Deregulation through Direct Democracy: Lessons from Liquor Markets^{*}

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Abstract

This paper examines the merits of state control versus private provision of spirits retail, using the 2012 deregulation of liquor sales in Washington state as an event study. This is the first shift from a public to a private system for spirits sales in the United States since Prohibition. We document effects along a number of dimensions: prices, product variety, convenience, substitution to other goods, state revenue, and consumption externalities. We estimate a demand system to evaluate the net effect of privatization on consumer welfare. Our findings suggest that deregulation harmed the median Washingtonian, even though residents voted in favor of deregulation by a 16% margin. Further, we find that vote shares for the deregulation initiative do not reflect welfare gains at the ZIP code level. We discuss implications of our findings for the efficacy of direct democracy as a policy tool.

This paper explores the tradeoffs between state monopoly and the private market in the context of liquor retail. On the one hand, competition among private retailers may lead to better location, employment, pricing, and variety decisions than state monopoly (Schleifer & Vishny (1994)). On the other, a state system may better mitigate the externalities associated with liquor consumption, such as drunk driving and domestic abuse (Megginson & Netter (2001)). A priori, therefore, it is unclear how best to organize the market for liquor, and consequently, the fifty states have adopted widely different approaches. We revisit this question using data from Washington state, where in 2011 ballot initiative 1183 asked voters

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[†]Researcher(s) own analyses calculated (or derived) based in part on data from The Nielsen Company (US), LLC and marketing databases provided through the Nielsen Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the Nielsen data are those of the researcher(s) and do not reflect the views of Nielsen. Nielsen is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

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whether they wanted to switch from a state to a private system. The initiative passed with 58% of the vote share, ending a 79-year-old state monopoly over liquor sales. This paper estimates the welfare consequences of deregulation in Washington, with particular attention to its distributional effects. We then consider implications for policy-making through direct democracy.

We begin by documenting how privatization changed spirits retail. Before deregulation, the Washington State Liquor Control Board ran both distribution and retail, overseeing approximately 330 retail outlets across the state. It implemented a 51.9% uniform markup across all spirits products, and levied two additional taxes: a 20.5% proportional tax and a \$3.7708 per liter tax. Ballot initiative 1183 mandated the WSLCB auction the licenses to existing state liquor stores¹ and sell any remaining inventory. As of June 2012, new licenses were to be issued to private retailers with premises larger than 10,000ft².² Additionally, two new fees were imposed on top of existing taxes: a 10% fee on distributors and a 17% fee on retailers.³ The stated purpose of these fees was to replace lost state profits from spirits sales.

Using both event-study and difference-in-differences approaches with Oregon as a control, we find that privatization increased spirits liters purchased by approximately 13%. This increase in purchasing is the combined effect of three striking changes in Washington's retail landscape: a four-fold increase in the number of liquor outlets across the state; an 11% increase in prices; and a dramatic reduction in product variety. However, we find no commensurate change in drunk driving or emergency room admissions.

Our second main result is that deregulation harms most consumers, although a few experience large benefits. These findings are somewhat hard to square with the overall increase in purchasing at deregulation, perhaps in part because we borrow estimates of demand parameters from Seim & Waldfogel (2013), whose data is from Pennsylvania. We plan to estimate a discrete choice model of spirits demand with heterogenous consumers using data from Washington state. Our model extends the Seim & Waldfogel (2013) framework to allow for two different sources of heterogeneity. First, welfare differences may stem from observed differences in market structure. As an example, the densest areas, such as Seattle, see the greatest entry. Second, different types of consumers may value changes in convenience and price differently.

These preliminary results on welfare are difficult to reconcile with median voter models, which predict that policy mirrors the median voter's preferences. Indeed, we find that ZIP code level vote shares do not reflect the distribution of utility estimates, even controlling for religiosity and other demographics. One theory consistent with our findings is that citizens are unable to anticipate the full consequences of liquor deregulation, as in Dal Bo *et al.*

¹Auction winners won the right to sell spirits on a premises less than 10,000 ft² within one mile of the former state store. ²With a \$316 application fee.

³"Selling Spirits in Washington State." Washington Department of Revenue. [https://dor.wa.gov/sites/default/files/legacy/Docs/Pubs/Spirits/SpiritTax_DeterminingTax.pdf]

(2017). The requisite counterfactual calculation is potentially quite complex, involving predictions of firm entry, product inventory choices, and pricing. If citizens vote naively, they may therefore elect suboptimal policies. Washington's deregulation provides some of the first evidence from the field on the efficacy of direct democracy, which is employed by some 37 states to adjudicate contentious policy issues, including marijuana legalization and the death penalty.⁴

The rest of the paper is structured as follows: section 1 frames our contribution relative to the literature. Section 2 describes the data, and reduced-form evidence on deregulation is provided in section 3. Section 4 develops a model of spirits purchasing, while section 5 presents estimates of consumer welfare. Section 6 provides evidence on voting behavior, and section 7 concludes.

1 Prior Literature

Our paper bridges two literatures: one on the costs and benefits of state control, and a second on the merits of direct democracy. We document that the costs and benefits of deregulation are distributed unevenly across the population, and then ask whether voting outcomes reflect this heterogeneity.

In a seminal paper, Schleifer & Vishny (1994) develop a model of state provision where the regulator experiences political pressure; in consequence, he chooses suboptimal retail locations, prices, and employment levels. These drawbacks must be weighted against the possibility that regulation may target the socially optimal level of consumption for goods like spirits, where consumption externalities loom large. Indeed, the WSLCB advertised the low-level of spirits purchases in ABC states as a feature of regulation. See Megginson & Netter (2001) for an overview of this literature, which includes other mechanisms for state distortions. One approach to quantifying the merits of state control is to exploit cross-sectional or inter-temporal variation in regulation. For example, La Porta & López-De-Silanes (1999) study a wave of privatizations in Mexico between 1983-1991. They find that privatization improves firms profitability due to worker layoffs, price hikes, and increased productivity. While our findings are consistent with theirs, we see our strategy as complementary. In contrast to La Porta & López-De-Silanes (1999), we drill down on a single industry. This focus allows us to explore other outcomes, such as product variety and access, and also to estimate consumer welfare directly.

A second strand of empirical work focuses on a deregulation in a single industry. This includes work on airlines (e.g. Borenstein & Rose (2014)), electricity (e.g. Davis & Wolfram

 $^{^4}$ For a list of the states which allow referenda and initiatives, see: http://www.ncsl.org/research/elections-and-campaigns/chart-of-the-initiative-states.aspx.

(2012)), and liquor itself: Aguirregabiria *et al.* (2016), Eckert & West (2008), Miravete *et al.* (2017), and Seim & Waldfogel (2013). Seim & Waldfogel (2013) ask how Pennsylvania's state monopoly retail locations compare to simulated private-market locations. Similarly, Miravete *et al.* (2017) compare prices under the Pennsylvania State Monopoly to profit-maximizing prices. Both papers rely on structural modeling because they do not observe variation in Pennsylvania liquor regulation. For example, Seim & Waldfogel (2013) struggles with the dimensionality of the problem – simulating the full free-entry benchmark involves solving a game across a large set of competitors. In our setting, no such modeling is required because we observe private market locations when Washington deregulated.

Other work has exploited the Washington deregulation to learn about related economic phenomena: Illanes & Moshary (2018) exploit the licensure threshold to estimate the effects of market structure; Huang *et al.* (2018) examine how private firms learn about liquor demand in the wake of deregulation; LoPiccalo (2016) studies tax-avoidance by examining changes in Oregon's tax revenue; and Seo (2016) examines the boon to consumers from one-stop shopping for groceries and spirits.

Finally, this paper relates to a literature on direct democracy. Proponents of initiatives (and their cousins, referenda, where voters decide on measures already passed by the legislature) argue that these mechanisms might solve principal-agent problems. If politicians are hard to monitor, then they may implement policies that they prefer at the expense of the electorate. Initiatives allow voters to undo harmful policies, and even the threat of an initiative might act as a check on such self-interested behavior. On the other hand, voters may enact harmful policies themselves if they are uninformed (Matsusaka (2005)). In the case of Initiative 1183, to fully understand the impact of the deregulation requires significant effort: predict the set of entrants in the private market and their product variety and price decisions. This task is difficult - Seim & Waldfogel (2013) is a case in point. In a series of recent lab experiments, Dal Bo *et al.* (2017) find that subjects predict general equilibrium effects but poorly. On the other hand, voters may decide based on simple cues or heuristics that proxy for their best interests.

Empirical evidence on the use of referenda generally compare outcomes, such as spending and taxation, across states where initiatives and referenda are and are not available (e.g. Bowler & Donovan (2004)). This approach raises two concerns: first, institutions, such as direct democracy, may be endogenous to other state characteristics; and second, it is hard to measure "good" policy. Kahn & Matsusaka (1997) identify the correlates of voting on environmental initiatives, which include proxies for the costs of the initiatives (in particular, exposure to industries affected by the regulation). Hastings *et al.* (2007) examine whether voting behavior in school board elections is driven by past experiences with the school system. In this paper, we implement an alternative approach to study voting: we directly measure gains of deregulation using a revealed preference approach, and then compare gains and voting behavior.

2 Data

2.1 WSLCB

Our data on spirits sales under the state monopoly come from the Washington State Liquor Control Board, which provides quantities and prices at the outlet-product (UPC) level from November 2010 - May 2012. During this period, 332 different outlets operated in Washington state, with average monthly revenue of \$337,090. Table 1 includes other summary statistics for these stores, such as the average number of unique products sold (1,299). For each outlet, the WSLCB also provides the street address and monthly revenues from 2007 through deregulation. During these five years, 26 stores enter and 22 stores exit spirit sales.

Summary Statistics for WSLCB Sales							
	# Observations	Mean	SD	Min	Max		
Product							
Price	5153971	20.8669	18.91022	.75	3649.95		
Quantity Sold	5153971	16.89424	45.14392	1	10293		
Store							
Revenue	3968	337.0899	360.3818	1.26015	4083.305		
Bottles Sold	3968	21943.66	21605.71	132	212630		
Number of Products	3968	1298.884	845.1331	10	4511		
Notes: Based on monthly data from 332 WSLCB retail outlets spanning November 2010 - May 2012.							

Table 1: Summary Statistics for WSLCB Sales

2.2 Nielsen Data

The Nielsen Panel and Scanner datasets provide information on prices and quantities of spirits sales following deregulation. The scanner dataset includes sales at 670 outlets from 10 chains that sell spirits between 2012 and 2015 in Washington state. The average outlet earned \$48,595 in monthly revenue, and the scanner stores accounts for approximately 40% of spirits revenue in the state.

We augment the Scanner data with information from Nielsen's Homescan, which tracks purchases of a revolving panel of households. Importantly, panel households report all spirits purchases, and so provide information about prices and assortment at non-Scanner stores. The Panel data includes some 2,700 households residing within Washington between January 2010 and December 2015. Of these households, 1,357 purchase liquor at least once during this five-year period. The panel data also includes demographic information, which provides insight into preference heterogeneity.

2.3 Voting Data

Finally, we incorporate publicly available voting data at the precinct level from the Washington Secretary of State website. Washington encompasses 6,784 precincts,⁵ of which we match 6,650 to 614 zip codes. Initiative 1183 earned 58% of the vote share statewide, although 20% of precincts voted against the reform.

3 Evidence on Deregulation

3.1 Changes in Purchasing

We first establish the impact of deregulation on spirits purchasing using quarterly data on the volume of liters sold from the Washington Department of Revenue. Figure 1 shows purchasing from January 2007 - January 2017. There is a clear pattern of seasonality, with the first quarter of the year consistently having lower volumes. Moreover, purchases are steadily rising over time. To account for these features, we estimate the change at deregulation using the following specification:

$$y_t = \alpha + \beta \cdot Post_t + \gamma \cdot t + \Lambda_t + \epsilon_t \tag{1}$$

where y_t is liters sold (in millions), Λ_t are quarter-of-the-year fixed effects, γ is a linear time trend, and β , the coefficient on an indicator variable for post-reform, is the coefficient of interest.⁶ We estimate that the volume sold increased by 13.35% at deregulation, which is consistent with reports from the Washington Office of Financial Management.⁷

⁵Sixty-nine precincts had zero votes cast, and so we exclude them from the analysis.

 $^{^{6}}$ Note that we exclude the second quarter of 2012 as deregulation occurred part-way through its duration.

⁷For example: The Impact of Initiative 1183, January 2015.





While these patterns speak to trends in aggregate consumption, we consider the distributional of purchasing changes using data from Nielsen's Homescan. The Homescan data allows us to examine how individual household purchasing changed at deregulation. Importantly, the Homescan data also contains out-of-state spirits purchases, which are missing from the DOR data. Indeed, LoPiccalo (2016) suggests that Oregon stores along the border experienced a boost in revenue when Washington deregulated, indicating the cross-border shopping behavior may have shifted precisely at deregulation. To allow for this possibility, we estimate the following regression specification, where y_{ht} is an outcome of interest for household h at time t:

$$y_{ht} = \alpha + \beta \cdot Post_t + \gamma \cdot t + \Lambda_h + \epsilon_{ht}.$$
(2)

We include household fixed effects Λ_h , which control for any time-invariant determinants of households' spirits purchasing. We estimate (2) using a six-month window around deregulation and cluster standard errors at the household level. Using a variety of measures for liquor purchasing (e.g. liters of ethanol, alcohol, and total spending), results indicate a significant boost in purchasing. Strikingly, the probability that a given household purchases liquor in a month doubles, from 7.6% to 15.1%, and changes in expenditures, number of liquor purchasing trips, and number of liquor products bought have similar magnitudes. Liters of ethanol purchased per person increase 62%, while average transacted prices increase by 28%.

We then group households into three bins, based on their pre-deregulation purchasing behavior: teetotal (no spirits purchases between January 2010 - May 2012), moderate drinkers (50th - 75th percentile in pre-period average monthly expenditures per person) and heavy

Change at Deregulation: Event Study							
	Purchase Liquor (1)	Quantity (L) (2)	Ethanol per Person (L) (3)	Spending (\$) (4)	Average Price per Product (\$) (5)	Number of Products (6)	Number of Liquor Trips (7)
Change at Deregulation	0.075*** (0.008)	0.163*** (0.036)	0.034*** (0.008)	3.793*** (0.615)	5.213*** (1.124)	0.128*** (0.018)	0.113*** (0.016)
State Monopoly Level	0.076	0.230	0.055	3.920	18.595	0.156	0.123
Observations:	19624	19624	19624	19624	2025	19624	19624
Notes: Monthly data spans December 2011 - January 2012. All regressions include a linear time trend and household fixed effects. Standard errors are clustered at the household level. Coefficients are significant at the *10%, **5%, and ***1% level.							

Table 2: Change at Deregulation: Household-Level Evidence

drinkers (top quartile in the same). We estimate (2) using interactions between $Post_t$ and an indicator for each group, and find that gains operate on both the extensive and the intensive margin. Households who did not consume liquor prior to deregulation now have a 4.7% probability of doing so, while the heaviest-drinking households exhibit an 8.6% increase.

Effect of Deregulation by Pre-Period Drinking Behavior							
Outcome is Log (1 +)	Quantity (L)	Ethanol per Person (L)	Expenditures (\$)	# Liquor Trips			
Post	0.047***	0.014***	0.169***	0.043***			
	(0.008)	(0.003)	(0.021)	(0.006)			
Post $ imes$ 50-75th Percentile	0.004	0.001	0.010	-0.005			
	(0.012)	(0.004)	(0.036)	(0.010)			
Post X Top Quartile	0.039**	0.019**	0.124**	0.029*			
	(0.019)	(0.009)	(0.049)	(0.016)			
Household Fixed Effects	х	х	х	х			
Observations	95376	95376	95376	95376			
Notes: Observations are at the month-household level. All regressions include a linear time trend and household fixed							
effects. Standard errors are clustered at the household level. Coefficients are statistically significant at the *10%, **5%, and							
***1% level. Data from January 2010 - December 2015.							

Table 3: Change at Deregulation: by Pre-Period Drinking Behavior

We also consider heterogeneity across demographics, such as income and education. Differences in the objective of the WSLCB compared to private firms (such as equity vs profitability) may lead to differential effects of deregulation across Washingtonians. For example, Miravete *et al.* (2017) find that the Pennsylvania Liquor Commission subsidizes liquor favored by African Americans. Unfortunately, minorities, very low and very high income households are underrepresented in the homescan data, so it is difficult to estimate effects for these groups using the Nielsen dataset. We therefore use Census data on ZIP code



Figure 2: Change at Deregulation across Demographics

Notes: At the zip code level.

demographics and revenue data at the retailer level. We employ the following specification:

$$logr_{zt} = \alpha + \delta' X_z \times Post_t + \Lambda_t + \Gamma_z + \epsilon_{zt}$$

where Λ_t are month fixed effects and Γ_z are ZIP code fixed effects. We are interested in estimates of δ , the coefficients on the interaction terms between a post indicator and demographics. These demographics include: % White, median household income, median age, % with a Bachelors degree or beyond, and % less than high school. Figure 2 presents coefficient estimates. We find that zip codes with higher median household income exhibit no differential consumption increase at deregulation, while zipcodes with higher percentage white, higher median age and higher fractions of the population with less than a high school degree have lower consumption increases at deregulation. Finally, zip codes where a higher fraction of the population have more than a BA degree have a greater consumption increase at deregulation.

3.2 Externalities

Concern that spirits consumption generates externalities constitutes the chief justification for state control of alcohol retail. In this section, we examine whether deregulation exacerbated these externalities in the Washington context.

We first consider how deregulation changed on-premise spirits purchasing. If consumers substitute away from imbibing spirits at bars or restaurants, the change in total alcohol consumption will be smaller than the effect on off-premise purchasing. Moreover, liquor consumption externalities might be different when liquor is consumed at home versus in an bar or restaurant. Figure 1 presents the quarterly sales of liters to on-premise licensees per capita.⁸ If anything, on-premise consumption appears to have increased after deregulation. We present regression estimates of specification (1) in table 4 column 4. The coefficient on the post indicator is positive but statistically insignificant, suggesting that consumers did not substitute away from on-premise consumption.

Spillovers of Liquor Deregulation							
	Log State Revenue (1)	Log 1+ Expend Out-of-State (2)	Log 1+ Beer Sales (Oz) (3)	Log On-Premise Liquor Sales (L) (4)	Log Domestic Violence Hotline Calls (5)	Log Car Accidents (6)	Sales to Minors (7)
Post	0.213	0.003***	-0.031	0.021	-0.281**	-0.110*	0.002**
	(0.342)	(0.001)	(0.021)	(0.014)	(0.085)	(0.059)	(0.001)
Post × Alcohol-Related	_	_	-	-	_	-	0.001 (0.001)
FE		Hhold	Hhold				
Level of Observation	Year	HHold x Month	HHold x Month	Quarter	Year	Month	Month
Observations	8	149395	149395	22	8	71	132
Notes: Heteroskedasticity-robust standard errors in parentheses in columns 1 & 4-7. Clustered standard errors at the household level in columns 2 & 3. Coefficients are significant at the * 10%, **5% and ***1% level. All outcomes measured per-capita. All regressions include a linear time trend. Columns 3, 4, 6 & 7 exclude June 2016. The omitted category in column 7 is sales of tobacco to individuals under 18.							

Table 4:	Spillovers	of Liquor	Deregulation
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A second possibility is that consumers might substitute away from off-premise beer and wine purchasing following liquor deregulation. To be clear, prior to referendum I-1183, private retailers could obtain a license to sell beer and/or wine, and may continue to sell these products following deregulation. Using data on monthly beer purchases for each household in Nielsen's Homescan as our dependent variable, we estimate (2) with standard errors clustered at the household level. The coefficient estimate is presented in table 4 column 3. We detect no break in purchasing following deregulation.

Our evidence so far has focused on changes in purchasing, and now we turn to directly estimating the effect of deregulation on adverse outcomes for consumers but also for bystanders. We collect data on three measures of externalities: domestic violence hotline call volumes, alcohol-related driving accidents, and spirits sales to minors.

While underreporting of alcohol-induced domestic is a concern, we employ data from the Washington State Department of Health and Social Services on crisis calls as a proxy for abuse. Annual call volumes include the state hotline but also other emergency domestic violence shelter programs from 2009-2016. In table 4, column 5 we present estimates of specification (1) with the log calls per capita as the left-hand-side variable. The coefficient on the post indicator is negative and suggests a decline of 20% in hotline calls in the years following deregulation. While we are cautious in interpreting the coefficient on the post

 $^{^{8}\}mathrm{Data}$ is from the Washington Department of Revenue.





indicator as the causal effect of deregulation on domestic violence (the data is coarse and only proxies for the underlying incidence of abuse), the sign is inconsistent with a large increase in violence at deregulation.

Figure 3 shows the log number of car accidents per capita involving alcohol over time, using data from the Washington Department of Transportation. Visual inspection suggests no break at deregulation, but rather a decline in January 2013. Although the coefficient estimate on the post indicator in table 4 is negative and marginally significant, it loses significance when we restrict to a 6-month window around deregulation. We therefore hesitate to interpret the drop in 2013 as causal, but again note that it the estimates are inconsistent with a rise in externalities at deregulation.

A second concern is that private retailers might be less vigilant in enforcing minimum drinking age requirements (21 years). Both before and after privatization, the WSLCB audited retailers to detect violations of this law. We collect these records to examine how violations changed at deregulation. One concern is that estimates based on the time series variation alone might be confounded by contemporaneous changes to Washington spirits demand or regulation. We therefore employ a difference-in-differences strategy, where we use tobacco violations (sales to under 18s) as a control group. The identification assumption is that absent deregulation, these two types of violations would follow similar trends to spirits violations. Let A_j take a value of 1 if the violation involves alcohol rather than tobacco and v_{jt} be the number of violations of type j in month t, which we model as

$$v_{jt} = \alpha + \beta \cdot Post_t \times A_j + A_j + \lambda_t + \gamma \cdot t + \epsilon_{jt}.$$

Visual inspection of figure 3 indicates that spirits violations peak following deregulation, but level well within a year of the change. One difficulty in interpreting this pattern is that we observe only those violations that are detected by the WSLCB, rather than the true number of violations. If the WSLCB intensified audits at deregulation, then the spike might merely reflect the change in auditing. Alternatively, the spike might signify a period of adjustment where private retailers learn how to prevent sales to minors. We present estimates of the regression coefficients in table 4. The coefficient on the interaction between violations and alcohol is small and statistically insignificant, indicating no long-run change in sales to minors.

Finally, the monopoly on spirits sales generated significant profits for the state, which helped to fund other programs. To compensate for this lost profit, the state levied two new fees at deregulation: 10% on distributors and 17% on retailers. Nonetheless, if privatization defunded important state programs, then it might have decreased overall welfare. Figure 4 shows the evolution of state revenue distributions plus tax collections before and after deregulation. If anything, deregulation was a windfall for the state government.



Figure 4: State Revenues from Spirits Sales

3.3 Price Changes

In this section, we compare prices under the State Monopoly to those under the private market. Ex ante, it is hard to predict how deregulation might affect prices. On the one hand, the state maintained a 51.9% markup on all spirits products, which is higher than the industry average (CITE). Competition between private firms might therefore lower markups. On the other hand, the new fees levied at deregulation should put upward pressure on prices. Finally, acquisition costs might differ between the state and private retailers, creating further price divergence.

Because the liquor category encompasses over 3,000 products, we construct a Tornqvist price index to assess the change in the price level. The Tornqvist is meant to capture substitution across products as relative prices shift. We construct the Tornqvist by merging two datasets: data from the WSLCB on the sales of each product under the State Monopoly, and Nielsen's ScanTrack, which contains all sales at a set of scanner stores following deregulation. We match WSLCB and Nielsen products using universal product codes (UPCs).⁹ Figure 6a shows the Tornqvist over time; prices jump 15% at deregulation and remain at the elevated level thereafter.

Of course, estimates based on the ScanTrack data do not include price data from retailers that do not partner with Nielsen. Based on comparisons with data on liters sold from the Department of Revenue, we estimate that Scanner Store sales compose approximately 45% of the spirits market in Washington (figure 1 shows the evolution of scanner and total sales over time). We therefore re-calculate the Tornqvist using the Homescan dataset, which includes sales at all stores. However, the price data in the Homescan data is sparse, as only 2,700 households in Washington State participate in the panel between January 2010 and December 2015. Reassuringly, the Homescan-based Tornqvist (figure 6c) shows the same pattern, an approximate 9% increase in prices at deregulation. As a robustness check, we construct the Tornqvist for Oregon households in the Homescan dataset and find no change at the date of Washington's deregulation (figure 6d).

We next consider heterogeneity in price changes across products. Miravete *et al.* (2017) suggest that the PLCB subsidizes prices for products favored by certain constituents - those with a taste for rum and tequila. Figure 6 plots the log of the State Monopoly price in 2012 against the log of the average price at Scanner stores in 2015 for each product. As a benchmark, we include a line with a slope of 1.27; this line reflects a counterfactual where retailers maintain WSLCB prices with perfect pass-through of the new fees. Consistent with Miravete *et al.* (2017), we find that relative prices increase most for low-end products (those with the lowest prices under the WSLCB). That is, the WSLCB subsidized the cheapest products relative to the private market.

 $^{^9 \}rm Only$ half of WSLCB products are assigned UPCs, but these account for 99.41% of bottles sold between November 2010-October 2011



Figure 5: State Monopoly vs Deregulated Prices

Figure 6: Distribution of Price Changes across Products: 2015 vs Early 2012



Finally, we consider variation in prices across the state. Different localities may have experienced deregulation differently for three reasons. First, they may experience differences in the prices for a fixed set of products. Second, product availability may differ across geographies. And third, tastes may differ across geographies. We calculate the variation in prices across the state using Nielsen's ScanTrak dataset. For each product, we calculate the coefficient of variation of prices across the state and report summary statistics in table 5. The average value across all stores is 9%, which suggests that prices for a given product are remarkably stable. Within chain, the average coefficient of variation is 3%. However, we find that store inventory varies considerably: within, the average fraction of products that overlap across pairs of stores is only 81%. These two facts are consistent the broad trends identified by DellaVigna & Gentzkow (2017) and Hitsch et al. (2017) for grocery retail, as well as Adams & Williams (2017) for home improvement. To capture differences in availability and tastes, we calculate a Laspevres price index separately for each ZIP code. Our benchmark bundle is the set of products purchased in November 2010 across all outlets within a particular ZIP code. Before deregulation, prices for the bundle are based on WSLCB list prices. Following deregulation, we use average 2013 prices at Nielsen Scanner stores within the same threedigit ZIP code. Figure 6b presents a histogram of the Laspeyres indices. While prices rise across the board, some ZIP codes experience increases on the order of 60%, while for others, the impact is far smaller.

Overall, we find that prices rose following deregulation, but did so heterogeneously. Relative prices - across products and ZIP codes - shifted, potentially disadvantaging certain consumers. These descriptives motivate our investigation of voting patterns in section 6.

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

Table 5: Price and Product Variati	on
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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 intervtories 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.

3.4 Changes in Convenience

Deregulation dramatically increased the number of spirits retailers. Figure 8a plots the number of locations over time. Under the State Monopoly, the WSLCB oversaw approximately 330 outlets. By January 2013, the number of retailers exceeded 1,500. These numbers do not reflect entry by entirely new retailers. Most spirits licensees are supermarkets, drugstores, and big box retailers that were already licensed to sell beer and wine (Illanes & Moshary (2018)). Following deregulation, retailers cluster in the densest parts of the state (see appendix figure 12 for a map). Residents of Seattle, for instance, enjoy an almost five-fold increase in the number of outlets.





(a) Number of Spirits Retailers

(b) Number of Spirits Retailers

3.5 Product Mix Changes

Finally, we examine how deregulation altered product variety. Our first comparison is between state stores and scanner stores, as our data provides the complete inventory for this set of retailers. Figure 8 reports product entries and exits following deregulation.¹⁰ Exit represents products¹¹ sold by the WSLCB that are not sold at the Nielsen Scanner stores. Conversely, entry represents products scanner stores sell that were not available at WSLCB stores in 2011. Exits outnumber entries in the first year of deregulation, but scanner stores continue to introduce new products over the next three years. The bottom panel of figure 8 shows the distribution of the number of products across state stores in 2011 compared to

 $^{^{10}}$ WSLCB state and contract stores sold other alcoholic beverages, including beer, wine and cider. However, we do not consider these products an important part of the State business - added together, these other products accounted for less than 2% of revenue for the State in 2011.

¹¹The WSLCB assigned each product a unique brand code.





scanner stores in 2013. Consumers face less than half the number of products at the typical scanner store compared to a state outlet.

This dramatic product churn may both affect average consumer surplus and also disparities in consumer welfare. Those consumers who value these new products potentially enjoy greater utility following deregulation, while those whose favorite products are less available potentially lose out.

In figure 9, we plot the price distribution of products by their availability under the state and private systems. Among goods carried by the state (panel a), those products with prices closer to median are more likely to be sold by Scantrak stores. Inexpensive items (84% of which are 0.05 liter "nips") are likely to exit, as are products priced at \$100 or more. Conversely, among products carried at Scantrak stores, newly introduced products are priced similarly to holdovers from the state system. The products not carried by Scantrak stores





(some 3,233 products) accounted for 8.4% of revenues under the WSLCB monopoly.

Finally, we examine changes in product variety in the Nielsen Homescan as a complement to our Scantrak analysis. Because the Scanner dataset composes roughly 45% of alcohol sales in Washington state, theirs are weak subset of total product offerings under the private system. In contrast, the Homescan dataset contains alcohol purchases at all retailers, albeit for a limited number of households. Figure 10 displays the total number of unique products purchased across all Washington panel households by year (note that we discard private store sales in the latter half of 2012). In contrast to figure 8, the variety of products purchased expands after deregulation. This suggests that even while total product variety may have fallen following deregulation, the households in the panel consume a wider assortment of goods.





4 Model of Spirits Purchasing

In this section, we develop a model of spirits purchasing that allows for heterogeneity in tastes across demographics and geographies. Estimation of the model in section 5 allows us to quantify the change in welfare at deregulation that derives from spirits purchases. In particular, we model consumer i's utility from purchasing product j from store s in month t as:

$$u_{ijst} = -\alpha_i p_{jt} + \gamma_i d_{is} + X_{jt} \beta_i + \Lambda_j + \Omega_t + \xi_{jst} + \epsilon_{ijst}$$
(3)

where p_{jst} is good j's price in store s in period t, d_{is} is individual i's distance to store s, X_{jt} is a matrix of product-time characteristics, Λ_j and Ω_t are product and time fixed effects, ξ_{jst} is a product-store-time unobservable, and ϵ_{ijst} is a logit error. We normalize the utility of not purchasing to zero, $u_{i0t} = 0$. As in Seim & Waldfogel (2013), we model consumers as purchasing at most one liquor product each month. Purchasing patterns in the Nielsen household panel also justify this assumption: over 70% of household liquor-purchasing trips include only a single product.

Let D_i denote an individual-level vector of demographic characteristics (and a constant). We assume that α_i and γ_i are independently log-normally distributed with mean $D_i \cdot \alpha$ and $D_i \cdot \gamma$ and variance σ_{α} and σ_{γ} , respectively. We also assume that each element k of β_i , β_i^k , is normally distributed with mean $D_i \cdot \beta^k$ and variance σ_{β^k} . In matrix form, β_i is normally distributed with mean $D_i \cdot \beta^k$ and variance Σ_{β} , where Σ_{β} is a diagonal variance-covariance matrix.

Let $\theta_i \equiv (\alpha_i, \gamma_i, \beta_i)$. Then product j's share in store s and period t can be written as:

$$s_{jst} = \int \int \frac{\exp(-\alpha_i p_{jt} - \gamma_i d_{is} + X_{jt} \beta_i + \Lambda_j + \Omega_t + \xi_{jst})}{1 + \sum_{j',s'} \exp(-\alpha_i p_{j's't} - \gamma_i d_{is'} + X_{j's't} \beta_i + \Lambda_{j'} + \Omega_t + \xi_{j's't})} df(\theta_i) df(d_i)$$
(4)

where $f(d_i)$ is the empirical distribution of distances between individuals and stores.

Note that we cannot estimate this model on our post-liberalization data, as the Nielsen datasets do not include all spirits retailers after privatization. Fortunately, our pre-liberalization data offers full coverage of the market. Furthermore, since we know the pricing and product assortment rules followed by the WSLCB, we can leverage this knowledge to aid identification of the demand system. Thus, our main assumption for the remainder of the analysis is that the underlying demand function does not change at privatization, only the inputs into the decision problem do. That is, prices, product assortment, and store locations change post liberalization, but the mapping of these features to choices and utility does not.

One limitation of the pre-period Homescan data is it does not include the location of the particular state liquor store visited by each household, only that the household visited a state liquor store.¹² To get around this issue, we assume that each liquor-purchasing consumer visits the state store that is closest to them, and remove all other stores from their choice set. We do not believe that this assumption is particularly restrictive, as all state liquor stores offer the same set of products, at the same prices, and with a joint promotional strategy, so there is little incentive to travel further. The disutility of distance is then identified by households that choose not to purchase liquor at all. In particular, we exploit changes in the share of the outside good as the distance to the nearest state liquor store increases.

The typical concern when estimating demand is the correlation between prices and unobserved (to the econometrician) product characteristics, captured by ξ_{jst} . This concern is less severe in our setting relative to most demand-estimation exercises, as prices and product assortment are fixed across locations. However, liquor manufacturers may consider product, location, and time specific unobservables when bargaining with the WSLCB over wholesale prices. Thus, we estimate demand assuming prices are exogenous conditional on the battery of fixed effects, and then repeat the exercise using prices of the same products in Texas and Florida (*Hausman* instruments).

To be more precise, we estimate this model as a "Micro-BLP" demand system, following Petrin (2002); Berry *et al.* (2004). Letting $\delta_{jst} \equiv X_{jt}\beta + \Lambda_j + \Omega_t + \xi_{jst}$, and M as the annihilator matrix for the set of fixed effects, then the exclusion restriction is $E[W'M\delta] = 0$, where W is the matrix of product characteristics in X and prices (either in Washington or in Texas/Florida). We also include "micro-moments," which restrict the relationship between predicted and observed demographics of liquor purchasers. In particular, we restrict the predicted age, gender, income and distance travelled of liquor purchasers to match the observed quantities from the Nielsen Homescan panel. For each demographic variable h, its' predicted level conditional on liquor purchase is:

$$\hat{D}^{h} = \frac{1}{NM} \sum_{i=1}^{N} \sum_{m=1}^{M} (1 - \hat{s}_{i0m}) D^{h}_{im}$$
(5)

where M is a market (i.e. a store-time combination). We also restrict the covariance between each pair of these demographic characteristics:

$$\hat{cov}(D^h, D^k) = \frac{\frac{1}{NM} \sum_{i=1}^{N} \sum_{m=1}^{M} (1 - \hat{s}_{i0m}) D^h_{im} \cdot D^k_{im}}{\frac{1}{NM} \sum_{i=1}^{N} \sum_{m=1}^{M} (1 - \hat{s}_{i0m})} - \hat{D}^h \hat{D}^k$$
(6)

as well as the covariance between each aforementioned individual characteristic and liquor type (vodka, tequila, whisky and rum), bottle size, and price level (cheap, medium, expen-

 $^{^{12}}$ All state liquor stores share the same store code.

sive):

$$\hat{cov}(D^h, X^k) = \frac{\frac{1}{NM} \sum_{i=1}^N \sum_{m=1}^M D_{im}^h \sum_{j=1}^J \hat{s}_{ijm} \cdot X_{jm}^k}{\frac{1}{NM} \sum_{i=1}^N \sum_{m=1}^M (1 - \hat{s}_{i0m})} - \hat{D}^h \cdot \frac{\frac{1}{NM} \sum_{i=1}^N \sum_{m=1}^M \sum_{j=1}^J \hat{s}_{ijm} \cdot X_{jm}^k}{\frac{1}{NM} \sum_{i=1}^N \sum_{m=1}^M (1 - \hat{s}_{i0m})}$$
(7)

The addition of these micro-moments help identify heterogeneity in sensitivity to prices and distance across demographics, a key input for calculating welfare effects. As we have yet to estimate the model, the following section presents preliminary welfare measures aimed at giving a sense of the magnitude of the effects of privatization. Future versions of this paper will discuss estimation and results below before proceeding to the counterfactual analysis.

5 Consumer Surplus

In this section, we use estimates of the model presented in section 4 to quantify how deregulation affected consumer welfare. As an interim step, we calculate the compensating variation using demand parameter estimates from Seim & Waldfogel (2013) as in Small & Rosen (1981). They estimate spirits demand using data from the Pennsylvania state retail monopoly, where they treat liquor as a homogenous good. Consumer *i* located in zip code *r* gets the following utility from consuming of a bottle of liquor at store *s*:

$$V_{ijrs} = X'_{jr}\beta_x - \beta_{d1} \cdot d_s - \beta_{d2} \cdot d_s \times NoCar_r - \beta_p \cdot p_t + \epsilon_{ijrs}.$$

where p_t is a price index all liquor products, d_s is the distance between consumer *i*'s Zip code centroid and nearest liquor store *s*, and $NoCar_r$ is the fraction of households without access to a motor vehicle in ZIP code *r* (taken from the Census). We borrow their parameter estimates, which include β_{d1} and β_{d2} , the parameters that govern the disutility of travel, and β_p , the disutility of price. Then the expected consumer surplus for consumers in zip code *r* is:

$$CS_{rs} = -\frac{1}{\beta_p} \ln(1 + \exp(V_{ijrs})).$$
(8)

We then estimate 8 using our dataset. Figure 11 plots the distribution of utility changes at deregulation; the modal precinct experienced a decline in welfare, although welfare from liquor consumption more than doubles in some areas.

Figure 11: Histogram of Utility Changes at Deregulation



6 Voting Behavior

In this section, we examine whether and to what extent, vote shares reflect the changes wrought by deregulation. Liquor regulation, like marijuana legalization and the death penalty, has been dictated by direct democracy in Washington state. The Washington constitution permits citizens to vote directly on certain state laws through ballot initiatives and referendum. Initiatives proposed by registered voters must garner 129,811 signatures for inclusion on the ballot.¹³ On November 8, 2011, Washingtonians voted on I-1183, which called for the dissolution of the state liquor monopoly and the creation of a private spirits market.

Skeptics of the ballot initiative system, which is employed in some form in 38 other states, fear that the electorate is ill-equipped to choose optimal policies. In the case of I-1183, voters might be unaware of what deregulation entails: which private firms will choose to sell spirits, the number and types of entrants, the products they will stock, and how they might set prices. Worse, corporations might influence voters through advertising to vote against their self-interest. In the Washington case, Costco invested some \$20 million in TV advertising. Despite concern in the popular press, there is little direct evidence on whether vote shares reflect the best interests of the electorate.

We take a first step to understanding voting behavior by examining the correlates of vote shares at the ZIP code level. Table 6 presents the results of a regression of the proderegulation vote share on two key regressors: the change in the price level, which we measure

¹³https://www.sos.wa.gov/elections/initiatives/faq.aspx

using the Laspeyres price index, and the change in convenience, which we measure as the percent change in the distance from the nearest liquor store to the ZIP centroid. The sign on the price coefficient in column 1 is negative, which is consistent with voters appreciating and disliking high post-deregulation prices, but the estimate is not statistically significant. The sign on the distance coefficient is also negative, implying that those for whom deregulation delivers the greatest increase in convenience were most likely to vote in favor of the reform. Column 2 includes demographic controls taken from the 2011 American Community Survey: median income, percent White, percent of residents with a BA or higher, and the percent of the population without a car. We also include the number of religious establishments per capita to proxy for religiosity, which we taken from the 2011 County Business Patterns in the US Census. The coefficient estimates on price level and convenience do not change.

Correlates of ZIP Deregulation Vote Share							
	(1)	(2)	(3)	(4)			
Laspeyres Price	-0.025	0.028					
Index	(0.025)	(0.026)					
% Δ in Distance	-0.019*	-0.018*					
	(0.011)	(0.010)					
Estimated Δ			-0.001	-0.005			
Welfare			(0.007)	(0.006)			
Dama maakia							
Controls		Х		Х			
Observations	525	525	525	525			
Notes: Robust sta	ndard errors	in Parenthese	s. Coefficients	are			
significant at the * 10%, ** 5%, and *** 1% level. Demographic controls							
include median household income, % White, % BA or higher, population							
density, % population without a car, and number of religious							
organizations per person at the ZIP code level.							

Table 6

One theory consistent with the regressions in table 6 is that residents voted based on their expectations of externalities. However, our findings in section 3 suggest that deregulation did not alter either domestic violence or drunk driving. If votes rest on expectations of externalities that did not materialize, then that casts a similar pall on direct democracy as votes based on faulty predictions of price or entry.

7 Conclusion

This paper documents differences between state and private market provision of spirits. We

find that relative to the invisible hand, the state monopolist provides wider product variety at lower prices, but dramatically limits accessibility. In 2011, Washingtonians were left to weigh these costs and benefits when the November ballot included an initiative to deregulate liquor retail, divesting the state of its monopoly. We estimate the net effect of deregulation on welfare using a revealed preference demand approach, using data on spirits sales from the Washington Liquor Control Board and Nielsen. Although our preliminary estimates imply that deregulation harmed the average resident, the initiative garnered 58% of votes. Further, we find that vote shares do not reflect utility gains at the ZIP code level.

Our results question the value of direct democracy as a legislative tool. Washington's liquor initiative was seemingly straight-forward as it concerned only a single industry, yet voting still required a shrewd calculus: forecasting entry and market conduct. As in the lab experiments of Dal Bo *et al.* (2017), our findings are consistent with a theory of voter naïveté. Perhaps more troublingly, they are also consistent with a model where corporate interest captures voters. Indeed, Costco spent over \$20 million to support advertising for Washington's liquor initiative. We would be interested to see more evidence on voting in initiatives and referenda, particularly as these policy tools are increasingly used to settle thorny issues, such as marijuana legalization and the death penalty.

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A Additional Tables & Figures

Figure 12: Map of Spirits Retail Locations



Figure 13: Household Liquor Bundling Behavior



Figure 14: Histogram of Precinct Vote Shares



B Calculating after-Tax Prices from the Consumer Panel Dataset

We calculate per-unit spirits prices using the following formulas before deregulation:

$$price = \frac{total_price_paid}{quantity} \tag{9}$$

Shelf prices under the WSLCB included excise taxes $(20.5\% \text{ proportional tax plus a $3.7708 per liter tax}).^{14}$ However, private retailers do not include excise taxes in sticker prices. We therefore calculate per-unit prices following deregulation as:

$$price = \frac{total_price_paid}{quantity} \times 1.205 + 3.7708 \times size1_amount \times units$$
(10)

where we scale $size1_amount$ by $\frac{1}{1000}$ if the $size1_unit$ is "ML."

As a check, we compare the number of liquor-only trips where the sum of expenditures based on equation 9 match the total spent (reported directly in the Nielsen data, this variable incorporates taxes) before and after deregulation, and vice versa for equation 10. We also consider the following two alternatives: adding only the proportional tax and adding only the per-liter tax.

$$price = \frac{total_price_paid}{quantity} \times 1.205$$
(11)

$$price = \frac{total_price_paid}{quantity} + 3.7708 \times size1_amount \times units$$
(12)



 $Census \ Data \ from \ Washington: \ https://www.ofm.wa.gov/washington-data-research/population-demographics/decennial-census/census-2010/census-2010-data$

C WSLCB Data Cleaning

The WSLCB provided monthly sales data for at the outlet-product level from October 2010 to May 2012. The WSLCB assigned each product a unique "brand code." When we map brand codes to UPCs in the Nielsen data, we have the following four case:

- 1. For 12,095 (3.59%) of the sales data observations, we do not have a UPC match in either of the crosswalk data. (Or if there is a match, the difference in the reported product sizes exceeds 1 ml.)
- 2. For 31,906 observations of the sales data, we have a match in exactly one of the crosswalks.
- 3. For 188,382 observations of the sales data, there is a match in both crosswalks and the following conditions hold: the difference in proofs reported in both data sets is less than 1 and one UPC is a (weak) subset of the other.
- 4. For 106,550 observations of the sales data, there is a match in both crosswalks and either the reported proofs differ by more than 1 unit or the UPCs are not subsets of one another. We create a flag for these cases.

For cases 3 & 4, we attempt to match the UPC in the Nielsen dataset to at least one UPC from the crosswalk. We consider a match a case where the UPCs match exactly and the difference in reported size between Nielsen and the crosswalk is less than .006L and the difference in the proof is less than 1.