Market Structure and Product Variety: Evidence from a Natural Experiment in Liquor Licensure*

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Abstract

This paper examines how market structure, measured as the number of firms, affects prices, quantities, and product assortment. Our analysis focuses on Washington's deregulation of spirit sales, which generated exogenous variation in the number of retailers across the state. We find that an additional firm increases purchasing because retailers respond by offering greater product variety. However, these effects exhibit strong diminishing returns. We find further that prices do not adjust to competition. Overall, our results suggest that entry restrictions curtail liquor consumption. However, Washington's licensure requirement appears a blunt policy instrument, as our estimates imply negligible effects in most markets.

Keywords: Liquor regulation, market structure, privatization, price discrimination, product variety.
JEL Codes: D43, D62, L43, L66.

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1 Introduction

A central question in Industrial Organization is how do market outcomes vary with the number of competitors. As the literature has long recognized, simply regressing outcomes on the number of firms does not provide a satisfactory answer, as entry is endogenous and responds to unobserved (to the econometrician) market and firm conditions. In this paper, we leverage a natural experiment in Washington State to revisit this question. We find that increasing the number of firms increases purchasing because enhanced competition spurs retailers to widen their product assortment. We also find that entry exhibits strong diminishing returns; there is no difference in product offerings in markets with five or more firms. Even in concentrated markets, however, firms do not appear to adjust prices in response to competition. This finding is consistent with work by Adams and Williams (2017), DellaVigna and Gentzkow (2017) and Hitsch et al. (2017). Our results further illustrate how chain stores nevertheless engage in price discrimination across retail locations through product variety adjustments.

We also shed light on the efficacy of licensure restrictions, a common policy instrument for curtailing liquor consumption. We find that households are half as likely to engage in heavy drinking in monopoly compared to duopoly markets. This evidence lends support to concerns that easier access to liquor increases excessive consumption, an argument voiced by opponents of liquor privatization in Washington. We also show that increases in consumption operate on the intensive margin: they are concentrated among the highest liquor-purchasing households in the period before privatization. However, we find no change in alcohol-related accidents, and no change in the purchasing of beer and wine. Taken together, these results suggest that restricting entry into retail liquor sales can successfully reduce off-premise liquor consumption, but only for the heaviest-drinking households and in markets with relatively few competitors.

Our analysis exploits a licensure threshold induced by liquor market deregulation in Washington State.[1] From the end of Prohibition through May 2012, the Washington State Liquor Control Board (WSLCB) held a monopoly on spirit sales similar to fourteen other "Alcohol Beverage Control" (ABC) states.[2] In November 2011, Washington became the first, and so far sole, ABC state to privatize sales. In the transition to the new regime, state stores were sold at auction or closed,[3] and private retailers were allowed to enter the market so long as their premises exceeded $10,000\text{ft}^2$. We adopt a regression discontinuity design to leverage this threshold; we compare prices, quantities, product variety, and liquor consumption externalities in markets with an incumbent just above versus just below the size requirement. Our identification argument is that incumbents just above $10,000\text{ft}^2$ are otherwise similar to stores just below, except in their license-eligibility.

We begin by establishing the power of the licensure threshold to estimate causal effects. We show there is little renovation or bunching just above $10,000\text{ft}^2$, suggesting that firms did not game the licensure requirement. There is, however, a large discontinuity in licensure probability at the threshold: a 27 percentage point jump across all stores, and an 86 percentage point jump for chain stores. Importantly, entry by marginally-eligible firms does not fully crowd out larger rivals; while large independent stores are 20% less likely to enter when facing an additional potential competitor nearby, chain store licensure is invariant to rival eligibility. Consequently, "treatment" markets, which have a store just above the $10,000\text{ft}^2$ threshold, boast 0.88 more spirits retailers on average than "control" markets, which have a store just below. We can then attribute differences in outcomes across treatment and control markets to this difference in the number of liquor retailers.

We investigate how prices and quantities respond to rival entry using data from Nielsen's Consumer Panel dataset. We compare purchases by households in ZIP codes with a firm sized just above versus just below $10,000\text{ft}^2$. Our findings indicate that an exogenous increase in the number of retailers increases consumption substantially: by as much as 60% in monopoly markets. Fur-

[1] Defined by Washington State as beverages above 24% ABV

[2] Alabama, Idaho, Maine, Maryland, Mississippi, Montana, New Hampshire, North Carolina, Ohio, Oregon, Pennsylvania, Utah, Vermont, and Virginia.

[3] The WSLCB oversaw approximately 360 outlets.

ther, estimated causal effects exhibit diminishing returns, as in Bresnahan and Reiss (1991). And we find no effect on prices, even in monopoly markets.

Instead, our results indicate that liquor retailers compete in product variety: consumers purchase half as many unique products (UPCs) in monopoly compared to duopoly markets. Our findings highlight the importance of variety in multi-product retail settings, adding to a literature that includes Berry and Waldfogel (2001), McManus (2007), Sweeting (2010), Sweeting (2013), Fan (2013), Eizenberg (2014), and Wollmann (2017). This result contrasts markedly with Berry and Waldfogel (2001), who find that radio station mergers increase product variety. Thus, our findings emphasize that the effect of competition on product variety is ambiguous, as is documented in the theory literature (see Stole (2007)). Importantly, it does not appear that this result is driven by a shift out in the demand curve due to increased convenience, as the marginal entrant is located within 0.1 miles of an existing store.

As a robustness check, we provide auxiliary evidence on pricing and product variety from the Nielsen ScanTrack dataset, which contains point-of-sales data for nine chains that sell spirits in Washington. There is little variation in prices for the same product either across or within chains (perhaps due to retail price maintenance restrictions). In contrast, product assortment varies considerably. On average, 20% of products carried in a single location are not available in larger stores from the same chain. Both the ScanTrak and Panel dataset suggest that firms tailor inventory to local market conditions, including the competitive environment.

The reduced-form approach adopted here complements a large literature examining the interplay between market structure and competition, a central ingredient in anti-trust and merger policy. Dating back to Bertrand and Cournot, theory demonstrates the myriad ways that the number of competitors might affect consumers: through equilibrium prices and quantities, but also other characteristics, such as location (beginning with Hotelling (1929) and Salop (1979)) and product variety (Mussa and Rosen (1978), Spulber (1989), Champsaur and Rochet (1989)). A rich literature employs structural econometric methods to deal with this endogeneity of entry (Bresnahan and Reiss (1991), Berry (1992), Seim (2006), Jia Barwick (2008), Ciliberto and Tamer (2009), among others). In contrast, we utilize variation in market structure created by the licensure discontinuity. One advantage of our approach is that estimation does not requires assumptions that are commonly imposed in structural work, such as the distribution of unobservables, the choice set, and the size of the market. These types of assumptions are particularly unpalatable in competitive second-degree price discrimination settings because they can alter theoretical predictions on the effect of competition on product variety (Stole (2007)).

Our results add to the burgeoning literature investigating the motives and efficiency of state-level liquor regulations across the United States, including Seim and Waldfogel (2013), Conlon and Rao (2015b), Conlon and Rao (2015a) and Miravete et al. (2017). This work complements these previous studies by focusing on documenting the effects of entry restrictions on private liquor market outcomes. Furthermore, our results have practical implications for the simulation of private-system counterfactuals, as in Seim and Waldfogel (2013). Free-entry counterfactuals pose an intractable computational burden because the set of locations and potential entrants is large. Our results suggest a simple heuristic for entry that has not been considered in the literature: using the set of chain stores that sell beer and wine as a proxy for the set of spirits retailers. In Washington's spirits markets, these firms comprise over 90% of retailers, and their entry does not hinge on rivals' entry decisions.

Finally, our paper also contributes to a literature exploiting Washington's deregulation: Seo (2016) analyzes how privatization increases the willingness to pay for liquor by increasing convenience, while Chamberlain (2014) analyzes the effects of increased liquor availability on crime. In contrast to these papers, our chief comparison is across privatized markets with different configurations, rather than between private and state-monopoly systems. In that sense, our work is most similar to Milyo and Waldfogel (1999), who study how advertising affects price competition in liquor markets, and Conlon and Rao (2015b) and Conlon and Rao (2015a), who study the effects of different regulations on private liquor markets.

The rest of the paper proceeds as follows: section 2 introduces our data sources, section 3 describes our empirical strategy and results, and section 4 concludes.

2 Data

2.1 Data on Beer, Wine and Liquor Licensure

Our data on beer, wine and liquor licensure comes from the Washington State Liquor Control Board (WSLCB) off-premise licensee list from January 2013, six months after liberalization. This list contains information on every retailer licensed to sell beer, wine and/or liquor for consumption outside of their store. For each licensee, this list provides the trade name, license number, store address and phone number, and dates for the following events: commence of business operations, liquor license application submission, license issue, license expiration, and (potential) license termination. We therefore observe all liquor licensees through January 2013, including former licensees that already ceased operating. From the WSLCB, we also obtain off-premise liquor revenues (excluding beer and wine sales) and on-premise licensee lists for this period.[4]

Our analysis focuses on the set of beer and wine retailers that began operating before 2012. These licensees compose the set of firms for whom we have a natural experiment on entry into spirits markets, as these firms plausibly did not set square footage in response to the licensure threshold in Referendum I-1193. Our identification strategy, presented fully in Section 3, rests on the assumption that stores sized just above the $10,000\text{ft}^2$ threshold are comparable to those just below. We therefore interpret any discontinuities in outcomes across this threshold as causal effects (for example, of license-eligibility on entry). In contrast, after 2011, the licensure threshold induces a discontinuity in the payoff to square footage for new beer and wine establishments. By revealed preference, new retailers just above the threshold value the spirits licensure option more than those just below. We might therefore suspect other differences between the two groups of new

[4]Including hotels, bars, and restaurants.

Table 1: Summary Statistics for WSLCB Stores

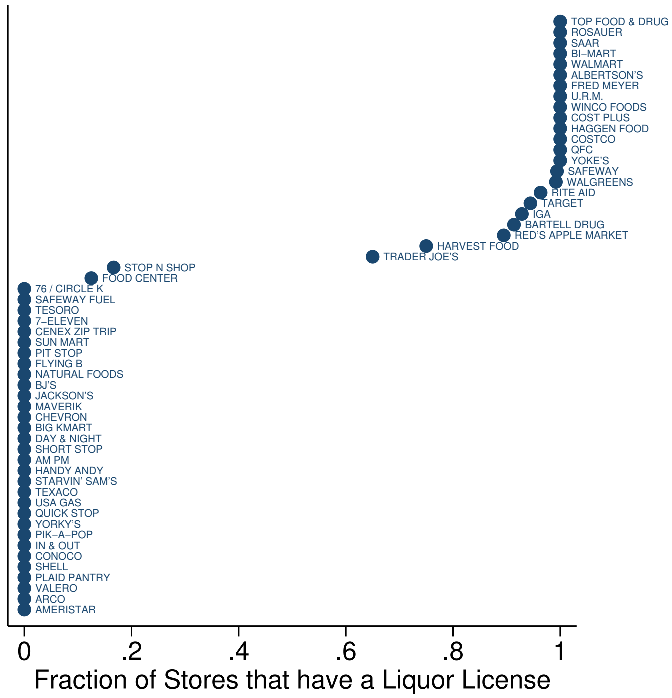
Summary Statistics for Beer, Wine and Liquor Licensure	
Prior to 2012: Beer and wine licensed retailers	4,978
Chain licensees	2,098
At Liberalization: Existing Beer/Wine Licensees	4,977
Liquor-licensed	1,075
Chain liquor licensees	924
At Liberalization: Entrants	570
Liquor-licensed	57
Beer and wine licensed	558
Chain stores	130

entrants. Therefore, we have no purchase on a control group for any establishments built after the licensure threshold is introduced, even when they are near the threshold.

Table 1 presents summary statistics for licensees over time. There are 4,978 beer and wine licensed retailers in December 2011, of which 2,098 are chains. At liberalization, on June 1st of 2012, 4,977 of these stores were still operating, and 1,075 of them obtained liquor licenses. Most of these entrants are chains (924 of 1,075). Our focus on existing beer and wine resellers captures the lion's share of entrants into Washington's nascent spirit market. While 570 new alcohol retailers enter during 2012, a mere 57 sell spirits. That is, only 5% of spirits retailers fall outside of our potential entry sample. Low levels of realized entry by stores that were not selling any alcohol prior to 2012 make us confident that the set of stores that we consider captures the majority of potential entrants.

An important characteristic of liquor retailers is their chain identity. We denote chains as groups of at least two stores in different locations with the same store name. Most chains are either fully spirits licensed or completely out of the spirits market, as Figure 1 shows. The smallest chain has 2 locations, the median chain has 12 locations, and the largest chain (7-Eleven) has 242 stores. Appendix Figure A.1 reports chain names and sizes (in number of stores) for all chains with 5 or more stores. Overall, there are 2,098 chain stores in the sample, and 44% of them obtain a

Figure 1: Chain Licensure



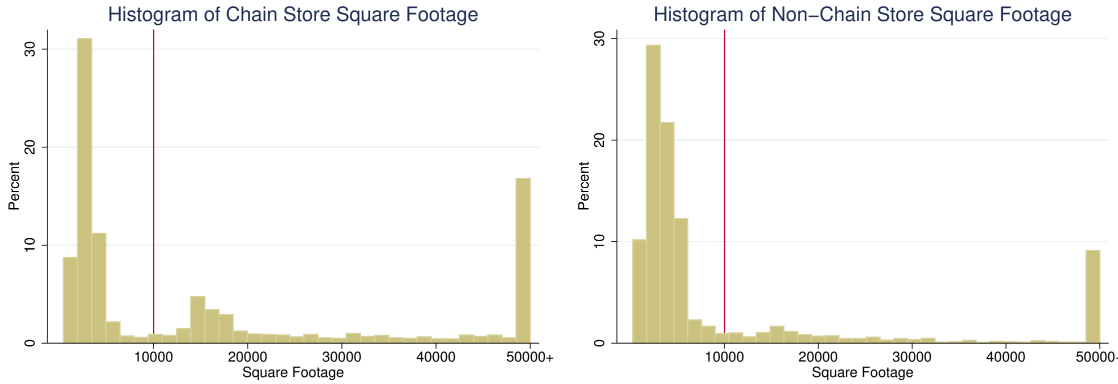
liquor license. Chains that never sell spirits, such as gas stations and convenience stores, typically feature formats that are quite small. In contrast, large format retailers, like Costco and Safeway, are always in. Variation in licensure is highest for chains of small grocery stores, like Trader Joe's. In section 3, we document that chain stores are close to perfect compliers, as the probability they sell liquor jumps from nearly 0 to 1 at the licensure threshold. In what follows, we will use the term "independent" stores to refer to non-chain stores.

2.2 Data on Square Footage

We employ Google Map Developers' Area Calculator to measure square footage for the stores in our sample.[5] This application overlays a tool for calculating square footage on top of Google Maps' satellite images. Figure D.1 in Appendix D presents an example of how we use the appli-

[5] `https://www.mapdevelopers.com/area_finder.php`

Figure 2: Histogram of Store Sizes



(a) Chain Stores (b) Independent Stores

cation to calculate store area. To obtain data for all 4,978 stores in our sample, in May of 2017 we hired Amazon Mechanical Turk (MTurk) workers to measure each store's square footage, a task that would otherwise have been prohibitively time-consuming. Appendix D details our procedure in the hopes that it may prove useful to other research requiring extensive data-gathering.

Figure 2 presents histograms of retailer sizes separately for chain and independent stores. Both distributions are skewed towards small formats, which are typical of gas stations and convenience stores. Overall, 73% of our sample consists of stores below $10,000\text{ft}^2$, which are not license-eligible. Chain stores are larger than independents, but the majority (54.6%) are still below the licensure threshold.

2.3 Data on Liquor Prices and Quantities

Our data on liquor sales comes from the 2010-2015 Nielsen Consumer Panel Dataset hosted by the Kilts Center. The data comprises all transactions for a revolving panel of households in the United States, including 2,700 households in Washington State over this time period. Our identification strategy, discussed in depth in section 3, compares outcomes in markets with stores below and above the liquor licensure size threshold. Our analysis therefore focuses on households

Figure 3: Panelist Summary Statistics

Panelist Summary Statistics					
		Mean	SD	Min	Max
WSLCB Stores before 06/2012		0.10	0.30	0	1
Number of Beer & Wine Licensees	Operating in 2011	22.18	10.96	1	51
	Selling Liquor in 2012	5.43	2.52	0	11
	5k - 15k ft^2	1.52	0.79	1	4
	10k - 15k ft^2	1.02	0.80	0	4
Monthly Liquor	Purchase Probability	0.09	0.28	0	1
	Total Expenditures ($)	6.00	31.35	0	558.71

Notes: Sample is 1,138 households who reside in a Washington State zip code with at least one chain store sized 5,000-15,000 ft^2 in 2012-2015.

who reside in zip codes with at least one chain store sized near the threshold, i.e. those between $5,000-15,000\text{ft}^2$. This includes some 141 zip codes and 1,138 households. Table 3 displays summary statistics for the relevant set of households, including the liquor selling configuration in their home zip code. As an example, only 10% live in a zip code that had a WSLCB store under the state monopoly, but these panelists average nearly 5.4 liquor retailers within their zip code after deregulation.

To calculate liquor consumption, we restrict attention to products in the Nielsen alcohol module that also qualify under the WSLCB definition of liquor. See appendix C for details on sample construction. The summary statistics in table 3 reveal that liquor purchasing is highly skewed. The average household spends only $6.00 on spirits per month, but the standard deviation of expenditures is $31.35. This pattern motivates our investigation of how the incidence of heavy drinking changes with market structure in Section 3.

Nielsen selects households to mirror the demographics of the overall United States population, each census-region, and several major markets (including Seattle). These demographics include race, household size, income, and head-of-household age. Table 4 includes a side-by-side display of the Washington households in our sample and state residents. A similar proportion of both

Figure 4: Demographics of Panelists versus Population for Washington State

Demographics of Panelists vs State Population		
Demographic	Consumer Panel	State
% White	83.5	82.5
<u>% Income</u>		
< 25k	15.7	20.3
> 100k	16.4	24.4
<u>% Education</u>		
< HS	3.7	10.6
HS	18.8	24.0
BA +	44.5	29.5

Notes: Data on the Washington State population comes from the 2010 census. Education is for male heads of household from the Consumer Panel. Panelist sample is 1,138 households who reside in a zip code with at least one chain store sized 5,000-

groups are white, but Nielsen households tend to be more educated (a higher fraction have earned a bachelors or beyond). The income distribution for Nielsen households is also more flat, as a lower proportion of panelists earn less than $25,000 or more than $100,000. Our analysis therefore speaks more to the median household, rather than to the richest or poorest Washington residents.

3 Empirical Strategy and Results

In this section, we present three sets of results: first, we document that the licensure restriction is binding, and that firms do not appear to game the size threshold. Second, we demonstrate how entry decisions and revenues respond to the license-eligibility of neighboring stores. Finally, we estimate how consumption, pricing, product variety and liquor consumption externalities change with the number of stores, using the licensure threshold as an instrument.

3.1 License Eligibility, Entry, and Liquor Revenue

3.1.1 Empirical Strategy

In this subsection we describe our estimation strategy, which leverages the discontinuity of license eligibility in store size at $10,000\text{ft}^2$. Ex ante, it is unclear whether stores just above $10,000\text{ft}^2$, which are marginally-eligible, will choose to sell spirits. For instance, large firms may deter entry by marginal firms, or marginally-eligible firms may face prohibitively high liquor acquisition costs as they may have little bargaining power with distributors. If the licensure threshold were not binding, then we would not expect it to affect market structure or outcomes. However, our results indicate that a stores just above $10,000\text{ft}^2$ are indeed more likely to obtain a liquor license than a store just below, so that the discontinuity in eligibility generates a discontinuity in entry.

Our basic model for estimating the effect of eligibility on entry is:

$$
\begin{aligned}
1\left[\text{Liquor Licensed}\right]_s = \alpha_0 + \alpha_1 \cdot 1\left[SqFt_s \geq 10,000\right]_s + \alpha_2 \cdot SqFt_s \\
+ \alpha_3 \cdot 1\left[SqFt_s \geq 10,000\right]_s \times SqFt_s + \varepsilon_s
\end{aligned}
\tag{1}
$$

where $1\left[\text{Liquor Licensed}\right]_s$ and $SqFt_s$ are a liquor licensure indicator variable and the square footage of store s, respectively. We are mainly interested in the coefficient on $1\left[SqFt_s \geq 10,000\right]_s$, an indicator variable for square footage above $10,000\text{ft}^2$, which captures any change in the likelihood of licensure at that threshold. The exclusion restriction that permits a causal interpretation of the discontinuity estimate is that stores sized close to $10,000\text{ft}^2$, but on different sides of the cutoff, are otherwise identical in expectation. For stores near the $10,000\text{ft}^2$ cutoff established before Referendum I-1193 introduced the threshold rule, being above or below the threshold should be as good as random.

One concern with this approach is that firms might game the licensure threshold, for example by building an annex. This behavior would create a selection problem, as only stores that enjoy profits from liquor sales would choose to expand. To test for manipulation of square footage, we examine whether there is bunching above $10,000\text{ft}^2$. Table 2 presents the results of a McCrary test (McCrary (2008)) for manipulation of the running variable around the threshold. For all specifications, we can reject the hypothesis that there is a discontinuity in the density of store square footage at $10,000\text{ft}^2$ at the 5% level.

We also analyze whether store characteristics are balanced around the licensure threshold. If stores just below $10,000\text{ft}^2$ differ from stores just above on dimensions correlated with liquor demand, then these stores would serve as a poor control group. We present results from these tests in appendix B. Overall, the results from these regressions leave us confident in the validity of our exclusion restriction.

3.1.2 Results on Entry Probabilities and Liquor Revenues

We present estimates of the licensure discontinuity at $10,000\text{ft}^2$ in Table 2. We employ a local linear regression discontinuity design model with robust, bias-corrected standard errors and an optimal bandwidth as in Calonico et al. (2014).[6]. There is a 38 percentage point jump in the probability of licensure at $10,000\text{ft}^2$ (column 1). The regulation binds for roughly 30% of stores near the threshold, but we learn that selling liquor is not profitable for all eligible firms; the probability of licensure above the threshold is approximately 40%, well below full compliance. We note that the likelihood of licensure is approximately 10 percentage point for stores just below, indicating that some measurement error in square footage remains (as these retailers must, in fact, be larger than $10,000\text{ft}^2$). In columns 2 and 3 of Table 2 we report estimates separately for chain and independent stores. Chain and independent stores may behave differently, as a portion of the fixed costs of spirits sales are likely to be sunk for chain stores. As an example, chains may be able to exploit established relationships with suppliers and distributors. Indeed, column 4 shows that the discontinuity for chain stores is 86 percentage points, statistically indistinguishable from perfect

[6]Estimated in Stata using the rdrobust command (Calonico et al. (2017))

Table 2: Regression Discontinuity Estimates of the Effect of License Eligibility on Entry

RD Estimates of the Effect of Licensure on Entry				
	(1)	(2)	(3)	(4)
	All Stores	Independent Stores	Chain Stores	Large Chains (10+ Stores)
Licensure Discontinuity	0.26**	-0.03	0.86***	0.88***
	(0.112)	(0.133)	(0.153)	(0.160)
Observations	4605	2599	2006	1870
Effective Observations – Below	194	102	103	23
Effective Observations – Above	130	87	55	40
Bandwidth	4149.9	3634.8	3397.6	2867.5
McCrary Test P-Value	0.379	0.620	0.545	0.981

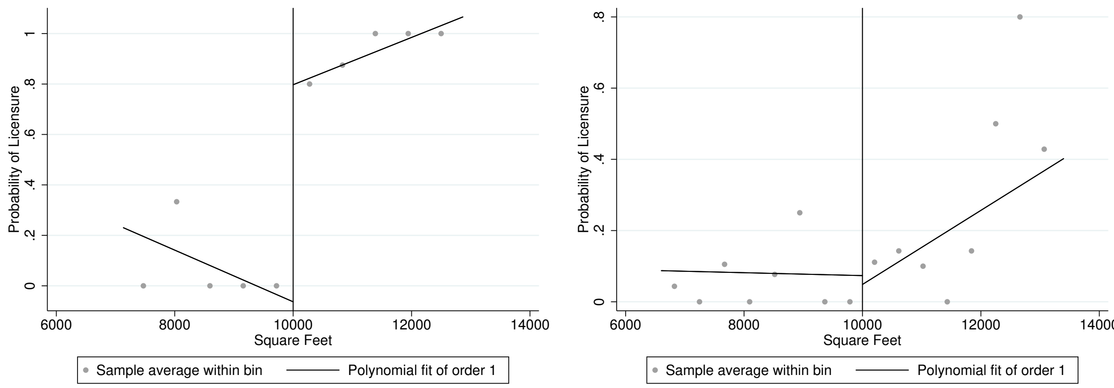
Notes: This table presents results of a local polynomial regression-discontinuity design model with robust bias-corrected confidence intervals and a MSE-optimal bandwidth, estimated in Stata via the “rdrobust” command using techniques in Calonico, Cattaneo and Titiunik (2014), Calonico, Cattaneo and Farrell (2016) and Calonico, Cattaneo, Farrell and Titiunik (2016). Licensure Discontinuity denotes the estimated change in licensure probability at the 10,000 square foot cutoff. Column 1 reports this estimated quantity for all stores in our sample. Column 2 considers only stores in cities where there is more than one alcohol-selling outlet. Column 3 considers only non-chain stores, while column 4 only considers chain stores and Column 5 considers only chain stores for chains with 10 stores or more. The row labelled “McCrary Test p-value” presents the p-value of a McCrary test of the density of the running value around the 10,000 square foot cutoff. Robust, bias-corrected standard errors in parentheses. Coefficients are significant at the * 10%, ** 5% and *** 1% levels.

compliance. Figure 5 plots the predicted probability of licensure for chain stores, the graphical analogue the estimates in Table 2. Both the plot and the estimates suggest that for chain stores, the licensure threshold forecloses stores that almost surely would enter absent regulation. Column 5 in Table 2 further restricts the sample to chains with 10 or more stores in Washington, with no significant change in the estimated licensure discontinuity. Figure A.2 in the appendix presents the predicted licensure probability plot for this subsample.

In contrast, independent store licensure exhibits no discontinuity at the threshold (column 3). Measurement error does not appear to cause this result; as shown in figure 5, the licensure probability hovers around 10% on both sides of the cutoff. Therefore, we conclude that the licensure threshold does not exclude independent stores from spirits sales.

To understand why independent stores just above $10,000\text{ft}^2$ have such a low entry probability, in table 6 we present results obtained from estimating equation 1 using total liquor revenues in 2012 as the outcome variable. For independent stores, the estimated change in liquor revenues induced by license eligibility is very small: approximately $2,000 for the seven month period in 2012 when spirits retail first becomes legal. These results suggest that spirits are relatively unprofitable for

Figure 5: Probability of Spirits Licensure by Store Size



(a) Chain Stores (b) Independent Stores

marginally-eligible independent stores, as profits are bounded above by these revenue estimates. The cost of spirits sales are also potentially large, as they include acquisition costs, taxes, and the opportunity cost of the shelf space. For chain stores, the sales discontinuity is larger, on the order of $70,000 for the same period. Chain entry appears more profitable than independent entry, both because chains earn higher revenues, but also because independent outlets may face fixed costs that are already sunk for the marginally-eligible chain outlet.

3.2 Neighbor License Eligibility and Entry

3.2.1 Empirical Strategy

In this section, we present evidence on firm responses to the license eligibility of their neighbors. That is, we examine whether, and to what extent, marginally license-eligible stores crowd out rival entry. Rival entry teaches us about the intensity of competition in spirits markets, a central ingredient in understanding how the licensure restriction affects consumers. At one extreme, shifting a store from below to above the threshold might drive out another potential entrant, preserving the total number of spirits retailers. In this case, the licensure restriction may affect consumers by shifting the composition of liquor retailers. On the other hand, if there is no crowd-out, then

Figure 6: Regression Discontinuity Estimates of the Effect of License Eligibility on Liquor Revenues

RD Estimates of the Effect of Licensure and License-Eligibility on Liquor Sales				
	(1)	(2)	(3)	(4)
	All Stores	Independent Stores	Chain Stores	Large Chains (10+ Stores)
License-Eligibility	26,164	2,290	71,538**	72,747**
	(17,707)	(14,523)	(32,898)	(33,284)
Observations	4,605	2,599	2,006	1,973
Effective Observations – Below	167	288	27	24
Effective Observations – Above	123	81	49	47
Bandwidth	4016.2	4723.5	3264.9	3195.8

Notes: This table presents results of a local polynomial regression-discontinuity design model with robust bias-corrected confidence intervals and an MSE-optimal bandwidth, estimated in Stata via the "rdrobust" command using techniques in Calonico, Cattaneo and Titiunik (2014), Calonico, Cattaneo and Farrell (2016) and Calonico, Cattaneo, Farrell and Titiunik (2016). Panel B reports the results of a regression discontinuity design with total liquor sales in 2012 (7 months) as the outcome variable, square footage as the running variable, and 10,000 square feet as the cutoff. Column 1 reports this estimated quantity for all stores in our sample. Column 2 considers only independent stores, while column 3 only considers chain stores and Column 5 considers only chain stores for chains with 10 stores or more. Robust, bias-corrected standard errors in parentheses. Coefficients are significant at the * 10%, ** 5% and *** 1% levels.

the licensure restriction induces exogenous variation in the number of firms across markets; those markets with a marginally-eligible firm would have an additional retail outlet compared to those with a marginally-ineligible firm.

To estimate how entry decisions depend on neighbor configurations, we employ an regression discontinuity-style argument, similar to equation 1. Our concern is that firms select locations in response to (potentially unobservable) market conditions, so we cannot simply compare stores with more or fewer competitors to establish causal effects. Instead, we condition on the number of competitors sized $5,000-15,000\text{ft}^2$, and compare firms with a different number above versus below the threshold. The exclusion restriction is that conditional on the number of rivals between $5,000$ and $15,000\text{ft}^2$, how many fall above or below the threshold is orthogonal to unobserved firm and market characteristics that affect entry decision. Our goal is to determine whether, and to what extent, a store that faces an additional potential competitor is less likely to sell spirits.

A challenge in this exercise is determining the relevant set of rivals for each license-eligible store s. We construct two sets of potential rivals: stores within a certain distance d of store s, and the n-nearest neighbors to store s. We present the methodology and results of the latter approach in appendix E as the results are quite similar. The distance-based regressions employ the following model:

$$
\begin{aligned}
1\left[\text{Has Liquor License}\right]_s = \alpha_0 + \alpha_1 \cdot 1\left[IsChain\right]_s + \alpha_2 \cdot N_s^{d,10-15} \\
+\alpha_3 \cdot 1\left[IsChain\right]_s \cdot N_s^{d,10-15} + \sum_k \lambda_k^d \cdot 1\left[N_s^{d,5-15} = k\right] + \varepsilon_s
\end{aligned} \tag{2}
$$

where $1\left[IsChain\right]_s$ is an indicator variable for whether store s belongs to a chain, $N_s^{d,10-15}$ is the number of stores within d miles of store s sized between $10,000 - 15,000\text{ft}^2$, and $N_s^{d,5-15}$ is the number of stores within d miles of store s sized $5,000 - 15,000\text{ft}^2$, so that λ_k^d is a fixed effect for stores that have k competitors within d miles sized $5,000 - 15,000\text{ft}^2$. We are interested in the coefficients α_2 and α_3, which capture the effect of rival eligibility on entry. The own-entry regressions results in table 2 suggest that chain and independent stores behave differently, so we allow for the effect on rival entry to be different for chains (α_3). Across all specifications, standard errors are clustered at the zip code level.

One advantage of the discontinuity design is that it recovers the causal estimates of rival eligibility on own entry decisions, but does not require full specification of each store's relevant set of competitors. In particular, equation 2 does not embed an assumption that store s competes only with stores sized $10,000 - 15,000\text{ft}^2$. Our argument is simply that the effects of all other factors, including larger competitors, is orthogonal to the number of stores just-above the cutoff, conditional on the total number of stores in the bandwidth.

As before, in Appendix B we present evidence that stores with a rival just-above versus just-below $10,000\text{ft}^2$ are similar on observables, to support the exclusion restriction outlined above. Taken together, the regression results in this Appendix lend support to validity of the licensure threshold strategy for identifying rival entry.

3.2.2 Results

We present estimates of neighbor license-eligibility on own entry decisions in table 3, which correspond to equation 2. As in table B.3, each column corresponds to a different radius around the store. Rows 1 and 2 (3 and 4) include results for independent (chain) stores. We split the sample because we have already determined that entry decisions differ for these two groups. Our results indicate that neighbor eligibility only impacts independent stores: an additional license-eligible competitor reduces the entry probability by around 20 percentage points if the rival store is within 0.2 miles, for example. The effect falls to around 10 percentage points for neighbors within 0.5-0.6 miles, and is indistinguishable from 0 for distances larger than 0.9 miles. These magnitudes are large: a 20 percentage point drop corresponds to a two-thirds reduction in the likelihood of spirits licensure. Moreover, competition appears fairly localized, as the estimated magnitude quickly dissipates as the distance bandwidth grows. In contrast, chain stores are insensitive to their neighbors' eligibility: the estimated effect of an additional eligible rival are both statistically and economically insignificant. This result dovetails with the full compliance finding in the previous section: to first order, license-eligible chain stores always enter. We replicate this analysis for the n-nearest neighbor metric in appendix E. The results are consistent with those presented here.

These results have implications on how best to simulate a free entry counterfactual in other states where liquor sales are currently administered by a state monopoly. Seim and Waldfogel (2013) describe the computational burden of simulation-based approaches, which stems from the large set of potential store locations and players. They propose a sequential myopic algorithm because the full problem is intractable. Our results suggest another simple rule: the set of chain stores that carry beer and wine proxy well for the set of spirits retailers under privatization.

Although chain entry is invariant to rival eligibility, their revenues are affected by market structure. Table 4 presents estimates of specification (2) using 2012 liquor revenues as the outcome variable.[7] These results indicate that an additional eligible rival increases liquor revenues in 2012

[7] Unconditionally (including zeros).

Table 3: Effect of License Eligibility of Nearby Stores on Own Entry Decisions

Effect of the License Eligibility of Nearby Stores on Own Entry Decisions Bandwidth = 5000 square feet											
stance to Store (miles):		0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
Independents	# of Marginally License Eligible Neighbors	-0.158	-0.218***	-0.181***	-0.170***	-0.114**	-0.102***	-0.064*	-0.067*	-0.027	-0.045
		(0.107)	(0.068)	(0.058)	(0.044)	(0.046)	(0.035)	(0.038)	(0.034)	(0.033)	(0.029)
	Baseline Entry Probability	0.323***	0.340***	0.345***	0.354***	0.349***	0.354***	0.346***	0.351***	0.341***	0.354***
		(0.025)	(0.025)	(0.026)	(0.027)	(0.027)	(0.027)	(0.028)	(0.029)	(0.030)	(0.031)
Chains	# of Marginally License Eligible Neighbors	0.073	0.012	-0.002	-0.009	-0.007	-0.001	0.002	0.014	0.012	0.002
		(0.051)	(0.036)	(0.036)	(0.032)	(0.027)	(0.021)	(0.021)	(0.019)	(0.017)	(0.016)
	Baseline Entry Probability	0.948***	0.951***	0.953***	0.954***	0.954***	0.952***	0.951***	0.947***	0.945***	0.950***
		(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.011)	(0.011)	(0.012)
# of Neighbors in the Bandwidth FE		x	x	x	x	x	x	x	x	x	x
N		1173	1173	1173	1173	1173	1173	1173	1173	1173	1173

Notes: This table presents results of a linear regression of a licensure dummy on a constant and the interaction between a chain store dummy and the number of neighbors who are within the relevant distance and who are above the 10,000ft^2 licensure threshold, but below 15,000ft^2. All specifications include fixed effects for the total number of stores 5,000-15,000ft^2 and who are also within the relevant distance. The sample is restricted to stores who are not former state liquor stores, are eligible to sell liquor, and have at least one neighbor within the relevant distance. Robust standard errors with clustering at the zip code level in parentheses. Coefficients are significant at the * 10%, ** 5% and *** 1% levels.

by around \$50,000 or 20%. The results for independent stores are noisier, but also point in the direction of higher revenues when facing additional competition.[8] This result is consistent with pricing at the elastic part of the demand curve, where marginal revenue is positive. Alternatively, these revenue effects could indicate that stores with few eligible competitors strategically withhold products that are valued by consumers. In the next section, we employ household-level purchasing data to better understand this competitive response.

3.3 Effect of License Eligibility on Liquor Sales

3.3.1 Empirical Strategy

In this section we adapt the previous RD-style argument to estimate the causal effect of market structure on prices and quantities. Our regression of interest specifies how a purchasing outcome y

[8]The results for the n-nearest neighbor metric are consistent with the results presented here, and are reported in appendix E

Table 4: Effect of License Eligibility of Nearby Stores on Own Liquor Revenue

	Effect of the License Eligibility of Nearby Stores on Own Sales of Liquor Bandwidth = 5000 square feet										
	istance to Store (miles):	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
Independents	# of Marginally License Eligible Neighbors	74,164*** (26,994)	26,344 (19,522)	9,939 (17,099)	23,061 (19,248)	17,613 (21,375)	15,317 (14,965)	30,489* (15,994)	15,789 (13,241)	27,740** (12,096)	12,942 (10,543)
	Baseline Sales	28,600*** (4,616)	29,876*** (5,086)	30,960*** (5,407)	29,757*** (5,148)	29,293*** (5,402)	30,063*** (5,129)	24,697*** (5,773)	28,024*** (5,607)	23,001*** (5,815)	27,330*** (6,161)
Chains	# of Marginally License Eligible Neighbors	125,407*** (33,004)	60,278** (26,185)	52,321** (24,356)	48,610** (24,185)	65,412*** (22,696)	48,615** (19,144)	45,496*** (17,682)	36,894** (14,994)	40,954*** (13,958)	35,933*** (13,100)
	Baseline Sales	245,564*** (9,374)	246,837*** (9,778)	245,818*** (10,184)	243,644*** (10,434)	238,400*** (10,085)	238,942*** (10,302)	236,633*** (10,476)	237,725*** (10,786)	232,210*** (9,714)	233,325*** (9,981)
# of Neighbors in the Bandwidth FE		x	x	x	x	x	x	x	x	x	x
	N	1173	1173	1173	1173	1173	1173	1173	1173	1173	1173

Notes: This table presents results of a linear regression of a licensure dummy on a constant and the interaction between a chain store dummy and the number of neighbors who are within the relevant distance and who are above the 10,000ft^2 licensure threshold, but below 15,000ft^2. All specifications include fixed effects for the total number of stores 5,000-15,000ft^2 and who are also within the relevant distance. The sample is restricted to stores who are not former state liquor stores, are eligible to sell liquor, and have at least one neighbor within the relevant distance. Robust standard errors with clustering at the zip code level in parentheses. Coefficients are significant at the * 10%, ** 5% and *** 1% levels.

for household h in month t changes with the number of firms in h's home zip code, denoted $z(h,t)$:[9]

$$y_{ht} = \alpha_0 + \alpha_1 \cdot NL_{z(h,t)} + \alpha_2 \cdot NL^2_{z(h,t)} + X'_{z(h,t)}\delta + \varepsilon_{ht} \tag{3}$$

where $NL_{z(h,t)}$ is the number of liquor outlets and $X_{z(h,t)}$ includes any market-level control variables. The quadratic term for the number of liquor outlets allows for diminishing returns to the number of competitors, as in Bresnahan and Reiss (1991). Because the number of liquor stores in a zip code is likely correlated with demand and cost unobservables, we construct instruments for $NL_{z(h,t)}$ and $NL^2_{z(h,t)}$ using the licensure threshold. In particular, we condition on the number of stores within a zip code sized $5,000-15,000\text{ft}^2$, and then employ the number sized $10,000-15,000\text{ft}^2$ as an instrument. The essence of our identification assumption is that unobserved demand and cost characteristics are similar between treatment markets (those with a store sized just above the licensure threshold) and control markets (those with a store just below the threshold). Any differences in outcomes across these markets we therefore attribute to differences in the number of spirits retailers.

[9] Approximately 5.7% of households switch zip codes at least once between 2010 and 2015.

The first stage regression specifies the number of liquor outlets in zip code z at time t as:

$$\begin{aligned} NL_{z(h,t)} = & \pi_0 + \pi_1 \cdot N^{10-15}_{z(h,t)} + \sum_i \tilde{\pi}_i \cdot N^{10-15}_{z(h,t)} \times 1\left[N^{15+}_{z(h,t)} = i\right] \\ & + \sum_k \lambda_k \cdot 1\left[N^{5-15}_{z(h,t)} = k\right] + \sum_j \gamma_j \cdot 1\left[N^{15+}_{z(h,t)} = j\right] + \varepsilon_{z(h,t)} \end{aligned} \quad (4)$$

where the regressor of interest is $N^{10-15}_{z(h,t)}$, the number of pre-existing chain stores sized $10,000-15,000\text{ft}^2$. Controls include indicator variables for $N^{5-15}_{z(h,t)}$, the number of chain stores sized $5,000-15,000\text{ft}^2$, so that λ_k is a fixed effect for zip codes that had k beer/wine licensees sized $5,000-15,000\text{ft}^2$ in 2011. We also include fixed effects for the number of large stores (above $15,000\text{ft}^2$) in the zip code ($N^{15+}_{z(h,t)}$). Finally, we allow for interactions between indicator variables for the number of large stores and the number of stores just above the threshold ($N^{10-15}_{z(h,t)}$). These interaction terms exploit variation in how pre-determined market characteristics mediate the effect of a marginally-eligible entrant, and provide additional instruments for the number of spirits retailers and its square.

Results of the first stage estimates are presented in panel C of table 5, separately for our two endogenous variables. In zip codes without a large beer or wine outlet, shifting a firm from just-below to just-above $10,000\text{ft}^2$ leads to an additional 0.88 liquor retailers. We do not report the full set of interactions out of space considerations, but the effects are economically smaller and statistically distinct in zip codes with large retailers (above $15,000\text{ft}^2$), which is consistent with crowd-out. The partial F-statistics are 15.77 and 16.88 for the number of liquor stores and its square, respectively.

One concern is that these instruments might affect market outcomes through channels beyond the number of retailers. For example, shifting a firm above the licensure threshold could elicit an entry deterrence response by larger firms. In that case, the two-stage least squares exclusion restriction will not hold, as license eligibility would affect entry into liquor sales not only through the number of firms. However, under this concern the reduced form, which captures the net causal effect of license-eligibility on equilibrium outcomes, is still valid. Therefore, we also provide results from the reduced form equation, and argue that their similarity with the two-stage least squares results alleviates this concern.

$$y_{ht} = \beta_0 + \beta_1 \cdot N_{z(h,t)}^{10-15} + \beta_2 \cdot N_{z(h,t)}^{10-15} \times N_{z(h,t)}^{15+} + \sum_k \lambda_k \cdot 1\left[N_{z(h,t)}^{5-15} = k\right] + \sum_j \gamma_j \cdot 1\left[N_{z(h,t)}^{15+} = j\right] + \varepsilon_{ht} \tag{5}$$

where β_2 captures the interaction between the number of stores just above the threshold and the number of large stores. In what follows, the economic magnitudes of the reduced form results are in line with the magnitudes of the two stage least squares, so we do not revisit this point further.

Our estimation strategy in equations 4 and 5 employs store characteristics within a household's zip code as explanatory variables. While it is typical in the IO literature to group consumers into larger markets, such as metropolitan areas, we look more narrowly for three reasons: first, there are relatively few cities within Washington state; second, earlier work demonstrates that most consumers shop within a few miles of home (Ver Ploeg et al. (2015)); and third, our results on rival entry suggest that firms beyond 0.6 miles distance have limited impact on rivals' decisions in this context. To be clear, we do not assume that consumers shop within their own zip code, but instead test whether the market structure within a household's home zip code affects its liquor purchases. This is important, as it frees us from having to define markets and specify choice sets. Finally, since our natural experiment induces variation at the zip code level, but the data lives at the household level, we cluster standard errors by zip code.

Before turning to estimates of equation (4), we first test whether zip codes (and the Neilsen Panelists that live in them) with a marginally eligible store (just above $10,000\text{ft}^2$) differ on observable characteristics than those with a marginally ineligible store (just below $10,000\text{ft}^2$).[10] Our aim is to provide suggestive evidence on the exclusion restriction that identifies equation (4). The results of this exercise are presented in Appendix B. Overall, zip codes and Nielsen households

[10] In fact, we compare zip codes with the same number of stores in the neighborhood of $10,000\text{ft}^2$ but a different number just above versus just below.

do not appear to be significantly different when we hold the number of stores around the licensure threshold fixed and vary the number of stores just above versus just below. Interestingly, this is also true for pre-period liquor consumption.

3.3.2 Quantity Effects

Table 5 shows estimates of how an additional firm affects quantities, which we measure three ways: liquor expenditures (in dollars), volume purchased, and ethanol purchased (both in liters). These outcomes correspond to columns 1, 2, and 3, respectively. All outcomes are measured in levels rather than logs because most households purchase liquor less than once a month. Panel A presents estimates of specification (4). Across the board, the coefficient estimate on the number of liquor outlets is positive and statistically significant at the 5% level, while the quadratic term is negative and also significant. As an example, our estimates imply that the first liquor outlet in a zip code increases average liquor expenditures by \$5.80 per month, or approximately 0.2 liters. These effects are large, as they amount to an increase of approximately 74% over baseline spending (\$7.88) and consumption (0.27 liters). However, they exhibit strongly diminishing returns. The switch from duopoly to triopoly constitutes a 52% expenditure boost (or \$4.12). Figure 7 displays results graphically for a range of market configurations. The marginal affect of an additional liquor outlet is statistically indistinguishable from zero when there are five or more stores.

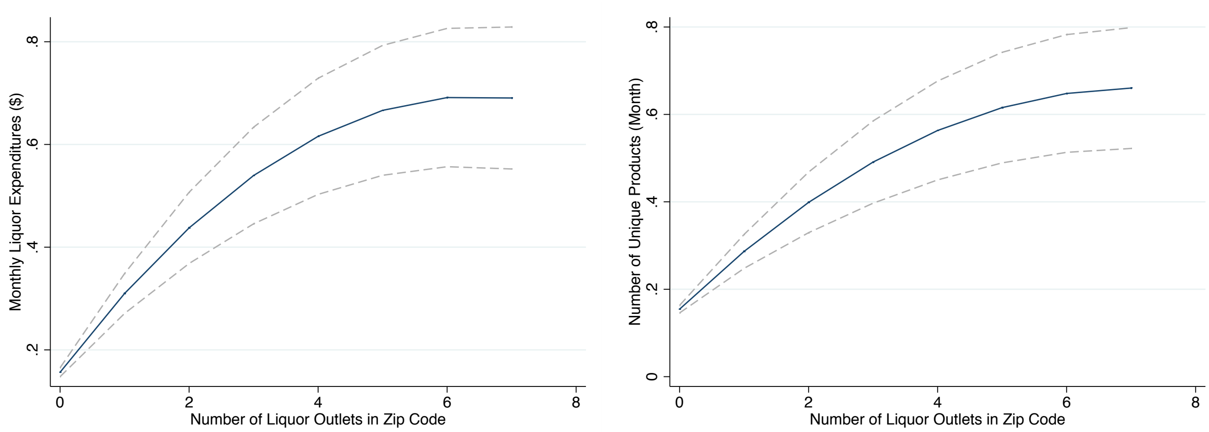
Panel B displays the estimates of the reduced-form analogue to the market configuration regressions, which corresponds to specification (5). The magnitudes and point estimates are very similar to those in panel A, which is consistent with near full compliance for chains. As an example, the estimates in panel C suggest that in markets without any large stores, shifting a beer/wine licensed chain from below to above the $10,000\text{ft}^2$ threshold leads to a 0.883 chance of an additional liquor outlet. As there is very little entry by new firms in 2012, this additional liquor outlet typically constitutes a local monopoly. Column 1 in table 5 indicates the shift also increase expenditures by \$6.25 in these markets. While we prefer the 2SLS estimates for ease of interpretation, it is

Table 5: Effect of License Eligibility on Purchasing

Effect of Market Structure on Consumption			
	(1)	(2)	(3)
Panel A: IV	Expenditures ($)	Volume (L)	Ethanol (L)
# of Liquor Retailers	4.605***	0.208**	0.086**
	(1.659)	(0.091)	(0.036)
# of Liquor Retailers^2	-0.322***	-0.014**	-0.006**
	(0.124)	(0.007)	(0.003)
# of Stores in the Bandwidth FE		X	X
# of Stores Above the Bandwidth FE	X	X	X
Mean	5.305	0.275	0.109
Observations	31875	31875	31875
Panel B: Reduced Form	Expenditures ($)	Volume (L)	Ethanol (L)
# of Marginally License-Eligible Stores	4.002**	0.223**	0.094**
	(1.631)	(0.101)	(0.040)
# of Marginally License-Eligible Stores × # Stores Above the Bandwidth	-0.457***	-0.026***	-0.011***
	(0.153)	(0.009)	(0.004)
# of Stores in the Bandwidth FE	X	X	X
# of Stores Above the Bandwidth FE	X	X	X
Observations	31875	31875	31875

Notes: Observations are at the panelist-month level for 06/2012-12/2015. Standard errors are clustered at the zip code level, and coefficients are statistically significant at the *10%, **5%, and ***1% level. Instruments in panel A are interactions between the # of marginally eligible firms and the # of stores above 15,000 ft^2. Partial F-statistics in Panel A are 15.57 for # liquor retailers and16.82 for # liquor retailers^2.

Figure 7: Effect of Number of Firms on Market Outcomes



Notes: Dashed lines indicate 95% confidence intervals based on standard errors from a nonparametric bootstrap.

reassuring that the effects appear stable across specifications.

These results illustrate how spirits purchases change as the number of retailers grows, which is particularly relevant to policy-makers concerned about negative externalities. Indeed, the WSLCB adopted the licensure threshold that we exploit precisely to curb liquor consumption. Some feared that if every corner store could sell hard liquor - potentially increasing convenience and lowering prices - the resultant bump in consumption could increase DUIs.[11] We find at least some basis for these fears; in markets with fewer stores, consumption does increase markedly with an additional outlet (for example, a move from monopoly to duopoly increases liquor consumption by 63%). However, this effect diminishes quickly as the number of stores increases, suggesting that licensure restrictions are a blunt policy instrument.

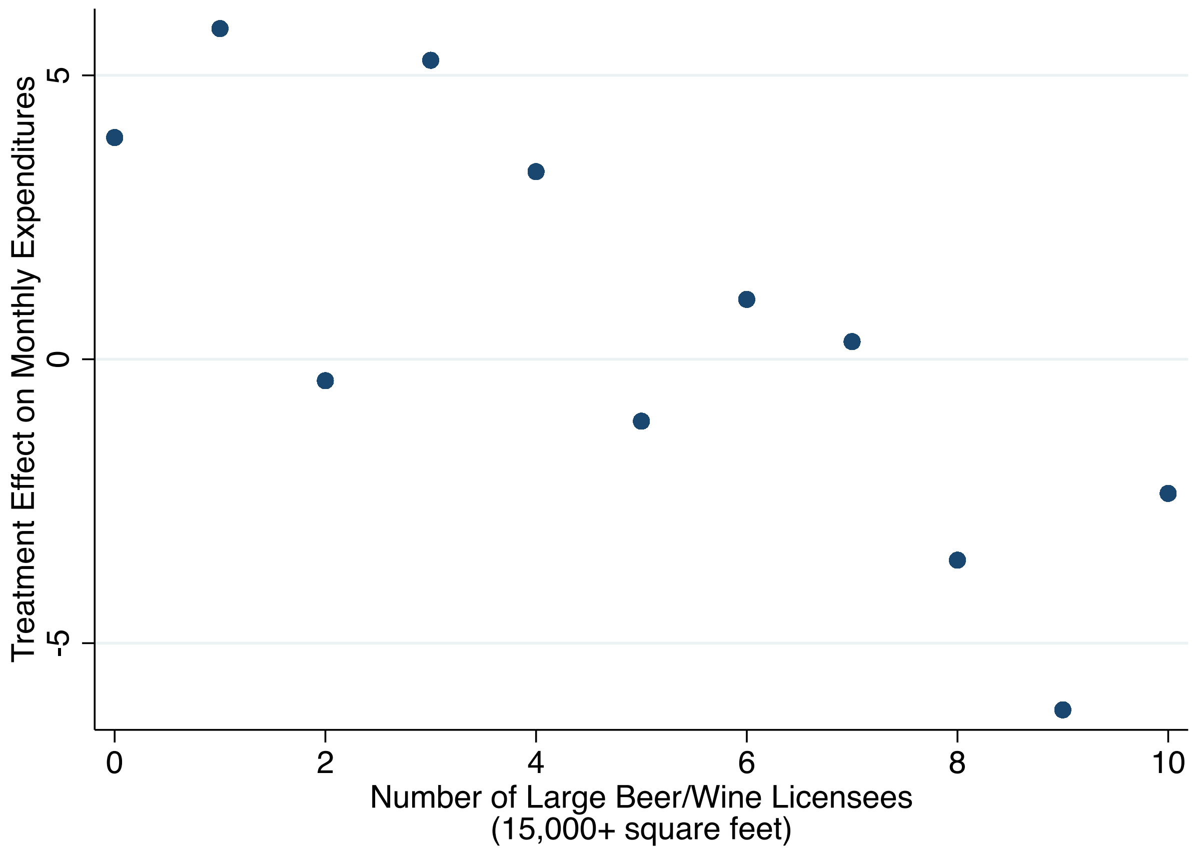
One concern with the previous results lies in the parametric functional form used to capture the effect of the number of firms. In particular, if most of our data lies in markets with more stores, then it is possible that the large effects we are finding when moving from monopoly to duopoly could be driven by the curvature that is needed to fit the smaller effects in larger markets. To address these concerns, figure 8 plots the reduced form treatment effect of increasing the number of license-eligible stores by one, for zip codes with different numbers of stores above 20,000 square feet. Unlike the two-stage least squares estimates, the reduced form does not rely on the quadratic functional form, and yet we still see drastically larger treatment effects for zip codes with few stores.

3.3.3 Effects on Adverse Behaviors

We next consider how entry affects consumption for different types of households. In particular, we investigate whether it encourages teetotal households to begin consuming alcohol or simply boosts consumption among households already at the high-end of the purchasing spectrum. We

[11] Harry Esteve. November 8, 2011. "Washington voters OK sales of liquor in big grocery stores." The Oregonian. http://www.oregonlive.com/politics/index.ssf/2011/11/washington_voters_ok_sales_of.html

Figure 8: Reduced-Form Treatment Effects on Liquor Expenditures



classify households according to their spirits purchasing behavior January 2010 through May 2012. Households are deemed "non-drinkers" if they never purchase liquor and "heavy drinkers" if they are in the top quartile of households in per-person ethanol purchases (among households who buy liquor at least once). For each group, we separately estimate the effect of market configuration on the likelihood of purchasing spirits using specification (4). Column 1 in table 6 reports results for the full population: compliers are 6.3 percentage points more likely to purchase liquor in duopoly compared to monopoly markets. The effect is nearly twice as large for heavy drinking households (14.8 percentage points, reported in column 2). There is no detectable effect for teetotal households; the 95% confidence interval for the coefficient on the linear term is 1.2 percentage points. Market configuration therefore appears to operate on the intensive margin, which is concerning if it leads to excessive drinking and alcohol-related fatalities.

We directly examine whether market structure affects the likelihood a household engages in "heavy drinking", and report estimates in columns 4 and 5 of table 6. Again, we make use of data on alcohol purchases under the state monopoly. Heavy drinking is classified by the 75th percentile of monthly alcohol and ethanol purchases (2.35 and 0.96 liters, respectively) prior to liberalization. Results indicate that households in duopoly markets are 3.3 percentage points more likely to exceed these heavy drinking thresholds than those in monopoly markets. However, the public health implications of this increase are ambiguous. Households may simply drink less at bars and restaurants and more at home, which might even lead to fewer alcohol-related driving accidents. Or they may be substituting away from beer and wine. Since Nielsen also tracks purchases of these products, we can address this point directly with the same estimation strategy as before. We do not find conclusive evidence of a shift in beer and wine consumption, although our estimates are noisy. And while we do not have data on total alcohol consumption, we can shed light on how on-premise liquor consumption changes using additional licensure data from the WSLCB. In column 9, we examine whether an additional off-premise spirits retailer affects the number of bars within the same zip code operating in January 2013. If the shock to residual demand at bars is

Table 6: Effect of License Eligibility on Adverse Behaviors

	Effect of Market Structure on Adverse Outcomes								
	Household Level (Monthly)						Zip Code Level		
	Buy Alcohol			Drink ≥		Beer & Wine Consumption (L)	Accidents 06/2012-12/2015		Bars Operating January 2013
	All	Heavy Drinkers	Non-Drinkers	2.35 L Alcohol	0.96 L Ethanol		All	Severe	
Panel A: IV	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
# of Liquor Outlets	0.074***	0.208***	0.012	0.037***	0.038***	-0.792	0.021	0.012	9.597
	(0.020)	(0.053)	(0.010)	(0.011)	(0.011)	(1.135)	(0.168)	(0.031)	(9.583)
# of Liquor Outlets^2	-0.005***	-0.017***	-0.001	-0.003***	-0.003***	0.111	-0.007	-0.000	-0.825
	(0.002)	(0.004)	(0.001)	(0.001)	(0.001)	(0.084)	(0.014)	(0.002)	(0.940)
# of Stores in the Bandwidth FE	X	X	X	X	X	X	X	X	X
# of Stores Above the Bandwidth FE	X	X	X	X	X	X	X	X	X
Panel B: Reduced Form									
# of Marginally License-Eligible Stores	0.058***	0.133***	0.014	0.020**	0.021***	0.728	-0.011	0.009	5.426
	(0.016)	(0.043)	(0.009)	(0.008)	(0.008)	(0.751)	(0.115)	(0.020)	(6.129)
# of Marginally License-Eligible Stores × # Stores Above the Bandwidth	-0.007***	-0.016***	-0.002***	-0.003***	-0.003***	-0.000	-0.007	-0.000	-0.795
	(0.002)	(0.004)	(0.001)	(0.001)	(0.001)	(0.068)	(0.014)	(0.002)	(0.931)
# of Stores in the Bandwidth FE	X	X	X	X	X	X	X	X	X
# of Stores Above the Bandwidth FE	X	X	X	X	X	X	X	X	X
Observations	31875	7981	17810	31875	31875	31875	141	141	141
Mean	0.095	0.198	0.031	0.021	0.021	2.690	1.820	0.074	34.589

Notes: Observations in column 1-5 are at the panelist-month level for May 2012 - January 2015, and standard errors are clustered at the zip code level. Observations in columns 6-8 are at the zip code level, and standard errors are heteroskedasticity-robust. In all specifications, coefficients are statistically significant at the * 10%, ** 5%, and ***1% level. Severe accidents include: "Dead at Scene," "Dead on Arrival," "Died in Hospital" or "Suspected Serious Injury." 2.35L of alcohol and 0.96L of ethanol are the 75th percentile of monthly per person consumption among households January 2010-May 2012. Heavy drinkers are households above the 75th percentile in average per-person consumption January 2010-May 2012. Instruments in panel A are interactions between the number of marginally eligible firms and indicator variables for the number of stores above 15,000 ft^2.

large enough, then these establishments may exit. However, the estimated coefficient on the linear term is not statistically significant, and it is positive, which contravenes the substitution story.

Finally, we obtain a comprehensive dataset of alcohol-related accidents from the Washington State Department of Transportation to directly examine whether liquor market configuration affects car accidents. For every accident between 2010 and 2015, we observe location, date and time, as well as the sobriety level and any resultant injuries. One challenge is linking accident locations to purchase locations; we observe the zip code where an accident occurred, not the zip code where a driver purchased any liquor he or she imbibed. We therefore estimate a modified version of equation (4) at the zip code level, where the number of accidents in that zip code is the dependent variable. Coefficient estimates for the number of firms and its square are economically small and statistically insignificant. The estimates imply that a shift from monopoly to duopoly does not change the likelihood of an accident at all. Taken together, these results suggest that additional competitors increase liquor consumption substantially, particularly in monopoly and duopoly markets, but find no evidence of spillovers, either to on-premise liquor purchasing or accidents.

3.3.4 Price Effects

We next examine whether, and to what extent, an additional spirits retailer translates to lower prices for consumers. A rich theory literature demonstrates that the effect of market configuration on prices depends on the nature of competition; predictions range from static Bertrand, where duopoly achieves the perfectly competitive outcome, to perfect collusion, where additional firms merely share in monopoly rents. To shed light on these effects in the liquor context, we modify equation (4) so that the level of observation is at the product-month rather than household-month level. The outcome variable of interest is the price of product j purchased by a household residing in zip code z in month t.

Our preferred specification is presented in column 3 of table 7. The point estimates imply that a switch from monopoly to duopoly leads to a 3.3% increase in the average price of spirits, although

Table 7: Effect of Market Configuration on Prices

Effect of Market Structure on Log Price				
Panel A: IV	(1)	(2)	(3)	(4)
# of Liquor Outlets	0.027	0.006	0.097	-0.026
	(0.057)	(0.011)	(0.093)	(0.017)
# of Liquor Outlets^2			-0.007	0.002*
			(0.007)	(0.001)
# of Stores in the Bandwidth FE	X	X	X	X
# of Stores above the Bandwidth FE	X	X	X	X
UPC FE		X		X
Panel B: Reduced Form				
# of Marginally License-Eligible Stores	0.023	0.006	0.042	-0.006
	(0.048)	(0.011)	(0.063)	(0.016)
# of Marginally License-Eligible Stores × # Stores above the Bandwidth			-0.004	0.002
			(0.008)	(0.002)
# of Stores in the Bandwidth FE	X	X	X	X
# of Stores above the Bandwidth FE	X	X	X	X
UPC FE		X		X
Observations	6037	6037	6037	6037

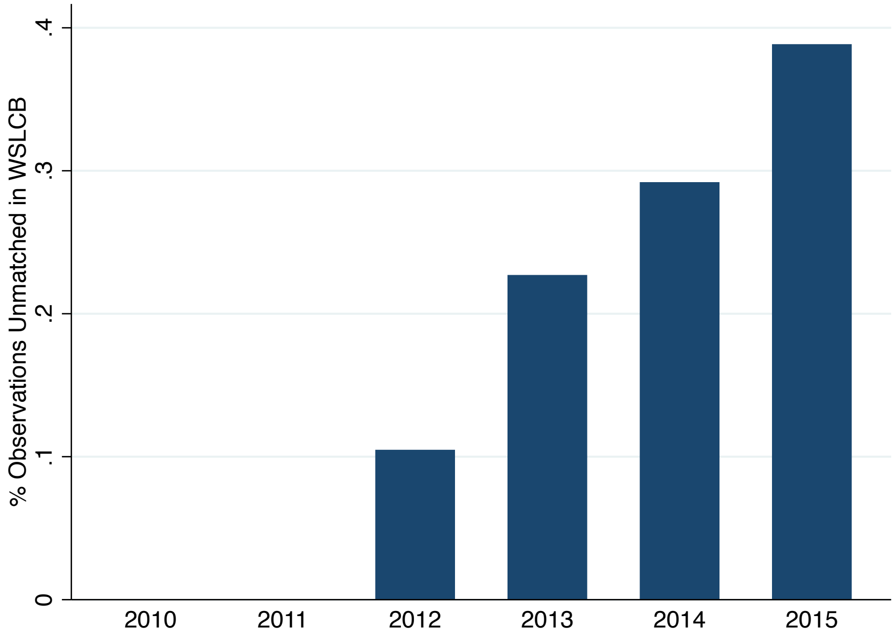
Notes: Standard errors clustered at the zip code level, and coefficients are statistically significant at the *10%, **5%, and ***1% levels. Observations are liquor transactions. The bandwidth is 5,000-15,000ft^2. The intruments in panel A include the interactions between the number of marginally license-eligible stores and a full set of indicators for the number of stores above 15,000ft^2.

neither the linear nor quadratic term are statistically significant. Average price changes potentially confound two effects: differences in the purchase bundle (selection) and differences in the prices of the same set of goods. To isolate the second force, in column 4 we present estimates of equation (4) that include product (UPC) fixed effects. Regression estimates with product fixed effects use only within-UPC variation in prices, allowing us to examine whether households in zip codes with an additional firm pay lower prices for the same goods. Results indicate no differences in price across market structures. For example, the estimates suggest an economically and statistically insignificant 1.2% price drop for duopoly compared to monopoly markets. Columns 1 and 2 present results for a linear version of this model, which are in line with the previous discussion.

3.3.5 Product Assortment Effects

In this subsection, we consider how product assortment responds to market configuration. While firms do not appear to adjust prices when facing another retailer within their same zip code, they may compete on other margins (as in Berry and Waldfogel (2001) or Wollmann (2017)). A first finding, displayed in column 1 of table 8, is that households buy a wider variety of products when they have an additional liquor outlet in their home zip code. There is an 80% increase in the number of unique products bought by households in duopoly compared to monopoly markets, although the effect diminishes with the number of stores. These new UPCs are not merely different formats of the same liquor variety (e.g. different sized bottles of Bacardi Silver) but include new brands. We find similar results of a regression on the number of unique brands, displayed in column 3. Because individual households purchase liquor infrequently, as a robustness check, we aggregate the data to the zip code level and report results of configuration on the total number of products observed across all households between June 2012 and December 2015 (column 2). The estimated effects are strikingly similar: a shift from monopoly to duopoly increases the number of unique products by 59%. Although not reported here, these estimates are robust to controls for the total number of liters purchased within the zip code. Our findings on product varierty dovetail

Figure 9: Incidence of New Liquor Products Purchased by Panelists



with the expansion of the product space following privatization. Figure 9 shows that the fraction of panelist purchases which correspond to new products (UPCs the WSLCB did not stock 2010-2012) increases dramatically each year. Our results suggest that gains from wider product variety accrue disproportionately to consumers in markets with more spirits retailers.

What kinds of products do households consume in markets with more competitors? Households are more likely to buy large bottles (1.75 liters) and high proof products (higher than 80 proof, the 75th percentile). Of particular interest is whether and how quality changes in response to enhanced competition. While it is difficult to measure quality directly, we create indicators for whether a product is "expensive" or "cheap" using the WSLCB price lists. A product is expensive (cheap) if it is above (below) the 75th (25th) percentile in price per bottle under the WSLCB. Since the WSLCB applied a uniform markup rule to pricing, expensive products are essentially those with high manufacturer prices. We also calculate these percentiles weighted by purchase volume under the state monopoly (to give a sense of relatively expensive products). Results are presented in

Table 8: Effect of License-Eligibility on Product Selection

Effect of Market Structure on Product Mix									
	# Unique UPCs		# Brand Codes	Purchase Indicator					
				1.75L Bottle	High Proof	Expensive (R)	Expensive	Cheap (R)	Cheap
Panel A: IV	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
# of Liquor Outlets	0.158***	13.440**	0.147***	0.057***	0.028***	0.018**	0.035***	0.038***	-0.001
	(0.041)	(5.341)	(0.040)	(0.015)	(0.010)	(0.009)	(0.012)	(0.012)	(0.001)
# of Liquor Outlets^2	-0.012***	-0.824	-0.011***	-0.004***	-0.002**	-0.001*	-0.004***	-0.003***	0.000
	(0.003)	(0.515)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
# of Stores in the Bandwidth FE	X	X	X	X	X	X	X	X	X
# of Stores Above the Bandwidth FE	X	X	X	X	X	X	X	X	X
Panel B: Reduced Form									
# of Marginally License-Eligible Stores	0.109***	5.968	0.104***	0.047***	0.024***	0.013**	0.044***	0.031***	-0.000
	(0.033)	(3.780)	(0.031)	(0.011)	(0.007)	(0.005)	(0.015)	(0.008)	(0.001)
# of Marginally License-Eligible Stores × # Stores Above the Bandwidth	-0.013***	-0.602	-0.012***	-0.005***	-0.002***	-0.001	-0.003***	-0.004***	0.000
	(0.004)	(0.599)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
# of Stores in the Bandwidth FE	X	X	X	X	X	X	X	X	X
# of Stores Above the Bandwidth FE	X	X	X	X	X	X	X	X	X
Observations	31875	125	31875	31875	31875	31875	31875	31875	31875
Mean	0.153	18.662	0.145	0.050	0.016	0.016	0.046	0.027	0.003

Notes: Observations in columns 1 & 3-9 are at the panelist-month level for May 2012 - January 2015. For these columns, standard errors are clustered at the zip code level. Observations in column 2 are at the zip code level for May 2012-January 2015 and standard errors are heteroskedasticity-robust. Coefficients are statistically significant at the * 10%, ** 5%, and ***1% level. Instruments in panel A are interactions between the number of marginally eligible firms and the number of stores above 15,000 ft^2.

columns 5-8 and suggest an increased likelihood of buying both high and low-end products. Taken together, these results suggest that multi-product retailers soften competition through diversifying product offerings.

3.3.6 Evidence on Price and Product Variety in Scanner Data

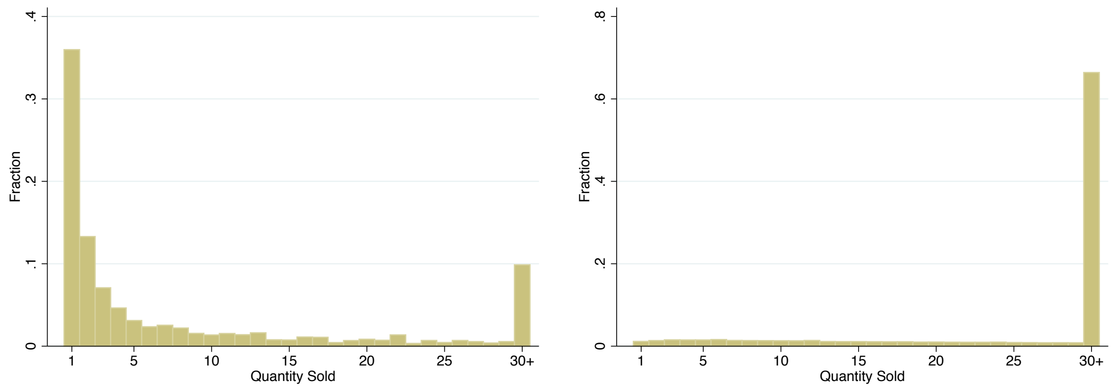
Our panelist analysis demonstrates that market configuration affects how much and what kinds of liquor consumers purchase. In a sense, these are the outcomes most relevant for those crafting policies to mitigate liquor externalities; our panelist analysis shows that limiting the number of firms reduces liquor consumption. One drawback of the panel analysis, however, is that the data comes from transactions for only a subset of consumers, potentially inducing selection bias. In this subsection, we provide auxiliary evidence in support of our findings using the Nielsen ScanTrack dataset.

The ScanTrack Dataset contains weekly volume and average selling price for each product (UPC) sold at each location of nine liquor-selling chains in Washington State. In contrast, the Consumer Panel only contains information about products purchased by one of the participating households (some 2,700 in Washington state during our sample period). Selection into the ScanTrack dataset is therefore much less severe than into the Panel dataset. To get a sense of magnitudes, in figure 10 we plot the distribution of annual quantity sold for each zip code-UPC in the Consumer Panel, which is the unit of analysis in equation (4). Many products appear only once, suggesting that there may be many other products on shelves absent from the Consumer Panel dataset. In contrast, we plot the distribution of annual quantity sold per store in the Scanner dataset and there is little bunching at low numbers. As an example, over 60% of products sell at least 30 units annually per store in the Scanner data. Relative to the Consumer Panel, very few products are sold in small quantities in the ScanTrack Dataset, suggesting that there are few products on shelves that are not transacted. Selection is therefore less of a concern in the ScanTrack dataset.

Inference about absolute price levels using the Consumer Panel is also limited. If household

Figure 10: Annual Quantity Sold by UPC

(a) Consumer Panel - by Zip Code (b) Scanner - by Store



A purchases good k at p_k^A and household B purchases good l at price p_l^B where $p_k^A < p_l^B$, we cannot infer whether household B faced higher absolute prices ($p_l^A < p_l^B$ and $p_k^A < p_k^B$) or lower prices ($p_l^B < p_l^A$ and $p_l^B < p_l^A$). Instead, we learn that the relative price of product k to l was lower for household A. The Nielsen ScanTrack dataset includes all transactions at 663 retailers in Washington State, including but not limited to panelist purchases.

Two salient facts emerge from the ScanTrack dataset: first, there is little variation in prices across stores, and second, variation in product selection is substantial. These industry descriptives support our findings from the Consumer Panel, but we do not re-estimate (5) using the ScanTrack dataset for two reasons: first, to protect retailer anonymity, Nielsen does not release store locations, except at the 3-digit zip code level. There are only fourteen 3-digit zip codes within Washington State (compared to 773 5-digit zip codes), and our own-entry regression results indicate this is far too wide a band to learn about competition. Second, the Scantrack dataset includes prices and quantities only for a subset of stores, and in particular only for chain stores. We therefore prefer the Panel dataset for our principal analysis, and use explore the ScanTrack dataset as auxiliary evidence.

The retail chains in the ScanTrack dataset are large, boasting 86 outlets a piece and selling an average of 1.84 million bottles of spirits annually. Table 9 provides descriptive statistics about prices and product selection at these chains. To measure price variation, we first calculate the average selling price at each outlet for a given year, and then calculate its coefficient of variation[12] across outlets for a given product-year. The average coefficient of variation across products and years is 9% (row 4), suggesting there is little variation in price relative to its level. Most of this variation is across chains. The average within-chain (i.e. across stores within the same chain) coefficient of variation is 3%.[13]

In contrast, retailers sell widely different assortments. On average, chains sell 678 different UPCs each year, but the average store sells only 327. This figure varies substantially across outlets. The coefficient of variation for the number of products sold annually is 47%, five-times larger than the coefficient of variation for prices. Even within chain, the coefficient of variation for the number of products is 18%.

What drives differences in product variety? Our analysis of the Consumer Panel Dataset suggests that retail outlets tailor their product assortment to local demand conditions. But it is also possible that differences across outlets simply reflects differences in store size, driven, for instance, by real estate costs. To disentangle these possibilities, we investigate product overlap between retail outlets in the same chain in the ScanTrack Dataset. We examine whether low-variety stores sell a subset of the inventory of stores with greater selection or if their product offerings are distinct. For each pair of stores in each retail chain, we calculate the overlap in inventory: the fraction of the smaller store's products also sold at the larger store. If inventory simply expands with store size, then this ratio is one. It is zero if the intersection of the two inventories is empty. The average overlap across chains is our sample is 81%, which means that one in five products carried by a small store is not available at larger outlets. In sum, the patterns in prices and varieties in the ScanTrack Dataset corroborate our analysis of the Consumer Panel Data: spirits retailers engage in product, not price, localization.

[12]the ratio of standard deviation over the mean

[13]Note that the sample size for this measure is the number of chain-years where there are multiple stores per chain (29 observations in our data).

Table 9: Price and Product Variety for Scanner Stores

Price and Product Variation within and across Chains							
	Variable	# Observations	Mean	SD	Min	Max
# Outlets per Chain		30	85.37	51.14	1	169
Annual Quantity Sold (mil)		30	1.84	1.77	0.00	6.17
Annual # Products - Chain		30	678	403	49	1,676
Annual # Products - Store		2,561	327	158	19	1,274
Coefficient of Variation	Price	6,442	0.09	0.09	0	1.24
	Price - within Chain	29	0.03	0.03	0	0.11
	# Products	4	0.47	0.03	0.44	0.51
	# Products - within Chain	29	0.18	0.14	0.02	0.43
Overlap - within Chain		8	0.81	0.12	0.63	0.94

Notes: Based on the sales of 9 retail chains in the Nilesen Scanner data operating in Washington State May 2012 - December 2015. Coefficient of variation for price is the average across UPCs of the following quotient: standard deviation of price divided by its mean. To calculate the within chain coefficient of variation, we recalculate the CoV separately by chain and then report the average across chains. "Overlap - within Chain" is a measure of similarity between intenvtories of two stores within the same chain. For any two stores within the same chain, we calculate the share of the smaller store's inventory also carried in the larger store, and then average that measure across branches within the chain.

4 Conclusion

This paper examines how market structure, measured as the number of firms, affects competition. We find that an exogenous shift from monopoly to duopoly increases consumption by 63%, but not because competition lowers prices. Instead, retailers adjust product offerings when facing an additional rival; consumers in duopoly markets purchase almost twice as many unique products as their counterparts in monopoly markets. However, the number of retailers increases both consumption and variety at a decreasing rate, so that there are no detectable effects of moving from five to six firms.

We establish causality using a quirk of Washington state's deregulation of liquor sales in 2012. Before 2012, only state stores could sell spirits, although private retailers sold beer and wine. At privatization, these retailers could apply for a spirits license, but only if their premises exceeded $10,000\text{ft}^2$. We employ the number of marginally eligible firms induced by the threshold as an instrument for the number of spirits retailers. We augment this analysis with descriptive evidence from Nielsen's ScanTrack dataset on retail chains in Washington, which reveals considerably more variation - both within and across chains - in product offerings than prices.

Taken together, our findings provide guidance on crafting liquor regulation. First, our results indicate that narrow market definitions, as small as the zip code, could be important for correctly gauging concentration and competition in other states and for similar products. Second, they suggest that Washington's licensure threshold is but a blunt instrument for reducing any negative externalities of liquor consumption, effective only in relatively concentrated markets. Even in these markets, where consumers are less likely to engage in heavy-drinking, we observe no reduction in alcohol-related car accidents. An important caveat, however, is that our findings speak to the marginal effect of entry. Removing the licensure threshold altogether would constitute a much larger shock to market structure. As an example, the threshold forecloses convenience stores, including 242 7-Eleven outlets[14]. Perhaps it is unsurprising that Costco, whose stores average $140,000\text{ft}^2$, spent $22 million on advertising to support an incarnation of the referendum with this particular entry requirement.[15]

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[14]Which also sell beer or wine

[15]Melissa Allison. July 18, 2011. "Costco revamps liquor-sales initiative." The Seattle Times. `http://www.seattletimes.com/seattle-news/costco-revamps-liquor-sales-initiative/`

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A Appendix Figures and Tables

Figure A.1: Chain Sizes

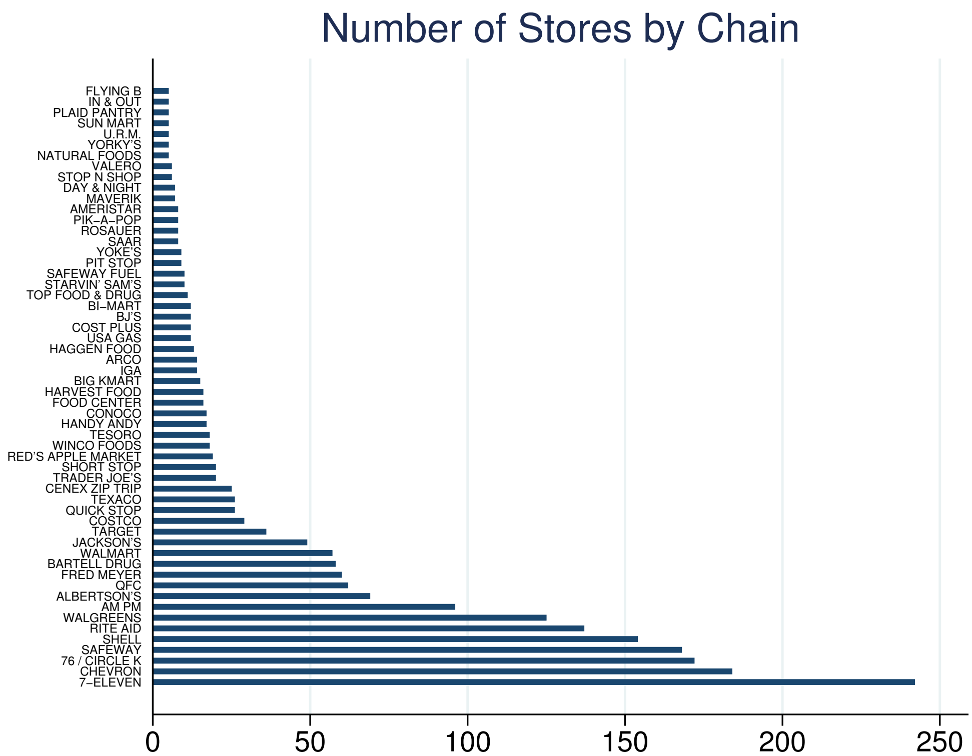
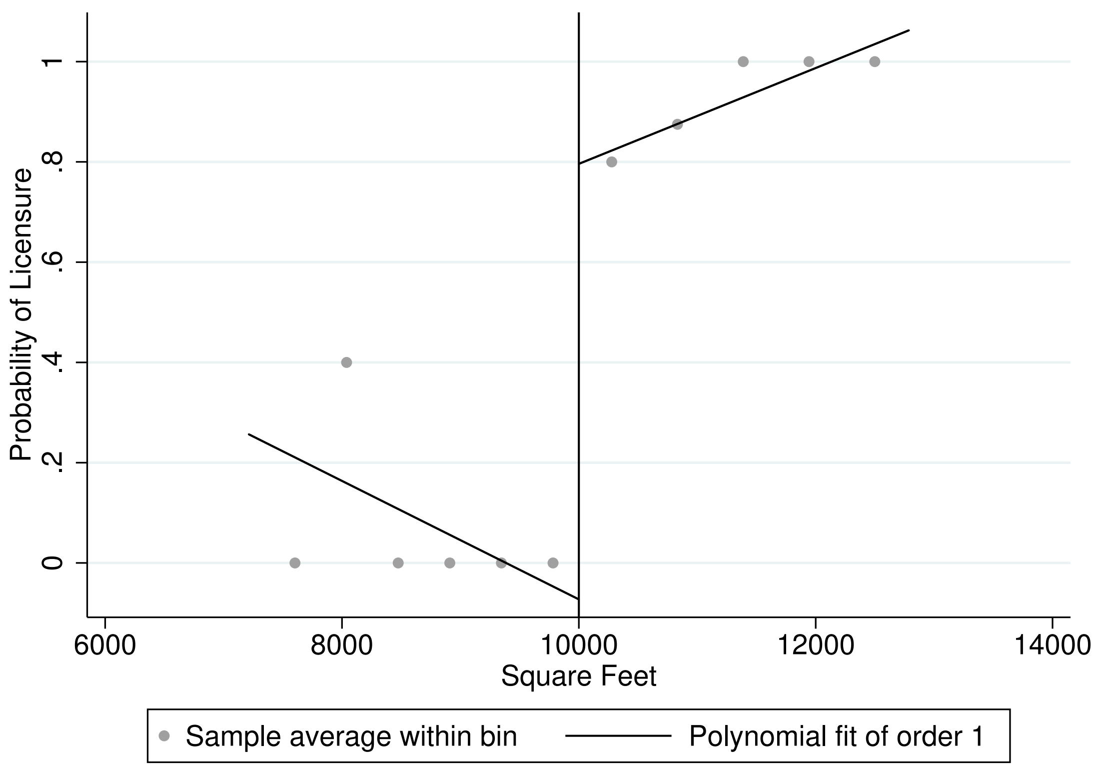


Figure A.2: Regression Discontinuity Plot, Chains with 10 or More Stores



B Covariate Balance

B.1 Covariate Balance for Stores Around the Licensure Threshold

This Appendix presents results from a battery of covariate balance tests that study whether store observables vary around the 10,000 square foot threshold. To begin, we estimate equation (1) using store characteristics reported by the WSLCB as outcome variables, and present results in Table (B.1). For example, the first row reports the discontinuity at $10,000\text{ft}^2$ in the probability that we can geolocate a store using the address provided by the WSLCB. We consider as store covariates whether the store is geolocated, the earliest date it receives any kind of beer or wine license, the total amount of alcohol-related fines paid in 2010, 2011 and for the pre-liberalization months of 2012, a series of zip code demographics, and the number of competitors within 0.5 miles that are either below 5,000 square feet, between 5,000 and 15,000 square feet, between 10,000 and 15,000 square feet, and above 15,000 square feet. The only significant discontinuity for the full sample is on poverty rate, as stores just above the threshold are more likely to be located in zip codes with higher poverty rates. This result is driven by independent stores, as for chain stores the discontinuity is statistically insignificant. Moreover, independent stores just above the threshold are also located in zip codes with lower median household income. Despite this, independent stores are balanced across all metrics related to the number of competitors. That is, these differences in zip code income are not correlated with differences in neighbor configuration, alleviating concerns about differences in demand around the threshold. As for chain stores, there is a discontinuity in total fines paid in 2011, but not in 2010 or 2012, and stores just above the threshold appear to have more competitors nearby. If anything, a systematic difference in the number of competitors ought to generate downward bias in our estimate of the causal effect of license eligibility on liquor licensure. Particularly as the estimate of uptake for chain stores is already close to one, this difference in competitors does not appear economically significant.

As an additional test for gaming the threshold, we leverage auxiliary data from CoreLogic to

Table B.1: Covariate Balance Across Licensure Threshold

Covariate Balance of Store Characteristics Around the Licensure Threshold			
	(1)	(2)	(3)
	All Stores	Independent Stores	Chain Stores
Is Geolocated	-0.05	0.00	-0.01
	(0.088)	(0.102)	(0.150)
Earliest Privilege Date (Days)	269.14	172.76	1,347.73
	(550.8)	(631.7)	(1670.4)
Total Fines Paid in 2010 ($)	-10.97	-28.72	36.11
	(34.1)	(60.4)	(40.8)
Total Fines Paid in 2011 ($)	-184.92	-49.43	-795.52**
	(150.7)	(166.0)	(337.2)
Total Fines Paid in 2012, Before June ($)	2.06	13.39	-2.37
	(9.4)	(16.1)	(5.4)
Zip Code Population	-968.15	-5,339.74	-3,986.55
	(4667.6)	(5747.6)	(7919.5)
Zip Code Population Over 21	-809.60	-4,004.47	-1,907.31
	(3438.3)	(3995.3)	(5576.2)
Zip Code African American Population	122.97	406.81	-2,531.51
	(459.3)	(459.6)	(2313.9)
Zip Code Hispanic Population	-255.49	280.56	-4,539.33
	(1187.0)	(1682.2)	(3849.1)
Zip Code Median Age	-3.62	-2.83	-3.02
	(3.3)	(4.5)	(4.1)
Zip Code Unemployment Rate	1.55	2.22	1.19
	(1.7)	(2.1)	(3.8)
Zip Code Median Household Income	-11,440.53	-23,472.87**	-5,930.57
	(8677.3)	(9328.7)	(18721.8)
Zip Code Percentage of Population with Less than High School Education	1.09	5.89	-10.19
	(3.5)	(4.6)	(9.1)
Zip Code Percentage of Population with High School Education	-4.00	1.13	-14.16
	(2.9)	(3.5)	(11.2)
Zip Code Percentage of Population with BA or Higher	5.90	-6.16	24.69
	(7.2)	(8.1)	(21.2)
Zip Code Percentage of Population in Poverty	11.28**	16.33***	5.14
	(4.54)	(5.43)	(7.30)
Number of Neighbors within 0.5 Miles with Square Footage between 5,000 and 15,000	-0.13	-0.20	0.37
	(0.229)	(0.290)	(0.235)
Number of Neighbors within 0.5 Miles with Square Footage between 10,000 and 15,000	0.16	0.05	0.39**
	(0.121)	(0.172)	(0.159)
Number of Neighbors within 0.5 Miles with Square Footage below 5,000	1.12	-0.62	6.53*
	(1.469)	(1.138)	(3.760)
Number of Neighbors within 0.5 Miles with Square Footage above 15,000	-0.22	0.02	-2.14
	(0.822)	(0.420)	(1.498)

Notes: This table presents results of a local polynomial regression-discontinuity design model with robust bias-corrected confidence intervals and a MSE-optimal bandwidth, estimated in Stata via the "rdrobust" command using techniques in Calonico, Cattaneo and Titiunik (2014), Calonico, Cattaneo and Farrell (2016) and Calonico, Cattaneo, Farrell and Titiunik (2016). Each row uses a different store characteristic as the dependent variable. Column 1 reports, for each dependent variable, the discontinuity at 10,000 square feet using our full sample. Column 2 considers only independent stores, and Column 3 considers only chain stores. Robust, bias-corrected standard errors in parentheses. Coefficients are significant at the * 10%, ** 5% and *** 1% levels.

test for store expansions. While the distribution of stores around $10,000\text{ft}^2$ is smooth, it is possible that small stores undergo large-scale expansions in response to I-1193. This type of manipulation might put stores far above the threshold, and would be consistent with large fixed costs and small marginal costs of renovation. We use CoreLogic to test whether retailers just below $10,000\text{ft}^2$ are more likely to renovate between 2012-2015 than those just above. CoreLogic pools County Assessor tax records for each parcel of land registered in the United States as of May 2015. It contains square footage, year of construction, and year of initial assessment with current configuration. Renovations are classified based on the difference in the date of initial assessment and construction.Unfortunately, we cannot accurately match CoreLogic and WSLCB records, precluding use of CoreLogic size measures in a regression on licensure (our main specification). We attempted a match based on trade names, addresses, latitude and longitude, but had little success. We restrict attention to stores likely to sell beer or wine using Property Indicator Codes, Land Use Codes, and Building Codes, three variables created by CoreLogic to describe the economic activity on a given parcel. For example, we exclude commercial parcels marked as "Hotel/Motel" or "Hospital". See appendix C for sample construction details. The final sample contains 18,224 commercial parcels in the state of Washington built prior to 2012. Table B.2 presents summary statistics for this sample. While roughly 37% of these parcels have been renovated at least once, only 0.04% have been renovated after 2011. Selective renovation therefore seems unlikely to be important in this setting.

For completeness, we run a battery of other tests using the CoreLogic data. Panel B of table B.2 reports estimates for discontinuities in other variables. We do not find a significant differences in year built, year renovated (conditional on renovation), or renovation after 2011. We repeat this exercise for smaller CoreLogic subsamples for which we assign a high probability of selling alcohol, such that the incentive to renovate is strongest. Again, the overall probability of renovating post-2012 is minute, and we cannot detect a discontinuity at the licensure threshold. The final row of this table reports the estimate from a McCrary test for bunching (in the number of stores) at $10,000\text{ft}^2$. Again, we find no evidence of this behavior. Overall, the information from this

Table B.2: Corelogic Covariate Balance

Covariate Balance of Store Characteristics Around the Licensure Threshold – Corelogic Sample			
	(1)	(2)	(3)
Panel A: Descriptive Statistics			
	All Potential Alcohol Retail Records	Selected Land Use Codes	Selected Building Codes
Number of Records	18,224	1,193	1,423
Square Footage, 10th Percentile	960	1,641	1,650
Square Footage, 50th Percentile	3,749	4,151	3,438
Square Footage, 90th Percentile	19,664	46,821	51,300
Year Built, 10th Percentile	1923	1929	1945
Year Built, 50th Percentile	1974	1974	1980
Year Built, 90th Percentile	2003	2000	2001
Percentage Ever Renovated	37.04%	57.67%	49.05%
Year Renovated, 10th Percentile	1964	1964	1970
Year Renovated, 50th Percentile	1982	1985	1988
Year Renovated, 90th Percentile	1997	2000	2000
Percentage Renovated Post 2012	0.04%	0.08%	0.00%
% Renovated Post 2012, If Ever Renova	0.10%	0.15%	0.00%
Panel B: Discontinuity at Licensure Cutoff			
	All Potential Alcohol Retail Records	Selected Land Use Codes	Selected Building Codes
Year Built	-0.559	-35.309**	-13.309
	(3.119)	(16.441)	(13.602)
Ever Renovated	0.096**	0.307	-0.204
	(0.046)	(0.221)	(0.218)
Year Renovated, If Ever Renovated	1.073	-5.280	-2.794
	(1.918)	(7.923)	(6.809)
Renovated Post 2012	-0.001	0.010	-
	(0.001)	(0.010)	-
Renovated Post 2012, If Ever Renovated	0.000	-	-
	(0.000)	-	
McCrary Test P-Value	0.30	0.48	0.26

Notes: This table presents results of a local polynomial regression-discontinuity design model with robust bias-corrected confidence intervals and an optimal bandwidth, estimated in Stata via the "rdrobust" command using techniques in Calonico, Cattaneo and Titiunik (2014), Calonico, Cattaneo and Farrell (2016) and Calonico, Cattaneo, Farrell and Titiunik (2016). The relevant sample is the set of Corelogic property tax records of potential alcohol retailers, as defined in Appendix B. Column 2 further restricts the sample to selected Corelogic "Land Use Codes" that are associated with retail sale of food (supermarket/food store/wholesale). Column 3 further restricts the sample to selected Corelogic "Building Codes" that are associated with retail sale of food (market/supermarket/food stand/convenience market, convenience store). For each sample, the dependent variable is different store record characteristics. More details regarding variable definitions and sample construction are in Appendix B. Robust, bias-corrected standard errors in parentheses.
Coefficients are significant at the * 10%, ** 5% and *** 1% levels.

auxiliary dataset makes us confident that our setting satisfies the exclusion restriction required for valid regression discontinuity inference.

B.2 Covariate Balance for Stores with Neighbors Around the Licensure Threshold

In this subsection, we present evidence that stores with a rival just-above versus just-below $10,000\text{ft}^2$ are similar on observables, to supports the exclusion restriction outlined above. We estimate equation 2 with different store s characteristics on the left-hand side, and report results in Table B.3. In appendix E we also present the analogous regressions for the n-nearest neighbor based specifications.

Each row of table B.3 presents results for different store characteristics: square footage, the earliest date it receives any kind of beer or wine license, the total amount of alcohol-related fines paid in 2010, 2011 and for the pre-liberalization months of 2012, and a series of zip code demographics. Each column considers a different distance bandwidth, from 0.1 to 1 mile. For example, the first two rows show that there is no statistically significant correlation between the number of license-eligible competitors in the bandwidth (our profit-shifter) and own-store size, conditional on the number of competitors in the bandwidth. This result holds for all distance thresholds between 0.1 and 1 miles, and for both chain and independent stores. Overall, we do not find many store characteristics that are associated with the number of competitors in the bandwidth who are above the licensure threshold. The main exception is across education levels: regardless of the distance bandwidth, both chain and independent stores with more license-eligible neighbors tend to be located in zip codes with higher educational attainment. Despite this, there is no significant correlation with zip code income, so it is not the case that stores that have more license eligible competitors within a certain distance are located in richer zip codes. This reassures us that this difference is more likely to be spurious than due to underlying differences in economic charac-

Table B.3: Covariate Balance Across License-Eligible Stores with Differing Numbers of License-Eligible Neighbors within Distance Bandwidths

Covariate Balance Across License-Eligible Stores with Differing Numbers of License-Eligible Neighbors within Distance Bandwidths											
Square Footage Bandwidth = 5000 square feet											
Distance to Store (miles):		0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
Own Square Footage	Independents	-148,609	-156,240	-144,910	-131,324	-120,365	-216,465	-214,798	-210,730	-169,579	-160,530
		(141,434)	(155,816)	(142,820)	(129,377)	(119,254)	(215,033)	(215,408)	(209,958)	(171,333)	(161,219)
	Chains	57,396	52,761	55,561	56,511	56,103	-77,600	-73,903	-61,106	-48,547	-41,103
		(61,103)	(55,096)	(54,851)	(55,885)	(57,455)	(76,861)	(75,644)	(62,317)	(50,479)	(42,379)
Earliest Alcohol Licensure Date (Days)	Independents	83.6	630.6*	339.1	324.0	288.8	161.8	73.6	212.6	86.9	92.5
		(512.2)	(323.8)	(271.0)	(222.2)	(179.6)	(148.5)	(148.4)	(134.8)	(125.8)	(114.4)
	Chains	-497.9	-95.1	-112.8	-25.2	66.5	78.1	-0.8	42.6	-1.2	-32.8
		(332.9)	(245.9)	(197.1)	(185.1)	(157.7)	(131.3)	(131.1)	(112.6)	(100.3)	(100.4)
Total Alcohol-Related Fines Paid in 2010	Independents	65.5	26.0	-8.6	-6.7	-0.2	1.9	-1.9	-2.4	-0.2	-6.7
		(52.3)	(26.6)	(23.3)	(20.3)	(15.0)	(13.1)	(13.0)	(12.7)	(11.6)	(9.8)
	Chains	14.0	9.1	-12.1	-14.4	-9.9	-3.6	-6.4	-6.5	-5.8	-6.7
		(12.4)	(11.0)	(13.7)	(11.4)	(8.5)	(6.7)	(6.5)	(6.3)	(5.7)	(5.2)
Total Alcohol-Related Fines Paid in 2011	Independents	-26.4	-4.9	-14.3	-21.4	-30.6	-34.4**	-15.4	-17.5	-13.9	-14.8
		(18.6)	(26.0)	(19.2)	(15.1)	(22.7)	(17.1)	(20.1)	(18.9)	(15.1)	(17.5)
	Chains	58.1	31.1	23.8	23.4	7.9	3.1	0.7	1.7	3.2	1.7
		(40.2)	(23.4)	(18.9)	(15.3)	(15.0)	(8.5)	(8.0)	(7.2)	(7.8)	(9.9)
Total Alcohol-Related Fines Paid in 2012 (Pre-Liberalization)	Independents	-9.5	-4.8	2.2	1.4	0.3	1.0	1.5	-0.8	-0.2	-1.5
		(8.8)	(4.3)	(7.2)	(4.1)	(3.0)	(2.2)	(2.3)	(2.0)	(1.5)	(1.6)
	Chains	-10.2	3.6	0.4	2.3	1.4	4.1	3.0	0.5	1.5	0.1
		(8.4)	(7.2)	(5.9)	(5.8)	(4.5)	(3.4)	(2.8)	(2.2)	(1.8)	(1.8)
Zip Code Population	Independents	3,913	-890	807	1,339	227	1,466	2,088	1,926	1,214	2,121
		(4699)	(3550)	(3269)	(2492)	(2431)	(1764)	(1711)	(1642)	(1429)	(1395)
	Chains	1,277	1,775	2,706	2,122	1,573	1,190	1,220	1,034	841	702
		(2158)	(1792)	(1668)	(1614)	(1665)	(1341)	(1370)	(1232)	(1220)	(1259)
Zip Code Population Over 21	Independents	4,239	812	1,781	1,852	980	1,791	2,160*	1,989*	1,492	2,114**
		(3371)	(2344)	(2168)	(1711)	(1635)	(1208)	(1165)	(1170)	(1001)	(953)
	Chains	2,139	2,228*	2,616**	2,139*	1,904*	1,507	1,376	1,207	1,133	1,015
		(1550)	(1236)	(1162)	(1138)	(1149)	(916)	(910)	(836)	(839)	(838)
Zip Code African American Population	Independents	75	152	-113	-157	-187	-74	-162	-190	-323**	-139
		(75)	(152)	-(113)	-(157)	-(187)	-(74)	-(162)	-(190)	-(323)	-(139)
	Chains	-195	173	236	137	5	-92	-169	-180	-170	-186
		-(195)	(173)	(236)	(137)	(5)	-(92)	-(169)	-(180)	-(170)	-(186)
Zip Code Hispanic Population	Independents	-1,831	-2,220	-1,463	-824	-1,408	-930	-685	-604	-599	-403
		(1176)	(1176)	(1176)	(1176)	(1176)	(1176)	(1176)	(1176)	(1176)	(1176)
	Chains	-1,567	-1,495	-851	-554	-862	-695	-311	-282	-378	-378
		(1977)	(1458)	(1226)	(951)	(1103)	(869)	(852)	(792)	(794)	(794)
Zip Code Median Age	Independents	1.6	-0.2	0.0	-0.4	-0.4	-0.5	-0.3	-0.1	0.0	-0.1
		(2.4)	(1.3)	(1.3)	(1.1)	(1.0)	(0.7)	(0.7)	(0.7)	(0.6)	(0.5)
	Chains	0.4	-0.1	-0.2	-0.1	0.1	0.4	0.2	0.4	0.4	0.3
		(1.0)	(0.8)	(0.7)	(0.7)	(0.6)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)
Zip Code Unemployment Rate	Independents	0.1	-0.7	-0.8	-0.3	-0.5	-0.7**	-0.6*	-0.4	-0.4	-0.5
		(1.3)	(0.7)	(0.6)	(0.5)	(0.4)	(0.3)	(0.4)	(0.4)	(0.3)	(0.3)
	Chains	-1.3**	-1.0**	-0.7*	-0.4	-0.6	-0.7**	-0.5	-0.5*	-0.5*	-0.5*
		(0.6)	(0.4)	(0.4)	(0.4)	(0.3)	(0.3)	(0.3)	(0.3)	(0.3)	(0.3)
Zip Code Median Household Income	Independents	3,866	-2,289	-2,215	-3,463	-2,412	957	1,297	813	1,261	1,557
		(4081)	(3823)	(3234)	(2964)	(2471)	(2174)	(2131)	(2116)	(1934)	(1914)
	Chains	9,755**	5,728.7*	3,042	610	1,983	3,299	2,225	1,998	1,720	1,437
		(4038)	(2965)	(3051)	(2924)	(2438)	(2034)	(1972)	(1805)	(1701)	(1749)
Zip Code Percentage of Population with Less than High School Education	Independents	-3.6	-4.1**	-4.7***	-2.2	-2.8*	-2.6**	-2.2*	-1.5	-1.6	-1.5
		(2.2)	(1.7)	(1.6)	(1.7)	(1.6)	(1.3)	(1.3)	(1.4)	(1.1)	(1.1)
	Chains	-2.9**	-2.5**	-2.1**	-1.4	-2.0*	-2.1**	-1.2	-1.1	-1.2	-1.1
		(1.3)	(1.0)	(1.0)	(1.1)	(1.0)	(0.9)	(1.0)	(0.9)	(0.9)	(0.9)
Zip Code Percentage of Population with High School Education	Independents	-2.7	-4.9**	-5.4***	-4.7***	-4.0***	-3.5***	-3.0***	-2.6***	-2.6***	-2.6***
		(2.6)	(2.0)	(1.8)	(1.3)	(1.1)	(0.9)	(1.0)	(0.9)	(0.8)	(0.8)
	Chains	-6.2***	-4.6***	-3.4***	-2.8**	-2.9**	-2.5***	-1.8*	-1.7**	-1.9**	-2.0**
		(1.8)	(1.3)	(1.3)	(1.3)	(1.2)	(1.0)	(0.9)	(0.8)	(0.8)	(0.8)
Zip Code Percentage of Population with BA or Higher	Independents	6.0	9.7**	10.0***	7.3***	6.7***	6.2***	5.5***	4.7**	4.9**	4.9**
		(5.4)	(4.2)	(3.7)	(2.7)	(2.4)	(2.0)	(2.1)	(2.2)	(2.0)	(1.9)
	Chains	13.4***	9.6***	7.4***	6.1**	6.9***	6.4***	4.4**	4.1**	4.3**	4.3**
		(3.7)	(2.8)	(2.8)	(2.7)	(2.4)	(2.0)	(2.0)	(1.9)	(1.9)	(1.9)
Zip Code Percentage of Population in Poverty	Independents	-3.4	0.8	0.6	1.7	1.2	-0.1	-0.3	0.3	-0.1	-0.3
		(2.2)	(1.8)	(1.5)	(1.6)	(1.3)	(1.1)	(1.1)	(1.0)	(0.9)	(0.9)
	Chains	-1.7	-1.4	-0.5	0.1	-0.6	-1.1	-0.5	-0.5	-0.7	-0.6
		(1.3)	(1.0)	(0.9)	(1.0)	(0.9)	(0.7)	(0.7)	(0.7)	(0.7)	(0.7)
N		1176	1176	1176	1176	1176	1176	1176	1176	1176	1176

Notes: This table presents results of a linear regression of different store characteristics on a constant and the interaction between a chain store dummy and the number of neighbors who are within the relevant distance and who are above the 10,000 square foot licensure threshold, but below 15,000 square feet. All specifications include a fixed effect for the total number of stores between 5,000 and 15,000 square feet and who are also within the relevant distance. The sample is restricted to stores who are not former state liquor stores, are eligible to sell liquor, and have at least one neighbor within the relevant distance. Robust standard errors with clustering at the zip code level in parentheses. Coefficients are significant at the * 10%, ** 5% and *** 1% levels.

teristics. We also check covariate balance for the n-nearest neighbor metric, and present results in appendix E. Taken together, the regression results in this section lend support to validity of the licensure threshold strategy for identifying rival entry.

B.3 Covariate Balance for Zip Codes with Stores Around the Licensure Threshold

We estimate equation (5) employing the following characteristics from the 2010 census as dependent variables: log population, percent white, log median income, and log median age. Results are reported in table B.4. As an example, the coefficient in column (6) implies that treatment zip codes boast 1.1% higher median income than control zip codes, but this difference is not statistically significant at the 10% level. Covariates are balanced across treatment and control zip codes, except for median age, as residents are 1.59 years older in treatment zip codes. Although this difference is statistically significant, it is economically small (a less than 5% difference). Zip codes are also similar in terms of representation in the Nielsen Panel (the number of households residing in the zip code), the number of beer and wine licensees in 2011, and the number of WSLCB stores pre-liberalization, which correspond to columns 1, 2, and 3 respectively. While we cannot test whether zip codes differ on unobservable characteristics, it is reassuring that they look similar both in terms of census population demographics and beer and wine market configurations before deregulation.

We next examine whether the panelists residing in treatment and control zip codes appear similar on observables. Panel A of table B.5 shows comparisons between households that live in zip codes with the same number of stores in the bandwidth, but different numbers of stores just above the cutoff, pooled across the entire sample period (2010-2015). Point estimates are small and statistically insignificant for differences in income levels and race, although heads of household in treated zip codes are 13.8% less likely to be married, a difference that is significant

Table B.4: Zip Code Covariate Balance

Covariate Balance of Zip Code Characteristics by Store Eligibility								
	# Households	# Stores	# WSLCB Stores	Log Population	% White	Log Median Income	Median Age	# Accidents per Month
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of Marginally License-Eligible Firms	2.194	0.760	-0.039	0.074	1.169	0.011	1.587**	-0.092
	(4.309)	(1.610)	(0.060)	(0.157)	(2.003)	(0.055)	(0.790)	(0.079)
Number of Stores in the Bandwidth FE	X	X	X	X	X	X	X	X
Mean	32.113	18.163	0.156	9.802	82.526	10.933	37.349	1.820
Observations	141	141	141	141	141	141	141	141

Notes: Sample includes zip codes with at least one chain store sized 5,000-15,000 ft^2. # households is the number of Nielsen Panel households in the zip code 2010-2012. # stores is the number of beer/wine licensees as of 12/2011. Demographic data come from the 2010 US Census. Coefficients are statistically significant at the *10%, **5%, and ***1% level.

at the 5 level. This difference in martial status threatens our identification strategy if it indicates differences in demand across treatment and control zipcodes. Fortunately, we can examine pre-liberalization alcohol consumption directly using data from January 2010 - May 2012. We find no statistically significant differences in the annual number of shopping trips (for any product), liquor purchase probabilities, or total liquor expenditures (Panel B). As an example, treated panelists engage in 0.45 more shopping trips per month (for any grocery item), a less than 5% difference[16]. Overall, households do not appear different in their shopping behavior across zip codes with stores just-above versus just-below the licensure threshold.

[16]To be precise, Panel B contains a proper subset of the households in Panel A, as some households included in Panel A enter the dataset after 2012 and have no pre-liberalization data.

Table B.5: Covariate Balance for Panelists

Covariate Balance of Panelist Characteristics by Local Store Eligibility					
Panel A: Full Sample Covariates (N=1,426)					
	(1)	(2)	(3)	(4)	(5)
				Income	
	Married	White	<25k	50k-100k	100k+
Number of Marginally License-Eligible Firms in Zip Code	-0.138***	-0.018	0.018	-0.010	-0.012
	(0.023)	(0.018)	(0.017)	(0.017)	(0.018)
Number of Stores in the Bandwidth FE	X	X	X	X	X
Mean	0.610	0.832	0.162	0.187	0.162
Panel B: Pre-Liberalization Covariates (N=1,092)					
	(6)	(7)	(8)		
	# Shopping Trips	Purchase Probability	Liquor Expenditures		
Number of Marginally License-Eligible Firms in Zip Code	0.450	0.035	0.425		
	(0.525)	(0.025)	(0.945)		
Number of Stores in the Bandwidth FE	X	X	X		
Mean	12.813	0.269	3.465		

Notes: Panel A includes households in Washington State in the Nielsen sample from 2010-2015. Panel B includes households in Washington State in the sample from 2010-2012. Both samples exclude households that switch zip codes during this six year period (5.71% of households). The sample includes only those residing in a zip code with at least one chain store 5,000-15,000 ft^2.

C Sample Restrictions

C.1 Corelogic Tax Records

This subsection describes the sample restrictions and variable definitions used to create Table B.2, which studies covariate balance across the 10,000 square foot licensure threshold using CoreLogic data. We access the 2015-04-22 version of the CoreLogic Tax Records dataset, which contains parcel-level property tax records for the entire United States. This dataset includes information regarding building square footage (”Universal Building Square Feet”), the construction year of the original building (”Year Built”) and the first year the building was assessed with its current components (”Effective Year Built”). We code a parcel as ”Ever Renovated” if the first year the building was assessed with its current components is greater than the construction year of the original building.

Our goal is to extract from these records a subset of parcels that contains the set of potential liquor retailers, and to study whether there is any significant variation in observables across the licensure threshold. To do so, we rely on three additional variables from the CoreLogic dataset: ”Property Indicator Code”, described as a ”CoreLogic general code used to easily recognize specific property types (e.g. Residential, Condominium, Commercial).”; ”Land Use Code”, described as a ”CoreLogic established land use code converted from various county land use codes to aid in search and extract functions”; and ”Building Code”, described as ”the primary building type (e.g. Bowling Alley, Supermarket).” Using different restrictions on the values of these variables, we construct three samples: ”All Potential Alcohol Retail Records”, ”Selected Land Use Codes” and ”Selected Building Codes”.

Table C.1 describes on the sample restrictions used to create the first sample, ”All Potential Alcohol Retail Records”, from the full set of Corelogic records. For each code described in the previous paragraph, we exclude all parcels with non-commercial code values, as well as parcels with commercial code values that are not associated with alcohol sales. We also exclude parcels with no square footage records and parcels that were built after 2012. This reduces the sample from 2,538,477 records to the 19,902 records that make up the ”All Potential Alcohol Retail Records” sample.

Table C.2 presents the values for the Property Indicator Code, Land Use Code, and Building Code variables in the ”All Potential Alcohol Retail Records” sample. As is discussed in the main text, this sample aims to include the full set of potential liquor-selling outlets, perhaps erring on the side of including too many outlets but without including any values that can be immediately dismissed, such as auto sales or department stores. The ”Selected Land Use Codes” sample further restricts the ”All Potential Alcohol Retail Records” sample by using only parcels with ”Supermarket”, ”Food Store” or ”Wholesale” land use code values. Finally, the ”Selected Building Code” sample further restricts the ”All Potential Alcohol Retail Records” sample by using only parcels with ”Market”, ”Supermarket”, ”Food Stand”, ”Convenience Market”, ”Convenience

Table C.1: CoreLogic Sample Restrictions

Corelogic Sample Restrictions		
Restriction	Observations	Excluded Values
Number of Records for Washington	2,538,477	
Excluding Non-Commercial Property Indicator Codes	190,268	Miscellaneous, Single Family Residence, Condominium, Industrial, Industrial Light, Industrial Heavy, Transport, Utilities, Agricultural, Vacant, Exempt
Excluding Selected Commercial Property Indicator Codes	155,704	Hotel/Motel, Service, Office Building, Warehouse, Financial Institution, Hospital, Parking, Amusement/Recreation
Excluding Non-Commercial Land Use Codes	77,137	Apartment/Hotel, Apartment, Duplex, Residence Hall/Dormitories, Multi Family 10 Units Plus, Multi Family 10 Units Less, Multi Family Dwelling, Mixed Complex, Mobile Home Park, Quadruplex, Group Quarters, Triplex, Time Share
Excluding Selected Commercial Land Use Codes	67,396	Auto Equipment, Auto Repair, Auto Sales, Condotel, Salvage Imprv, Auto Wrecking, Business Park, Cemetery, Convention Center, Department Store, Greenhouse, Kennel, Medical Building, Medical Condo, Laboratory, Office Condo, Public Storage, Store Franchise, Misc. Improvements
Excluding Non-Commercial Building Codes	28,484	Type Unknown, Agricultural, Fruit, Building, House, Storage, Out Building, Equipment Building, Equipment Shed, Barn, Barn Pole, Creamery, Storage Building, Shed, Utility, Utility Storage, Farm, Cocktail Lounge, Caf, Fast Food, Club, Lounge/Nite Club, Fraternal, Tavern, Bar, Bar Cocktail Lounge, Basketball Court, Clubhouse, Country Club, Convention Center, Fitness Center, Recreation, Restaurant, Theater, Theater/Cinema, Gymnasium, Health Club, Skating Rink, Arcade, Government, City Club, Fire Station, Community Center, Community Service, Post Office, Elderly/Senior Housing, Loading Dock, Multi Family, Multi Family Low Rise, Multi-Plex, Apartment, Apartment Low Rise, Condo Apartment, Duplex, Rooming/Boarding House, Triplex, Residential, Manufactured Home, Cabin/Cottage, Cabin/Apartment, Mobile Home, Mobile Home Single Wide, Mobile Home Double Wide, Single Family, Hangar, Hangar Maintenance, Truck Terminal, Truck Stop, Distribution, Cold Storage, Industrial Light, Industrial Office, Processing, Industrial Condo, Bulk Storage, Food Storage, Manufacturing, Manufacturing Heavy, Manufacturing Light, Other, Research & Development, Warehouse, Warehouse Distribution, Mini Warehouse, Warehouse Storage, Mixed Type, Group Home, Auditorium/Gymnasium, Classrooms, Center, Convalescent, Dental, Museum, University, Veterinarian, Medical, Surgical Center, Office Medical, Office Dental, College, Church/Synagogue, Day Care Center, Hospital, Hospital Convalescent, Hospital Public, Veterinary Hospital, Dormitory, Kennel, Kennel Veterinary, Fraternity, Library, Library Museum, Nursing Home, Retirement Home, Mortuary, School, School Classroom, Elementary School, Clinic Dental, Dispensary, Dispensary Medical, Ymca/Ywca, Telephone, Mixed Use, Condo & Single Family Residenc, Miscellaneous Industrial, Office/Shop, Apartments & Residential
Excluding Selected Commercial Building Codes	22,287	Storage, Commercial Greenhouse, Lumber Store, Lumber Storage, Office, Medical Office, Auto, Auto Agency, Auto Showroom, Auto Sales, Auto Sales & Service, Auto Service, Laundromat/Dry Cleaners, Bank, Garage, Repair Garage, Barber Shop, Barber & Beauty Shop, Shop Office, Retail Office, Car Wash, Car Wash Drive Thru, Car Wash Automatic, Car Wash Self Service, Parking, Parking Garage, Marina, Hotel, Hotel/Motel, Motel, Department Store, Auto Repair, Garage Service
Excluding Parcels with Missing Square Footage or Missing Year Built	18,451	
Excluding Parcels Built After 2011	18,224	

Store", "Pharmacy" or "Warehouse Store" building code values. These two sets of restrictions aim to generate a sample of parcels for which the probability of selling alcohol is high, and who may have the greatest incentive to game their square footage in order to become license-eligible.

C.2 Nielsen Consumer Panel

Nielsen's Consumer Panel tracks household purchases of a wide array of products (including both food and non-food items), and it contains an entire product module labeled "liquor". Unfortunately, the liquor module corresponds only loosely to the WSLCB definition of spirits. For our principal analysis, we are interested in products formerly sold exclusively by the state monopoly. We therefore restrict our sample based on the following three criteria:

C.2.1 Coolers

Products that Nielsen describes as coolers ($product_module_descr = $"$COOLERS - REMAINING$") are not included, some 1,627 UPCs. 99.8% of these observations were not sold by WSLCB stores under the state monopoly, and none have an associated proof. 51% of cooler purchases before liberalization correspond to stores with 2-digit zip codes within Washington state, so it appears that Washington households purchased these goods at non-state stores before deregulation. Further, purchases by panelists in border and interior counties were equally likely to fall under the cooler category under the WSLCB (t-stat of 0.108). We therefore conclude these are products that were legally sold by Washington state supermarkets before liberalization.

C.2.2 Prior Purchases

Products purchased by households before liberalization that were not sold by the WSLCB state monopolist are not included in the sample. The WSLCB provides monthly price lists for products sold in state liquor stores from February 2010 May 2012. These lists include 3,973 unique products (UPCs). We merge WSLCB prices with the Nielsen panelist dataset on UPC.

Table C.2: CoreLogic Codes for ”All Potential Alcohol Retail Records” Sample

Corelogic Code Values – All Potential Alcohol Retail Records Sample		
Panel A: Property Indicator Code		
Type	Frequency	Percentage
Commercial	5,583	30.64%
Commercial Condominium	203	1.11%
Retail	12,438	68.25%
Panel B: Land Use Code		
Type	Frequency	Percentage
Commercial (NEC)	3,542	19.44%
Multiple Uses	10	0.05%
Commercial Building	391	2.15%
Commercial Condominium	203	1.11%
Misc. Building	103	0.57%
Misc. Commercial Services	1,398	7.67%
Shopping Center	590	3.24%
Strip Commercial Center	297	1.63%
Store Building	755	4.14%
Retail Trade	9,742	53.46%
Supermarket	167	0.92%
Food Stores	887	4.87%
Wholesale	139	0.76%
Panel C: Building Code		
Type	Frequency	Percentage
Commercial	7,078	38.84%
Market	309	1.70%
Supermarket	247	1.36%
Commercial Condo	96	0.53%
Store	17	0.09%
Food Stand	56	0.31%
Service	1	0.01%
Service Station	13	0.07%
Service Garage	180	0.99%
Shops	185	1.02%
Retail	4,445	24.39%
Retail Store	3,821	20.97%
Convenience Market	408	2.24%
Convenience Store	260	1.43%
Shopping Center	345	1.89%
Discount	339	1.86%
Discount Store	269	1.48%
Pharmacy	15	0.08%
Retail & Warehouse	12	0.07%
Warehouse Store	128	0.70%

Observations without WSLCB prices either correspond to spirits bought out-of-state or to products the WSLCB does not classify as spirits (and therefore potentially bought in-state). In the latter case, these products experience no regulatory changes and therefore ought to be excluded from our principal analysis. In the former case, we would tend to lose power by excluding part of the sample. To differentiate these theories, we check whether any of these products were purchased at retailers with non-Washington 3-digit zip codes: none do.

However, Nielsen notes that store zip codes are sometimes imputed from a panelist's home zip code, so we cannot rule out inter-state shopping trips. In total, 78.52% of purchases are matched to WSLCB prices - 86.67% have matches before liberalization 69.94% have matches after liberalization. This pattern is consistent with the introduction of new products in the private market post-liberalization.

C.2.3 Proof

We use regular expressions to extract proof from the Nielsen *upc_descr* string. We exclude 4,067 observations that correspond to product that are less than 48 proof, as per the state definition of spirits.

D Google Maps Square Footage Calculations

This appendix section presents further details on our square footage calculations using Google Maps Developers' Square Footage Calculator and Amazon Mechanical Turk. Google Maps Developers' Square Footage Calculator allows us to overlay a tool for calculating square footage on top of Google Maps, as shown in Figure D.1. Over an 2 week period in May, 2017, we hired workers on Amazon Mechanical Turk to perform this calculation for each store in our sample.

We hired workers on a per-task basis. To ensure high quality responses, we screened out workers whose acceptance rate for previous work was lower than 98%, and required them to have

Figure D.1: Example of a Square Footage Calculation

performed at least 1,000 tasks in the past. Furthermore, workers had to pass a qualification test, where they were asked to calculate the square footage of a set of 5 stores that we had previously done ourselves and found to require attention to detail. Finally, we announced (and paid out) bonuses for the 10 most accurate workers.

A task consists of calculating the square footage of a given store. Upon accepting a task, workers clicked-through to the Google Map Developers' Area Calculator website and inputted the store address. Then, they had to zoom in to an appropriate distance from the store, check that the store name appeared in the map, calculate the area, and enter the square footage into a text box. In cases where the store name did not appear on the map, workers could click-through to a new instance of the square footage calculator website where the store name had been inputted into the search box. If the store was still not found, the workers returned to the address-based search and calculated square footage for the given address.

The instructions used for the qualification test are found at the end of this Appendix section.

Instructions for other stores were mostly the same, but sometimes tailored to the specific characteristics of the store type. For example, we added instructions not to consider the pumps for calculating gas station square footage.

To ensure data quality, we hired multiple workers to calculate square footage for each store and use the average across their reports. After collecting data from MTurk, we also double-checked each store with recorded square footage between $5,000-15,000\text{ft}^2$, to ensure accurate responses around the licensure threshold. Despite these checks, some measurement error remains: 36 out of the 3,292 stores we code to be below $10,000\text{ft}^2$ are licensed to sell liquor. Based on our conversations with the WSLCB, we are confident that we have mismeasured square footage for these stores (in reality, they exceed $10,000\text{ft}^2$). Miscategorizing a store below (above) the threshold as above (below) weakly lowers (raises) the average entry probability above (below) $10,000\text{ft}^2$. We therefore expect measurement error to bias our regression discontinuity estimates downwards, as in Pei and Shen (2017)).

The MTurk dataset contains square footage for 94% of our sample (303 firms are missing). Since we measure square footage using the Google Maps in 2017, stores may be absent if they closed between 2012 and 2017. Missing data is therefore correlated with survival and other associated store characteristics: 12% of former state liquor stores are missing data, compared to 5% of the rest of the sample; 1% of chain stores, compared to 7% of independents. This measurement error is unlikely to be classical. If selling spirits is profitable, then survival should discontinuously increase at the licensure threshold. In that case, our discontinuity estimates are conservative, as we are missing more stores below the threshold (that do not sell liquor) than stores above it. However, the low incidence of missing stores allays our concerns that measurement error affects our estimates in section 3, particularly as we focus on chain stores, which have near complete coverage.

Figure D.2: Sample Instructions

Website data collection - Instructions (Click to collapse)

Click the link below to review the Website. Collect the following information if it's available:

- Go to the link below
- Copy and paste the Address, then click "Go!"
- Zoom into the pin and confirm the label matches the Store Name
- Outline the store by clicking on the corners.
- Corners can be rearranged with the white and grey circles
- Record the area in square feet as it appears below the address bar

Store Name	Address
STAMPEDE MINI MARKET	111 RIVERSIDE DR OMAK WA 98841

The store can sometimes be away from the pin:

Store Name	Address
HAROLD'S GENERAL MERCHANDISE	4080 HARRAH RD HARRAH WA 98933

Stores can sometimes share a building. Use Streetview (the yellow person icon) to approximate the store size:

Figure D.3: Sample Instructions (cont.)

Store Name	Address
TRADER JOE'S #134	1758 S 320TH ST FEDERAL WAY WA 98003

E N-Nearest Neighbor Based Specifications

E.1 Empirical Strategy

One challenge with estimating Equation 2 is choosing a distance radius d appropriate to the entire state. As an example, within Seattle, firms may compete only with other firms within walking distance, compared to Snohomish, where rival five or ten miles apart might compete intensely. We therefore estimate a second version of the rival entry regressions that does not rely on a driving distance radius. Instead, we create a metric based on the license eligibility of the n-nearest neighbors to store s. That is, for every store we calculate the distance to all other stores, and then focus on the n-nearest neighbors, analyzing entry decisions based on their license eligibility. We adapt Equation 2 as follows:

$$\begin{aligned} 1\left[\text{Has Liquor License}\right]_s = \quad & \alpha_0 + \alpha_1 \cdot 1\left[IsChain\right]_s + \alpha_2 \cdot N_s^{n,10-15} \\ & + \alpha_3 \cdot 1\left[IsChain\right]_s \cdot N_s^{n,10-15} + \sum_k \lambda_k^n \cdot 1\left[N_s^{n,5-15} = k\right] + \varepsilon_s \end{aligned} \tag{6}$$

where $N_s^{n,10-15}$ is the number of store s's n-nearest neighbors sized $10,000 - 15,000\text{ft}^2$. For example, if $n = 2$ and store s's two nearest neighbors nearest neighbors are $23,000\text{ft}^2$ and $12,000\text{ft}^2$, then $N_s^{n,10-15} = N_s^{n,5-15} = 1$. As before, we include fixed effects λ_k^n for the number of store s's n-nearest neighbors in the bandwidth. As in Equation 2, standard errors are clustered at the zip code level.

E.2 Covariate Balance and Results

Figure E.1: Covariate Balance Across License-Eligible Stores with Differing Numbers of License-Eligible N-Nearest Neighbors

Covariate Balance Across License-Eligible Stores with Differing Numbers of License-Eligible N-Nearest Neighbors
Bandwidth = 5000 square feet

	Neighbors Included:	1	2	3	4	5	6	7	8	9	10
Own Square Footage	Independents	-148,609	-156,240	-144,910	-131,324	-120,365	-216,465	-214,798	-210,730	-169,579	-160,530
		(141,434)	(155,816)	(142,820)	(129,377)	(119,254)	(215,033)	(215,408)	(209,958)	(171,333)	(161,219)
	Chains	57,396	52,761	55,561	56,511	56,103	-77,600	-73,903	-61,106	-48,547	-41,103
		(61,103)	(55,096)	(54,851)	(55,885)	(57,455)	(76,861)	(75,644)	(62,317)	(50,479)	(42,379)
Earliest Alcohol Licensure Date (Days)	Independents	83.6	630.6*	339.1	324.0	288.8	161.8	73.6	212.6	86.9	92.5
		(512.2)	(323.8)	(271.0)	(222.2)	(179.6)	(148.5)	(148.4)	(134.8)	(125.8)	(114.4)
	Chains	-497.9	-95.1	-112.8	-25.2	66.5	78.1	-0.8	42.6	-1.2	-32.8
		(332.9)	(245.9)	(197.1)	(185.1)	(157.7)	(131.3)	(131.1)	(112.6)	(100.3)	(100.4)
Total Alcohol-Related Fines Paid in 2010	Independents	65.5	26.0	-8.6	-6.7	-0.2	1.9	-1.9	-2.4	-0.2	-6.7
		(52.3)	(26.6)	(23.3)	(20.3)	(15.0)	(13.1)	(13.0)	(12.7)	(11.6)	(9.8)
	Chains	14.0	9.1	-12.1	-14.4	-9.9	-3.6	-6.4	-6.5	-5.8	-6.7
		(12.4)	(11.0)	(13.7)	(11.4)	(8.5)	(6.7)	(6.5)	(6.3)	(5.7)	(5.2)
Total Alcohol-Related Fines Paid in 2011	Independents	-26.4	-4.9	-14.3	-21.4	-30.6	-34.4**	-15.4	-17.5	-13.9	-14.8
		(18.6)	(26.0)	(19.2)	(15.1)	(22.7)	(17.1)	(20.1)	(18.9)	(15.1)	(17.5)
	Chains	58.1	31.1	23.8	23.4	7.9	3.1	0.7	1.7	3.2	1.7
		(40.2)	(23.4)	(18.9)	(15.3)	(15.0)	(8.5)	(8.0)	(7.2)	(7.8)	(9.9)
Total Alcohol-Related Fines Paid in 2012 (Pre-Liberalization)	Independents	-9.5	-4.8	2.2	1.4	0.3	1.0	1.5	-0.8	-0.2	-1.5
		(8.8)	(4.3)	(7.2)	(4.1)	(3.0)	(2.2)	(2.3)	(2.0)	(1.5)	(1.6)
	Chains	-10.2	3.6	0.4	2.3	1.4	4.1	3.0	0.5	1.5	0.1
		(8.4)	(7.2)	(5.9)	(5.8)	(4.5)	(3.4)	(2.8)	(2.2)	(1.8)	(1.8)
Zip Code Population	Independents	3,913	-890	807	1,339	227	1,466	2,088	1,926	1,214	2,121
		(4699)	(3550)	(3269)	(2492)	(2431)	(1764)	(1711)	(1642)	(1429)	(1395)
	Chains	1,277	1,775	2,706	2,122	1,573	1,190	1,220	1,034	841	702
		(2158)	(1792)	(1668)	(1614)	(1665)	(1341)	(1370)	(1232)	(1220)	(1259)
Zip Code Population Over 21	Independents	4,239	812	1,781	1,852	980	1,791	2,160*	1,989*	1,492	2,114**
		(3371)	(2344)	(2168)	(1711)	(1635)	(1208)	(1165)	(1170)	(1001)	(953)
	Chains	2,139	2,228*	2,616**	2,139*	1,904*	1,507	1,376	1,207	1,133	1,015
		(1550)	(1236)	(1162)	(1138)	(1149)	(916)	(910)	(836)	(839)	(838)
Zip Code African American Population	Independents	75	152	-113	-157	-187	-74	-162	-190	-323**	-139
		(75)	(152)	-(113)	-(157)	-(187)	-(74)	-(162)	-(190)	-(323)	-(139)
	Chains	-195	173	236	137	5	-92	-169	-180	-170	-186
		-(195)	(173)	(236)	(137)	(5)	-(92)	-(169)	-(180)	-(170)	-(186)
Zip Code Hispanic Population	Independents	-1,831	-2,220	-1,463	-824	-1,408	-930	-685	-604	-599	-403
		(1176)	(1176)	(1176)	(1176)	(1176)	(1176)	(1176)	(1176)	(1176)	(1176)
	Chains	-1,567	-1,495	-851	-554	-862	-695	-311	-282	-378	-378
		(1977)	(1458)	(1226)	(951)	(1103)	(869)	(852)	(792)	(794)	(794)
Zip Code Median Age	Independents	1.6	-0.2	0.0	-0.4	-0.4	-0.5	-0.3	-0.1	0.0	-0.1
		(2.4)	(1.3)	(1.3)	(1.1)	(1.0)	(0.7)	(0.7)	(0.7)	(0.6)	(0.5)
	Chains	0.4	-0.1	-0.2	-0.1	0.1	0.4	0.2	0.4	0.4	0.3
		(1.0)	(0.8)	(0.7)	(0.7)	(0.6)	(0.5)	(0.5)	(0.5)	(0.5)	(0.5)
Zip Code Unemployment Rate	Independents	0.1	-0.7	-0.8	-0.3	-0.5	-0.7**	-0.6*	-0.4	-0.4	-0.5
		(1.3)	(0.7)	(0.6)	(0.5)	(0.4)	(0.3)	(0.4)	(0.4)	(0.3)	(0.3)
	Chains	-1.3**	-1.0**	-0.7*	-0.4	-0.6	-0.7**	-0.5	-0.5*	-0.5*	-0.5*
		(0.6)	(0.4)	(0.4)	(0.4)	(0.3)	(0.3)	(0.3)	(0.3)	(0.3)	(0.3)
Zip Code Median Household Income	Independents	3,866	-2,289	-2,215	-3,463	-2,412	957	1,297	813	1,261	1,557
		(4081)	(3823)	(3234)	(2964)	(2471)	(2174)	(2131)	(2116)	(1934)	(1914)
	Chains	9,755**	5,728.7*	3,042	610	1,983	3,299	2,225	1,998	1,720	1,437
		(4038)	(2965)	(3051)	(2924)	(2438)	(2034)	(1972)	(1805)	(1701)	(1749)
Zip Code Percentage of Population with Less than High School Education	Independents	-3.6	-4.1**	-4.7***	-2.2	-2.8*	-2.6**	-2.2*	-1.5	-1.6	-1.5
		(2.2)	(1.7)	(1.6)	(1.7)	(1.6)	(1.3)	(1.3)	(1.4)	(1.1)	(1.1)
	Chains	-2.9**	-2.5**	-2.1**	-1.4	-2.0*	-2.1**	-1.2	-1.1	-1.2	-1.1
		(1.3)	(1.0)	(1.0)	(1.1)	(1.0)	(0.9)	(1.0)	(0.9)	(0.9)	(0.9)
Zip Code Percentage of Population with High School Education	Independents	-2.7	-4.9**	-5.4***	-4.7***	-4.0***	-3.5***	-3.0***	-2.6***	-2.6***	-2.6***
		(2.6)	(2.0)	(1.8)	(1.3)	(1.1)	(0.9)	(1.0)	(0.9)	(0.8)	(0.8)
	Chains	-6.2***	-4.6***	-3.4***	-2.8**	-2.9**	-2.5***	-1.8*	-1.7**	-1.9**	-2.0**
		(1.8)	(1.3)	(1.3)	(1.3)	(1.2)	(1.0)	(0.9)	(0.8)	(0.8)	(0.8)
Zip Code Percentage of Population with BA or Higher	Independents	6.0	9.7**	10.0***	7.3***	6.7***	6.2***	5.5***	4.7**	4.9**	4.9**
		(5.4)	(4.2)	(3.7)	(2.7)	(2.4)	(2.0)	(2.1)	(2.2)	(2.0)	(1.9)
	Chains	13.4***	9.6***	7.4***	6.1**	6.9***	6.4***	4.4**	4.1**	4.3**	4.3**
		(3.7)	(2.8)	(2.8)	(2.7)	(2.4)	(2.0)	(2.0)	(1.9)	(1.9)	(1.9)
Zip Code Percentage of Population in Poverty	Independents	-3.4	0.8	0.6	1.7	1.2	-0.1	-0.3	0.3	-0.1	-0.3
		(2.2)	(1.8)	(1.5)	(1.6)	(1.3)	(1.1)	(1.1)	(1.0)	(0.9)	(0.9)
	Chains	-1.7	-1.4	-0.5	0.1	-0.6	-1.1	-0.5	-0.5	-0.7	-0.6
		(1.3)	(1.0)	(0.9)	(1.0)	(0.9)	(0.7)	(0.7)	(0.7)	(0.7)	(0.7)
N		1176	1176	1176	1176	1176	1176	1176	1176	1176	1176

Notes: For a given retailer, define N-nearest neighbors as the N closest stores to it. This table presents results of a linear regression of different store characteristics on a constant and the interaction between a chain store dummy and the count of the N-nearest neighbors who are above the 10,000 square foot licensure threshold, but below 15,000 square feet. All specifications include fixed effects for the total number of stores between 5,000 and 15,000 square feet. The sample is restricted to stores who are not former state liquor stores, are elegible to sell liquor, and have at least one neighbor in the bandwidth. Robust standard errors with clustering at the zip code level in parentheses. Coefficients are significant at the * 10%, ** 5% and *** 1% levels.

Figure E.2: Effect of License Eligibility of N-Nearest Stores on Own Entry Decisions

	Effect of N-Nearest Neighbors' License Eligibility on Own Entry Decision Bandwidth = 5000 square feet										
	Neighbors Included	1	2	3	4	5	6	7	8	9	10
Independents	# of Marginally License Eligible Neighbors	-0.015 (0.121)	-0.140* (0.073)	-0.118* (0.062)	-0.081 (0.056)	-0.101* (0.052)	-0.112** (0.045)	-0.108** (0.044)	-0.091** (0.043)	-0.087** (0.039)	-0.093** (0.036)
	Baseline Entry Probability	0.311*** (0.023)	0.324*** (0.024)	0.327*** (0.025)	0.327*** (0.026)	0.334*** (0.026)	0.343*** (0.027)	0.344*** (0.028)	0.341*** (0.028)	0.346*** (0.029)	0.349*** (0.029)
Chains	# of Marginally License Eligible Neighbors	-0.011 (0.056)	-0.003 (0.038)	-0.021 (0.030)	-0.010 (0.028)	-0.022 (0.025)	-0.016 (0.022)	-0.019 (0.021)	-0.020 (0.022)	-0.017 (0.020)	-0.010 (0.017)
	Baseline Entry Probability	0.952*** (0.008)	0.952*** (0.008)	0.956*** (0.008)	0.954*** (0.009)	0.957*** (0.009)	0.956*** (0.009)	0.958*** (0.009)	0.959*** (0.010)	0.958*** (0.010)	0.957*** (0.010)
# of Neighbors in the Bandwidth FE		x	x	x	x	x	x	x	x	x	x
N		1223	1223	1223	1223	1223	1223	1223	1223	1223	1223

Notes: For a given retailer, define N-nearest neighbors as the N closest stores to it. This table presents results of a linear regression of a licensure dummy on a constant and the interaction between a chain store dummy and the count of the N-nearest neighbors who are above the 10,000ft^2 licensure threshold, but below 15,000ft^2. All specifications include fixed effects for the total number of stores 5,000-15,000ft^2. The sample is restricted to stores who are not former state liquor stores, are eligible to sell liquor, and have at least one neighbor in the bandwidth. Robust standard errors with clustering at the zip code level in parentheses. Coefficients are significant at the * 10%, ** 5% and *** 1% levels.

Figure E.3: Effect of License Eligibility of N-Nearest Stores on Own Entry Decisions

	Effect of N-Nearest Neighbors' License Eligibility on Liquor Sales Bandwidth = 5000 square feet										
	Neighbors Included	1	2	3	4	5	6	7	8	9	10
Independents	# of Marginally License Eligible Neighbors	15,756 (27,353)	-4,382 (19,702)	10,761 (14,049)	22,160 (16,180)	17,729 (12,958)	7,483 (11,474)	9,531 (10,976)	16,278 (11,577)	11,527 (10,338)	9,518 (9,234)
	Baseline Sales	30,955*** (4,350)	32,486*** (4,707)	30,470*** (4,779)	28,050*** (4,562)	28,280*** (4,407)	29,773*** (4,571)	28,945*** (4,800)	27,033*** (5,040)	27,302*** (5,172)	28,464*** (5,130)
Chains	# of Marginally License Eligible Neighbors	38,017 (41,293)	61,232* (33,987)	81,539*** (27,240)	66,247*** (22,702)	57,657*** (21,453)	50,376** (19,926)	45,883** (19,099)	37,321** (17,642)	44,825*** (14,711)	33,976** (13,439)
	Baseline Sales	249,678*** (9,562)	245,130*** (10,070)	238,317*** (9,901)	238,337*** (9,782)	238,036*** (9,796)	237,670*** (9,691)	237,292*** (9,776)	238,334*** (9,975)	234,089*** (9,861)	236,926*** (9,843)
# of Neighbors in the Bandwidth FE		x	x	x	x	x	x	x	x	x	x
N		1223	1223	1223	1223	1223	1223	1223	1223	1223	1223

Notes: For a given retailer, define N-nearest neighbors as the N closest stores to it. This table presents results of a linear regression of a licensure dummy on a constant and the interaction between a chain store dummy and the count of the N-nearest neighbors who are above the 10,000ft^2 licensure threshold, but below 15,000ft^2. All specifications include fixed effects for the total number of stores 5,000-15,000ft^2. The sample is restricted to stores who are not former state liquor stores, are eligible to sell liquor, and have at least one neighbor in the bandwidth. Robust standard errors with clustering at the zip code level in parentheses. Coefficients are significant at the * 10%, ** 5% and *** 1% levels.