

# 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 payday lenders. By focusing on states with state-run liquor monopolies, we account for endogenous supply-side variables that are typically unobserved. Further analysis of consumer-level data indicates that the lending restrictions reduce alcohol expenditures without affecting total household spending. This is consistent with a distinct relationship between payday lending access and alcohol purchases, and suggests that present biased motivations underlie some loan use. The finding is significant because it shows that payday loan access is associated with unproductive borrowing, and directly links payday loan access to public health issues.

*Keywords:* payday lending, consumer credit, alcohol

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

# 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, which include many state legislatures, 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.

Despite uncertainty surrounding the benefits to borrowers, there exists a surprising lack of research 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 a very different margin: spending on liquor.

To identify the causal effect of lending restrictions on liquor sales, we exploit a change in payday lending laws in the State of Washington that limited payday loan access for frequent borrowers. Leveraging 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 two states with state-run liquor monopolies, we can address endogenous supply-side responses, such as price changes, or store openings and closings, that normally would confound this identification strategy. We find that restricting access to payday loans reduces overall spending on liquor, with the largest effects observed at liquor stores neighboring payday lenders at the time of the law change. We then turn to individual-level data using the US Consumer Expenditure Interview Survey to determine whether or not the effects on liquor spending are part of broader reductions to overall consumption. We find that reduced loan access does not appear to affect overall household expenditures, which suggest a distinct

relationship between payday lending and alcohol spending.

The household level findings are consistent with the hypothesis that borrower productivity changes or financial difficulties caused by credit constraints do not underlie the reduction in liquor demand we observe. Therefore, to explain our results, we point to an existing literature which argues that economic theories of time inconsistency and impulsivity underlie some payday loan use (Skiba and Tobacman (2008), Zinman (2014)). This leads to two direct contributions of the paper for policy makers. First, targeted payday lending restrictions are effective at reducing at least one form of unproductive borrowing. Second, targeted payday lending restrictions reduce liquor consumption. The first speaks directly to the efficacy of the type of restrictions that Washington State implemented. The second speaks directly to the costs and benefits associated with access to payday lending.

Our results contribute to a number of ongoing debates. First, our findings provide causal evidence that payday lending affects spending on alcohol, that lender-store proximity is a salient characteristic, and that restrictions to payday loan access can reduce spending on alcohol. We are not the first to posit a link between liquor and payday lending. Collocation of lenders and liquor stores, including under the same storefront, occurs in the U.S. and elsewhere.<sup>1</sup> Local municipalities have recognized this relationship and enacted zoning restrictions to keep lenders away from liquor stores. Oakland, CA, for example, prohibits payday lenders from operating within 500 feet of banks, schools, churches and liquor stores. North Kansas City, MO goes further, specifying that lenders must locate more than 1000 feet from liquor stores.<sup>2</sup> Internationally, there exists explicit recognition of linkages between payday lending, liquor and poverty, but which is justified only anecdotally. For example, from a report on payday lending by the New Zealand government, an interviewee states, “There are cash loans in liquor stores. Why? Because a lot of low-income families, they like

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<sup>1</sup>Leyva, J. “Campbell: City considering new rules for payday lending and check cashing businesses in city.” (San Jose Mercury News. Nov. 11, 2015.) discusses proposed legislation in California to stop liquor stores and payday lenders from operating out of the same store front.

<sup>2</sup>Griffith, Kelley, Linda Hilton and Lynn Drysdale. *Controlling the Growth of Payday Lending Through Local Ordinances and Resolutions - A Guide for Advocacy Groups and Government Officials*. Nov. 2007.

to drink, so they go to a liquor store. If they can't pay for it...they keep borrowing money from a liquor store," (Couchman and Baker (2012)). That communities legislate minimum distances between lenders and liquor stores implies a belief that parallel exposure to both products generates antisocial decision making. Existing local ordinances which establish minimum distances between payday lenders and liquor stores represent an area where policy has outpaced research. Our research moves to fill this gap by providing evidence that targeted payday lending restrictions can be effective in promoting productive borrowing.

Second, there exist broader questions about the benefits that payday lenders provide to borrowers, and the communities in which lenders operate. Arguments in favor of payday lending link payday loans to increased economic productivity and financial flexibility. Arguments against claim that lenders leverage borrowers' impulsivity, present-bias or limited financial literacy to encourage harmful over-borrowing. These arguments may justify interventions in the payday lending market. Our finding that some individuals use expensive credit to finance non-productive consumption supports the latter argument, and will be of interest to researchers studying the behavior of credit-constrained individuals more generally.

Finally, which factors shift demand for alcohol is of wide interest to researchers of public health and have large economic implications. The Centers for Disease Control (CDC) estimates that the cost of excessive drinking in the US reached \$249 billion in 2010, and the World Health Organization estimates that there is a causal relation between alcohol consumption and more than 200 medical conditions. For example, among males, approximately 7.6% of world-wide mortality in 2012 was attributable to alcohol (World Health Organization (2014)). The economics literature 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 educa-

tional attainment (Renna (2007)). Evidence supports that some of these consequences result from both own as well as peer alcohol use (Waddell (2012)). The fact that restricting payday loan access can reduce spending on alcohol suggests that the effects of lending laws extend beyond traditional consumer finance, and may be large.

The remainder of 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 discuss and summarize the findings.

## 2 Background

The term “payday loan” describes a form of short-term consumer credit traditionally offered at storefronts specializing in the loans. 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, minus 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 meaningful improvements 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 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 payday loan-financed consumption of 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, Zinman (2010) finds that nearly twenty percent of Washington borrowers take loans primarily for food/groceries, shopping or entertainment, prior to the law change. 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.<sup>3</sup> Second, Olafsson and Pagel (2016) presents empirical evidence for payday loan financed alcohol consumption using a panel dataset from a financial accounts aggregation app that allows the authors to observe borrower liquidity and consumption decisions. They document that borrowers do not necessarily experience large decreases in liquidity prior to borrowing, that they often have access to cheaper forms of credit, and that they use loans primarily on non-essential consumption including alcohol. Finally, if payday lending increases financial

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<sup>3</sup> The literature studying tax refund spending among low income households finds similar spending patterns. In particular, Cole et al. (2008) using transactions data find that groceries and entertainment are the first and fourth largest spending categories by value, respectively. However, survey evidence from tax returns shows bill payment (Schneider and Tufano (2006)) or debt repayment (Barr and Dokko (2006)) as the primary uses. This inconsistency may represent a disconnect between the recipients’ intentions and actions regarding how the money is spent.

distress,<sup>4</sup> research suggests that greater alcohol use and abuse may be expected.<sup>5</sup>

## 2.1 Lending in Oregon and Washington

Washington State enacted HB 1709 on January, 1<sup>st</sup> 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. In the year prior to the law, the State of Washington estimated that roughly one third of all payday loan borrowers took out more than eight loans.<sup>6</sup> Finally, the law mandated that borrowers were entitled to a 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.<sup>7</sup> 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. In the following year, the number of locations fell again to

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<sup>4</sup>Research 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.

<sup>5</sup>In 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 alcohol consumption. Dee (2001a) and Ruhm and Black (2002) show that, although the income effect dominates during economic downturns leading to pro-cyclical alcohol consumption, financial stress related drinking appears to rise with binge drinking increasing and moderate drinkers increasing their consumption.

<sup>6</sup>2009 Payday Lending Report authored by the Washington State Department of Financial Institutions.

<sup>7</sup>Washington's reports are available online at <http://www.dfi.wa.gov/reports/payday-lending-reports>.

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.

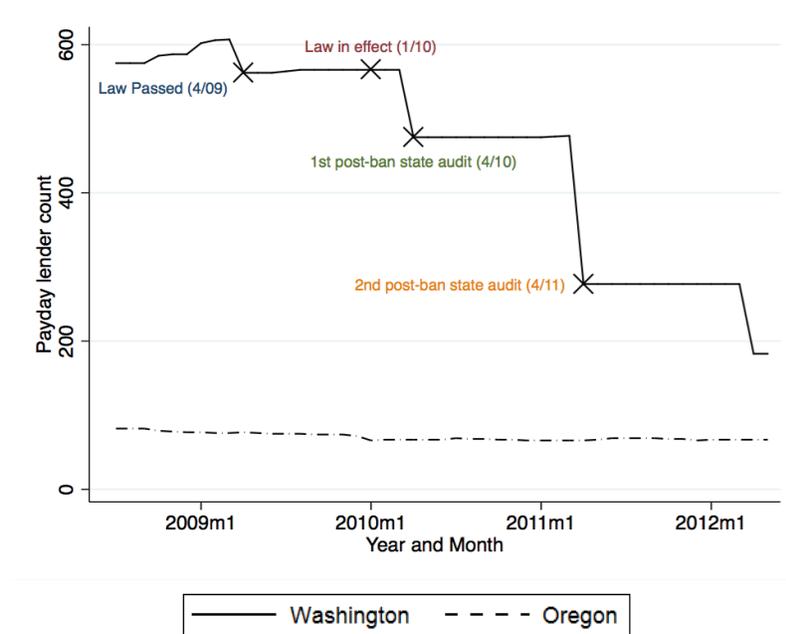
Oregon enacted similar payday lending restrictions in August of 2007. The law capped loans at 30% of income or \$700, whichever is less; borrowers could only apply for eight loans in a calendar year, and were entitled to similar instalment plans as Washington borrowers. The Oregon law also effectively 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.<sup>8</sup> Zinman (2010) employs a similar identification strategy to the one we employ using survey data from borrowers to study the impact of Oregon's law change on consumers' finances using Washington as a valid counterfactual. The results confirm the efficacy of rate and loan caps, and extended repayment periods, as payday loan users report substantively restricted access to credit in the wake of the law change.

Throughout our observation period, both states require payday lenders to 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

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<sup>8</sup>We do not possess data to similarly estimate the effect of Oregon's law change on liquor sales using Washington as a counterfactual.

Figure 1  
Total payday lenders by state



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

unchanged.<sup>9</sup>

Although in recent years, online payday lenders have grown in popularity, they remain a small portion of the market. If borrowers readily transition from brick-and-mortar to online lenders, then the expected efficacy of Washington’s law change could be diminished. However, nationally representative survey evidence suggests that most individuals opt to forego loans rather than use online lenders when states restrict payday lenders. According to the PCT survey, 95% of surveyed would-be borrowers from states restricting lending indicate that they would not use payday loans at all, versus 5% who would find an alternate credit provider (Bourke et al. (2012)). The authors report that “in states that enact strong legal protections, the result is a large net decrease in payday loan usage.”

<sup>9</sup>The 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. Having received our data from the same agency, we have no reason to doubt its comprehensiveness.

## 2.2 Liquor Sales in Oregon and Washington

A crucial feature of our identification strategy involves accounting for potentially endogenous supply side factors that challenge efforts to separately identify a change in demand from the stores' responses to that 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 control states until June of 2012 (Washington privatized liquor sales in June 2012).<sup>10</sup> In what follows, we describe the relevant operation and price setting policies in effect during the period of interest.

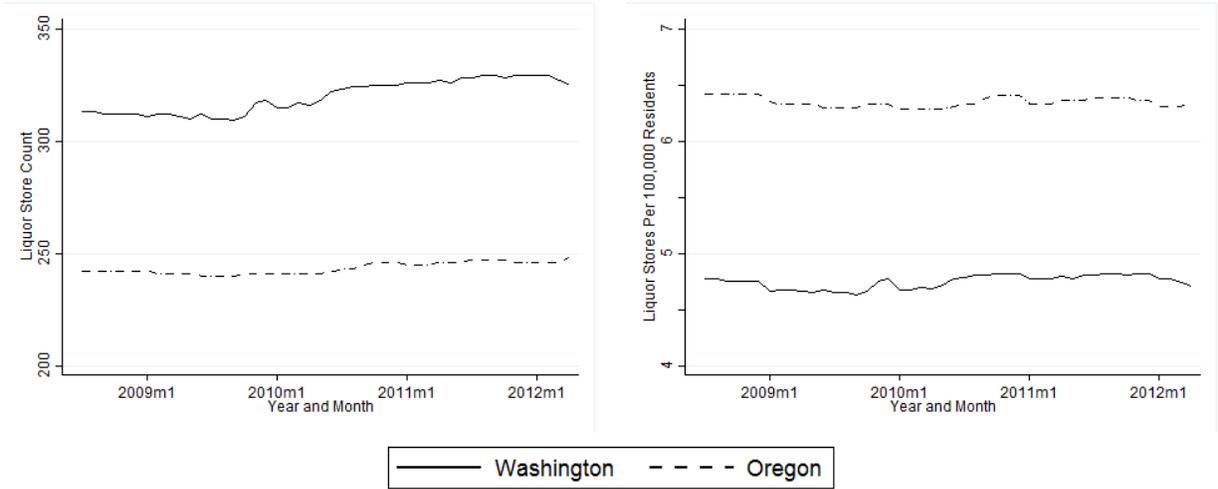
The prices in each state are set according to publicly known formulas and published in advance. These formulas and the applicable taxes differ between the two states and are periodically revised. On average, liquor prices in Oregon during the period of study are \$1.80 per bottle less than in Washington 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 mark-up (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. 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 in the public health literature as a key factor in reducing overall

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<sup>10</sup>The 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 stores where sales were conducted by state employees.

Figure 2  
Liquor store counts by state



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

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)).<sup>11</sup> Therefore, the absence of diverging prices between the two states leads us to believe that changes in prices over time do not underlie 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 increases 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.<sup>12</sup> 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.

<sup>11</sup>In 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.

<sup>12</sup>Source: Press release from Washington Liquor Control Board dated June 24, 2009.

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.<sup>13</sup> Figure 3 shows total monthly sales in Oregon and Washington over our sample period adjusted for inflation.<sup>14</sup> 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.

To assess the effect of the law on individual consumption, we supplement our liquor sales data by studying the responses in the Consumer Expenditure Interview Survey for Oregon and Washington, over the same time period. We look at the effect of the law change on household total consumption expenditure, household alcohol expenditure, and use household characteristics to look at the response of populations most likely to use payday loans.

## 2.3 Mechanisms

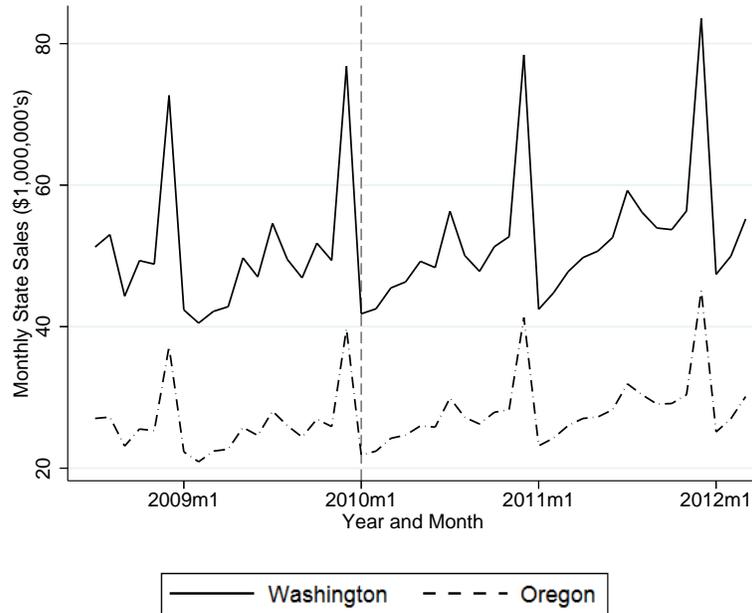
We next consider the theoretical mechanisms through which Washington’s payday lending law change may affect spending on liquor. As the lending restrictions did not set out to target liquor sales, any link between liquor sales and payday lending occurs as a byproduct of the loss of credit access. For borrowers who use the loans to smooth consumption, the predicted response is ambiguous and depends critically on whether there exists a positive or negative link between payday loan access and a borrower’s ability to generate and/or maintain income. If greater loan access contributes to higher productivity (by enabling borrowers to pay for unanticipated car repairs or medical expenses to avoid missing work, for example), then restricting payday loans should reduce average consumption along all margins, including liquor. However, if loans do not impact productivity or have some negative effect, then the

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<sup>13</sup>Although 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.

<sup>14</sup>We 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 of our regressions.

Figure 3  
Total liquor sales by state and month



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

predicted response of average consumption to lending restrictions is the opposite. Average consumption of goods and services which are unrelated to servicing payday loans rise over time since borrowers no longer incur payday loan interest payments, yet possess the same or higher average disposable income.

The existing empirical literature generally supports a negative relationship between payday loan access and productivity. For example, loan access is found to reduce job performance and retention, and the ability to afford medical care (Carrell and Zinman (2014) and Melzer (2011), respectively). Therefore, if payday lending affects productivity, we expect that lending restrictions increase incomes for borrowers, which leads to greater consumption (including of liquor) on average following the law change.

Alternatively, unanticipated productivity shocks may not motivate borrowers' use of payday loans. Instead, there may exist some degree of complementarity between loans and liquor in borrowers' preferences. Then, even holding incomes fixed, credit access may catalyze liquor purchases that otherwise would not take place. In this case, liquor sales are

predicted to fall in response to the law change. We see two compelling reasons for such a link to exist. First, there exists significant overlap in the behavior of both frequent payday loan users and heavy users of alcohol. Specifically, 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.<sup>15</sup> In these models, individuals can objectively benefit from a restricted choice set that limits their access to loans and liquor, with clear benefits in the case of liquor given the health consequences, antisocial behaviors and negative externalities associated with over-consumption.

Second, there is empirical evidence that liquidity and problem alcohol use are related. Following the randomized arrival of payments from the 2008 Economic Stimulus program, Gross and Tobacman (2014) find that the probability of an adult being hospitalized for an alcohol or drug related incident significantly increased. They conclude that liquidity constraints are not a barrier for medical care but instead liquidity increases the need for care through its interaction with anti-social behaviors. Relatedly, a large literature finds that regular within-month variation in liquidity generated by paydays and transfer payments coincides with patterns of substance abuse and mortality. For example, Stephens (2006) considers U.K. household expenditure surveys and finds that spending, particularly on instantaneous consumption goods, which includes alcohol, rises in the weeks in which payday occurs for young, and less affluent workers. Halpern and Mechem (2001) find substance-related morbidity spikes at the beginning of each month, which coincides with increases in household liquidity from the receipt of a monthly pay check. With respect to mortality, Phillips et al. (1999) find that deaths from substance abuse and other external causes, such as motor vehicle accidents and suicide rises by fourteen percent in the first week of each month, while total deaths increase by only one percent. More recently, Evans and Moore (2012) show a more general connection between within-month changes in liquidity and mor-

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<sup>15</sup> See Laibson (2001), MacKillop et al. (2010) and Zinman (2014) for cue theory and Skiba and Tobacman (2008) and Vuchinich and Simpson (1998) for hyperbolic preferences, payday lending, and consumption.

tality. They show that changes in liquidity are associated with changes in economic activity that drive increases in mortality due to a wide range of factors, which includes substance abuse.<sup>16</sup> Finally, Dobkin and Puller (2007) find that California’s move to distribute transfer payments uniformly throughout the month eliminated the clustering of substance-abuse related hospitalizations that historically occurred at the beginning of the month when the payments were previously disbursed. The authors argue that the illusion of a “full wallet” positively affects payment recipients’ substance abuse, despite permanent income remaining constant. In the next section, we empirically examine the effects of Washington’s law change on liquor sales, and present empirical evidence supporting a negative effect on sales from restricted access, while total household expenditures remain unchanged.

### 3 Empirics

To retrieve the causal effect of the lending restrictions on liquor sales, we employ a difference-in-differences (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 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:

$$\ln(Sales_{it}) = \alpha + \beta_1 Post\ Law_t + \beta_2 WA_i + \beta_3 Post\ Law_t \times WA_i + \epsilon_{it} \quad (1)$$

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,  $\beta_3$ , represents the causal effect of the lending restrictions.

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<sup>16</sup> Andersson et al. (2015), however, do not find evidence of increased substance abuse related mortality using Swedish data in response to variation in monthly pay receipt. However, the authors note that their study looks at relatively high socio-economic status individuals.

However, this specification may be susceptible to potential estimation bias from unobserved store characteristics, fixed over time, and correlated with sales and the  $Post\ Law_t \times WA_i$  term. 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,  $\alpha_{im}$ :

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

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.<sup>17</sup> 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, while sales in both states maintain a common upward trend.

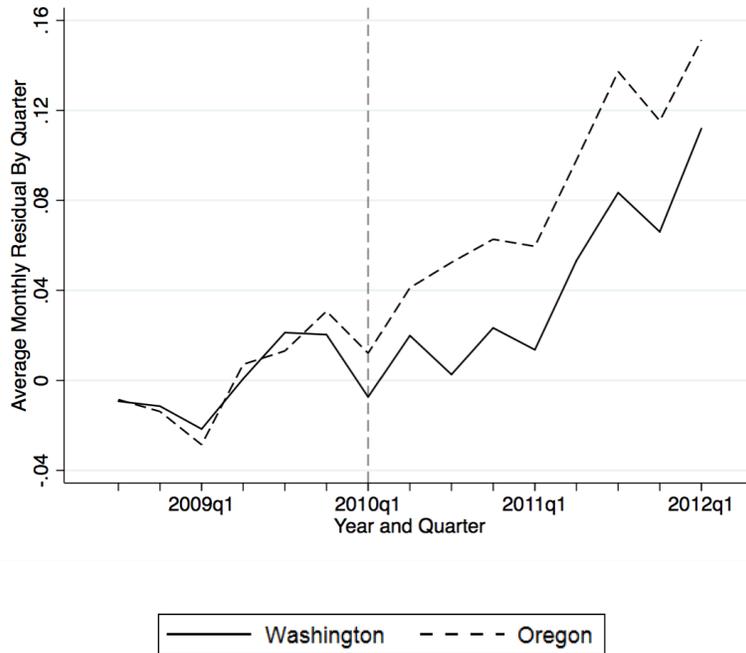
Although Figure 4 shows that sales in both states’ follow common trends prior to the lending restrictions, we can further relax the requirements for identification and likely reduce residual variance with the inclusion of relevant control variables. Therefore, we introduce both store-specific and regional 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 the ZIP code level. For store operations, we include indicator

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<sup>17</sup>As one would expect, store fixed effects alone fail to remove the apparent seasonality. Appendix Figure A1 plots log consumer sales, net of store fixed effects, and averaged by month and year.

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, normalized relative to states' pre-treatment means, and averaged by quarter.

variables that account for changes in sales driven by temporary store closures, store location moves, and possible competitive effects of new stores entering the market. In particular, to control for temporary store closures we introduce store specific indicator variables that take a one during a closure and zero otherwise. For a store location move, we create a store specific indicator variable that takes a one for all periods following the store move to capture any permanent changes in sales that may occur due to the new location. To control for the effect of new entrants, including entrants that occur due to a store move, 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.<sup>18</sup> Finally, to correct for possible bias in the estimated parameters of these controls driven by time-specific shocks, we remove

<sup>18</sup>The 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.

arbitrary trending over time with the inclusion of year-by-month fixed effects,  $\tau_t$ . Like the control variables, these fixed effects may provide additional efficiency gains. Our preferred specification is therefore given by the following equation:

$$\ln(\text{Sales}_{it}) = \alpha_{im} + \tau_t + \delta_1 \text{Post Law}_t \times \text{WA}_i + \mathbf{x}_{it}\mathbf{b} + \epsilon_{it}. \quad (3)$$

Table 1 reports the estimated causal effect of Washington’s payday lending restrictions on consumer liquor sales using the full sample of stores. 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.<sup>19</sup> 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. It is reassuring that this number is small, as large declines in liquor spending relative to the change in payday lending would imply either extreme changes in borrowers preferences for liquor when access to payday loans is restricted, or large declines in productivity.

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

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<sup>19</sup>Table 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.

Table 1  
The effects of the PDL restrictions on liquor sales

	(1)	(2)	(3)
(N=25,330)			
WA × Post Law (Year -1)			-0.007 (0.010) [0.469]
WA × Post Law (Year 1)			-0.036*** (0.011) [0.002]
WA × Post Law (Year 2+)			-0.047** (0.018) [0.010]
WA × Post Law	-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).

credit constraints.

We also consider and rule out three other possible explanations for the 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 experience little change in per-capita store numbers over time, a possible concern with competitive effects not captured by our controls is that the distribution of liquor sales in Washington shifted during the period relative to Oregon. If this shift was systematically related to liquor store sales volume, i.e., small stores were out competed by large stores, then given our log specification, such a shift could result in a negative effect on sales being observed at the store level even if there was no effect on total sales in the state. To investigate this possibility, we consider varying levels of aggregation of the sales data.

Aggregating to the state level, sales in Washington for the year 2010 are relatively flat compared to 2009, while Oregon sales increase. In fact, the year 2010 is the only year in our data for which Washington sales do not show a year-over-year increase in sales similar to the one experienced in Oregon.<sup>20</sup> Estimates in columns 3 and 4 of Table 2 show our preferred specification with all applicable controls estimated on sales aggregated to the city level. The estimates indicate that, while entry may play a small role in determining store-level sales, significant reductions in liquor sales occur at the city-level.<sup>21</sup> In addition, the total count of observations in the Table 2 is not much smaller than the total number of individual store observations (25,330), which indicates that most liquor stores do not have competitors in the same city, further minimizing any concern about competitive effects biasing our estimates.

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<sup>20</sup>The average year-over-year monthly growth rate of Oregon sales in the sample is 5.5%. The average year-over-year monthly growth rate of Washington sales in the sample excluding the year 2010 is 5.2%. Average sales growth in Washington in 2010 is just 0.01%

<sup>21</sup>It 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.

Table 2  
Specification robustness checks

	(1)	(2)	(3)	(4)
	No WA-OR			
	Border Counties		City-level Data	
WA × Post Law (Year -1)		-0.003 (0.011) [0.784]		-0.013 (0.009) [0.140]
WA × Post Law (Year 1)		-0.025** (0.012) [0.037]		-0.034*** (0.012) [0.008]
WA × Post Law (Year 2+)		-0.032* (0.018) [0.083]		-0.039** (0.018) [0.037]
WA × Post Law	-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).

Estimates in 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 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.

Finally, 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, about alternative explanations. The only relevant changes we identified from our research and conversations with regulators were those already discussed in Section 2.

### **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 as a result of discrimination in traditional credit markets, for example, or merely proxy for characteristics that do. Therefore, separating counties with large metropolitan areas from those with smaller communities will capture differences in the intensity of payday loan reliance simply due to the fact there is a higher concentration of payday loan users in larger metropolitan areas.

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	
WA × Pre Law (Year -1)		-0.005 (0.016) [0.774]		-0.012 (0.010) [0.211]
WA × Post Law (Year 1)		-0.054*** (0.011) [0.001]		-0.030** (0.015) [0.045]
WA × Post Law (Year 2+)		-0.074*** (0.021) [0.007]		-0.037* (0.021) [0.078]
WA × Post Law	-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).

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).<sup>22</sup> 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, representing the non-uniform geographic distribution of residents.<sup>23</sup>

In line with greater loan usage, per capita, and greater access in densely populated and more racially diverse 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).<sup>24</sup> 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 Store Sales and Lender Proximity

We next consider the role that distance between lenders and stores plays in determining the negative effects. Distance may be a salient feature for a number of reasons. First, individuals intent on using payday loans to purchase liquor may seek to limit travel costs by visiting lender-liquor store pairs which locate near to one another. If the lending restrictions eliminate credit access for these borrowers, then stores nearest to lenders should experience a relatively greater decline in demand. Second, the colocation of liquor stores and lenders may generate new demand for both products which would not occur if greater separation existed. In this case, individuals (with or without a prior demand for a loan) receive a cue for alcohol and at the same time possess the means to make an immediate purchase. The exit of

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

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

<sup>24</sup>Data from the 2010 US census confirms the greater racial diversity in large metro counties versus smaller counties (whites make up 76 and 84 percent of the populations, respectively).

nearly two thirds of all payday lenders from the market significantly reduces the convenience and likelihood of these types of transactions. Finally, theories of anchoring posit that some retailers generate positive economic spillovers on nearby businesses as they draw people to the area. When a lender’s contribution to the localized economic viability of a shopping center is diminished as a result of the restrictions on lending (by closing, for example), then borrowers with pre-existing demand for liquor may shop elsewhere causing a redistribution of sales across liquor stores.

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.<sup>25</sup> We then repeatedly estimate 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.<sup>26,27</sup> We believe that a two kilometer walking distance represents a reasonable upper bound for which distance remains a salient predictor of sales.<sup>28</sup> 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%

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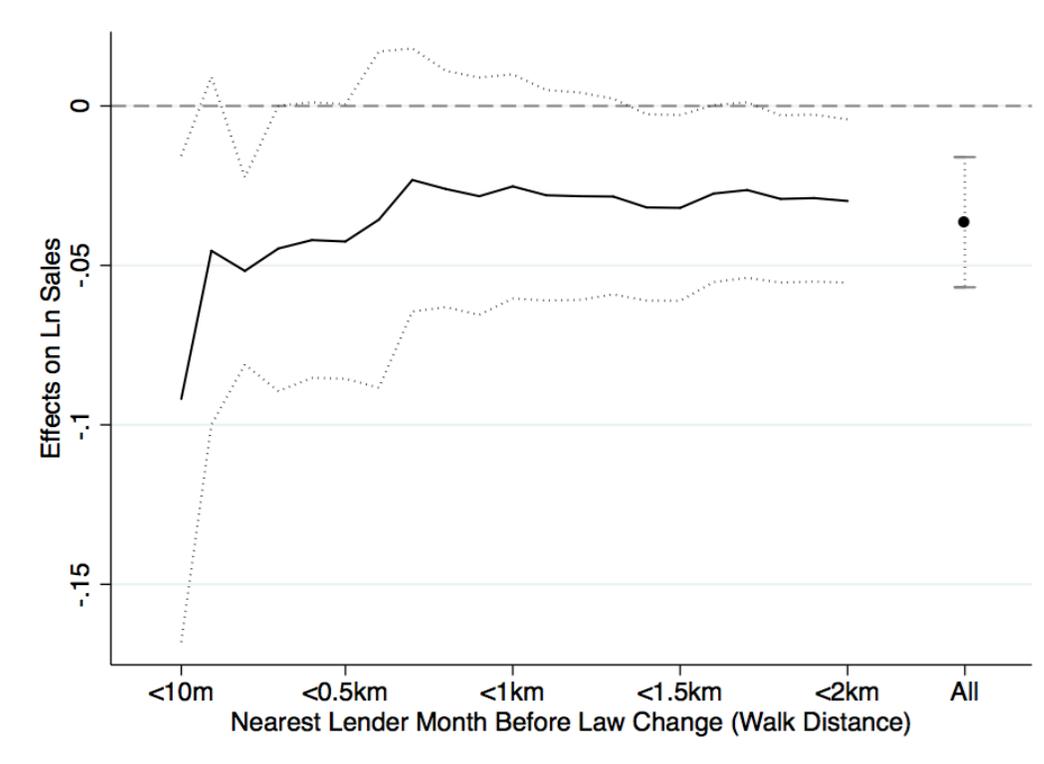
<sup>25</sup>See the data appendix for a description of the geocoding procedure.

<sup>26</sup>Appendix Figure A2 gives the cumulative number of liquor stores by distance to the nearest lender in the month before the law change.

<sup>27</sup>We use the walking distance in the month before the Washington law change, rather than a more time-dynamic measure such 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.

<sup>28</sup>For robustness, Figure A3 expands this window out to five kilometers.

Figure 5  
The relationship between distance and effect



*Notes:* Graph gives the 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. The right-most scatter point gives the estimated effect using the full sample of liquor stores. Regressions include store-by-month and month-by-year fixed effects and controls. Dotted lines give the 95% confidence interval, calculated to account for clustering at the county-level.

levels), an effect almost three times as large as that observed overall.<sup>29</sup> The larger effect rapidly declines with distance suggesting that even a small degree of separation between liquor store and lender before the law is enacted is significant in determining the magnitude of its eventual impact.

If unobserved state-wide factors, rather than reductions in payday loan access, drive the drop in liquor spending observed in Table 1, then distance should be orthogonal to the magnitude of the decline in sales. We therefore interpret the relatively large decline at stores

<sup>29</sup>In 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. A 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.

nearest to lenders as providing further evidence of the causal relationship between loans and liquor.

### 3.3 Effect of the law on other types of consumption

How broadly reductions in access to payday lending affect spending remains an important question for policy makers, and an open question in the literature. If reduced access to payday lending diminishes spending across the spectrum of consumption goods, this would signal that borrowing is positively correlated with productivity, and that the decline in liquor that we observe is potentially part of a broader decline in overall consumption. The liquor store data do not allow us to fully investigate the effects of Washington’s lending restrictions on broader categories of spending. However, we are able to consider whether similar declines in liquor consumption occur more widely at locations catering to on-site consumption of alcohol (for example at restaurants and nightclubs).

Throughout the sample period, licensed establishments serving alcohol purchase all liquor from the state monopolies, generally from the same stores that consumers shop at. Using the data on sales to licensees, we find that the states’ sales trend in parallel pre-law as before. But, when re-estimating equation (3) using log sales to licensees as the dependent variable, we find a small and statistically insignificant effect of the lending restrictions ( $\delta_1 = 0.018$ , p-value = 0.61).<sup>30</sup> The result suggests that wider declines in consumer temptation spending at bars and restaurants do not occur.<sup>31</sup>

In addition, to further explore the broader effects of Washington’s lending restrictions, we also analyze data from the Bureau of Labor Statistics’ Consumer Expenditure Interview Survey (CE). The CE is a rotating panel that surveys U.S. households about their spending habits. Households participate for one year and are surveyed every three months about

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<sup>30</sup>Figure A5 in the appendix shows the analogue to Figure 4 for licensee sales.

<sup>31</sup>We note that unlike direct sales to consumers for at-home consumption, bars and restaurants are free to lower prices to counteract drops in patronage. Therefore, we cannot rule out that spending declines while consumption remains the same. This would explain the apparent inconsistency with Olafsson and Pagel (2016), which finds that consumer’s use payday loans to make restaurant purchases.

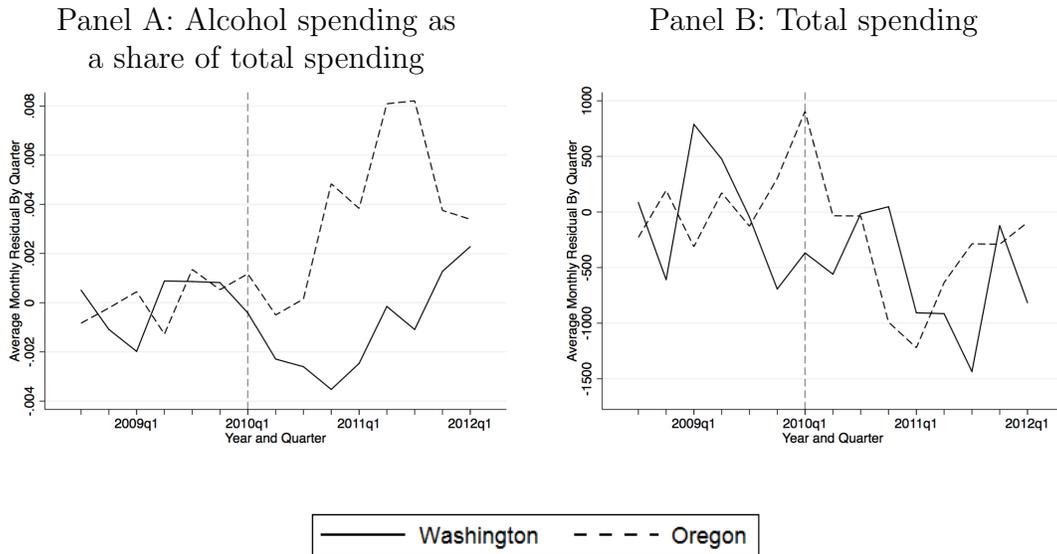
their spending in the previous quarter. We believe the CE data can help reveal whether the declines in liquor spending occur in isolation, or as a result of wider reductions in household consumption. We restrict our attention to households with positive reported earnings who can, therefore, borrow against a future paychecks.

Panel A of Figure 6 plots the share of total expenditures devoted to alcohol, by state over time, net of state-specific monthly cyclicalities. This figure represents the CE analog to Figure 4. Consistent with the DD parallel trends assumption, the share of total expenditures devoted to alcohol in Oregon and Washington exhibit similar pre-law trends. However, the states diverge following the law change, implying a disproportionate reduction in alcohol spending by Washington consumers, relative to Oregon consumers. This finding indicates that lending restrictions do not cause proportionate reductions in consumption across the full range of goods which comprise a household's consumption basket. Further, in Panel B, we show that not only did alcohol spending exhibit disproportionate declines, but total spending in Washington and Oregon follow similar trends throughout the sample period. This implies that reductions in total consumption spending in Washington do not underlie the divergence in alcohol spending over time shown in Panel A.

Table 4 presents the DD point estimates of the effect of Washington's law change with state-by-interview month and month-by-year fixed effects, which corresponds closely to the identification strategy employed for liquor sales in equation 3. The point estimate in column 1 indicates that Washington's payday lending restrictions cause a one-half percentage point reduction in the share of consumption devoted to alcohol spending in Washington, relative to the amount expected in the absence of the law change ( $p$ -value = 0.019). Columns 2 and 3 report the effects of the law on total and alcohol-specific expenditures, respectively. The point estimate for total expenditure is negative and insignificant (-\$279 per quarter with a standard error of \$878), meaning we cannot reject the null of no change. The point estimate for alcohol spending is also negative and insignificant (-\$19 with a standard error of \$21.30), but unsurprisingly proportionally larger than the drop in total expenditures relative to their

Figure 6

Share of households' total spending on alcohol, net state-by-month fixed effects, averaged quarterly



*Notes:* Figure shows the residuals from a regression of quarterly alcohol expenditures divided by total quarterly expenditures on a set of state-by-interview month fixed effects, normalized relative to states' pre-treatment means, and averaged by quarter. Data is from the Consumer Expenditure Interview Survey.

pre-law means, in accordance with the point estimate in column 1.

However, these results represent the effect of the law on the full sample of households with positive earning. Since lower income households are more likely to rely upon payday loans, we believe the effects of Washington's lending restrictions should be concentrated among lower income households, leaving higher earning households relatively unaffected. Therefore, in Table 5, we reestimate the results presented in Table 4 separately for households stratified by income terciles. We find that indeed the reduction in alcohol consumption is primarily driven by the lowest income households, which is consistent with this group's greater reliance on payday loans. Households in the lowest income tercile, with average household incomes of \$20,315 in our sample, reduce the proportion of their budget dedicated to alcohol by nearly one percentage point on average (p-value = 0.067) following the law change. For the remaining terciles, we fail to reject the null of no response to the law change at conventional significance levels. Although, the point estimates for the middle income tercile are also negative, while the point estimates for the high income tercile are positive.

Table 4  
The effects of the PDL restrictions on consumption expenditures

	(1)	(2)	(3)
(N=2,745)	Alcohol share of total expenditures	Total expenditures (\$)	Alcohol expenditures (\$)
WA × Post Law	-0.0048** (0.0020) [0.019]	-278.6 (878.0) [0.751]	-19.0 (21.3) [0.372]
Pre-law WA mean outcome	.0094	11,527	98

*Notes:* All regressions include state-by-interview month and month-by-year fixed effects. Data is from the Consumer Expenditure Interview Survey. Estimated standard errors are reported in parentheses, adjusted for any clustering that may occur at the family-level. \*\*\* significant at 1%; \*\* significant at 5%; \* significant at 10% (with p-values in brackets).

The CE results, together with licensee sales results, provide corroborating evidence for our main conclusion that payday lending restrictions reduce spending on alcohol for home consumption, but do not harm broader household consumption, and is in-line with the argument that for some borrowers, there exists complementarities between payday loans and liquor.

Table 5  
The effects of the PDL restrictions on quarterly alcohol expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Lower-tercile Earners			Middle-tercile Earners			Upper-tercile Earners		
	Alcohol share of total expenditures	Total expenditures (\$)	Alcohol expenditures (\$)	Alcohol share of total expenditures	Total expenditures (\$)	Alcohol expenditures (\$)	Alcohol share of total expenditures	Total expenditures (\$)	Alcohol expenditures (\$)
WA × Post Law	-0.0092* (0.0050) [0.067]	257.2 (797.4) [0.747]	-37.7 (27.9) [0.176]	-0.0055 (0.0034) [0.105]	-338.7 (912.5) [0.711]	-28.9 (34.8) [0.407]	0.0003 (0.0025) [0.902]	69.3 (1,650.9) [0.967]	27.4 (45.8) [0.550]
Observations	906	906	906	908	908	908	931	931	931
Pre-law WA mean outcome	.013	6,768	71	.007	9,190	67	.009	15,835	135

*Notes:* All regressions include state-by-interview month and month-by-year fixed effects. Data is from the Consumer Expenditure Interview Survey. The household income tercile ranges are estimated from the CE data. The low income cutoff is \$37,149 and the middle income cutoff is \$80,407. Average annual incomes of households in the low-, middle-, and high-tercile brackets are \$20,315, \$57,791, and \$141,352, respectively. Estimated standard errors are reported in parentheses, adjusted for any clustering that may occur at the family-level. \*\*\* significant at 1%; \*\* significant at 5%; \* significant at 10% (with p-values in brackets).

## 4 Conclusion

Our analysis provides the first empirical evidence of a connection between payday lending and aggregate spending on liquor using store-level data. We uncover a persistent reduction in liquor sales resulting from payday lending regulations that restricted access for frequent payday loan users. Our investigation finds heterogeneity in the response to the law changes consistent with known borrowing patterns in urban environments. In particular, the effect of the law change is approximately twice as large in densely populated urban areas, which also possess higher rates of payday loan use. Further, for liquor stores located nearest to a payday lender, the effect of the law change is almost three times as large as that observed overall. Looking at other measures of consumption, we find no corresponding effect on liquor stores' sales to bars and restaurants, and no evidence of an effect on total expenditure using data from the Consumer Expenditure Interview Survey.

Since wider changes to total households' expenditures do not accompany the regulations' effects on liquor spending, we conclude that productivity changes cannot explain our results. Instead, we believe that access to payday lending causes some borrowers to allocate more of their fixed budgets to liquor than they otherwise would. Consistent with this finding is the fact that the marginal affected borrower - by design of the law - is not an individual who obtains an occasional loan in response to unforeseen productivity shocks. To explain our results, we point to an existing literature which finds that economic theories of time inconsistency and impulsivity underlie some payday loan use. That these theories also underlie over-consumption of alcohol provides further evidence for this conclusion. As such, regulations which limit the scope for lenders to leverage borrowers' behavioral preferences to both over-borrow and over-consume can be effective in reducing unproductive borrowing. Policy makers should recognize that there exist interconnections between the market for payday loans and other areas of regulatory interest (e.g. public health), which means that the effects of interventions in this industry extend beyond consumer finance concerns and may be large.

# Acknowledgements

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## 5 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	

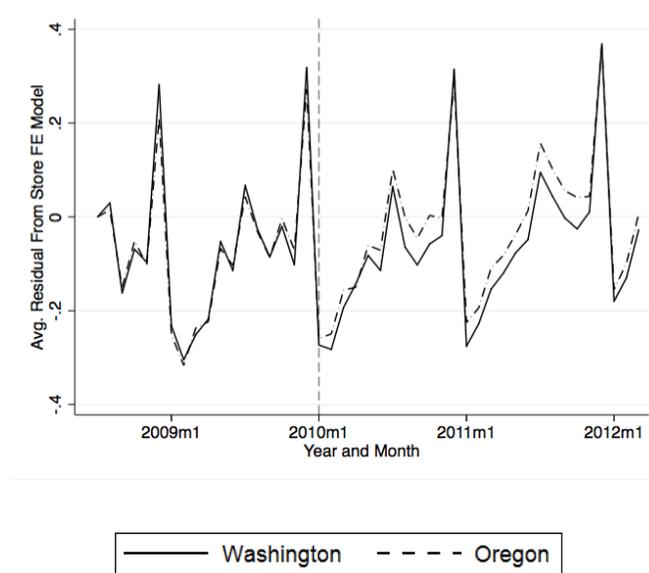
*Notes:* The mean price includes all alcoholic beverages for sale in the states during a given year. This includes mini-bottles that retail for under \$2.00 to fine whiskey that retails for over \$2,000. The “Min Price” is the minimum price for a 750ml bottle of 80 proof liquor available in each state. 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.

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 <a href="http://www.oregon.gov/OLCC/pages/news.aspx">http://www.oregon.gov/OLCC/pages/news.aspx</a> and <a href="http://www.liq.wa.gov/pressreleases/main">http://www.liq.wa.gov/pressreleases/main</a> 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 <a href="http://www.oregon.gov/OLCC/pages/news.aspx">http://www.oregon.gov/OLCC/pages/news.aspx</a> and <a href="http://www.liq.wa.gov/pressreleases/main">http://www.liq.wa.gov/pressreleases/main</a> 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 <a href="http://www.oregon.gov/OLCC/pages/news.aspx">http://www.oregon.gov/OLCC/pages/news.aspx</a> and <a href="http://www.liq.wa.gov/pressreleases/main">http://www.liq.wa.gov/pressreleases/main</a> for Oregon and Washington, respectively.	Authors' calculations

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

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



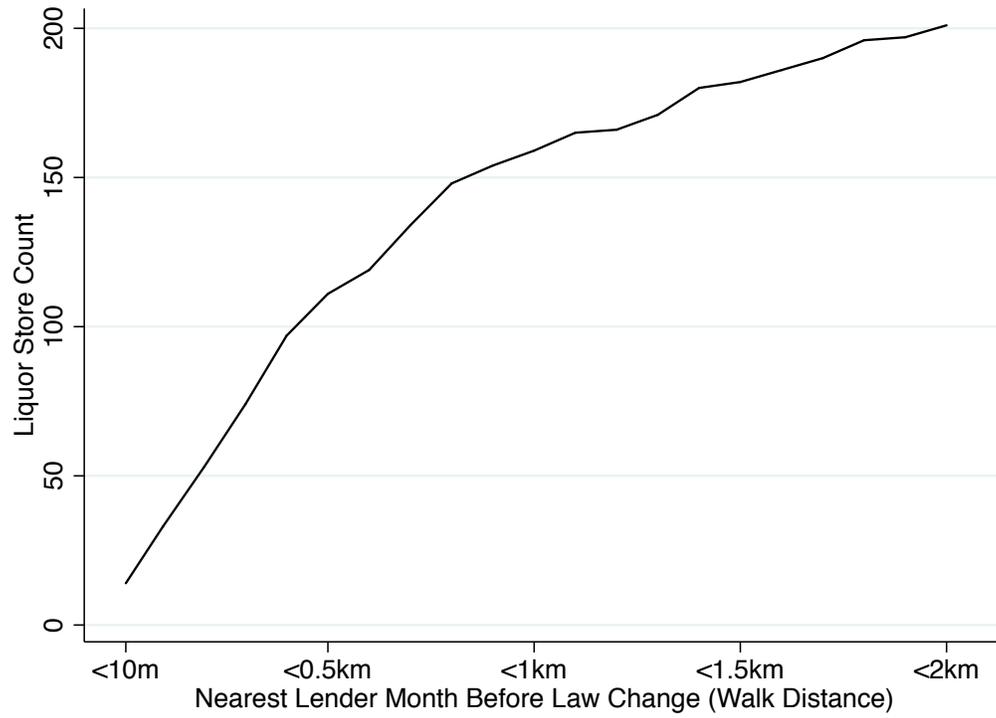
Notes: Average log sales for Washington and Oregon net of store fixed effects

Table 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

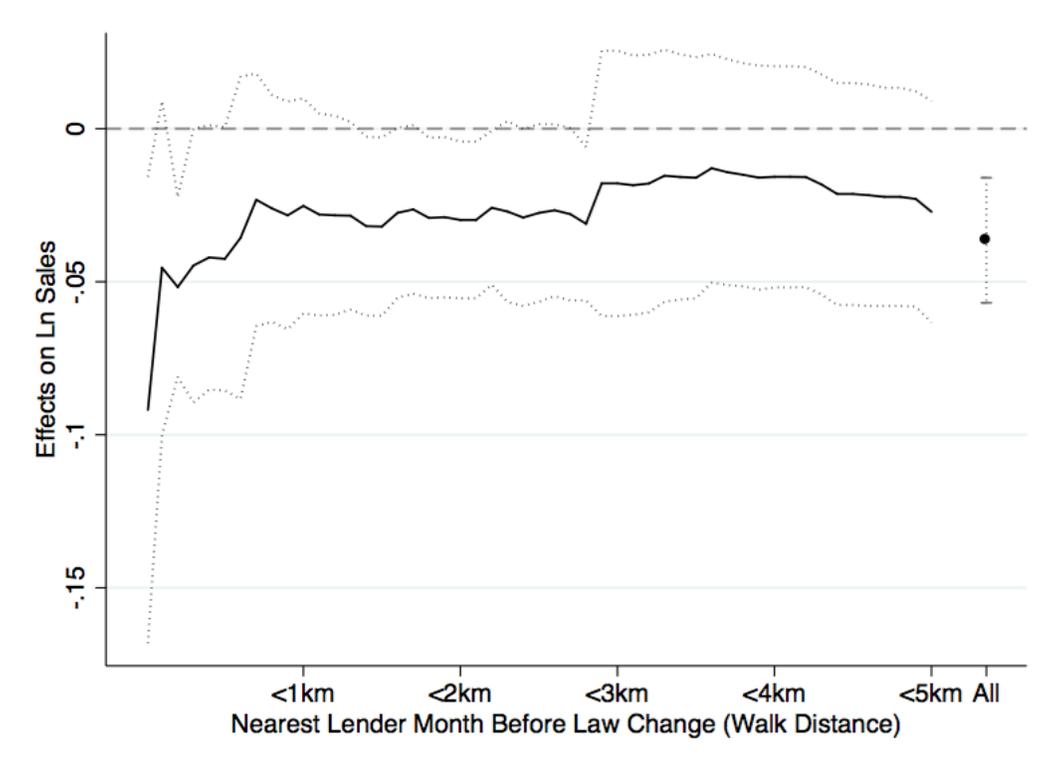
Notes: All regressions include year-by-month and store-by-month fixed effects and controls. Estimated standard errors are reported in parentheses, adjusted for any clustering at various levels. \*\*\* significant at 1%; \*\* significant at 5%; \* significant at 10% (with p-values in brackets).

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).

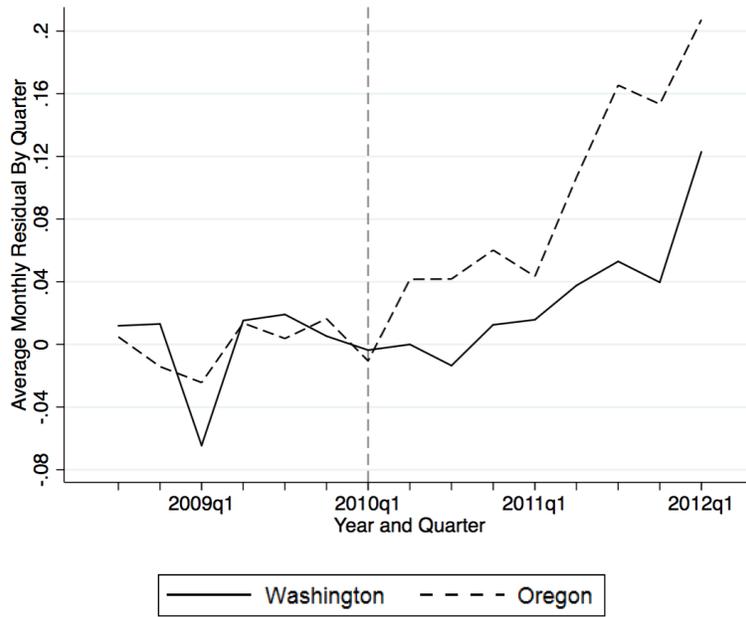
Figure A3  
The relationship between distance and effect magnitude



*Notes:* Graph gives the 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. The right-most scatter point gives the estimated effect using the full sample of liquor stores. 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

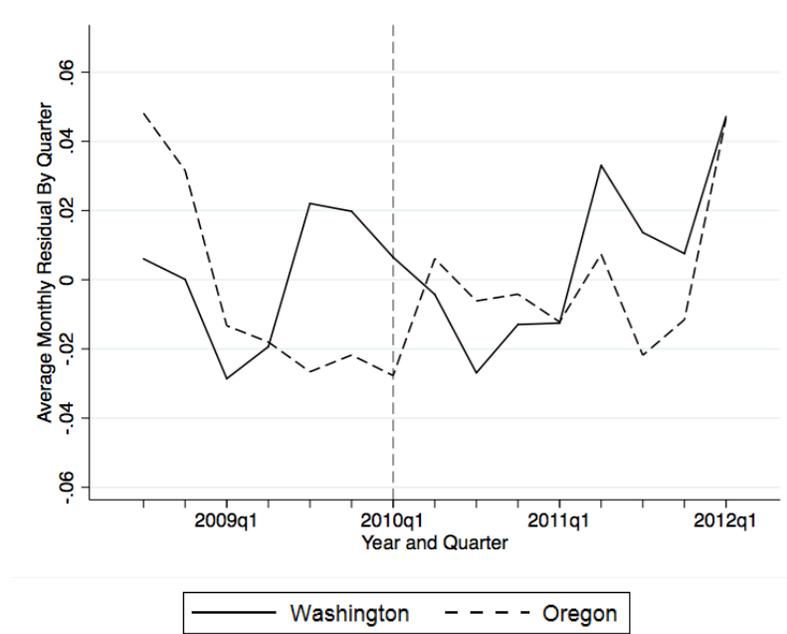
Detrended log sales over time, for stores within 10 meters of a PDL, net of store-by-month fixed effects, averaged quarterly



*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 relative to states' pre-treatment means, and averaged by quarter.

Figure A5

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



*Notes:* This figure shows the average residuals from a regression of log licensee liquor store sales on a set of store-by-month fixed effects, normalized relative to states' pre-treatment means, and averaged by quarter..