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The effect of payday lending restrictions on liquor sales

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Abstract

We exploit a change in lending laws to estimate the causal effect of restricting access to payday loans on liquor sales. Leveraging lender- and liquor store-level data, we find that the changes reduce sales, with the largest decreases at stores located nearest to lenders. By focusing on states with state-run liquor monopolies, we account for supply side variables that are typically unobserved. Our results are the first to quantify how credit constraints affect spending on liquor, and suggest mechanisms underlying some loan usage. These results illustrate that the benefits of lending restrictions extend beyond personal finance and may be large.

Keywords: payday lending, consumer credit, alcohol

JEL Classifications: D18, G23, H70, I12, K23

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1 Introduction

The practice of short-term consumer financing known as payday lending remains controversial because the theoretical gains in welfare from greater credit access stand in opposition to anecdotal evidence that many borrowers are made worse off. Advocates for the industry assert that the loans fill a gap in credit access for underserved individuals facing temporary financial hardship. Opponents, who include many state legislatures and the Obama administration, argue that lenders target financially vulnerable individuals with little ability to pay down their principal, who may end up paying many times the borrowed amount in interest and fees.¹

Despite uncertainty surrounding the benefits to borrowers, there exists a surprising lack of evidence on the impact of payday lending on consumption. An exception is Melzer (2011), which finds that extending access to payday lending leads to more instances of individuals delaying purchases of necessities such as medical care, dental care and prescription drugs. In this paper, we explore the ways that access to payday lending affects consumption along another margin: the demand for liquor. This is a particularly interesting market to study because, far from being a necessity, liquor is widely regarded as a non-essential consumption good whose use and abuse is associated with a vast

¹In his March 28, 2015 weekly address, President Obama stated that “while payday loans might seem like easy money, folks often end up trapped in a cycle of debt.”

array of health consequences, anti-social behaviors and negative externalities.²

Regulations restricting both payday loan and liquor access seek to minimize the potential for overuse.³ To justify intervention in the two markets, policy makers note a host of negative externalities associated with each product, and cite behavioral motivations underlying individuals' consumption decisions. In particular, researchers have shown that the same models of impulsivity and dynamically inconsistent decision making - hyperbolic preferences and the cue theory of consumption - used to describe the demand for alcohol, also describe patterns of payday loan usage.⁴ In these models, individuals can objectively benefit from a restricted choice set that limits their access to loans and liquor. The overlap in behavioral characteristics of over users of both products suggests that liquor represents an area particularly responsive to changes in payday lending regulations.

To identify the causal effect of lending restrictions on liquor sales, we exploit a change in payday lending laws in the State of Washington. Leverag-

²The World Health Organization estimates that there is a causal relation between alcohol consumption and more than 200 medical conditions. Among males, approximately 7.6% of world-wide mortality in 2012 was attributable to alcohol (World Health Organization (2014)). The economics literature, in work largely though not exclusively focusing on young drinkers finds alcohol to be a contributor to a diverse range of problems. While not exhaustive, this includes more premature mortality, vehicle fatalities, and suicides (Dee (1999); Carpenter (2004); Carpenter and Dobkin (2009)), crime and violence (Carpenter (2005b); Carpenter (2007); Biderman et al. (2010), Luca et al. (2015)), risky sexual activity and teenage pregnancy (Dee (2001b); Chesson et al. (2000); Carpenter (2005a)), poorer infant health (Fertig and Watson (2009)), and lower earnings and educational attainment (Renna (2007)). Evidence supports that some of these consequences result from both own as well as peer alcohol use (Waddell (2012)).

³For example, the Washington State Liquor Control Board lists reduced state-wide per-capita consumption of alcohol as a primary benefit of the state's alcohol distribution monopoly (*Washington State Liquor Control Board Business Plan 2007-2009*).

⁴ See Laibson (2001) for cue theory and Skiba and Tobacman (2008) and Vuchinich and Simpson (1998) for hyperbolic preferences, payday lending, and consumption.

ing lender- and liquor store-level data, we estimate a difference-in-differences model comparing Washington to the neighboring State of Oregon, which did not experience a change in payday lending laws during this time. Importantly, by focusing on states with state-run liquor monopolies, we can address supply-side competitive effects, such as price changes, or store openings and closings, that normally would confound this identification strategy for a generic consumption good. We find that the law change leads to a significant reduction in liquor sales, with the largest decreases occurring at liquor stores located very near to payday lenders at the time the law took effect. Our results provide compelling evidence on how credit constraints affect consumer spending, suggest a behavioral mechanism that may underly some payday loan usage, and provide evidence that the Washington law likely reduced one form of loan overuse.

The paper proceeds as follows: in Section 2, we review previous work studying the payday lending industry, its impact on communities, and potential links to liquor demand. We also explain the legal landscape for payday lenders and liquor stores in Washington and Oregon, establish the impact of the Washington law change on the state-wide prevalence of loans and lenders, and discuss the data. We present our empirical results in Section 3, and in Section 4, we conclude with a summary of the findings, and implications for policy.

2 Background

The term “payday loan” describes a form of short-term consumer credit traditionally offered at storefronts specializing in the loans.⁵ In a typical transaction, a borrower writes a personal check to the lender to be cashed on the borrower’s next payday. In exchange for the check, the borrower receives cash equal to the loan amount net of all fees and interest subtracted upfront.

While on average, loans are small (around \$375), three factors make them potentially more risky than other forms of credit. First, the cost of borrowing is high when converted into an annual percentage rate (APR). Individuals typically pay \$15 to \$25 in fees per \$100 borrowed (an APR of approximately 390% to 650% for a typical loan). Second, the short duration of the loans (generally 14 days or fewer) leaves little time for a meaningful improvement in borrowers’ finances. This contributes to a third issue which is that most loans are taken to repay a prior payday loan (Burke et al. (2014)). In some states, this pattern can continue, *ad infinitum*, transforming a relatively small loan into a regular liability paid two or three times per month.

Estimates of the size of the payday loan industry vary. Bair (2005) and Stegman (2007) put annual loan volume as high as 50 billion dollars in 2004 spread across 22,000 lending storefronts. More recent estimates made by the Pew Charitable Trust (PCT) put the number of store fronts at 20,000 in 2010 with a smaller loan volume. The slow down in the industry’s growth likely

⁵In recent years, online payday lenders have grown in popularity, however, they remain a small portion of the market with approximately 16% of surveyed individuals indicating that they rely exclusively on internet payday lenders for loans. Additionally, survey evidence suggests that most individuals opt to forego loans rather than utilize online lenders when states restrict payday lenders (Bourke et al. (2012)).

represents the effect of greater regulation enacted in a number of states. Despite the new regulations, PCT estimates that 12 million Americans received a payday loan in 2010, including nine percent of individuals with annual earnings less than \$15,000, and eleven percent of people earning between \$15,000 and \$25,000 (Bourke et al. (2012)).

While sound financial conduct would likely dictate refraining from using payday loans to obtain liquor, it is plausible that not all borrowers act with such restraint. First, survey evidence finds that eight percent of borrowers indicate that their first loan was used primarily for “something special, such as a vacation, entertainment, or gifts,” and five percent said that they used it to purchase food and groceries (Bourke et al. (2012)). In another survey of borrowers, Bertrand and Morse (2009) find that more than nine percent of respondents are “temptation spenders” who report using loans for vacations, eating out, entertainment, gifts, apparel or electronics. Additionally, both payday loan users and heavy drinkers exhibit behavior consistent with present-biased hyperbolic preferences (Skiba and Tobacman (2008) and Vuchinich and Simpson (1998), respectively), further suggesting a willingness to use expensive credit to finance liquor purchases. Furthermore, that some drinkers may choose to satisfy impulsive desires for alcohol through payday loans is consistent with behavioral explanations of impulsivity and addiction such as Laibson (2001) cue theory of consumption. Finally, if payday lend-

ing increases financial distress,⁶ research suggests that greater alcohol use and abuse may be expected.⁷

2.1 Lending in Oregon and Washington

Washington State enacted HB 1709 on January, 1st 2010, which introduced three new major restrictions to the payday loan industry. First the law limited the size of a payday loan to 30% of a person's monthly income or \$700, whichever is less. Second the law created a statewide database to track the issuance of payday loans in order to set a hard cap on the number of loans an individual could obtain in a twelve month period to eight, and eliminated multiple concurrent loans. This effectively prohibited the repayment of an existing loan with a new one.⁸ In the year prior to the law, the State of Washington

⁶Research on the impact of payday lending on indicators of financial distress finds mixed results. Evidence supporting a positive correlation (i.e. loan use positively correlated with distress) includes Campbell et al. (2012), Melzer (2011), and Carrell and Zinman (2014) which find that loan access leads to more involuntary bank account closures, difficulty paying bills, and lower job preparedness, respectively. Skiba and Tobacman (2011) and Morgan et al. (2012) link greater loan access to higher bankruptcy rates, though Hynes (2012) finds the opposite. Alternatively, the volume of bounced checks (Morgan et al. (2012)), and borrower-reported financial distress (Zinman (2010)) go up after payday loan bans (even though the probability of making a late payment on a bill appears to go down). Results from Morse (2011) indicate that the presence of lenders mitigates foreclosure and crime rate rises after unforeseen natural disasters. Bhutta (2014) and Bhutta et al. (2015) find no effects of payday lending on credit scores.

⁷In particular, Peirce et al. (1994) and San Jose et al. (2000) both document a positive relationship between stress and alcohol consumption in survey data on alcohol use. This relationship also appears in studies of the effect of unemployment on alcoholic consumption. Dee (2001a) and Ruhm and Black (2002) show that, although the income effect dominates during economic downturns leading to procyclical alcohol consumption, financial stress related drinking appears to rise with binge drinking increasing and moderate drinkers increasing their consumption.

⁸Prior to the new law, Washington prohibited lenders from issuing roll-over loans that allowed borrowers to pay a fee to delay cashing existing checks. Rather than deter financial irresponsibility, we believe this feature may have strengthened the link between loans and impulse spending because customers obtaining new loans to repay existing debts needed to physically handle a greater volume of cash.

estimated that roughly one third of all payday loan borrowers took out more than eight loans.⁹ Finally, the law mandated that borrowers were entitled to an 90 day instalment plan to pay back loans of \$400 or less or 180 days for loans over \$400.

The effect of the law on the industry was severe. There were 603 payday loan locations active in Washington in 2009 that were responsible for 3.24 million loans worth \$1.366 billion according to Washington Division of Financial Institutions.¹⁰ In the year following the law change, the number of payday lenders dropped to 424, and loan volume fell to 1.09 million loans worth only \$434 million. The following year the number of locations fell again to 256 with a loan volume of roughly 900,000 worth \$330 million. Today there are fewer than 200 lenders in Washington and the total loan volume and value has stabilized close to the 2011 values.

Oregon enacted similar payday lending restrictions in August of 2007. The law capped loans at 30% of income or \$700, whichever is less; borrowers could only apply for eight loans in a calendar year, and are entitled to similar instalment plans as Washington borrowers. The Oregon law also effectively reduced the number of payday lenders. Payday lender numbers dropped from 370 in 2006 to 82 by 2008. Oregon's payday laws have remained unchanged since, and the number of active payday lenders has held constant.¹¹

Throughout our observation period, both states require payday lenders to

⁹2009 Payday Lending Report authored by the Washington State Department of Financial Institutions.

¹⁰Washington's reports are available online at <http://www.dfi.wa.gov/reports/payday-lending-reports>.

¹¹We do not possess data to similarly estimate the effect of Oregon's law change on liquor sales using Washington as a counterfactual.

hold special licenses to operate. The licensing information is publicly available and includes information on when a license was issued and when it expired, the street address of the store, and the parent company. Figure 1 shows the payday lender counts in Oregon and Washington from July 2008 through March 2012 from the licensing data. The time frame corresponds to the period for which we have liquor sales data. Entry into the count occurs on the date a license is issued and exit occurs when the last observed license expires. The majority of Washington lenders exit in April, which coincides with the annual audit of lenders by the state. According to the Washington Division of Financial Institutions, there are no rules, however, that force firms to exit in this month. Washington experiences major declines in lenders beginning in the years following the payday lending restrictions, while Oregon's count is practically unchanged.¹²

2.2 Liquor Sales in Oregon and Washington

A crucial feature of our estimation strategy involves accounting for potentially endogenous supply side factors that challenge efforts to separately identify changes in demand from the store response to the change. To do so, we focus on liquor control states, in which the state determines the number and location of liquor stores, the products offered, and harmonizes prices across stores to regulate and restrict liquor access. Oregon and Washington were both liquor

¹²The counts given in the Washington Division of Financial Institutions annual reports cited previously differ slightly from our own. We believe differences are due to the counts being done at different points in the year, or differences in counting temporarily expired licenses, and as we have received our data from the same agency, we have no reason to doubt its comprehensiveness.

control states until June of 2012 (Washington privatized liquor sales in June 2012).¹³ In what follows, we describe the relevant operation and price setting policies in effect during the period of interest.

The prices in each state are set according to publicly known formulas and published in advance. These formulas and the applicable taxes differ between the two states and are periodically revised. On average liquor prices in Oregon during the period of study are \$1.80 per bottle less than in Washington including tax. A majority of this difference reflects the absence of sales tax in Oregon.

There were two revisions to the pricing formulas in Oregon and Washington that occurred around the time of the Washington law change. Oregon added a \$0.50 per bottle surcharge in April of 2009 and Washington increased the markup (over wholesale price not including taxes) on all bottles from 39.2% to 51.9% in August 2009, which resulted in an average total price increase of \$0.67 for all items observed at the universal product code level. No additional changes in formulas occurred through the end of our sample (March 2012), although, observed retail prices in Washington fell by an average of \$0.60 in the two years following the initial price increase, while Oregon's remained steady.¹⁴ In addition, the formula revisions had an equally small effect on the minimum price of liquor in each state. The minimum price of liquor is noted

¹³The agencies for Washington and Oregon are the Washington State Liquor Control Board and the Oregon Liquor Control Commission. The structure of the state monopolies are slightly different. Oregon owns all alcohol sold in the state and contracts individuals or firms to operate stores where the alcohol is sold. Washington on the other hand operated mostly state owned store where sales were conducted by state employees.

¹⁴Although we possess liquor sales data through June 2012, we end our sample in March to avoid any changes in relative sales that may have occurred due to the privatization of liquor sales in Washington.

in the public health literature as a key determinate in reducing overall alcohol consumption since the variety of price points in the alcohol market allow for easy substitution in response to price changes (Stockwell et al. (2012)).¹⁵ Therefore, the absence of diverging prices between the two states leads us to believe that changes in prices over time do not underly the changes in monthly liquor sales we observe.

Figure 2 shows the total number of liquor stores and liquor stores per 100,000 residents in the two states over time. Oregon and Washington both increase the number of stores in operation over our sample. However, the per capita graph shows that these increase are largely offset by population growth. The increase in Washington stores was part of a slow push to increase liquor access beginning in 2009 and was accompanied by an increase in store open hours and days of operation.¹⁶ The effect of this push with respect to our estimates of the effect of the payday lending law change is ambiguous. The increase in stores may bias our estimates towards finding a negative effect of the law change, while the increase in store hours and days of operation may bias the estimates towards finding a positive effect. As a result, we consider a number of specifications to address these sources of bias.

For this study, we use monthly store-level sales data provided by Oregon's and Washington's respective liquor control agencies from July 2008 through March 2012. Figure 3 shows total monthly sales in Oregon and Washington

¹⁵In both states during our sample period, a 750ml bottle of 80 proof whiskey, vodka, gin, rum, and tequila were each available for under \$10 including tax. A summary table of liquor prices is included in the Appendix. Historical liquor prices are available for the two states on request.

¹⁶Source: Press release from Washington Liquor Control Board data June 24, 2009.

over our sample period adjusted for inflation.¹⁷ Besides the level difference in sales between the states, Oregon sales appear to be a reasonable counterfactual for Washington sales. Both states exhibit similar but large seasonality in liquor sales, with December sales nearly twice as large as January’s in some years, and increasing sales over time.

3 Empirics

To retrieve the causal effect of the lending restrictions on liquor sales, we employ a difference-in-difference (DD) style estimator, with Oregon stores serving as the counterfactual for Washington stores. This is a valid identification strategy if the trend in Oregon’s liquor store sales follow the same trend that would have been observed in Washington’s stores in the absence of a reduction in credit access. The most parsimonious DD OLS regression model estimates the effects of the payday lending restrictions on store i ’s log consumer liquor sales in month t without conditioning on controls or fixed effects:

$$\ln(\text{Sales}_{it}) = \alpha + \beta_1 \text{Post Law}_t + \beta_2 \text{WA}_i + \beta_3 \text{Post Law}_t \times \text{WA}_i + \epsilon_{it} \quad (1)$$

where Post Law_t is a binary variable equalling one in the months after Washington’s law change, and WA_i is an indicator variable for Washington liquor stores. The coefficient associated with the interaction, β_3 , represents the causal

¹⁷We adjust the sales data for inflation using the CPI measure for Food and Beverages. The available liquor price data though suggests that movements in liquor prices were much smaller than those implied by CPI inflation. However, we find that adjusting for inflation has little effect on the point estimates or our regressions.

effect of the lending restrictions. However, this specification may be susceptible to potential unobserved store characteristics, fixed over time, and correlated with sales and the $Post\ Law_t \times WA_i$ term, which may bias estimates of the causal effect of the law change. Additionally, as demonstrated in Figure 3, there exists substantial seasonality in liquor sales. The degree to which each store is affected by this seasonality may vary (e.g. liquor stores next to shopping malls may experience greater seasonality than those located in suburbs), which motivates a specification that includes store-by-month (e.g. January) fixed effects, α_{im} :

$$\ln(Sales_{it}) = \alpha_{im} + \gamma_1 Post\ Law_t + \gamma_2 Post\ Law_t \times WA_i + \epsilon_{it} \quad (2)$$

Figure 4 plots estimated residuals from a regression of log liquor store sales on a set of store-by-month fixed effects, averaged over state and quarter.¹⁸ The graph possesses three notable features. First, prior to Washington’s lending restrictions (indicated by the vertical dashed line), both states’ log sales trend in parallel, confirming the plausibility of the “common trends” assumption of the DD model. Second, a persistent gap in the states’ sales appears in the same quarter as the law change. This gap is the result of a relatively large downward movement in Washington’s sales compared to Oregon’s, consistent with a negative effect of the law on sales. Finally, the effect appears to be a permanent level shift down, yet sales in both states maintain a common upward trend.

¹⁸As one would expect, store fixed effects alone fail to remove the apparent seasonality. Figure A1 plots log consumer sales, net of store fixed effects, and averaged by month and year.

Although Figure 4’s apparent confirmation that sales in both states’ follow common trends prior to the lending restrictions, we can further relax the requirements for identification and likely reduce residual variance with the inclusion of relevant control variables. Therefore, we introduce both store-specific and regional control variables to equation (2). For regional economic factors, we include the monthly unemployment rate and annual household median income measured at the county level, and the annual proportion of residents living below the U.S. poverty line measured at zip code level. For store operations, we include indicator variables to account for changes in sales driven by temporary store closures, store locations moves, and possible competitive effects of new stores entering the market. To control for the effect of new entrants, we create an indicator variable that takes a one in all months following the opening of a new liquor store for any existing liquor store within a one kilometer radius to separate changes in sales due to competition from those associated with the law change.¹⁹ Finally, in order to correct for possible bias in the estimated parameters of these controls driven by time-specific shocks, we remove arbitrary trending over time with the inclusion of year-by-month fixed effects, τ_{my} . Like the control variables, these fixed effects may provide additional efficiency gains. Our preferred specification is therefore given by the following equation:

$$\ln(Sales_{it}) = \alpha_{im} + \tau_{my} + \delta_1 Post\ Law_t \times WA_i + \mathbf{x}_{it}\mathbf{b} + \epsilon_{it}. \quad (3)$$

¹⁹The information on new stores, store closures, changing hours, and store moves is obtained from archived press releases published on the two states liquor control websites. Further explanation of the variables are given in Table A2 in the Appendix.

Table 1 reports the estimated causal effect of Washington’s payday lending restrictions on store consumer liquor sales. Columns 1 and 2 report the results without and with control variables, respectively. Estimated standard errors account for any clustering that may occur at the county-level.²⁰ Our preferred specification with controls (column 2) indicates that the payday lending restrictions reduced liquor store sales by approximately 3.6% (statistically significant at the 1% level). As average Washington liquor sales were approximately \$163,000 in the months prior to the law change, this represents a \$5,900 decline per store each month. At the state level, the point estimate implies a \$23.5 million dollar ($\$5,900 \times 333$ liquor stores $\times 12$ months) annual decrease in liquor sales. As Washington State reported that the law decreased payday loans by \$932 million from 2009 to 2010, this decline represents approximately 2.5% of the change in total value of loans issued.

The final column of Table 1 reports the dynamic effects of the lending restrictions in the year prior to the law change, and after, relative to months more than one year before. There is no evidence of an effect on sales prior to the law change, and the effect on liquor sales appears to increase somewhat in years after the law change. Because payday lenders continued to exit through 2011, and the nature of the cap limiting borrowers to eight loans per twelve-month period, this larger effect on liquor sales over time is consistent with progressively tightening credit constraints.

We also consider and rule out three other possible explanations for the

²⁰Table A3 in the Appendix shows that while unadjusted standard errors are less than half the size of these, there is little difference between correcting for clustering at the store-, city- or county-level.

estimates not captured by the controls: unobserved competition effects not captured by the new entrants indicator variable, other law changes affecting liquor sales, and changes in cross-border liquor purchases following the law change due to the fact that Oregon and Washington share a populated border. While Figure 2 indicates that both states have little change in per-capita store numbers over time, we further investigate the role of new entrants on existing liquor store sales by estimating the effect of the law change on sales collapsed to the city-level. Estimates in columns 3 and 4 of Table 2 indicate that, while entry may play a small role in determining store-level sales, significant reductions in liquor sales occur at the city-level.²¹

To account for other possible law changes, we conducted a review of Washington and Oregon legislation proposed and passed during the 2009 legislative sessions and contacted the Washington State Liquor Control Board and the Washington State Division of Financial Institutions, the regulator of payday lenders, to specifically ask about alternative explanations. The only relevant changes we identified from our research and conversations with regulators were those already discussed in Section 2. Finally, Columns 1 and 2 of Table 2 use store-level observations with our preferred specification to demonstrate that the results are robust to dropping the border counties from the sample. This is important as Melzer (2011) finds significant evidence of individuals' willingness to traverse state borders to obtain payday loans. If the observed effect

²¹It is important to note that we do not expect the point estimates in Table 2 to equal those in our main specification unless heterogeneity in response to the lending restrictions is orthogonal to the number of stores operating in a city. When the data is collapsed to the city-level, observations from cities with few liquor stores receive relatively greater weight than those compared to the store-level regressions.

of lending restrictions is primarily driven by reduced economic activity at the border, dropping these stores should yield insignificant estimates. However, this is not observed.

3.1 Effects in urban versus rural counties

Heterogeneity in payday loan usage suggests that not all communities' liquor stores may experience the effects of lending restrictions similarly. One margin along which stratification might prove informative is between the states' large metropolitan areas versus smaller or more rural communities. The nation-wide survey of borrowers by PCT finds seven percent of individuals living in urban areas have used payday loans, which "is significantly higher than the three percent of suburban-area residents who report having used payday loans." The report implicates race as another observable predictor of payday loan usage (twelve percent of African Americans report using a payday loan compared to four percent of whites). Race and geography may directly influence usage patterns or merely proxy for characteristics that do.

Table 3 presents estimates of the effects of payday loan restrictions on individual liquor store sales from equation (3) separately for counties with a large metro area and counties considered part of the fringe (columns 1 and 2), and counties with smaller populations (columns 3 and 4).²² Apparent from this table is that while liquor stores are more numerous in smaller counties, average sales per store are higher in the high population density counties,

²²County size definitions follow the Centers for Disease Control's National Center for Health Statistics county urbanicity classification definitions.

representing the non-uniform geographic distribution of residents.²³

In line with greater loan usage, per capita, and access in densely populated areas, the effect of the credit ban reduces liquor sales by slightly less than six percent (significant at the 1% level), on average in large metro core and outlying counties, whereas the restrictions reduce sales in smaller metropolitan and non-core areas by only two-and-a-half percent (p-value=0.06). The estimates of the dynamic effect pre- and post-law change again point to no statistically significant effect prior to the law change, and a slightly increasing effect from year one to two-plus years post restrictions.

3.2 Discussion

We see two primary explanations (not mutually exclusive) for the decline in Washington liquor sales in response to the law change. First, the effect may be part of a wider permanent reduction in consumption as households lose their ability to cope with unforeseen negative income shocks. Alternatively, the drop in spending may indicate a more direct financing of liquor purchases by individuals with present-biased preferences. The first explanation implies that restrictions on payday lending negatively affect consumer welfare, while the second allows for a positive impact, since individuals with present-biased preferences may be made objectively better off with a restricted choice set.

Zinman (2013) highlights Laibson (2001)'s theory of Pavlovian cues as a particularly intriguing explanation for payday loan usage. In these models, consumer "impulsivity" makes instant gratification a special case during dy-

²³Over the considered time period, total sales of alcohol was thirty percent higher in the large metropolitan counties.

namic utility maximization, where exposure to a cue can explain dynamically inconsistent behavior. Indeed, Laibson uses liquor as a prime example of consumption thought to be influenced by cues, and subsequent experimental research on liquor provides evidence consistent with this hypothesis (MacKillop et al. (2010)). In situations where payday lenders locate very near to liquor stores, individuals may be exposed to a cue for alcohol, and then see the lender as a means to satisfy the urge to make an immediate purchase. A lender and liquor store separated by even a brief walk may be far enough apart to allow an individual to resist the urge to obtain both the loan and liquor. Of course, cue-theory of consumption makes lender-liquor store distance relevant even in circumstances where individuals experience a cue only after borrowing. Lenders locating near liquor stores increase the likelihood that an individual exposed to a cue is financially liquid, and able to act on an impulse.

In the following section, we investigate the relevancy of a store’s distance to a lender on the magnitude of the effect of the law change. We hypothesize that a relationship between distance and effect size that is sufficiently non-linear such that it cannot be justified based upon increased travel costs alone would be consistent with behavioral explanations of demand.

3.3 Store Sales and Lender Proximity

To investigate liquor store and lender proximity, we geocode the stores’ and lenders’ street addresses, and calculate walking distances for all liquor store-lender pairs within two kilometers of one another.²⁴ We then repeatedly esti-

²⁴See the data appendix for a description of the geocoding procedure.

mate equation (3) with our full set of controls on an ever expanding window of liquor stores beginning with the stores that were located within a ten meter walking distance of a lender in the month prior to the law change, then within 100 meters, within 200 meters, etc., to two kilometers.^{25,26} We believe that a two kilometer walking distance represents a reasonable upper bound for which distance remains a salient predictor of cue-driven sales.²⁷ These estimates are presented in Figure 5. The graph demonstrates a negative effect of 9.2% on those liquor stores that had a payday lender located within ten meters in the month before the law change (significant at the 1% levels), an effect almost three times as large as that overall.^{28,29} The larger effect rapidly declines in distance suggesting that even a small degree of separation is significant. The degree of nonlinearity in the relationship between distance and liquor sales supports the behavioral explanation of demand.

²⁵Appendix figure A2 gives the cumulative number of liquor stores by distance to the nearest lender in the month before the law change.

²⁶We use the walking distance in the month before the Washington law change, rather than a more time-dynamic measure such as as openings and closings post-law because these are likely to be endogenous decisions. We see distance prior to the law change as a plausible proxy for a liquor store’s reliance on lenders leading up to the lending restrictions, and then measure the reduced form impact on sales, which includes potential endogenous relocation of stores throughout the state. Substantial within-store increases in distance over time may evidence a weak relationship between sales and lending if lenders close after the law change, whereas declining distance over time could indicate that lenders cluster around liquor stores which are most reliant upon lenders, even if the law change served to reduce credit access and therefore liquor sales. This type of selection would lead to the incorrect conclusion that a decrease in distance causes sales to decline.

²⁷For robustness, figure A3 expands this window out to five kilometers.

²⁸In addition, we investigated the timing of the decline in sales at the closest stores compared to all stores and found that decline corresponds to the law change. Figure A4 in the appendix shows the normalized average residuals from the closest stores as done for all stores in Figure 4. The closest stores average log sales exhibit qualitatively the same behavior as all stores in the full sample.

²⁹A triple-difference model does not reveal this larger effect to be statistically significant at conventional levels. This is not surprising given the 95% confidence interval apparent in Figure 5

4 Conclusion

Our analysis provides the first empirical evidence of the connection between payday lending and spending on liquor. We uncover a clear reduction in liquor sales resulting from payday lending restrictions. Further investigation finds heterogeneity in the response to the law changes consistent with known borrowing patterns in larger urban environments. In addition, we find that those liquor stores located very near to lenders at the time of the law change experience declines in sales almost three times as large as the overall average.

This finding is significant because it highlights that a segment of borrowers may be willing to assume significant risk by borrowing in order to engage in alcohol consumption - an activity which carries significant personal risk of its own. The connection between payday lending restrictions and reduced liquor purchases, therefore, suggests that the benefits to payday lending restrictions extend beyond personal finance and may be large.

Effective payday loan regulation should recognize the potential for greater credit access to help or harm consumers. As Carrell and Zinman (2014) highlight, heterogeneity likely exists within the pool of payday loan users, and external factors will influence the ratio of “productive and counter-productive borrowers.” Lending restrictions can seek to reduce the proportion of counter-productive borrowers through the prohibition of practices known to harm consumers, including those that rely upon leveraging behavioral responses such as addiction and impulsivity. The behavioral overlap identified in the literature between counter-productive payday loan borrowers and heavy alcohol users suggests that there exists a logical link between the two markets. The decline

in liquor sales documented here provides evidence that these regulations may be effective in promoting productive borrowing.

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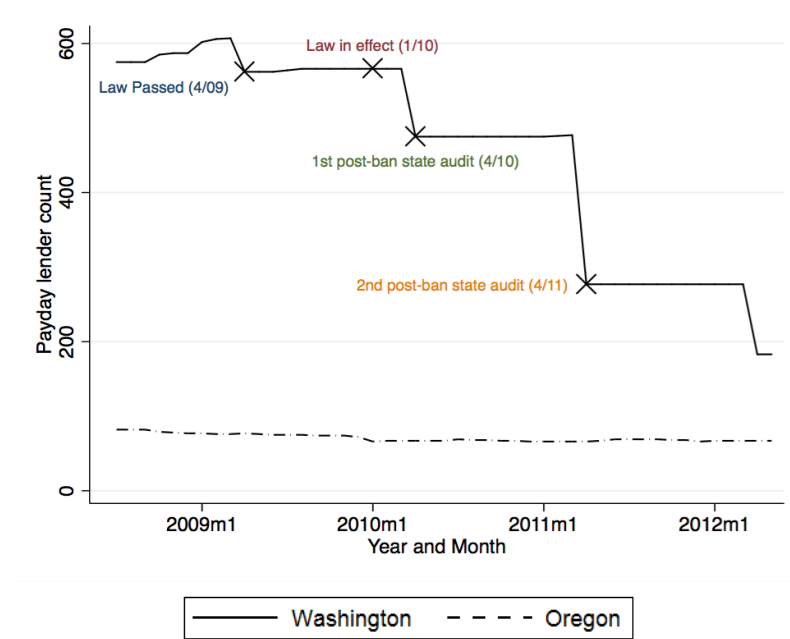
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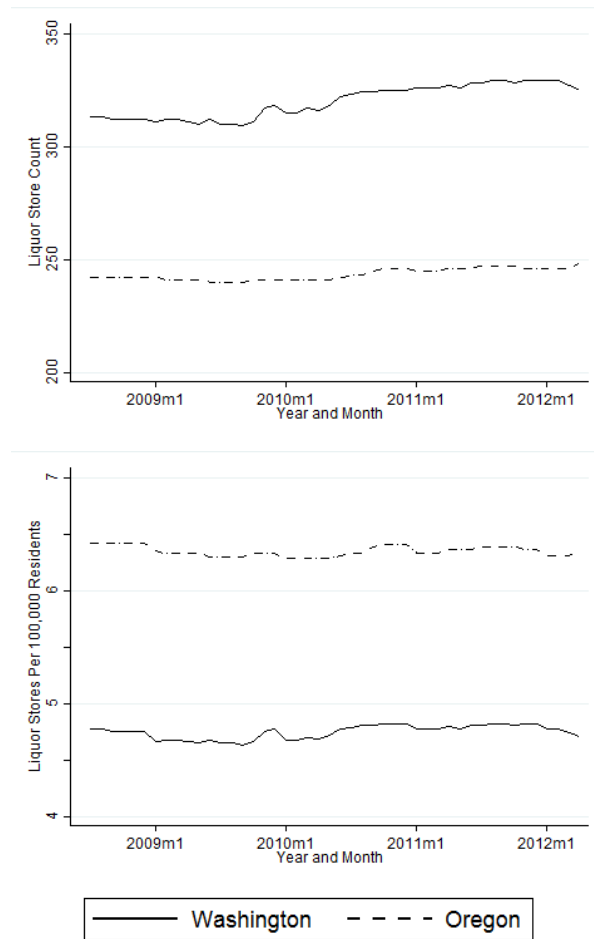
5 Figures

Figure 1
Total payday lenders by state



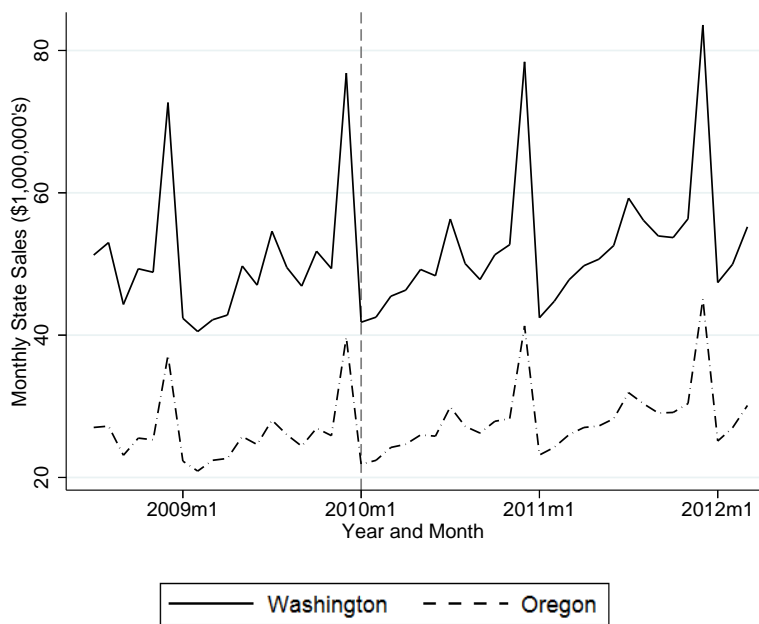
Notes: This figure shows total payday lenders by state and month.

Figure 2
Liquor store counts by state



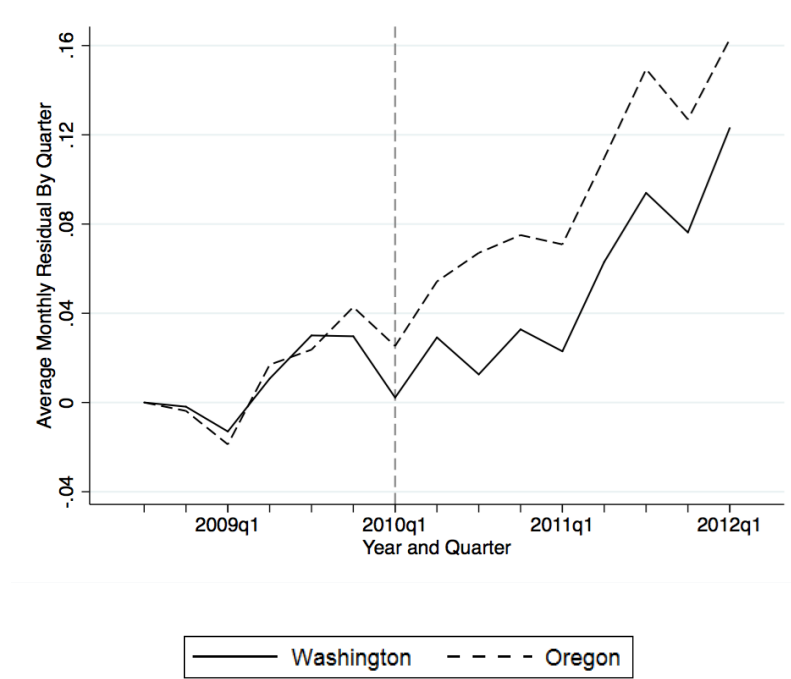
Notes: This figure shows total liquor stores and per capita liquor stores by state and month.

Figure 3
Total liquor sales by state and month



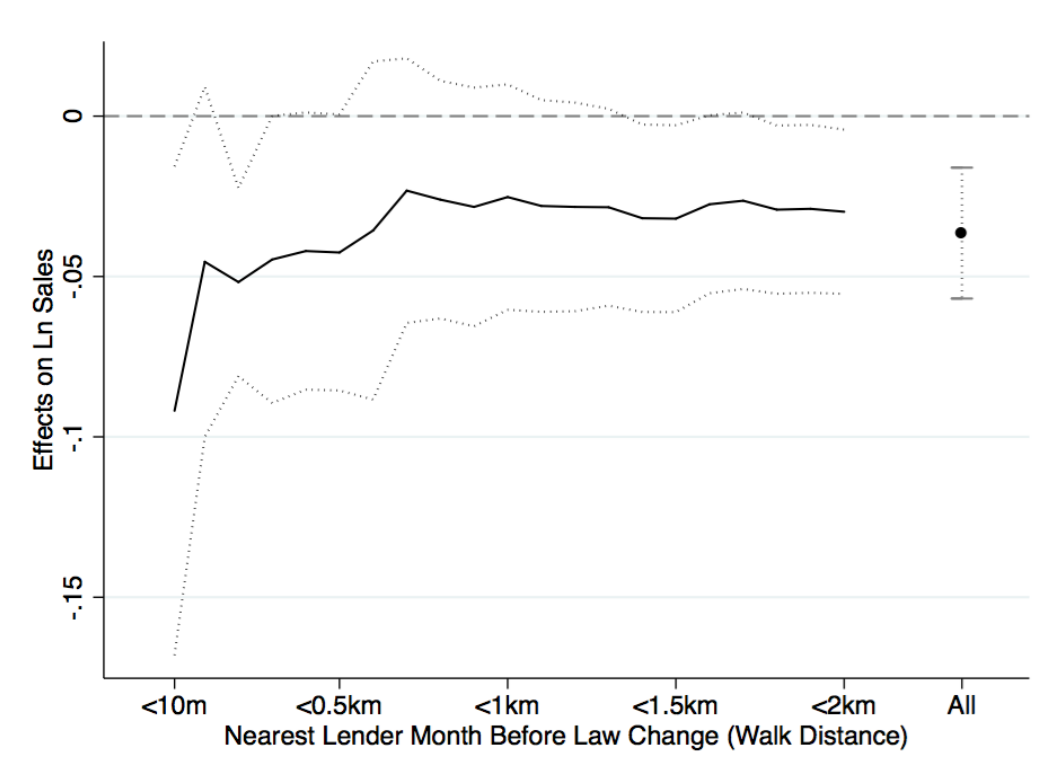
Notes: This figure shows total liquor sales by state and month, adjusted for inflation.

Figure 4
Detrended log sales over time, net of store-by-month fixed effects, averaged quarterly



Notes: This figure shows the residuals from a regression of log liquor store sales on a set of store-by-month fixed effects, averaged by quarter, and normalized so that the first observation's residual is centered at zero.

Figure 5
The relationship between distance and effect



Notes: Graph gives estimated effect by distance to a store's nearest lender in the month before Washington's law change beginning with liquor stores nearest to a lender. Moving left to right, estimates include a greater number of stores by adding those that were increasingly further from lenders at the time of the law change. Regressions include store-by-month fixed effects and controls. Dotted lines give the 95% confidence interval, calculated to account for clustering at the county-level.

6 Tables

Table 1
The effects of the PDL restrictions on liquor sales

	(1)	(2)	(3)
(N=25,330)			
Post Law×WA (Year -1)			-0.007 (0.010) [0.469]
Post Law×WA (Year 1)			-0.036*** (0.011) [0.002]
Post Law×WA (Year 2+)			-0.047** (0.018) [0.010]
Post Law×WA	-0.044*** (0.011) [0.000]	-0.036*** (0.010) [0.001]	
Avg. Pre-law WA Sales	162,547	162,547	162,547
Sales Impact	-7,091	-5,927	.
Controls	no	yes	yes

Notes: All regressions include store-by-month and year-by-month fixed effects. Estimated standard errors are reported in parentheses, adjusted for any clustering that may occur at the county-level. *** significant at 1%; ** significant at 5%; * significant at 10% (with p-values in brackets).

Table 2
Specification robustness checks

	(1)	(2)	(3)	(4)
	No WA-OR			
	Border Counties		City-level Data	
Post Law \times WA (Year -1)		-0.003 (0.011) [0.784]		-0.013 (0.009) [0.140]
Post Law \times WA (Year 1)		-0.025** (0.012) [0.037]		-0.034*** (0.012) [0.008]
Post Law \times WA (Year 2+)		-0.032* (0.018) [0.083]		-0.039** (0.018) [0.037]
Post Law \times WA	-0.027** (0.011) [0.015]		-0.027** (0.011) [0.020]	
N	21,621	21,621	17,960	17,960
Avg. Pre-law WA Sales	161,518	161,518	225,884	225,884
Sales Impact	-4,297	.	-6,038	.

Notes: Regressions in columns 1 and 2 include store-by-month fixed effects, and city-by-month fixed effects in columns 3 and 4. All specifications include year-by-month fixed effects and controls. Estimated standard errors are reported in parentheses, adjusted for any clustering that may occur at the county-level. *** significant at 1%; ** significant at 5%; * significant at 10% (with p-values in brackets).

Table 3
The effects of the PDL restrictions on liquor sales by metropolitan status

	(1)	(2)	(3)	(4)
	Large Metro Core & Fringe		Smaller Metro/ non-Core	
Post Law \times WA (Year -1)		-0.005 (0.016) [0.774]		-0.012 (0.010) [0.211]
Post Law \times WA (Year 1)		-0.054*** (0.011) [0.001]		-0.030** (0.015) [0.045]
Post Law \times WA (Year 2+)		-0.074*** (0.021) [0.007]		-0.037* (0.021) [0.078]
Post Law \times WA	-0.058*** (0.007) [0.000]		-0.025* (0.013) [0.060]	
N	9,501	9,501	15,829	15,829
Avg. Pre-law WA Sales	242,540	242,540	108,534	108,534
Sales Impact	-14,064	.	-2,698	.

Notes: Estimates are stratified using the Centers for Disease Control's National Center for Health Statistics county urbanicity classification definitions. All regressions include store-by-month and year-by-month fixed effects and controls. Estimated standard errors are reported in parentheses, adjusted for any clustering that may occur at the county-level. *** significant at 1%; ** significant at 5%; * significant at 10% (with p-values in brackets).

7 Appendix

Geocoding Procedure: Geocoding of the address data obtained from payday lender licenses and liquor store data was done using Google Maps API service. The service provided us longitude and latitudes for all addresses as well as allowed us to calculate walking and driving distances between locations. To quality control the geocoding provided by Google, we compared the original addresses from our data to the Google matched addresses. When discrepancies were found, we manually geocoded the address. The manual geocoding usually involved correcting an error in the street address and then using Google Maps to find the correct address and longitude and latitude. In some cases, Google Street View was used to find the actual store front of the business to obtain the address.

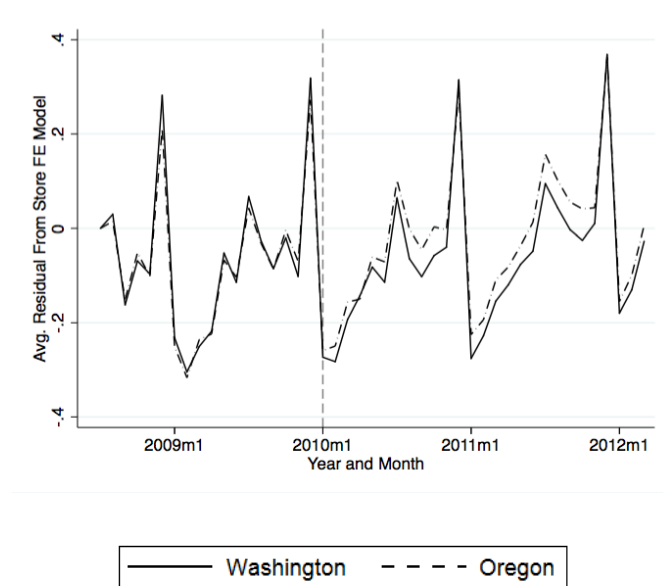
To calculate distances between liquor stores and payday lenders, we first narrowed the pairs using straight line distance obtained from the longitudes and latitudes. Specifically, we kept all liquor store and lender pairs within a given radial distance of one another. We then obtained the Google walking distance between all locations. We use Google walking distances because it provides a better measure of proximity than straight line or driving distance. The two major cities in our data, Portland and Seattle, both have large bodies of water in close proximity. Therefore, straight line distances may be misleading because locations on the opposite sides of a body of water may be linearly close, but for our purposes quite far apart. Likewise driving distance provided by Google factor in one-way streets. Therefore, a payday lender that is across the street from a liquor store may for example have a driving distance of half a kilometer if they are located on a one-way street, where a driver must circle around the block to reach the destination. Walking distances solves both of these concerns.

Table A1
Oregon and Washington Liquor Prices

	OR	WA	OR	WA	OR	WA	OR	WA
Mean	\$20.58	\$24.44	\$21.24	\$25.35	\$23.99	\$24.44	\$23.91	\$22.71
Min Price	\$6.80	\$9.10	\$6.80	\$9.40	\$6.80	\$9.65	\$6.75	\$9.55
	2009		2010		2011		2012	

Notes: The mean price includes all alcoholic beverages for sale in the states during a given year. This includes mini-bottles that retail for under \$2.00 to fine whiskey that retails for over \$2,000. The “Min Price” is the minimum price for a 750ml bottle of 80 proof liquor available in each state. Vodka, rum, gin, tequila, and whiskey are all available for within about 20 cents of the minimum prices. The Oregon price in 2009 includes the 50 cent per bottle surcharge. The Washington prices in 2009 do not include the change in markup. The reason for this discrepancy is due to the dating of our liquor price data.

Figure A1
Residuals over time from store fixed effects model, averaged by state



Notes: Average log sales for Washington and Oregon net of store fixed effects

Table A2
Control Variable Definitions

Controls	Frequency	Description	Source
Unemployment Rate	Monthly	Non-seasonally adjusted county unemployment rate as measured by the US Bureau of Labor Statistics	Federal Reserve Economic Database
HH Median Income	Yearly	County-level household median income.	U.S. Census Bureau
Pop. Below Poverty Line	Yearly	Proportion of the population with incomes below the national poverty threshold by county.	U.S. Census Bureau
Temporary Closures	Monthly	Indicator variable for liquor stores that takes a one in every month in which a store does not operate for all state mandated business days. The variable is identified by press releases announcing store closures available at http://www.oregon.gov/OLCC/pages/news.aspx and http://www.liq.wa.gov/pressreleases/main for Oregon and Washington, respectively. The indicator variable also takes a one for liquor stores mentioned in press releases as an alternative to visit during the store's closure.	Authors' calculations
Store Moves	Monthly	Indicator variable that takes a one in all months following a location change of a liquor store. Stores that do not move, but may be affected by a moving store also receive a one for all months following a move. Store moves are identified by press releases available at http://www.oregon.gov/OLCC/pages/news.aspx and http://www.liq.wa.gov/pressreleases/main for Oregon and Washington, respectively. Stores affected by a store move are identified by the press releases themselves which often offer alternative store suggestions and by using Google maps driving and walking distances. If a store moves within 500 meters of an existing store with no other stores within that distance, then indicator variable is set to one	Authors' calculations
New Entrants	Monthly	Indicator variable that takes a one in all months for a liquor store affected by a new entrant to the market after January 1st 2010. Any store within 1 kilometer (straight line distance) of the new entrant is deemed as affected. New stores are identified by press releases available at http://www.oregon.gov/OLCC/pages/news.aspx and http://www.liq.wa.gov/pressreleases/main for Oregon and Washington, respectively.	Authors' calculations

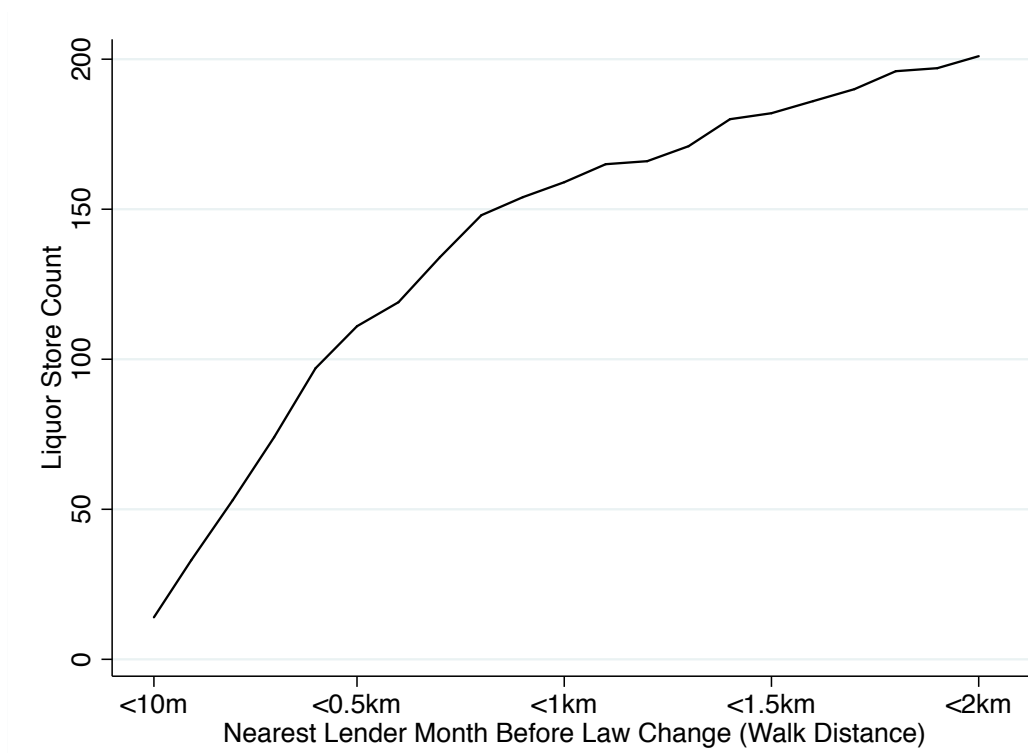
Notes: Control variable descriptions. Data and replication files available from the authors upon request.

Table A3
Standard error cluster check

	(1)	(2)	(3)	(4)
(N=25,330)				
Post Law × WA	-0.036*** (0.005) [0.000]	-0.036*** (0.011) [0.001]	-0.036*** (0.010) [0.000]	-0.036*** (0.010) [0.001]
SE Cluster Level	none	Store	City	County

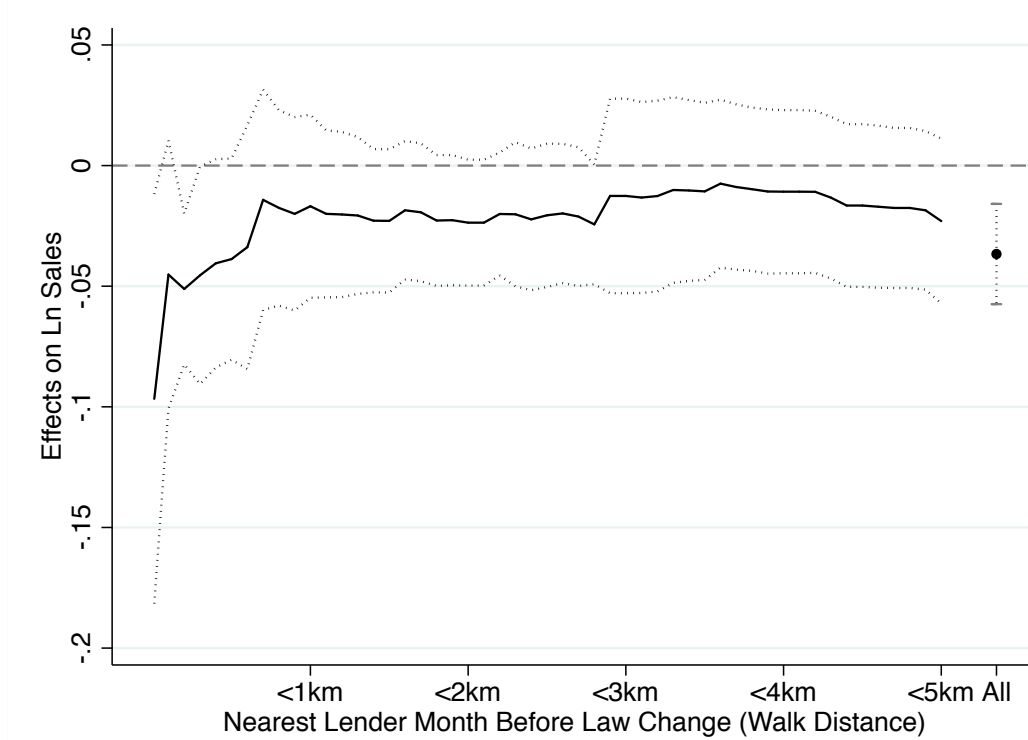
Notes: All regressions include year-by-month and store-by-month fixed effects and controls. Estimated standard errors are reported in parentheses, adjusted for any clustering at various levels. *** significant at 1%; ** significant at 5%; * significant at 10% (with p-values in brackets).

Figure A2
Cumulative store counts by distance to a lender



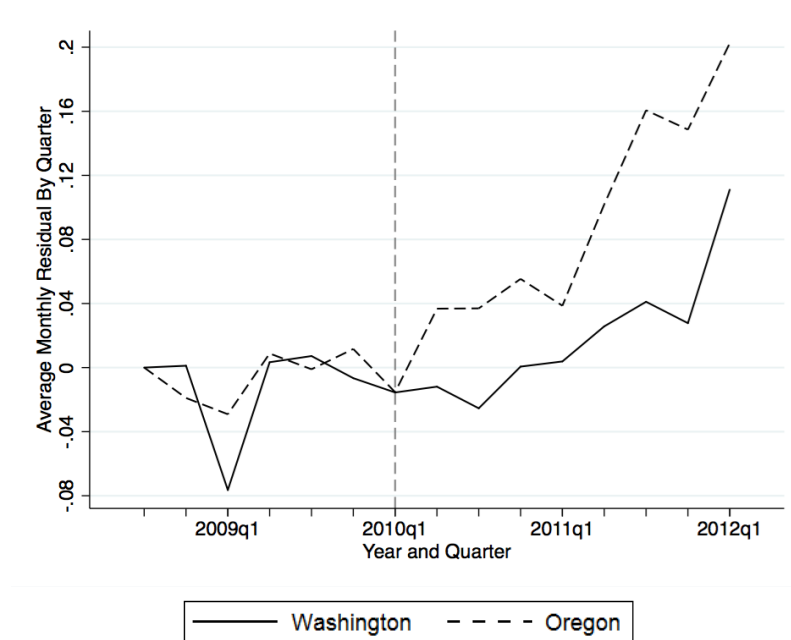
Notes: Graph gives the number of liquor stores within various walking distances of any lender in the month prior to Washington's lending restrictions (December 2009).

Figure A3
The relationship between distance and effect magnitude



Notes: Graph gives estimated effect by distance to a store's nearest lender in the month before Washington's law change beginning with liquor stores nearest to a lender. Moving left to right, estimates include a greater number of stores by adding those that were increasingly further from lenders at the time of the law change. Regressions include year-by-month and store-by-month fixed effects, and controls. Dotted lines give the 95% confidence interval, calculated to account for clustering at the county-level.

Figure A4
 Normalized average de-trended log liquor store sales for stores within 10 meters of
 a PDL



Notes: This figure shows the average residuals from a regression on liquor stores within 10 meters of PDL the month prior to the law change (December 2009) of log liquor store sales on a set of store-by-month fixed effects, normalized so that the first observation's residual is centered at zero.