alcohol and marijuana mirror the theoretical ambiguity. Focusing on recreational marijuana laws (RML) in the US, early studies concluded the two goods were substitutes as alcohol use declined after legalization (Dragone et al. 2019; Alley, Kerr and Bae 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, Trangenstein and Kerr 2025). For example, Zhu, Trangenstein and Kerr (2025) conclude that *recreational* marijuana users engage in complementary use patterns, while *medical* marijuana users exhibit substitution use patterns. Overall, the inconsistencies are often identified as differences in data sources, methods, policy environments, and the populations studied. However, the recent wave of marijuana legalization for recreational use also provides a new opportunity to examine how frictions influence observed consumer relationships with legal and available goods.

Our contribution to the literature is twofold. First, we formalize a mechanism that has been acknowledged but rarely modeled explicitly: the role of transaction costs in shaping the relationship between goods. Prior studies note that frictions may affect consumption patterns, but these are treated as implicit or secondary costs. We extend this by incorporating transaction costs directly into the budget constraint, offering an explanation for why the relationship between goods may not be fixed but may shift according to frictions faced by consumers.

Second, we apply our framework to the marijuana-alcohol relationship in a legalized retail setting. We combine retailer-level spirit sales data across the state of Washington with the staggered rollout of marijuana dispensaries to examine the relationship between alcohol and legalized marijuana. 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 dispensary market entry across space and time to improve upon conventional difference-in-differences design that identify all retailers as simultaneously treated. Finally, we incorporate our theoretical rationale into our empirical model by measuring spatial distances between marijuana dispensaries and alcohol retailers, treating accessibility between the two goods as a consumer friction that affects the "full price" of use.

Our results reveal spatial heterogeneity: spirit sales increase by 2–6% for retailers located within 0.5 miles of a marijuana dispensary, while sales decrease by 7–10% for retailers that are geographically isolated from 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 sales increase by 10% for "chain" alcohol retailers close to a marijuana dispensary, with effects primarily driven by supermarkets and pharmacies. Together, our findings add novel, disaggregated evidence of the relationship, highlighting spatial access as a transaction cost proxy systematically influences observed patterns of co-use.

Our empirical contribution demonstrates practical relevance for policy and market design. 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, Hansen and Rees 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-location and 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 the relationship between alcohol and marijuana across space.

The remainder of the paper proceeds as follows. The next section presents our theoretical framework emphasizing the role of transaction costs, which guides our empirical measurement of costs related to co-location and familiarity with alcohol retailers in shaping the relationship. Section 3 summarizes the literature examining the relationship between marijuana and alcohol,

and highlights our contribution to this literature. Sections 4 and 5 describe our data sources and empirical approaches to examine the role of transaction costs in determining the relationship, 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 Theoretical Model

We begin by examining the conditions necessary to define two goods as either complements or substitutes based on their relationship with 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:

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

This augmented log-linear utility function satisfies standard economic properties: it is strictly monotonic and exhibits diminishing marginal utility ($0 < \alpha < 1$ and $0 < \beta < 1$). 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 and cross-partial derivative of the utility function:

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

From these, $\alpha > 0$ and $\beta > 0$ are preference weights that determine an individual's base utility from each good, and γ captures the relationship between these two goods: $\gamma > 0$ implies the goods are utility-based *complements*, $\gamma < 0$ implies utility-based *substitutes*, and the goods offer no joint utility when $\gamma = 0$. This is in line with the standard *utility*-driven approach for examining the relationship between alcohol and marijuana.[2]

To understand how the frictions faced by consumers can change the relationship between two

[2]While we use an augmented log-linear specification for tractability and expositional clarity, the mechanism extends to any utility function with nonzero cross-partial derivatives. We connect our addition with the quadratic sub-utility approach by DiNardo and Lemieux (2001) in Appendix A, but a Constant Elasticity of Substitution utility function yields identical qualitative results.

goods, we incorporate a transaction cost (τ) directly into the purchase decision to show the key result does not depend solely on the elasticity of substitution, but rather on the interaction between intrinsic complementarity (γ) and transaction costs (τ). Intuitively, when transaction costs are sufficiently large relative to γ, the marginal cost of joint consumption may dominate the utility gain, leading the consumer to optimally consume a single good. To maintain differentiability, we model this as a smooth cost proportional to the consumption of q_A and q_M, directly incorporated into the budget constraint as:

$$P_A q_A + P_M q_M + T(q_A, q_M; d) \leq m \tag{3}$$

where $T(q_A, q_M; d)$ represents the additional costs associated with *joint* purchase. We explicitly include d to capture one relevant source of friction: the spatial separation between the retail locations of the two goods. T can be defined in a general form as:

$$T(q_A, q_M; d) = \kappa(d) q_A q_M, \quad \kappa(d) > 0,\ \kappa'(d) > 0 \tag{4}$$

The function $\kappa(d)$ captures the intensity of transaction costs associated with joint consumption and increases with distance. Intuitively, $\kappa(d)$ represents logistical frictions such as additional travel time, search effort, or reduced retail visibility when alcohol and marijuana retailers are farther apart. In addition, the marginal transaction costs are defined as:

$$\tau_M(q_A, q_M; d) \equiv \frac{\partial T(q_A, q_M; d)}{\partial q_A} = \kappa(d) q_M \tag{5}$$

$$\tau_A(q_A, q_M; d) \equiv \frac{\partial T(q_A, q_M; d)}{\partial q_M} = \kappa(d) q_A \tag{6}$$

These marginal transaction costs apply only when both goods are purchased together within a shopping trip and increase the effective full price of joint consumption, with no additional transaction cost to the sole purchase of either good when $q_A = 0$ or $q_M = 0$. However, as distance increases, $\kappa(d)$ rises, increasing the marginal cost of joint consumption. This can reduce interior consumption or induce corner solutions even when the goods are intrinsically complementary. With these considerations, first-order conditions are reduced to:

$$\frac{\frac{\alpha}{q_A} + \gamma q_M}{\frac{\beta}{q_M} + \gamma q_A} = \frac{P_A + \tau_M}{P_M + \tau_A} \tag{7}$$

Equation (7) illustrates how a positive marginal transaction cost ($\tau > 0$) distorts the effective marginal rate of substitution between goods; τ moderates the observed complementarity or substitutability of goods in empirical estimation. If $\gamma > 0$ (utility-based complements), an increase in τ raises the effective marginal cost of joint consumption.[3] That is, an increase in the transaction costs when the goods are utility-based complements either (i) reduce the interior levels of both goods or (ii) lead to a corner solution in which the consumer purchases only one good. The latter case is expected for sufficiently large τ and could lead researchers to observe these utility-based complements as substitutes when empirically measuring their relationship. When $\gamma = 0$, joint consumption does not provide an intrinsic utility gain. In this case, even modest transaction costs may induce corner solutions in which only one good is purchased. Thus, goods that are utility-independent may appear behaviorally substitutable in the presence of transaction costs faced by consumers.

2.1 Mapping to Empirical Identification

We can summarize the relationship for individuals by representing alcohol demand as a function of the price of goods, the intrinsic complementarity (γ) of these goods, as well as the transaction costs (including the distance between consumers and alcohol-marijuana retail pairs) associated with purchasing both goods as:

$$q_A = D_A(P_A, P_M, m, \tau_M; \gamma). \tag{8}$$

Up to now, we have characterized *individual* choices. Our data, however, are at the *retailer-quarter* level, aggregating the demand of consumers into a retailer's local market. The retailer outcome we observe is the aggregation of local consumers:

$$Y_{it} \equiv \sum_{z \in \mathcal{L}_i} \mathbb{E}[q_A(z,t)] \approx \sum_{z \in \mathcal{L}_i} D_A\big(P_A, P_M, m, \tau_M(z,t); \gamma\big). \tag{9}$$

[3]The finding that an increase in τ raises the effective marginal cost of joint consumption also holds with $\tau_A = \tau_M$, as long as $P_A \neq P_M$. This is important since our measure of transaction cost implies equality between τ_A and τ_M.

where Y_{it} is the aggregate of individual demand for retailer i and time t, with $\mathcal{L}_i$ representing the local market served by retailer i. We treat (P_A, P_M, m) as locally constant as they are absorbed by retailer and time fixed effects, while the heterogeneity in transaction costs $\tau_M(z,t)$ varies with spatial access to dispensaries. A dispensary opening at time t changes $\tau_M(z,t)$ in $\mathcal{L}_i$ for consumers as the distance falls, represented as:

$$\Delta Y_{it} \approx \sum_{z \in \mathcal{L}_i} \frac{\partial D_A}{\partial \tau_M}\Big|_{(z,t)} \cdot \Delta \tau_M(z,t) \tag{10}$$

Our interest is in the average demand response to τ, not an estimate of the structural parameter γ. When goods are utility-based complements ($\gamma > 0$) and $\partial D_A / \partial \tau_M \leq 0$, lower frictions raise alcohol demand for affected consumers and sufficiently high transaction costs can suppress joint purchase. That is, high friction can cause intrinsically complementary goods to appear behaviorally substitutable in aggregate measures. Table 1 highlights our empirical expectations corresponding to the prevailing transaction cost regime, where β captures the effect of reductions in transaction costs induced by proximity of the dispensary.

2.2 Retail Visibility/Familiarity as a Moderator

The effective substitutability or complementarity of goods is not only shaped by utility interactions (γ) and marginal transaction costs (τ), they may also be shaped by the visibility and retail context in which the goods are purchased. 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. Consider the case where goods A and M are utility complements ($\gamma > 0$), but good A is only readily available at a small "mom and pop" store at a time when the consumer is most interested in purchasing good M. 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 and visible retailer, 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 estimation.

3 Empirical Application: A Mixed Literature

The debate over 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 B.[4] 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 relationships using taxes or consumer indices, while others rely on enforcement proxies or decriminalization status to estimate consumer responses; 2) population - results often differ between adolescents, college students, and adults, suggesting age- or context-specific patterns; 3) policy context - studies exploit different sources of variation (e.g., beer taxes, minimum legal drinking ages, campus bans, decriminalization); and 4) empirical strategy. In short, as noted by Guttmannova et al. (2016), the relationship is complex. Differences across findings underscore the need for continued empirical work that integrates modern data sources, methods, and policy changes to understand this relationship.

Focusing on an abbreviated overview of the literature, Table 2 highlights three different policies with mixed outcomes despite the use of similar data. For example, Yamada, Kendix and Yamada (1996) and Pacula (1998) both use National Longitudinal Survey (NLSY) data to examine U.S. youth behavior through decriminalization. Yamada, Kendix and Yamada (1996) conclude alcohol and marijuana are substitutes while Pacula (1998) concludes they are complements. Focusing on RML, Miller and Seo (2021) use Nielsen retail scanner data in Washington and find legalization is associated with a reduction in alcohol use and demand. This is consistent with other research examining the relationship between alcohol and marijuana in the early stages of recreational marijuana legalization (e.g, Dragone et al. 2019; Alley, Kerr and Bae 2020). However, Calvert and Erickson (2021) record mixed conclusions using Nielsen consumer panel data comparing RML states with non-RML states. They conclude locational differences should be considered, while others finding mixed results have also suggested classification of the product (i.e., recreational or medical) may be important (e.g., Zhu, Trangenstein and Kerr 2025).

[4]The literature highlighted in Appendix B 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 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.

Our primary contribution to this strand of literature is the hypothesis explaining why results examining the relationship between goods may differ, even when using the same dataset. We propose that alcohol and marijuana can appear to be both complements and substitutes, and the mixed evidence on whether alcohol and marijuana are economic complements or substitutes may 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, medical, and recreational laws), 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. When a policy or market shock primarily raises *joint* frictions (e.g., increased enforcement), both uses can fall. This would suggest the goods act as complements. When a shock *differentially* lowers frictions for one good (e.g., nearby marijuana dispensary openings), consumption of the other may fall and suggest a substitution effect. We address this gap by modeling transaction costs explicitly using proximity between alcohol and marijuana retailers as a proxy for costs. Our spatially-staggered design at the retailer level helps isolate the role of spatial access costs, and our results provide a potential explanation for why the empirical literature may be mixed.[5]

There is a small strand of literature recognizing the importance of transaction costs through implicit frictions that affect the "full price" (i.e., monetary plus non-monetary costs) of substance use.[6] 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 researchers to formally consider transaction costs within their theoretical and empirical frameworks. They examine youth substance use under varying

[5] While our results and approach *may* reconcile the previous literature finding differing effects, note that we do not make this strong claim since we utilize different data and methods.

[6] Some also frame this as opportunity costs, though most studies summarized in Appendix B 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.

legal constraints, treating frictions as implicit in the full price of consumption.[7] We build on this foundation by explicitly modeling transaction costs as a separate parameter in the budget constraint, allowing us to isolate how spatial frictions (i.e., co-location and retail visibility) distort observed complementarity or substitution. Unlike DiNardo and Lemieux (2001), who focus on legal risk and enforcement as frictions, our framework captures logistical and informational barriers in a legalized retail environment. This distinction is critical in the post-legalization context where both goods are legally available but transaction costs may still shape behavior. Our model explains how complementarity may be masked by spatial frictions, offering a potential explanation for the mixed empirical findings in the literature.

Beyond transaction costs and the contribution to the empirical economic literature measuring the relationship between goods, there is additional value in examining the relationship within the small set of alcohol-marijuana 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, Kerr and Bae (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 longer 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.

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 relative costs can influence consumer behavior. Beyond our earlier reference to the Chakraborty, Devanur and Karande (2010) model with different effective prices, consider also the Alchian–Allen Effect; 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

[7] Pacula (1998), Farrelly et al. (1999a), 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. For completeness, Appendix A shows how our framework relates to the quadratic specification used by DiNardo and Lemieux (2001) while also showing that they do not explicitly incorporate transaction costs into their budget constraint. Both approaches highlight the sign of the cross-partial that determines intrinsic complementarity or substitutability. We add transaction costs τ as a price distortion when both goods are legal and available, distorting observed behavior.

fixed cost (such as a transportation fee) reduces the relative price difference and makes the higher-quality good more 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.

We also recognize the work of Allen and Rehbeck (2022), who show that in multi-good discrete-choice settings, latent complementarity can be obscured in observed data absent appropriate variation or exclusion restrictions. That is, mean demands can be consistent with either complements or substitutes. This helps explain why aggregate evidence may sometimes suggest substitution even when some consumers value joint consumption. Our work builds on this insight that latent complementarity can be masked by frictions Allen and Rehbeck 2022, but extends this discussion in two ways: 1) by explicitly modeling transaction costs through spatial access as a determinant of observed relationships, where goods that are legally and readily available may be complements in utility, and 2) by providing empirical evidence in a legalized retail setting.

Finally, our framework is closely related to Gentzkow (2007). Gentzkow focuses on bundle-level interactions governing the intrinsic substitutability or complementarity between goods (represented as γ in our model). However, we explicitly incorporate transaction costs in the budget constraint, allowing the full price of joint consumption to vary with spatial access and retail visibility.

Collectively, these studies, along with our approach, underscore the role that 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.

4 Data

4.1 Washington as an area of study

The State of Washington has recently been a leader in marijuana liberalization policies. Washington was one of the first states to approve medical marijuana (MML) 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. This effectively allowed for the retail sale of both medical and recreational marijuana through approved and licensed dispensaries.

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 budget and the remaining sixty percent is dedicated to substance-abuse prevention, research, education, and health care. In July 2014, the first month of marijuana dispensary sales, 18 locations across the state 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, generating nearly $900 million in yearly sales ($3.18 per capita) and over $330 million in annual tax revenue. Figure 1 presents the total number of dispensaries by quarter-year for Washington state.

Figure 2 presents the average per-capita alcohol and marijuana sales by zip code and 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 observe 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 Spirit Alcohol Sales

The primary data for this study comes from Washington State's Liquor and Cannabis Board (LCB), which classifies alcoholic beverages into three categories: beer, wine, and spirits.[8] 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.[9] 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 quarter level from 2013 to 2017. We identify the exact location of each alcohol retailer using a reverse geocoding.

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 in 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.

4.3 Legal Marijuana Sales

The secondary data for this study include aggregated quarterly marijuana sales at the dispensary level from the LCB from 2014 to 2017. Legal retail medical marijuana sales were not permitted before July 8, 2014, only home grown marijuana for medical use was allowed. We rely on sales through licensed marijuana dispensaries, which may serve both recreational and medical users. Similar to the alcohol retailer data, we identify the exact location of each marijuana dispensary using a reverse address to geospatial lookup.[10] Once a marijuana dispensary opens, we are able to measure the arc distance between each retailer and dispensary. We use these locations to pair with our retailer alcohol sales data above based on proximity.

[8] This administrative data from LCB provides universal coverage of licensed spirit sales at the retailer level, avoiding retail participation and selection concerns inherent in commercial scanner datasets previously employed (e.g., Miller and Seo 2021). The data are limited to spirits and do not capture beer or wine sales beyond certain direct-from-producer transactions; therefore, our estimates pertain to the spirits segment of the off-premise alcohol market.

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

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

Marijuana dispensary locations in Washington are also shaped by state and local zoning regulations. At the state level, licensed dispensaries are prohibited from locating within 1,000 feet of certain sensitive uses, including elementary and secondary schools, playgrounds, recreation centers, child care centers, public parks, libraries, and transit centers (Washington Administrative Code 314-55-050).[11] Local jurisdictions retain additional authority over land use and may impose further zoning restrictions, such as limiting dispensaries to commercial or industrial districts, requiring conditional use permits, establishing minimum spacing requirements between dispensaries, or capping the number of retail licenses within a municipality. As a result, the spatial distribution of marijuana dispensaries reflects both market forces and regulatory constraints that vary across local jurisdictions.

These regulatory features primarily affect the feasible locations where dispensaries may operate rather than short-run changes in alcohol demand. In our empirical design, fixed effects alcohol retailers absorb time-invariant characteristics of alcohol retailer locations, including local zoning environments, while quarter-year fixed effects account for statewide shocks affecting both the alcohol and marijuana markets. Identification therefore relies on the staggered timing of dispensary openings and the resulting changes in proximity between alcohol retailers and newly operating marijuana dispensaries. Consistent with substantial variation in access to dispensaries between markets, Table 3 reports a considerable dispersion in the distance between alcohol retailers and the nearest marijuana dispensary.

4.4 Covariates

Since our data do not include information on 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.

[11] For more information about Washington State zoning restrictions can be found here: https://apps.leg.wa.gov/wac/default.aspx?cite=314-55-050 and for more information on application licensing here: https://app.leg.wa.gov/rcw/default.aspx?cite=69.50.331

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 dispensaries provide an opportunity for a well-defined natural experiment. We exploit the staggered opening and geography of 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 in addition to purchasing marijuana, allowing us to estimate how *localized changes* in marijuana transaction costs affect alcohol sales.

We calculate the distance from an alcohol retailer to the nearest marijuana dispensary (d_i^{AM}) to proxy for 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, Jaravel and Spiess (2024) imputation estimator (BJS), which uses not-yet-treated period observations to construct the never-treated counterfactual 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:

$$Y_{izt|d} = Y_{izt|d}(0) + RMD_{t|d} \cdot \beta_{t|d} \tag{11}$$

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 marijuana dispensary (i.e., treated if $d_i^{AM} \leq d$), and $\beta_{t|d}$ is the cohort- and time-specific causal effect. BJS estimates $\beta_{t|d}$ by first modeling the untreated outcome as:

$$Y_{izt|d}(0) = \delta_i + \mu_t + X_{izt}\gamma + \epsilon_{izt|d} \tag{12}$$

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

$$\mathbb{E}[\epsilon_{izt|d} \mid X_{izt}, \delta_i, \mu_t] = 0. \tag{13}$$

BJS uses only not-yet-treated observations to estimate the parameters in (10), then imputes $Y_{izt|d}(0)$ for treated units in post-treatment periods. Treatment effects are the differences between observed outcomes and these imputed counterfactuals, which are aggregated across groups to produce an average treatment effect on the treated (ATT).[12] 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:

$$\tau_h = E(Y_{izt+h|d}(1) - Y_{izt+h|d}(0)) \tag{14}$$

We define the treatment date as the quarter in which 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 are centered around the overall marijuana dispensary start 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,

[12] 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 C. To check the robustness of our estimates, we also provide results from a Gardner (2022) regression and Callaway and Sant'Anna (2021) for the isolated retailer event studies.

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 composed of chain retailers. To minimize biasing estimates in our main analysis, we drop retailers that toggled treatment on or off after July 2014 or exited prematurely.[13]

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 retailer 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). The estimates for chain retailers grew in magnitude, with an increase of 10% at the closest threshold. The positive effect on alcohol sales for those retailers diminishes quickly and is similar to the overall 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 C.

Figure 3 presents the event study results for alcohol retailers in close proximity to marijuana dispensaries. Mirroring Table 4, Panels A through E display the one-tenth to half-mile models with controls and analyze a six-quarter pretreatment and eight-quarter post-treatment window around

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

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 shows a clear pattern in the closer models (0.10 to 0.30 miles) that there is a strong, statistically significant upward trend after three quarters of treatment.

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 and high transaction costs) should experience a negative sales effect.[14] 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.

The event studies for non-chain retailers in the 17.5+ through 22.5+ mile isolation rings are presented in Figure 4.[15] Although the estimates are less noisy than our close proximity figures, with no significant pretrend and a decrease after the first year, the confidence intervals displayed are downward biased (artificially small) due to the small number of observations within each group. This is a known problem inherent in the BJS model and most noticeable in Panel C. To account for this, we test the robustness of the downward trend using the staggered timing method developed by Callaway and Sant'Anna (2021). The estimates and event studies are found in Appendix C. Though the magnitude of dispensary isolation is greater, the event study shows a similar negative impact for non-chain retailers.

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.,

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

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

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 that 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 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, Callaway and Sant'Anna (2021), and Gardner (2022) estimator in Tables C.1 and C.2 for close and isolated retailers. The Callaway-Sant'Anna estimates reports similar, but attenuated positive values for the close retailer results and greater negative values for isolated retailers. Though in some cases the Gardner estimates lose statistical significance at further distances with close retailers, 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 assess 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 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 lead firms to exit early or temporarily shut down. In Appendix D, Tables D.1 and D.2 keep 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 reports 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 that 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 their 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; the inclusion of only border zip codes amplifies our estimates. This reinforces the idea of complementary purchasing for travelers who may have traveled a long 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 spirit sales, Panel E of Appendix E.1 reports the effects of treatment intensity. There are up to eight marijuana dispensaries varying the effects stated in our main estimations depending on the distance of the treatment applied. Higher exposure to marijuana dispensaries surrounding an alcohol retailer could have compounding effects. We use the Gardner (2022) method to test this theory by including

treatment intensity within the second stage, allowing 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.

6.3.6 Random-Inference Simulation

To test the credibility of our identification strategy, we implement a Monte Carlo randomization-inference exercise for the 0.10 mile proximity specification. We generate 1,000 placebo assignments by randomly selecting treated retailers and randomly assigning treatment timing, subject to the same pretreatment and post-treatment support observed in the data. For each draw, we re-estimate our main specification and record the resulting treatment effect. Our findings are shown in Appendix Figure F.1. The observed estimate from the actual treatment assignment lies in the extreme right tail of the placebo distribution, with a one-sided randomization-inference of p-value of 0.006 and two-sided randomization-inference p-value of 0.010. The effects of our measured magnitude rarely occur under random assignment of treatment status and timing. These results provide additional confidence that our findings are unlikely to be driven by coincidental timing or spurious correlation.

6.4 Descriptive Evidence of Co-Location and Localized Demand

To assess whether alcohol and marijuana retail activity co-move within the same local commercial corridors, we regress dispensary quarterly sales to a population- and distance-weighted measure of nearby alcohol sales.[16] Although these specifications are not causal, they provide descriptive evidence on retail co-location and localized demand environments. Table 8 shows that across specifications including dispensary and quarter-year fixed effects and controlling for zip code-level demographics, a one standard deviation increase in highly localized alcohol exposure (decay of $1/d^2$) is associated with approximately an 8–9% increase in marijuana sales.

Appendix Table G.1 reports estimates using alternative distance decay parameters. Higher powers of decay infer that closer proximity to a dispensary matters more than general exposure in

[16] For a better interpretation of alcohol exposure, since it is distance weighted, we report effects of a standardized change in alcohol exposure to the natural log of marijuana dispensary sales. To account for a reasonable catchment area, and the spatial influence based on proximity, we use standard distance decay measures where alcohol retailers nearby hold more weight than retailers several miles away.

a catchment area. The association strengthens as the decay becomes steeper, indicating that the relationship is driven by very close proximity rather than broader market conditions. Because alcohol exposure is not randomly assigned, these estimates should be interpreted as correlational patterns consistent with shared local demand and retail clustering, rather than evidence of individual-level joint consumption.

6.5 Distance Induced Switching: Control as Treated

Finally, we examine the possibility that consumers may change their consumption habits from distant alcohol retailers to those closer to a marijuana retailer, as suggested in our main findings. The shift to easy, one-stop-shopping for consumers could effectively "treat" a section of our control group, specifically in areas where consumers are indifferent in their behavior of co-consumption or substitution. We examine this in two ways; (i) consumers merely *shifting* alcohol purchases to a retailer closer to a dispensary, or (ii) indifferent consumers opting for a one-stop shopping experience. First, we limit the control group to alcohol retailers with a marijuana dispensary at most fifteen miles away for close retailers and at minimum two miles away for isolated retailers. This indicates whether our observed result is a shift toward more convenient retailers (i.e., those near dispensaries) or a change in total alcohol consumption. Second, we limit the control group by excluding retailers with dispensaries one to five miles away for close retailers and five to ten miles for isolated. This excludes consumers who were relatively close to an alcohol retailer but may have shifted consumption patterns to a more convenient one-stop-shopping experience (i.e., those most likely to be indifferent to travel).

We report on close retailers with the two versions of the restricted distance control groups in Appendix H Tables H.1 and H.2. The results are similar to our main specification, with minimal differences across all estimates. We report isolated retailers with restricting the control group in Tables H.3 and H.4. We find slightly attenuated estimates across all distances for all and non-chain retailers when truncating retailers within two miles of a dispensary, and a magnitude increase in the reduction of alcohol sales when truncating the potentially indifferent control group. These indicate a small shift in alcohol consumption away from isolated retailers to close retailers, enforcing the idea of convenient, one-stop-shopping as well as increased consumption related to dispensaries.

7 Discussion

Our findings highlight the importance of transaction costs and spatial frictions in determining whether alcohol and marijuana act as economic complements or substitutes. Consistent with the theoretical framework developed in Section 2, consumer behavior depends on the cost of jointly acquiring both goods. Empirically, we observe that alcohol spirit sales increase for retailers located in close proximity to newly opened marijuana dispensaries and decrease for retailers that are geographically isolated from dispensaries. When marijuana dispensaries and alcohol retailers are co-located, implying low transaction costs, alcohol sales increase following dispensary entry. This pattern is consistent with complementarity. Conversely, when alcohol retailers are spatially isolated from dispensaries, sales decrease, indicating substitution. Our results show that complementarity and substitution can coexist within the same market depending on spatial access, suggesting that the relationship between goods should be viewed as conditional rather than fixed.

The idea that market frictions in consumer access shape observed relationships may provide a rationale for the seemingly conflicting results across previous studies.[17] Focusing on the recent wave of RML research discussed in our literature review, early studies often relied on aggregate sales data or survey responses. Both approaches may mask the spatial frictions consumers face when purchasing these goods (Dragone et al. 2019; Alley, Kerr and Bae 2020; Miller and Seo 2021). Our approach directly incorporates spatial access as a proxy for transaction costs using retailer-level data and staggered dispensary entry. In doing so, we provide empirical evidence consistent with the theoretical predictions of DiNardo and Lemieux (2001) and recent findings that the alcohol–marijuana relationship varies across contexts (Calvert and Erickson 2021; Zhu, Trangenstein and Kerr 2025). If consumers can purchase both goods in a single trip, joint use may become more common. If co-purchasing requires greater search costs in the form of increased travel time or lack of street presence, substitution will dominate.

The heterogeneity we document across retailer types and market environments further underscores the role of visibility and convenience as moderators. Large chain retailers, particularly supermarkets and pharmacies, exhibit the strongest positive effects following nearby dispensary

[17] We emphasize that our results do not directly reconcile the existing literature, as previous studies rely on different data sources and identification strategies. Future research may consider incorporating spatial access measures into earlier datasets, although this may be difficult with highly aggregated data (e.g., the approach of Miller and Seo 2021).

entry. These retailers facilitate "one-stop shopping" (Seo 2017) and reduce the logistical barriers associated with purchasing both goods in a single trip. In terms of our model, this reduces the effective transaction cost parameter (τ), bringing the full price of joint acquisition closer to listed prices. Smaller or less visible retailers lack these advantages and experience either smaller or negative effects. Differences between urban and non-urban retailers reinforce this mechanism: retailers located in dense urban areas face lower spatial frictions and exhibit stronger complementarity, while retailers in less dense markets face higher access costs, producing patterns consistent with substitution.

While our findings provide evidence consistent with transaction cost mechanisms, some limitations should be acknowledged. First, our data measure retailer-level spirit sales rather than individual consumption or joint purchase behavior. As a result, we infer complementarity or substitution through changes in alcohol sales rather than directly observing joint consumption. Second, while retailer and time fixed effects account for many local characteristics, dispensary placement may still reflect regulatory constraints and local zoning environments that influence where marijuana retailers can locate. These factors likely affect the feasible locations of dispensaries rather than short-run changes in alcohol demand, but they nonetheless shape the spatial structure of the market. Despite these limitations, the consistent patterns observed across proximity thresholds, retailer types, and robustness exercises provide strong evidence that spatial access and transaction costs play an important role in shaping observed relationships between alcohol and marijuana.

8 Conclusion

Our analysis revisits the long-standing question of whether alcohol and marijuana act as economic substitutes or complements. Rather than treating this relationship as fixed, we develop a framework in which the observed relationship between goods depends on transaction costs associated with joint acquisition. While the importance of frictions in consumer behavior is well understood, we show formally how transaction costs can alter the observed relationship between goods even when the underlying utility relationship remains unchanged. In particular, spatial frictions can *suppress joint purchase* when goods are intrinsically complementary or *amplify substitution* when acquisition costs differ across locations.

We apply this framework to the legalization of recreational marijuana in Washington State. Using retailer-level spirit sales data combined with the staggered entry of marijuana dispensaries, we estimate how spatial proximity to dispensaries affects alcohol sales. Our results reveal substantial heterogeneity consistent with the role of transaction costs. Spirit sales increase by approximately 2–6% for alcohol retailers located within 0.5 miles of a dispensary, with effects reaching roughly 10% for large chain retailers. In contrast, alcohol retailers located far from dispensaries experience a decrease in spirit sales of approximately 7–10%. These patterns are consistent with a mechanism in which lower spatial frictions facilitate joint purchasing while higher frictions discourage it. A series of robustness checks—including alternative staggered-adoption estimators, placebo simulations using randomization inference, and tests for retailer switching support the stability of these findings.

These results help explain why the empirical literature has reached mixed conclusions regarding the alcohol–marijuana relationship. Much of the existing research relies on aggregate data or survey responses that do not account for localized retail environments. Our findings suggest that spatial access and other transaction costs can meaningfully shape observed purchasing patterns, allowing both complementarity and substitution to emerge within the same market depending on the local context.

Understanding this relationship has implications for public health and fiscal policy. If alcohol and marijuana function as substitutes in some settings, increased marijuana availability could reduce certain alcohol-related harms, such as chronic alcohol-related health conditions or alcohol use disorders (Anderson, Hansen and Rees 2013). However, substitution away from alcohol may also reduce alcohol tax revenues, which could be partially offset by increased marijuana tax collections (Miller and Seo 2021). In contrast, complementary purchasing in low-friction environments could increase joint use and associated health risks. Our results therefore suggest that policy evaluations of marijuana legalization should consider local retail environments and spatial access when assessing broader behavioral responses.

Our findings also highlight the importance of incorporating transaction costs into empirical analyses of related goods. Differences in search costs, travel time, retail visibility, or regulatory environments can significantly alter observed demand relationships even when consumer preferences remain unchanged. Future research could extend this framework by examining heterogeneity across

consumer groups, incorporating additional sources of informational frictions, or applying similar approaches to other markets where the relationship between goods has historically appeared ambiguous.

Together, this study provides both a theoretical framework and new empirical evidence showing how transaction costs shape observed relationships between goods. Recognizing the role of spatial access and market frictions can improve our understanding of consumer behavior and help reconcile seemingly contradictory empirical findings across related markets.

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9 Figures

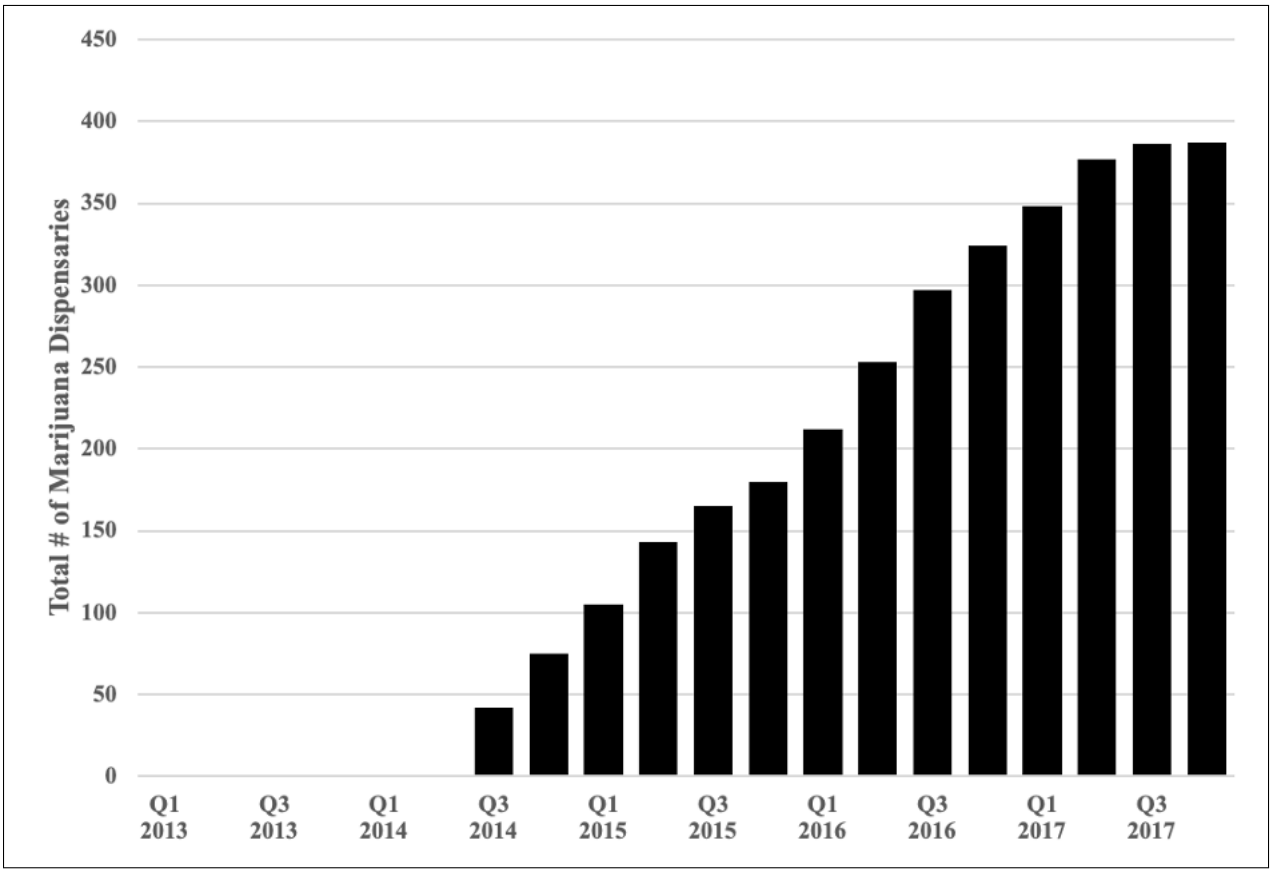


Figure 1: Total Number of Dispensaries in Washington, by quarter-year

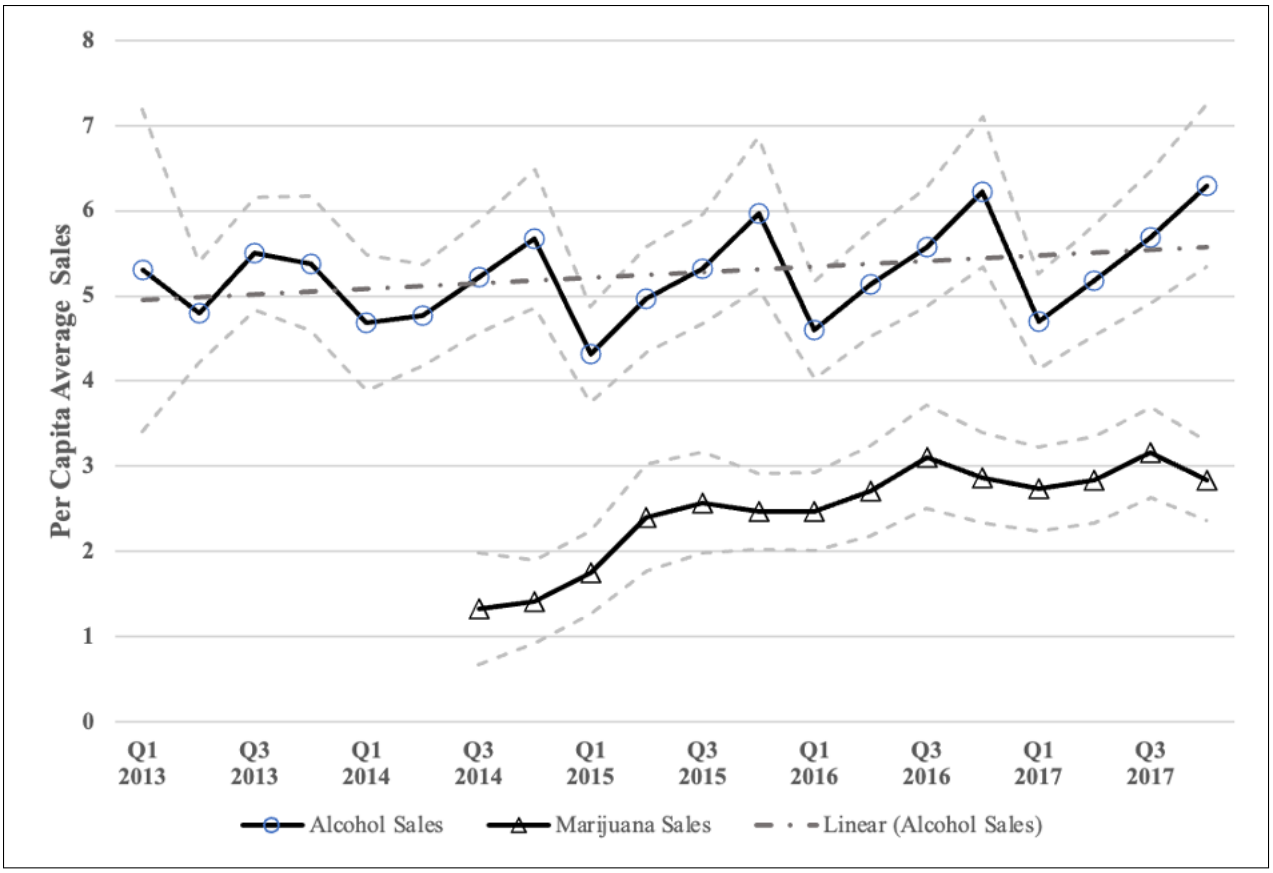
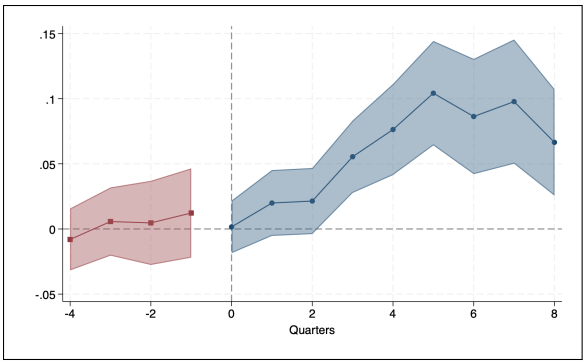
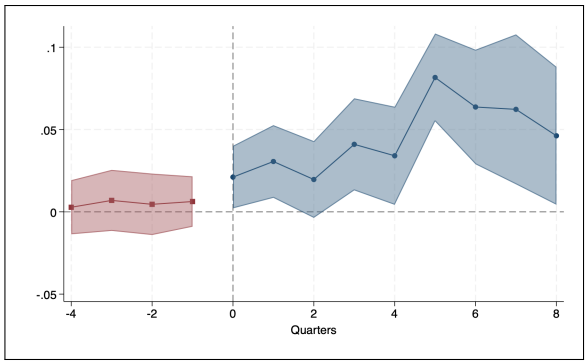


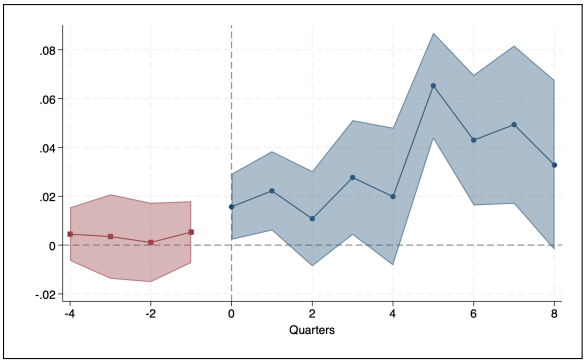
Figure 2: Average Spirit Alcohol and Marijuana Sales, per capita by quarter



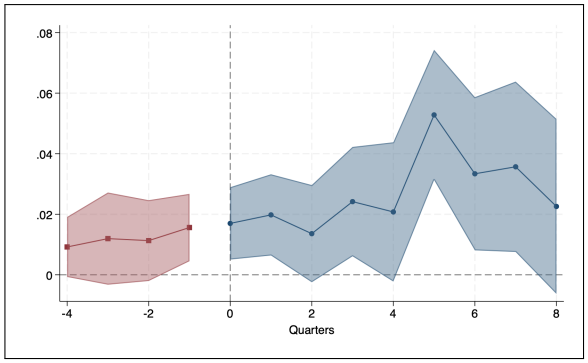
(a) "Treatment" $\leq$ 0.1-miles



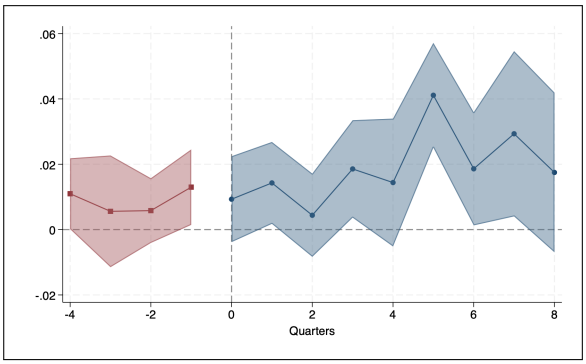
(b) "Treatment" $\leq$ 0.2-miles



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



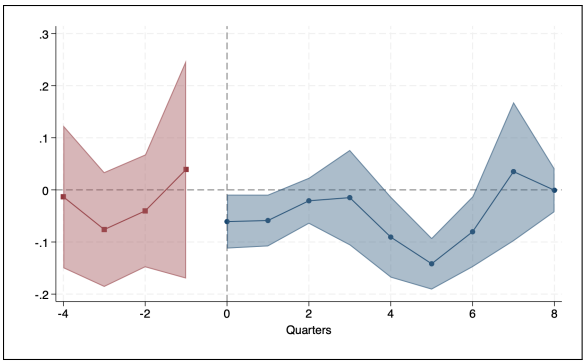
(d) "Treatment" $\leq$ 0.4-miles



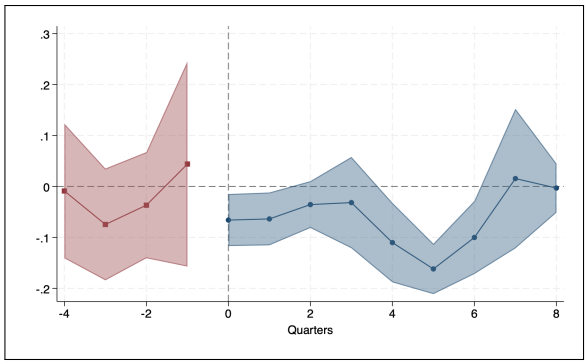
(e) "Treatment" $\leq$ 0.5-miles

Figure 3: Event Study for All Alcohol Retailers

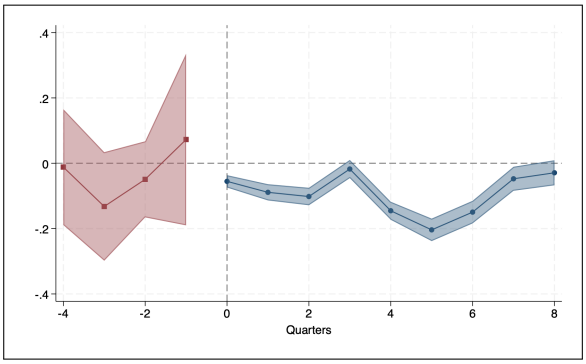
Note: Each point in the figure represents the difference between "treated" alcohol retailers relative to "untreated" retailer, before and after treatment. Each specification includes the covariates of percent age 20-24, percent male population, percent with at least a high school education, and percent 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" $\geq$ 17.5-miles



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



(c) "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.

10 Tables

Table 1: Theoretical Bridge to Model Predictions for Observed Alcohol Response

Intrinsic Relationship	**Low Transaction Costs**	**High Transaction Costs**
Utility Complements		
$\gamma > 0$	$\beta > 0$	$\beta \leq 0$ (possible reversal)
Utility Independent		
$\gamma = 0$	$\beta = 0$	$\beta < 0$
Utility Substitutes		
$\gamma < 0$	$\beta < 0$	$\beta < 0$ (stronger)

Table 2: Abbreviated Alcohol-Marijuana Literature Review

Study	**Policy**	**Data**	**Population**	**Conclusion**
Thies and Register (1993)	Decrim.	NLSY (1979, 1984, 1988)	U.S. males (ages 14–21)	Mixed
Yamada, Kendix and Yamada (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 and Rees (2013)	MML	FARS (1990-2010); BRFSS (1993-2010); Beer Institute's Brewers Almanac (1990-2010)	U.S. population	Substitutes
Sabia, Swigert and Young (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 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, Jaravel and Spiess (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, Jaravel and Spiess (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, Jaravel and Spiess (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, Jaravel and Spiess (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 8: Dispensary Sales and Alcohol Exposure Results

	Dispensary Sales and Alcohol Sales Exposure			
	Coef./SE	*Coef./SE*	*Coef./SE*	*Coef./SE*
Alcohol Exposure	0.147***	0.227***	0.087***	0.079*
	(0.012)	(0.028)	(0.026)	(0.032)
Constant	3.018***	3.018***	3.018***	3.428+
	(0.105)	(0.000)	(0.000)	(1.834)
Observations	3,294	3,294	3,294	3,294
Dependent Mean		3.018		
Covariates	No	No	No	Yes
Dispensary FE	No	Yes	Yes	Yes
Quarter FE	No	No	Yes	Yes

Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes*: Dispensary / Quarter FE represents dispensary and quarter by year fixed effects. The outcome variable is the log of dispensary sales per capita (by zip code). All models use a high-dimensional FE linear estimation method. Alcohol exposure is standardized and weighted by zip code population and the inverse distance to dispensary, squared. Covariates are zip-level variables, including: percent age 20-24, percent male population, percent with at least a high school education, and percent married. Standard errors are clustered at the zip code level. Symbols denote significance at the 0.5% (***), 1% (**), 5% (*), and 10% (+) levels.

Appendix A: 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 ($\gamma_{am} < 0$).

DiNardo and Lemieux (2001) rely on the utility framework along for identifying the relationship between these goods. That is, their budget constraint is the traditional:

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

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 in the illegal context of DiNardo and Lemieux (2001) (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 when both goods are legal and available:

$$I \;=\; q_0 \;+\; p_a q_a \;+\; p_m q_m \;+\; T(q_A, q_M; d) \tag{A.4}$$

where T remains a non-negative, concave function of q_A and q_M, as discussed in the text.

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

$$\frac{v_a}{v_m} \;=\; \frac{p_a + \tau_M}{p_m + \tau_A)} \tag{A.5}$$

where

$$v_a \;=\; \frac{\partial v}{\partial q_a} = \gamma_a + \gamma_{am} q_m + \gamma_{aa} q_a, \qquad v_m \;=\; \frac{\partial v}{\partial q_m} = \gamma_m + \gamma_{am} q_a + \gamma_{mm} q_m \tag{A.6}$$

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 discrete shifts in the "full" price of goods while not considering τ explicitly because these were not measurable. Rather, the transaction costs were modeled as implicit frictions at the individual level. By contrast, the joint transaction-cost parameter τ captures potentially explicit frictions that arise when both goods are legally available and acquired/consumed together, such as additional search time. When $\tau > 0$, the cost of joint consumption may rise with the consumption of both goods, so even goods that are complements in preferences may behave as substitutes in observed demand. Similar arguments can be made for other papers in the alcohol-marijuana literature. For example, Pacula's (1998) framework is another natural examination of a setting where both goods were (at least partly) illegal and the transaction cost variation was proxied through legal risk.

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

Study	MJ/Alcohol Policy Leveraged	Data	Population	Conclusion
Stephens, Roffman and Simpson (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. (1999b)	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 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, Brady and Li (2015)	MLDA	Fatality Analysis Resporting 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, Kendix and Yamada (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, Hansen and Rees (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, Hockenberry and Cummings (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, Swigert and 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 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, Chong and Kwon (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, Kerr and Bae (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, Trangenstein and Kerr (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 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:

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

and (2) the cohort-specific regression estimator of Gardner (2022). In all cases, the treatment date is defined as the effective date of RMD implementation by distance proximity d. The results are presented in the following tables.

Table C.1: Close Results for Callaway-Sant'Anna, Gardner, and TWFE

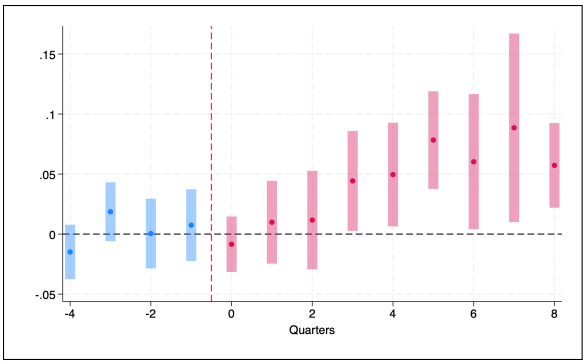
	Transaction cost treatment defined by distance (distances shown)					
	0.1 miles	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
	Coeff./SE	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*	*Coeff./SE*
Panel A: BJS						
Treatment Start	0.059**	0.060**	0.047**	0.034**	0.028*	0.021*
	(0.022)	(0.022)	(0.017)	(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: Callaway-Sant'Anna						
Treatment Start	0.046**	0.046**	0.040+	0.027	0.016	0.011
	(0.017)	(0.017)	(0.021)	(0.018)	(0.014)	(0.011)
Observations	19,253	19,253	19,251	19,234	19,212	19,200
Dependent Mean			1.417			
Panel C: 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 D: 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. Panel B only includes the number of close alcohol retailers as a covariate. Standard errors are clustered at the county level. Symbols denote significance at the 0.5% (***), 1% (**), 5% (*), and 10% (+) levels.

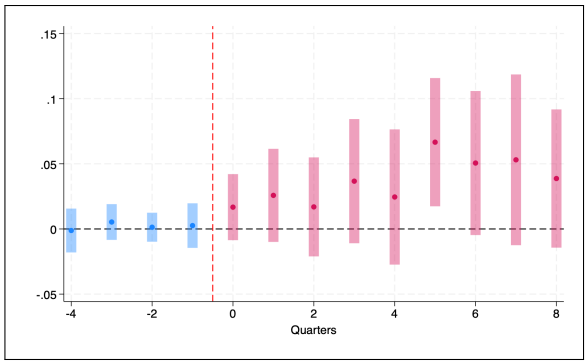
Table C.2: Isolated Results for Callaway-Sant'Anna, Gardner, and TWFE

	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: Callaway-Sant'Anna					
Treatment Start	-0.197+	-0.243	-0.245+	-0.244+	-0.369*
	(0.103)	(0.224)	(0.136)	(0.133)	(0.165)
Observations	19,221	19,221	19,216	19,230	19,241
Dependent Mean			1.417		
Panel C: 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 D: 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

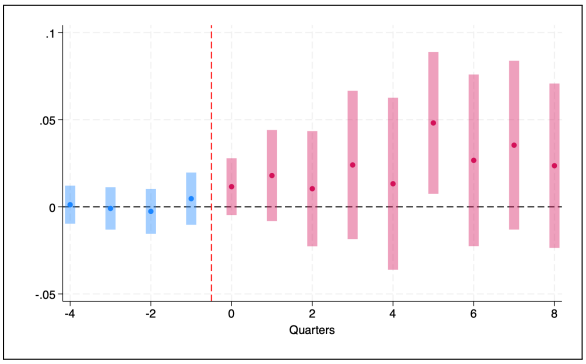
Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes*: R-Q FE represents retailer and quarter by year fixed effects. The outcome variable is the log of spirit sales per capita (by zip code). Covariates are zip-level variables, including: percent age 20-24, percent male population, percent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



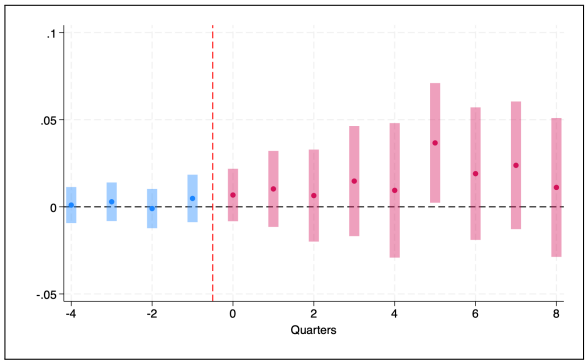
(a) "Treatment" $\leq$ 0.1-miles



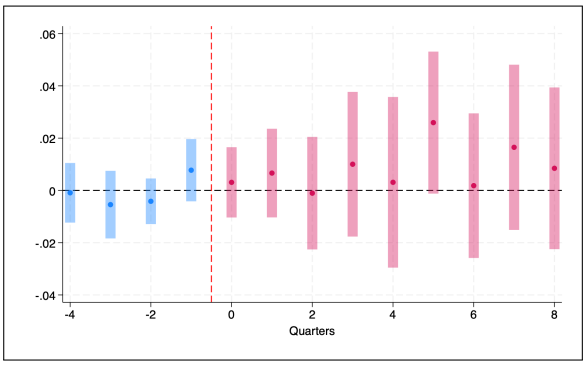
(b) "Treatment" $\leq$ 0.2-miles



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



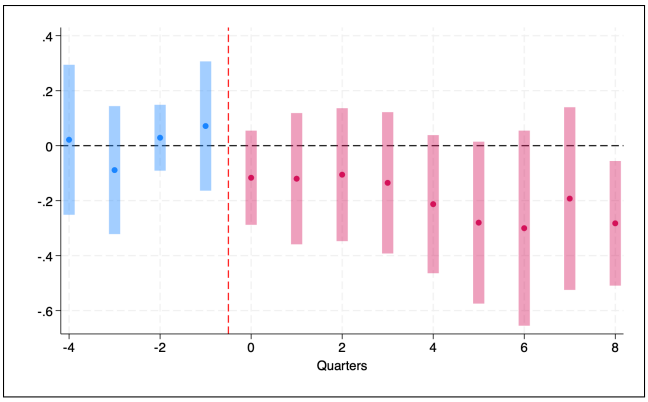
(d) "Treatment" $\leq$ 0.4-miles



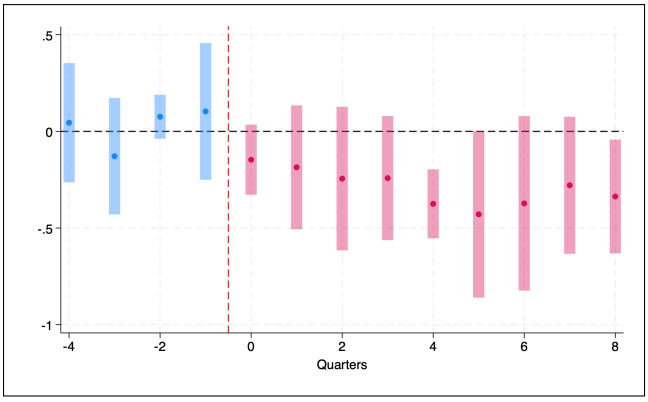
(e) "Treatment" $\leq$ 0.5-miles

Figure C.1: Event Study for Close Alcohol Retailer, Callaway-Sant'Anna

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 covariate of the number of nearby alcohol retailers ($\leq$5 miles), as well as retailer fixed effects. Standard errors are clustered at the county level. 90% confidence intervals reported as band around point estimates.



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

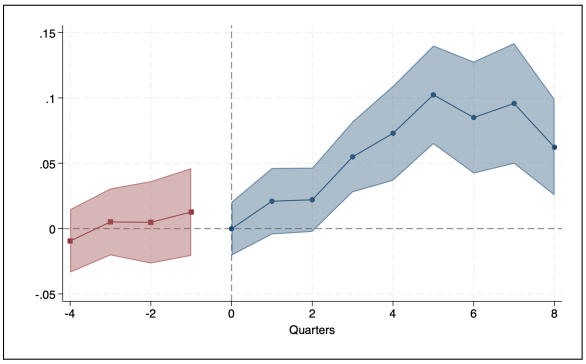


(b) "Treatment" $\geq$ 22.5-miles

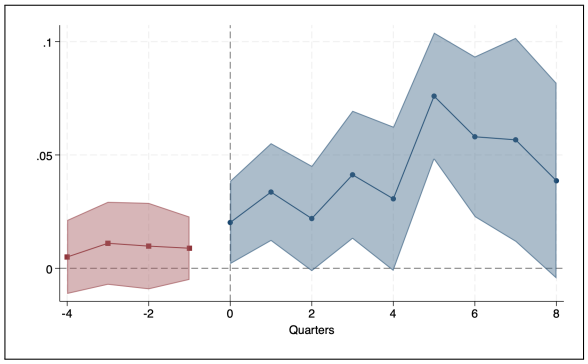
Figure C.2: Event Study for Non-Chain, Isolated Alcohol Retailers, Callaway-Sant'Anna

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 covariate of the number of nearby alcohol retailers ($\leq$5 miles), as well as retailer fixed effects. Standard errors are clustered at the county level. 90% confidence intervals reported as band around point estimates.

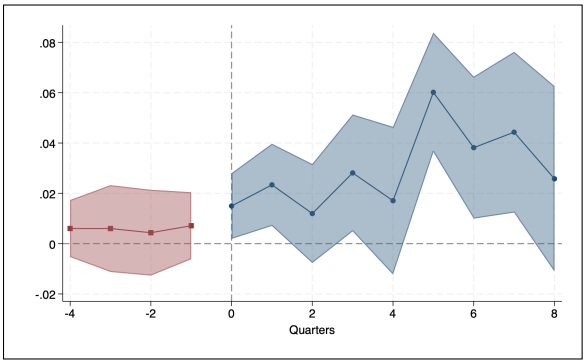
Appendix D: No Exit Results



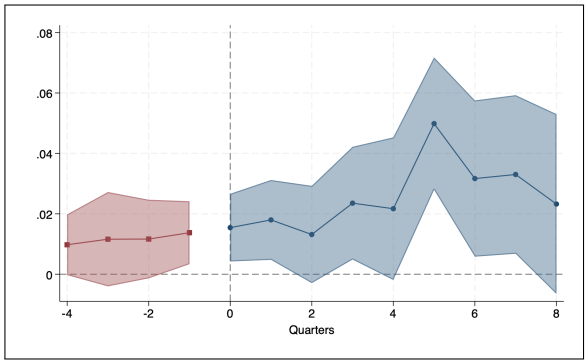
(a) "Treatment" $\leq$ 0.1-miles



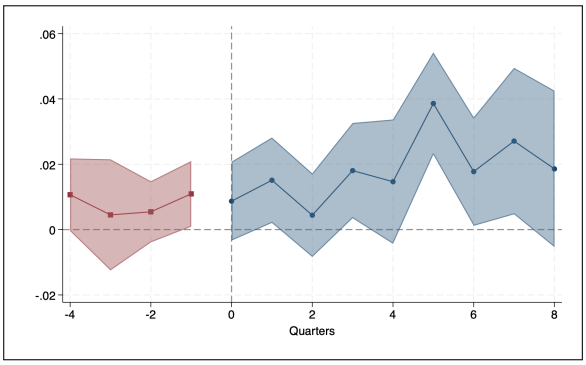
(b) "Treatment" $\leq$ 0.2-miles



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



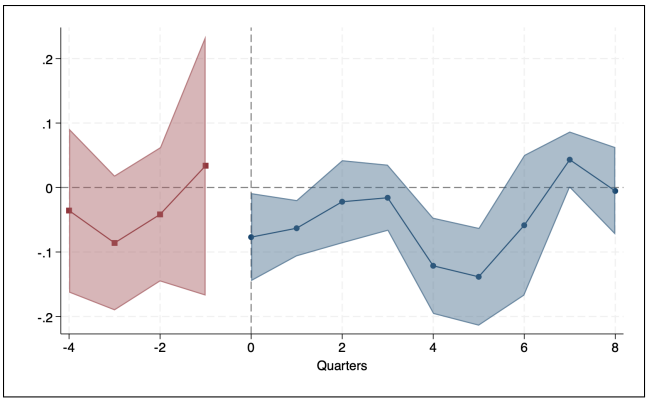
(d) "Treatment" $\leq$ 0.4-miles



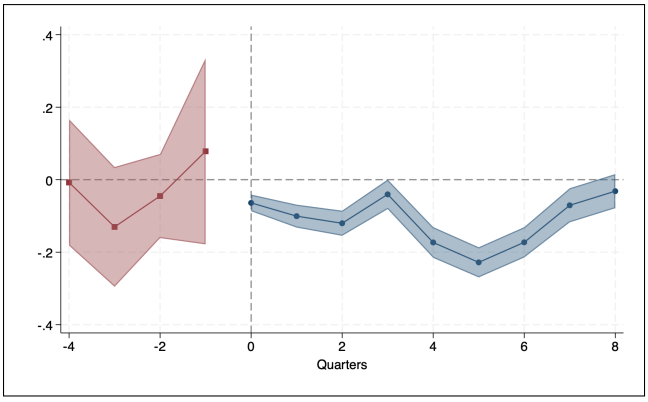
(e) "Treatment" $\leq$ 0.5-miles

Figure 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 retailer fixed effects. 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 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: Close Retailer 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, Jaravel and Spiess (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, Jaravel and Spiess (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, Jaravel and Spiess (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.

Appendix F: Randomization-Inference Simulation

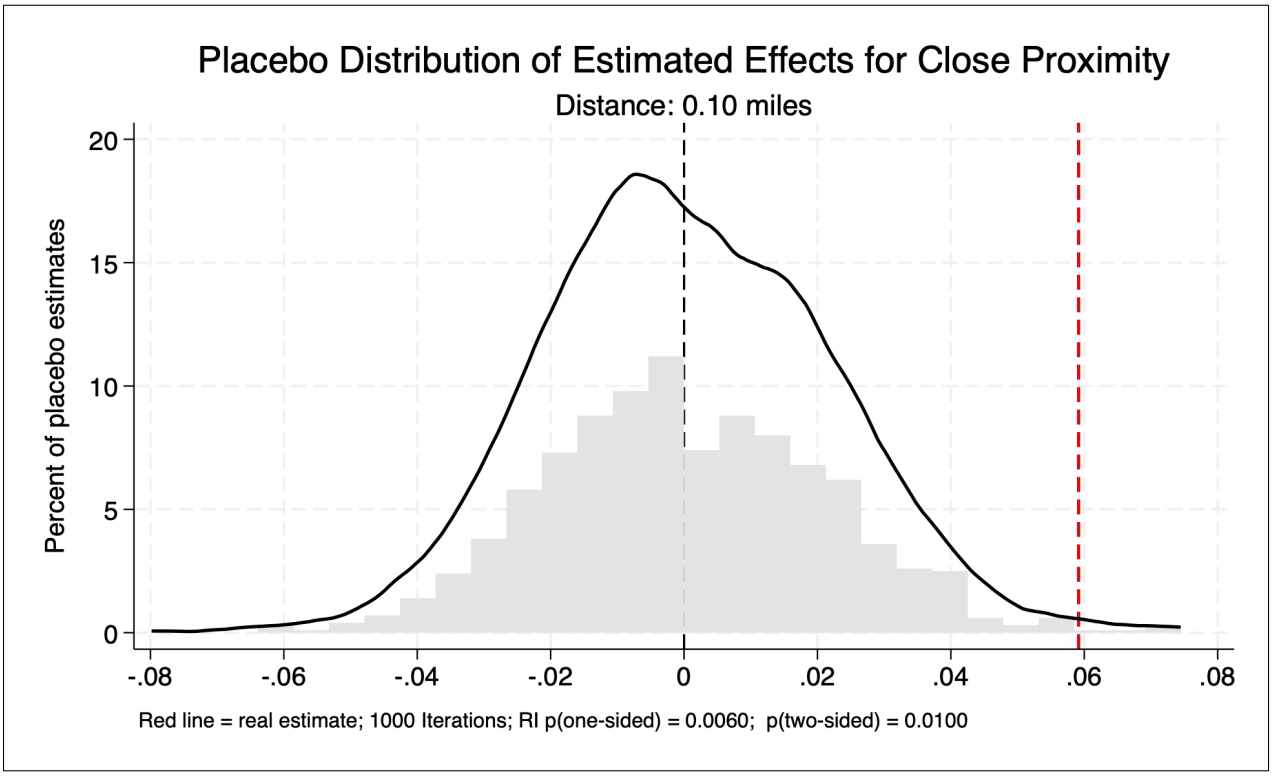


Figure F.1: Random Treatment and Timing Assignment

Appendix G: Alternative Spatial Decay Specifications

Table G.1: Distance Decay for Alcohol Sales Exposure

	Distance Decay for Alcohol Sales Exposure			
	$1/d^3$	$1/d^2$	$1/d$	$1/d^{0.7}$
	Coef./SE	*Coef./SE*	*Coef./SE*	*Coef./SE*
Alcohol Exposure	0.096***	0.087***	0.070*	0.054
	(0.026)	(0.026)	(0.031)	(0.040)
Constant	3.018***	3.018***	3.018***	3.018***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	3,294	3,294	3,294	3,294
Dependent Mean	3.018			
Covariates	No	No	No	No
Dispensary FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes

Source: Washington State Liquor and Cannabis Board, 2013-2017. *Notes*: Dispensary / Quarter FE represents dispensary and quarter by year fixed effects. The outcome variable is the log of dispensary sales per capita (by zip code). All models use a high-dimensional FE linear estimation method. Alcohol exposure is standardized and weighted by zip code population and the inverse distance to dispensary; $1/d^3$, $1/d^2$, $1/d$, and $1/d^{0.7}$, respectively. Standard errors are clustered at the zip code level. Symbols denote significance at the 0.5% (***), 1% (**), 5% (*), and 10% (+) levels.

Appendix H: Variations in Control Groups

Table H.1: Close: Control Max Dispensary Distance

	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
	Coef./SE	*Coef./SE*	*Coef./SE*	*Coef./SE*	*Coef./SE*	*Coef./SE*
Panel A: All Alcohol Retailers						
Treatment Start	0.059**	0.062**	0.048**	0.034**	0.028*	0.022*
	(0.022)	(0.022)	(0.017)	(0.012)	(0.011)	(0.009)
Observations	18,779	18,779	18,779	18,772	18,750	18,750
Dependent Mean			1.398			
Panel B: Chain Alcohol Retailers						
Treatment Start	0.099***	0.100***	0.056***	0.040***	0.030***	0.021**
	(0.031)	(0.031)	(0.017)	(0.009)	(0.010)	(0.007)
Observations	13,839	13,839	13,839	13,839	13,826	13,826
Dependent Mean			1.381			
Panel C: Non-Chain Alcohol Retailers						
Treatment Start	-0.041+	-0.036	0.020	0.011	0.018	0.015
	(0.022)	(0.023)	(0.058)	(0.049)	(0.038)	(0.031)
Observations	4,940	4,940	4,940	4,933	4,924	4,924
Dependent Mean			1.446			
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. Control group is limited to alcohol retailers that have a closest marijuana dispensary of at most 15 miles. The outcome variable is the log of spirit sales per capita (by zip code). All models use Borusyak, Jaravel and Spiess (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 H.2: Close: Control Mid Dispensary Distance

	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
	Coef./SE	*Coef./SE*	*Coef./SE*	*Coef./SE*	*Coef./SE*	*Coef./SE*
Panel A: All Alcohol Retailers						
Treatment Start	0.062**	0.063***	0.051***	0.038***	0.032**	0.024*
	(0.022)	(0.022)	(0.018)	(0.013)	(0.011)	(0.010)
Observations	11,565	11,565	11,565	11,558	11,536	11,536
Dependent Mean			1.468			
Panel B: Chain Alcohol Retailers						
Treatment Start	0.099***	0.101***	0.057***	0.042***	0.030*	0.020*
	(0.031)	(0.031)	(0.018)	(0.011)	(0.012)	(0.010)
Observations	7,972	7,972	7,972	7,972	7,959	7,959
Dependent Mean			1.401			
Panel C: Non-Chain Alcohol Retailers						
Treatment Start	-0.03	-0.028	0.031	0.022	0.031	0.03
	(0.023)	(0.023)	(0.060)	(0.050)	(0.039)	(0.033)
Observations	3,593	3,593	3,593	3,586	3,577	3,577
Dependent Mean			1.616			
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. Control group excludes alcohol retailers that have a closest marijuana dispensary between 1 and 5 miles. The outcome variable is the log of spirit sales per capita (by zip code). All models use Borusyak, Jaravel and Spiess (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 H.3: Isolated: Control Min Dispensary Distance

	Transaction cost treatment defined by distance (distances shown)				
	12.5+ miles	15+ miles	17.5+ miles	20+ miles	22.5+ miles
	Coef./SE	*Coef./SE*	*Coef./SE*	*Coef./SE*	*Coef./SE*
Panel A: All Alcohol Retailers					
Treatment Start	-0.018	0.021	-0.061***	-0.061***	-0.090***
	(0.026)	(0.025)	(0.021)	(0.021)	(0.019)
Observations	5,623	5,623	5,623	5,637	5,649
Dependent Mean			1.567		
Panel B: Chain Alcohol Retailers					
Treatment Start	0.010	-0.022	0	0	0
	(0.014)	(0.025)	(.)	(.)	(.)
Observations	3,481	3,481	3,481	3,481	3,481
Dependent Mean			1.454		
Panel C: Non-Chain Alcohol Retailers					
Treatment Start	-0.013	0.045	-0.068**	-0.068**	-0.107***
	(0.034)	(0.028)	(0.026)	(0.026)	(0.028)
Observations	2,142	2,142	2,142	2,156	2,168
Dependent Mean			1.755		
Covariates	No	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. Control group is limited to alcohol retailers that have a closest marijuana dispensary of at least 2 miles. The outcome variable is the log of spirit sales per capita (by zip code). All models use Borusyak, Jaravel and Spiess (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 H.4: Isolated: Control Mid Dispensary Distance

	Transaction cost treatment defined by distance (distances shown)				
	12.5+ miles	15+ miles	17.5+ miles	20+ miles	22.5+ miles
	Coef./SE	*Coef./SE*	*Coef./SE*	*Coef./SE*	*Coef./SE*
Panel A: All Alcohol Retailers					
Treatment Start	-0.038	-0.004	-0.076***	-0.076***	-0.106***
	(0.028)	(0.028)	(0.021)	(0.021)	(0.015)
Observations	17,954	17,954	17,954	17,968	17,980
Dependent Mean			1.402		
Panel B: Chain Alcohol Retailers					
Treatment Start	0.005	-0.027	0	0	0
	(0.020)	(0.035)	(.)	(.)	(.)
Observations	13,288	13,288	13,288	13,288	13,288
Dependent Mean			1.389		
Panel C: Non-Chain Alcohol Retailers					
Treatment Start	-0.057	0.006	-0.088***	-0.087***	-0.136***
	(0.036)	(0.036)	(0.026)	(0.026)	(0.027)
Observations	4,666	4,666	4,666	4,680	4,692
Dependent Mean			1.444		
Covariates	No	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. Control group excludes alcohol retailers that have a closest marijuana dispensary between 5 and 10 miles. The outcome variable is the log of spirit sales per capita (by zip code). All models use Borusyak, Jaravel and Spiess (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.