Restricting Consumer Credit Access:
Household Survey Evidence on Effects Around the Oregon Rate Cap[*]

Jonathan Zinman
Dartmouth College

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ABSTRACT

Many policymakers and some behavioral models hold that restricting access to expensive credit helps consumers by preventing overborrowing. I examine some short-run effects of restricting access, using household panel survey data on payday loan users collected around the imposition of binding restrictions on payday loan terms in Oregon. The results suggest that borrowing fell in Oregon relative to Washington, with former payday loan users shifting partially into plausibly inferior substitutes. Additional evidence suggests that restricting access caused deterioration in the overall financial condition of the Oregon households. The results suggest that restricting access to expensive credit harms consumers on average.

Keywords: payday loan, subprime credit market, predatory lending, usury, interest rate ceiling, behavioral economics, psychology and economics, financial sophistication, financial literacy, household finance, consumer finance, behavioral finance

I. Introduction

Expanding access to credit is a key ingredient of financial development strategies worldwide. The Small Business Administration and comparable small and medium-enterprise (SME) initiatives target billions of dollars of commercial credit in developed economies. The microcredit industry targets billions of dollars of commercial credit in developing economies. A widely shared presumption of these efforts is that expanding access to "productive" credit makes entrepreneurs and small business owners (weakly) better off.

There is less consensus on whether access to *consumer* credit does borrowers more good than harm. Market forces have spurred dramatic growth in subprime nonmortgage consumer credit in the U.S.— there are now more outlets offering small, two-week "payday loans" at 400% APR than McDonald's and Starbucks combined.[1] Revealed preference logic says that this growth should be welfare-improving: a consumer borrows only if she will benefit (weakly, in expectation). Some behavioral models say not necessarily: biases in preferences or cognition may lead consumers to overborrow.[2] These models can motivate restricting access.

Indeed, policymakers often raise concerns about "unproductive" lending at "usurious" rates in subprime markets. Usury laws have existed for millennia.[3] At least 13 states currently have binding restrictions on payday loan terms. New Hampshire and Ohio enacted their restrictions in 2008, and several more states are considering legislation that would restrict access in this $40 billion market. A 36% APR federal interest rate cap on loans to military households took effect in 2007, and presidential candidate Barack

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[1] Payday loans typically extend a few hundred dollars in return for a check post-dated to borrower's next pay date in the amount of the loan principal + a finance charge of at least $15 per $100. See Section II for details on the product and the market.

[2] Behavioral biases may produce borrowing that is excessive relative to a normative (e.g., neoclassical , or long-term self) benchmark. For example: Skiba and Tobacman (2008b) find that payday borrowing patterns are most consistent with partially naïve quasi-hyperbolic discounting. Laibson, Repetto, and Tobacman (forthcoming) find that consumers with present-biased preferences would commit $2,000 to not borrow on credit cards; Ausubel (1991) argues that over-optimism produces excess credit card borrowing. Stango and Zinman (2007; forthcoming) find that consumers tend to underestimate the interest rate on short-term loans and borrow more expensively and heavily as a result. The preceding discussion draws heavily on the Introduction in Karlan and Zinman (2008).

[3] Price ceilings can benefit borrowers and improve efficiency even in the absence of behavioral biases, if insurance markets are incomplete and ceilings do not produce rationing that is too severe (Glaeser and Scheinkman 1998).

Obama seeks to **"Cap Outlandish Interest Rates on Payday Loans"** by extending that cap to all Americans.[4]

A growing empirical literature on the effects of access to expensive credit on borrowers has added fuel to this debate. Several studies find that access to expensive credit exacerbates financial distress (Melzer 2007; Campbell et al. 2008; Carrell and Zinman 2008; Skiba and Tobacman 2008a). These findings suggest that psychological biases lead consumers to do themselves more harm than good when handling expensive liquidity, and hence that restricting access will help consumers by preventing overborrowing. But several other studies suggest otherwise. They find that, on average, access to expensive consumer loans helps borrowers smooth negative shocks (Morse 2007; Wilson et al. 2008), make productive investments in job retention (Karlan and Zinman 2008), or better manage liquidity to alleviate financial distress (Morgan and Strain 2008). These findings suggest that restricting access will harm borrowers by preventing them from financing valuable investment and consumption smoothing opportunities.

I add to this literature by examining the effects of restricting access to expensive consumer credit, using household survey data collected around new binding restrictions imposed by the state of Oregon in 2007 (the "Cap", below).[5] The neighboring state of Washington considered enacting similar restrictions but did not. Before- and after-Cap panel data, on a sample of Oregon and Washington respondents who were payday borrowers before-Cap, allow for difference-in-differences (DD) estimates of the effects of the Cap (and of access to expensive credit more generally) on borrower choices and outcomes.

The data provide two key advantages over comparable studies on the effects of access to subprime credit in the U.S. First, it measures usage of several different types of expensive loan products, permitting analysis of substitution (or complementarity) between payday loans and other liabilities. Second, it permits construction of a summary measure of financial condition based on a combination of an objective measure

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[4] http://www.barackobama.com/issues/economy/. To my knowledge presidential candidate John McCain has not issued a position on payday loans as of this writing.
[5] The data collection was funded by Consumer Credit Research Foundation (CCRF). CCRF is a non-profit organization, funded by payday lenders, with the mission of funding objective research. CCRF did not exercise any editorial control over this paper.

(employment status), and two subjective respondent assessments.[6] Employment status is a useful proxy for (financial) well-being here because unemployment is likely to be involuntary in this sample; subjects are relatively poor and credit constrained, and they have some recent attachment to the workforce (they are all recent payday loan users, and getting a payday loan requires a documented steady job). The subjective assessments help address the issue that financial condition may be difficult to infer from objective choices and outcomes without a complete accounting of the intertemporal optimization problem or strong related assumptions.[7]

Nevertheless several issues complicate the DD estimation. Dissimilarities across treatment (Oregon) and control (Washington) groups in baseline characteristics and attrition motivate matching and weighting estimators (along with the standard simple means comparisons). The short-run follow-up period (5 months), and trend in lender exit from Oregon, motivate attempts to identify Oregon respondents who were most affected (i.e., most likely rationed) by the Cap. Overall the results are robust to various DD estimation strategies.

I find that the Cap dramatically reduced access to payday loans in Oregon, and that former payday borrowers responded by shifting into incomplete and plausibly inferior substitutes. Most substitution seems to occur through checking account overdrafts of various types and/or late bills. These alternative sources of liquidity can be quite costly in both direct terms (overdraft and late fees) and indirect terms (eventual loss of checking account, criminal charges, utility shutoff). Under the broadest measure of liquidity in the data, the likelihood of any expensive short-term borrowing fell by 7 to 9 percentage points in Oregon relative to Washington following the Cap. This jibes with respondent perceptions, elicited in the baseline survey, that close substitutes for payday loans are lacking.

Next I examine the effects of the Cap on the summary measures of financial condition that are available in the data: employment status, and respondents' qualitative

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[6] Karlan and Zinman (2008) find that treatment effects on quantitative and qualitative measures of household well-being are positively correlated; see Section VI for more details. See Kahneman and Krueger (2006) for a more general discussion of subjective well-being measures and their uses.

[7] E.g., to evaluate the optimality of a consumer borrowing decision given the possibility of psychological biases, in principle one would need complete data on that consumer's preferences, expectations, cost perceptions, problem-solving approach, budget and liquidity constraints, and opportunity set.

assessments of recent and future financial situations. Estimates on individual outcomes are noisy but consistent with large declines in financial condition. Estimates on a summary measure of any adverse outcome— being unemployed, experiencing a recent decline in financial condition, or expecting a future decline in financial condition— suggest large and significant deterioration in the financial condition of Oregon respondents relative to their Washington counterparts.[8] As such the results suggest that restricting access harmed Oregon respondents, at least over the short term, by hindering productive investment and/or consumption smoothing.

The paper proceeds with a brief overview of the payday loan market. Section III then details the Oregon policy change and subsequent lender exit. Section IV describes the sample frame and survey data. Section V details my approaches to estimating treatment effects and related threats to identification. Section VI presents the main results: estimates of the five-month impacts of the Cap on credit access, credit use, and financial condition. Section VII discusses how and why longer-run impacts might differ, and presents results using predicted-rationed Oregon respondents as the treatment group, and Washington payday borrowers in the follow-up period as the control group. Section VIII concludes with a brief discussion of directions for future research.

II. The Payday Loan Market

In a standard payday loan contract the lender advances the borrower $100-$300[9] in return for a post-dated check, dated to coincide with the borrower's next paycheck, in the amount of $115-$345. The market rate is about $15 per $100 advanced (390% APR for a 2-week loan), although fees as high as $30 per $100 are not uncommon.[10] Nearly all transactions are face-to-face in retail outlets, although internet lending is growing.[11]

Payday lending has grown explosively in the U.S. since the early 1990s and is now prevalent. The market barely existed in the early 1990s; there are now over 20,000

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[8] The impact studies cited above also find evidence consistent with large treatment-on-the-treated effects.

[9] Stegman (2007) estimates that 80% of payday loans are for $300 or less, and much of the information in this section draws on his overview of the industry. See also Barr (2004) and Caskey (1994; 2005).

[10] See DeYoung and Phillips (2006) for evidence on strategic pricing in the payday loan industry.

[11] Stephens Inc. (2007) estimates that Internet payday lending is growing at 40% annually and comprised 12% of total volume in 2006.

lending outlets (Stephens Inc. 2007).[12] As others have noted, this means that there are now more payday lending outlets in the U.S. than McDonald's and Starbucks combined.[13]

Micro data on payday borrowers are limited, but the available evidence suggests that perhaps 5 to 7 percent of the U.S. population has used a payday loan, with very prevalent serial borrowing (Tanik 2005; Stegman 2007). Many (potential) payday borrowers are served by social programs like Food Stamps and the Earned Income Tax Credit, and annual payday loan volume of $40-$50 billion now exceeds the annual amount transferred by those programs.[14] The potential payday market comprises perhaps 10% of U.S. households (Stephens Inc. 2007). Payday borrowers must have documented steady employment and a checking account. They generally face severe credit constraints, and have poor credit histories and household annual incomes (well) below $50,000 (Section IV presents baseline characteristics for the payday borrowers in my sample).[15]

The closest substitute for a payday loan is arguably overdraft protection on a bank account (Stegman 2007; Morgan and Strain 2008).[16] Other expensive loan products require collateral (pawn, auto title, subprime home equity), a durable purchase (rent-to-own), or are available only once a year (tax refund anticipation).

State laws are an important determinant of access to payday loans. At least 13 states currently have laws that effectively prohibit payday lending with outright bans, or with binding interest rate caps on payday loans or consumer loans more generally.[17] These laws prohibiting or discouraging payday lending are generally well-enforced, if not

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[12] Most payday lenders are non-depository institutions. Many are check-cashers ("multi-line" lenders), but stand-alone ("mono-line") lenders are common as well.

[13] The McDonald's 2007 annual report shows U.S. 13,862 restaurants at year-end 2007. Horovitz (2006) reports that Starbucks had 7,950 U.S. stores during 2006; a graph in the 2006 Starbucks annual report (p. 16) suggests a comparable number.

[14] The fiscal year 2007 costs of the Food Stamp and EITC programs were $33 billion and $38 billion.

[15] None of the studies cited in the Introduction has national data on borrowing or extensive detail on borrower characteristics; the evidence cited above comes from Stegman's review of descriptive studies of payday borrowers. See also Brown and Cushman (2006).

[16] *Bouncing* checks is quite costly due to insufficient funds and returned-check fees, the potential for criminal charges, and negative effects on the credit score (CheckSys) banks use to screen applicants for a deposit account (Campbell et al. 2008). With *overdraft protection* a bank pays overdrawn checks rather than returning them. In exchange the bank often charges the account holder fee of $20 or more; hence in many cases getting a payday loan is cheaper than overdrawing the checking account (particularly if the account holder runs the risk of overdrawing multiple checks).

[17] See http://www.ncsl.org/programs/banking/paydaylend-intro.htm and http://www.ncsl.org/programs/banking/PaydayLend_2008.htm for updates.

always perfectly enforced (King and Parrish 2007), and hence provide a source of substantial variation in availability of payday loans across states and time (see Carrell and Zinman (2008) for more details). In contrast, many states have laws that restrict *serial* payday borrowing and/or lending, but until recently only three states had the means to enforce these restrictions (a central loan database, most critically).

III. The Oregon Policy Change and Lender Exit

The Oregon policy change (the "Cap" hereafter) constrained the set of permissible terms on consumer loans under $50,000. Effective July 1, 2007, the maximum combination of finance charges and fees that can be charged to Oregon borrowers is approximately $10 per $100, with a minimum loan term of 31 days (for a maximum APR of 150%).[18] Ex-ante, these were plausibly binding restrictions on payday lenders that typically charged at least $15 per $100 for two-week loans pre-Cap, since there does not seem to be any compelling evidence that payday lenders make excess profits (Flannery and Samolyk 2005; Skiba and Tobacman 2007). Fixed costs, loan losses, and related risks can account for market rates of 390% APR.

Ex-post, the binding nature of the Cap is evidenced by payday lenders exiting Oregon. Data from the Consumer and Business Services Department, Division of Finance and Corporate Securities (DFCS), indicate that there were 346 licensed outlets on December 31, 2006, 6 months prior to the effective date of the Cap. This count dropped to 105 licensed outlets in February 2008 (7 months after the effective date), and further to 82 licensed outlets by September 2008.

The State of Washington has also considered restricting loan terms in recent years[19] but has ultimately left the relevant laws unchanged as of this writing. Washington still permits $15 per $100 on loan amounts up to $500, with no minimum loan term.

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[18] The Cap appears to be closely enforced by Oregon regulators. The Oregon Division of Finance and Corporate Securities (DFCS) licenses and supervises payday lenders, responding to consumer complaints and conducting routine examinations of licensees at least every two years. The DFCS has taken several enforcement actions against payday lenders in the past; see, e.g., http://www.cbs.state.or.us/dfcs/securities/enf/orders/cf_enforcement_orders_index.html.

[19] See H.B. 1817 in 2008, several bills in 2007, and the 2006 hearing described in http://www.pliwatch.org/news_article_061213B.html . Introduced legislation is tracked and summarized by the National Conference of State Legislatures at http://www.ncsl.org/programs/banking/paydaylend-intro.htm#Bills.

IV. Sample Frame and Descriptive Statistics

I use data from two phone surveys of Oregon and Washington respondents who were payday borrowers prior to the effective date of the Oregon Cap. The data collection was funded by Consumer Credit Research Foundation (CCRF).[20]

A. Sample Frame and Resulting Samples

The baseline ("before" Cap) surveys were conducted between June 22 and July 11, 2007. The sample frame for the surveys was drawn from four major payday lenders and included all borrowers who had obtained loans in the prior three months. The lenders provided names and contact information to a survey firm, which randomly drew 17,940 clients (stratifying by state of residence). The survey firm tried to reach each of these clients by phone to complete a short survey of "opinions and experiences with short-term credit services". Baseline surveys were completed with 6% of the sample frame (7% in Oregon, 5% in Washington), creating a study sample of 1,040 payday borrowers. 873 agreed to be contacted for the follow-up survey, with a small and insignificant difference between Oregon and Washington respondents.

The follow-up ("after") Cap surveys were conducted about five months later, between November 19 and December 2. The survey firm reached 400 of the 873 baseline respondents who agreed to be contacted for the follow-up survey (46%), with 200 respondents each in Oregon and Washington.

B. Sample Characteristics

Table 1 presents descriptive statistics on nearly all of the information collected from the 1,040 respondents to the baseline survey.

Respondents report using their payday loan proceeds for bills, emergencies, food/groceries, and other debt service. Only 6% say "shopping or entertainment". Self-reported outside options appear to be thin; when asked "if a payday loan had not been available… what was your second choice to obtain money?", 70% responded with "none" or "don't know". Only 5% replied that a payday lender in another state or online

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[20] CCRF is a non-profit organization, funded by payday lenders, with the mission of funding objective research. CCRF did not exercise any editorial control over this paper.

would be their second choice, and only 8% stated "bank" or "credit union" (although it is not obvious that respondents would think of checking account overdrafts as a source of liquidity). 15% gave more evident potential substitutes (pawn shop, credit card, or car title loan) as their hypothetical 2nd choice.

In keeping with the prior studies of payday borrowers summarized in Section II, the households in my sample have low-to-moderate income (nearly 50% report total household income between $20,000 and $50,000). 50% of respondents have educational attainment of a high school degree or less. The mean age is about 47, and over 60% of borrowers are female. Fewer than 50% are married, and the mean number of dependents is only slightly above one.[21] The remaining variables in Table 1 are outcomes that might be measurably affected by the contraction of payday credit in Oregon, and I discuss them in Section VI below.

V. Identification

In this section I focus on issues related to identifying short-run average effects of the Oregon Cap on household financial condition. I defer discussion of longer-run and heterogeneous effects until Section VII.

The surveys described in Section IV were designed with a difference-in-differences (DD) strategy in mind for estimating the effects of the Cap on borrowing behavior, employment status, and subjective assessments of financial well-being (I detail each outcome of interest in Section VI below). There are before- and after-Cap data from Oregon (the "treated" state) and Washington (the "control" state), suggesting that one might obtain unbiased estimates of treatment effects by differencing 5-month changes in the outcomes for Oregon respondents from 5-month changes for Washington respondents. Since the treatment varies at the state level, and I have data from only two states, I start by simply calculating differences using the state mean for each variable of interest, allowing the variance to differ across states. A DD estimator will produce unbiased estimates of the Cap's average effects under the assumption of no differential unobserved trends in the outcomes of interest across Oregon and Washington.

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[21] The Oregon/Washington surveys did not inquire about race or homeownership status.

There are some reasons for optimism that the DD identifying assumption will hold in this setting. I could not find any contemporaneous policy changes that might affect the borrowing and financial condition of payday loan borrowers.[22] Oregon and Washington are neighboring states that were on similar economic trajectories at the time of the surveys: both states had experienced 4 consecutive years of employment growth, and both states forecasted a flattening of employment rates for the latter half of 2007 (Oregon Office of Economic Analysis 2007; Washington Economic and Revenue Forecast Council 2007).

Nevertheless Table 1 highlights two potential symptoms of violations of the DD identifying assumption. One symptom is some *observable* dissimilarities between Oregon and Washington respondents in the baseline data; baseline differences in observables may indicate proclivities toward differential unobserved trends in the outcomes. Column 3 shows that the Oregon and Washington respondents differ significantly in reported loan purpose, perceived outside options, education, income, marital history, internet access, employment status, and financial outlook. Another symptom is differential attrition across the two states. Columns 4 and 5 (7 and 8) take the 520 baseline respondents in Oregon (Washington) and report survey variables separately for those who completed a follow-up survey (Columns 4 and 7) and those who attrited (Columns 5 and 8). Column 6 (Column 9) then reports the estimated difference between survivors and attriters for Oregon (Washington). Comparing Columns 6 and 9 suggests that attrition may have been correlated with several outcomes of interest.

I address these potential confounds by constructing weights designed to balance the sample. I attempt to make the Oregon survivors representative of the Oregon baseline sample by predicting survival (*s*) among Oregon respondents using baseline characteristics, and then weighting Oregon respondents by $1/s$ when estimating a DD. This puts more weight on respondents in the follow-up survey who are observably similar to the attriters, and permits valid inference if attrition is not correlated with the treatment

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[22] E.g., the Oregon State Bar's "Highlights of the 2007 Legislative Session… Issues of Importance" mentions land use, anti-discrimination protection for gays and lesbians, identity theft, non-competition agreements, corporate taxes, and the Cap.

and the outcome.[23] I then balance Washington and Oregon respondents by estimating a propensity score p for being an Oregon respondent, using baseline characteristics on all Oregon and surviving Washington respondents, and then weighting surviving Washington respondents by $1/(1-p)$ when estimating a DD. This weight balances the survivor sample on observable characteristics, thereby maximizing the observable similarity between treatment (Oregon) and control (Washington), and hopefully minimizing the likelihood of differential trends.

VI. Main Results: Five-Month Average Treatment Effects of the Oregon Cap

A. Effects on the use of Payday Loans and Substitutes

Table 2 present estimates of the Cap's effects on the use of payday loans and several potential substitutes. For reference, columns 1 and 2 (3 and 4) present baseline and follow-up means for Oregon (Washington) subjects who responded to both surveys. Columns 5-7 present difference-in-differences (DD) estimates of the five-month average treatment effects on borrowing. Column 5 estimates the DD without any adjustment for matching or attrition. Column 6 weights to adjust for attrition and other observable differences (as detailed in Section V), dropping observations in the top percentile of weights in each state to reduce the influence of outliers. Column 7 weights without dropping outliers.

The first row shows that the likelihood of recent payday borrowing in Oregon fell by 26 to 29 percentage points relative to Washington, after the Cap. The unweighted likelihood fell from 1 to 0.79 in Washington, and from 1 to 0.51 in Oregon. Subsequent rows explore the degree to which former payday borrowers in Oregon substituted other sources of credit.

I first look at potential alternative sources of liquidity one-by-one. The use of a specific alternative will rise in Oregon relative to Washington if it is a close-enough substitute; conversely, use of the alternative will fall if it and payday borrowing are complements. There is little evidence of a significant effect on auto title or credit card

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[23] E.g., say the Cap restricts access to payday credit in Oregon and thereby worsens financial condition. If survey response is negatively correlated with financial condition, then declines in financial condition will be underrepresented in the survey, and estimates of the treatment effect will be biased upward (downward in absolute value).

cash advance borrowing. The baseline prevalence of these products is low in both Oregon and Washington (Columns 1 and 3), and the DDs (Columns 5-7) do not find significant increases from baseline to follow-up in Oregon relative Washington. But the DD confidence intervals are large on these and all other outcomes, so that insignificant results are not precise zeros.

The sign pattern for the next three outcomes (has a bank overdraft line of credit, bounced a check, bounced two or more checks in the last three months) suggests the possibility of a shift to checking account overdrafts. But again none of the DDs is significant. Interestingly, many more respondents use paid overdrafts or bounced checks as a source of liquidity than auto title or credit card cash advances (Columns 1-4). The likelihood of any late bill payment in the last three months is very high (75% or greater). This likelihood drops significantly in Oregon relative Washington in the unweighted DD (Column 5), but not in the weighted DDs (Columns 6 and 7). The likelihood of frequent late bill payments ranges from 19 to 30% in the baseline and follow-up samples (Columns 1-4); the DD point estimates here are all negative but none are significant.

The next four rows of Table 2 estimate DDs for increasingly inclusive measures of *any* recent borrowing (from "loans" only, to loans + checking overdrafts + late bills). Given the reduction in payday credit we expect total borrowing to fall unless alternative sources of liquidity are perfect substitutes. The results on any "loans only" in the last three months again suggest that credit card cash advances and auto title loans are poor substitutes for payday loans (the DD for any "loan" is about the same as the DD for payday borrowing alone). This meshes with the results on title loans and cash advances individually, and with the baseline assessments of payday loan alternatives (fewer than 10% of borrowers reported that a title loan or cash advance would be their second choice if they could not get a payday loan).

Checking account overdrafts of various types, and/or late bill payment, seem to be more likely, but imperfect, substitutes for rationed payday credit. The DDs on these more inclusive measures of borrowing are less than half of the DDs on payday borrowing alone. There several reasons why overdrafts and late bills may be imperfect, and inferior, substitutes for payday loans. Overdrafts are often more expensive than payday loans in pure pecuniary terms: fees are often $25-$35 per *transaction* (Campbell et al. 2008).

Repeated overdrafts or bounced checks can lead to the loss of checking account privileges (Campbell et al report 6.4 million involuntary closures nationwide in 2005) and criminal charges (Morgan and Strain 2008). Late bills can also produce substantial costs (late fees, utility shutoffs, reactivation fees, credit score declines).

The last row of Table 2 shows that the proportion of Oregon respondents reporting that it was harder to get a short-term loan recently rose by 17 to 21 percentage points relative to Washington.

So by any measure the survey data show that overall borrowing has fallen substantially in Oregon relative to Washington post-Cap.

The welfare implications of these results are unclear, as they hinge on one's underlying model of consumer choice. In most models with neoclassical (traditionally rational) consumers, the Cap reduces welfare for Oregon households by removing an option for which there is no perfect substitute. In some behavioral models reducing access to payday loans (and thereby to expensive liquidity more generally) may prevent overborrowing; hence the credit reductions we see in Table 2 may benefit Oregon households. The data do not permit direct tests of these competing hypotheses, and I turn to other outcomes for clues.

B. Effects on Employment, and Qualitative Assessments of Financial Condition
Table 3 presents estimates of the Cap's effects on employment status and respondents' subjective assessments of their financial condition. As in Table 2: Columns 1 and 2 (3 and 4) present baseline and follow-up means for Oregon (Washington) subjects who responded to both surveys. Columns 5-7 present difference-in-differences (DD) estimates of the five-month average treatment effects. Column 5 estimates the DD without any adjustment for matching or attrition. Column 6 weights to adjust for attrition and other observable differences (as detailed in Section V), dropping observations in the top percentile of weights in each state to reduce the influence of outliers. Column 7 weights without dropping outliers.

Proponents of payday loans argue that even expensive credit can be quite productive if it enables borrowers to avoid missing work (and thereby avoid losing daily wages or their jobs). The loan purpose self-reports are consistent with this story: 31% of borrowers

report financing emergency needs like auto repair or medical expenses. In Table 3 I look directly at employment status (the survey's measure of income is too coarse to use as an outcome measure).[24] The weighted and unweighted DD point estimates on two measures of unemployment or underemployment are all positive, which is consistent with the hypothesis that reducing payday loan access in Oregon hindered productive investments or consumption smoothing that facilitated job retention (or search). But the estimates here are severely underpowered: given the low baseline prevalence of unemployment (12%) and the sample size, the effects on unemployment would have to be quite large to be statistically significant.

Next I examine respondents' overall assessments of their financial situation in the past six months, and of their prospects for the future. Using subjective summary measures of financial condition is attractive given the difficulty of measuring overall (or even financial) well-being, particularly in short surveys. Karlan and Zinman (2008) find positive treatment effects of expensive credit access for both qualitative and quantitative measures of financial condition.[25] Michigan Surveys of Consumers show robust positive correlations between respondent expectations of their overall financial situation a year from now, and their expectations of income a year from now.[26] So it seems plausible that qualitative assessments are positively correlated with actual financial well-being. Perhaps surprisingly, these measures indicate low *levels* of recent or expected deterioration in financial condition (Columns 1-4). Fewer than 20% say that their situation has been getting worse, and fewer than 10% expect their situation to get worse in the future.

The DD point estimates on these qualitative assessments suggest that the Cap produced declines in financial condition for Oregon respondents relative to their Washington counterparts. The proportion of respondents saying their financial situation had been getting worse in the last 6 months increased by 6 to 8 percentage points in Oregon relative to Washington, but the estimates are very imprecise. The proportion

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[24] Karlan and Zinman (2008) find large positive effects of randomized access to 200% APR consumer loans on job retention and income 6-12 months later, in South Africa.

[25] The quantitative outcomes include job retention and income; and going to bed hungry in the last month. The qualitative outcomes are a "control and outlook" index of self-assessed control over household resources and decisions, optimism, and socio-economic status; and an ordinal measure of changes in food quality over the past year.

[26] Source: author's tabulations from Michigan surveys from 2006 and 2007. Correlations range from 0.18 to 0.25 throughout the income distribution.

saying that they expect their financial situation to get worse in the future increased significantly, by 5 or 6 percentage points, in Oregon relative to Washington.

The last row of Table 3 shows large and significant relative increases in the proportion of Oregon respondents reporting *any* adverse outcome: assessing recent financial situation as getting worse, assessing future financial prospects as worse, *or* being unemployed.[27] E.g., the unweighted proportion increased from 0.28 to 0.35 for Oregon respondents, while declining from 0.31 to 0.26 for Washington respondents, for a DD of 12 percentage points (Column 5). The weighted DDs produce similar estimates (14 and 15 percentage points). These magnitudes imply large treatment-on-the-treated effects of access to expensive credit; in keeping with prior impact studies.

C. Another Outcome: Phone Disconnects

Another outcome that might be of interest is the proportion of phone lines that are disconnected. I do not include this in the summary measure of adverse outcomes because phone disconnects might well be correlated with productive investments (e.g., moves to a better residence, change from landline to cell phone) rather than adverse outcomes like financial distress or eviction.

I estimate a DD for phone disconnects using information from the survey sample *frame* as well as from the survey sample itself. Among the survey sample frame of 17,940 borrowers called for the baseline survey, 18.6% of Oregon lines and 29.3% of Washington lines were disconnected, for difference of -10.7 percentage points (with a standard error of 0.006). The second difference comes from the follow-up survey sample frame. Everyone who completed a baseline survey had a working phone (since it was a phone survey!). Of the 873 borrowers who agreed to be contacted for the follow-up survey, 16.6% of Oregon respondents and 23.8% of Washington respondents had disconnected lines at the time of the follow-up survey, for a difference of -7.3 percentage points (with a standard error of 2.7). The difference between these two differences gives an imprecisely estimated 3.4 percentage point increase (standard error: 2.8) in Oregon disconnects relative to Washington.

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[27] As noted in the Introduction, unemployment is likely to be an adverse (rather than voluntary) condition in this sample, given subjects' strong attachment to the labor force (getting a payday loan requires a documented steady job), low incomes, and credit constraints.

VII. Longer-Run Treatment Effects: Discussion and Exploratory Analysis

The five-month results above suggest that the Oregon Cap reduced the supply of credit for payday borrowers, and that the financial condition of borrowers (as measured by employment status and subjective assessments) suffered as a result.

The longer-run impacts of policy initiatives to restrict credit access might differ from the five-month results for at least two reasons. First, the treatment effects of credit access might have gestation periods. The benefits of productive investments might not be realized for several months or years. The costs of systematically counterproductive loan uses (e.g., negative NPV investments borne of excessive optimism or biased underestimation of borrowing costs, or time-inconsistent consumption splurges) might also take time to materialize, particularly if they compound through the channel of serial expensive borrowing and debt traps. The best way to address this issue is to collect outcomes data over longer horizons.[28]

A second issue is that short-run measures may capture transitional rather than equilibrium outcomes. Borrowers may need time to adjust to the new regime (e.g., to find substitutes that blunt the effects of restricted payday loan access). Lenders may also take time to adjust their supply response. This has been the case in Oregon; as documented in Section III, lenders exited after the effective date of the Cap, but payday credit has not completely dried up. Recall that 50% of Oregon respondents had a payday loan in the follow-up survey. And per the new regulation, these Oregon borrowers were using a product that was cheaper (150% APR) and longer-term (minimum 31 days) than that used by their Washington counterparts. So short-term credit access may have actually *improved* for some Oregon borrowers. The challenge for interpreting the average treatment effects is that these borrowers are pooled with "already-rationed" former borrowers who can not get a payday loan as a result of the Cap.[29] The effects on already-rationed respondents may provide a better indication of longer-term impacts, particularly if payday lenders continue to exit.

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[28] Administrative data may complement survey data here; e.g., Karlan and Zinman (2008) examine treatment effects on credit scores one and two years after treatment (to complement the short-run survey outcomes) and find that increased access increased the likelihood of having a score, and had no effect on the score conditional on having one.

[29] Strictly speaking the Oregon borrowers in the follow-up survey may be rationed as well, on the intensive margin and/or on the extensive margin (given that the survey looks back over the prior three months).

I estimate effects on already-rationed respondents by defining new treatment and control groups. I set the treatment group by predicting would-be Oregon payday borrowers in the follow-up period (i.e., respondents who would have gotten payday loans in the absence of the Cap),[30] and flagging those who did not actually get a loan. There are 75 such predicted already-rationed borrowers. I then estimate DDs for this treatment group using the 157 Washington respondents who were payday borrowers in the follow-up period as the control group.

Table 4 Panel A shows DD estimates on the summary borrowing outcomes for the already-rationed. Column 5 uses the simple means comparisons, and Columns 6 and 7 weight to adjust for differential attrition and baseline characteristics across treatment and control. As expected, the declines in overall borrowing in Oregon relative to Washington are larger here, among the predicted already-rationed, than in the full sample (compare to Table 2). Panel B shows DDs on employment status and the qualitative assessments of financial condition. The results are qualitatively similar to the full sample (compare to Table 3) but not precise enough to identify anything but very large differences in effect sizes.

VIII. Conclusion

I examine some effects of restricting access to expensive consumer credit on payday loan users, using household survey data collected around the imposition of binding restrictions on loan terms in Oregon but not in Washington. The results suggest that the policy change decreased expensive short-term borrowing in Oregon relative to Washington, with many Oregon payday borrowers shifting into plausibly inferior substitutes. Oregon respondents were also significantly more likely to experience an adverse change in financial condition (where an adverse outcome is defined as being unemployed, or having a negative subjective assessment about one's overall recent or future financial situation). The results suggest that restricting access to consumer

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[30] Specifically, I estimate the likelihood of payday borrowing for Washington respondents in the follow-up survey using baseline characteristics. Then I use the coefficients to predict counterfactual (i.e., in the absence of the Cap) payday borrowing for Oregon respondents in the follow-up survey, using their baseline characteristics. I define Oregon respondents with a predicted probability of > 0.5 as the would-be borrowers. This produces a would-be borrowing rate of 78% in Oregon, as compared to the actual borrowing rate of 79% in Washington.

credit hinders productive investment and/or consumption smoothing, at least over the short term.

Much work remains to address the questions of whether access to expensive credit improves (consumer) welfare, and why.

The likelihood of additional policy changes at the state (and possibly federal) level seems high, suggesting that difference-in-differences (DD) approaches like the one used in this paper will continue to be useful. Future studies would benefit from larger sample sizes and a richer set of proxies for consumer welfare and financial condition. Viable examples of proxies to collect from household surveys include postponed medical care and forced moves as used by Melzer (2007), shutoffs of heat or other utilities, dunning as used by Morgan and Strain (2008), and hunger and subjective well-being as used by Karlan and Zinman (2008). Future studies would also do well to track outcomes of interest over longer horizons, since the costs and benefits of investment and consumption smoothing activities may have gestation periods, or compound over time.

Finally, it is critical to begin reconciling findings across different studies. Are the differences due to methodology, market context, and/or borrower heterogeneity? Field experiments randomized at the individual level would help, by providing clean variation in credit access and more statistical power than state-level natural experiments. Additional data collection on a richer set of outside options (for borrowing and economic activity) and decision inputs (for intertemporal choice models) would help address whether heterogeneity across consumers and markets drives the results.

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Table 1. Sample Composition and Attrition: Means for Baseline Survey Responses

Respondent's state of residence:	OR	WA	OR-WA difference	OR			WA		
Reached for follow-up survey?	all	all	all	in	not in	difference	in	not in	difference
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
any payday loan last 3 months	1.000	1.000	0.000	1.000	1.000	0.000	1.000	1.000	0.000
			0.000			*0.000*			*0.000*
agreed to be contacted for follow-up survey	0.848	0.831	0.017	1.000	0.753	0.247***	1.000	0.725	0.275***
			0.023			*0.024*			*0.025*
loan purpose: "regular bills like utilities, phone"	0.358	0.352	0.006	0.368	0.352	0.016	0.345	0.356	-0.011
			0.030			*0.044*			*0.044*
loan purpose: "emergency need: car, medical, etc."	0.304	0.322	-0.018	0.290	0.313	-0.022	0.299	0.337	-0.038
			0.029			*0.042*			*0.043*
loan purpose: "food/groceries"	0.189	0.138	0.051**	0.214	0.174	0.039	0.149	0.131	0.019
			0.023			*0.036*			*0.032*
loan purpose: "pay credit card or other loan bills" or "mortgage/rent payment"	0.085	0.124	-0.039**	0.129	0.121	0.008	0.073	0.092	-0.020
			0.019			*0.030*			*0.025*
loan purpose: "shopping or entertainment"	0.056	0.056	0.000	0.052	0.059	0.007	0.077	0.042	0.035
			0.015			*0.021*			*0.022*
other option if no payday loan: none	0.487	0.443	0.045	0.487	0.489	-0.002	0.449	0.438	0.011
			0.031			*0.046*			*0.045*
other option if no payday loan: not sure	0.215	0.285	-0.070***	0.213	0.217	-0.004	0.263	0.300	-0.037
			0.027			*0.037*			*0.041*
other option if no payday loan: pawn	0.067	0.080	-0.013	0.046	0.080	-0.034	0.101	0.066	0.035
			0.016			*0.023*			*0.025*
reason payday loan vs. another source: "fast approval"	0.339	0.367	-0.028	0.323	0.350	-0.027	0.353	0.376	-0.023
			0.031			*0.044*			*0.045*
reason payday loan: "more convenient location"	0.229	0.222	0.007	0.250	0.217	0.033	0.230	0.217	0.013
			0.027			*0.039*			*0.039*
reason payday loan: "cheaper"	0.156	0.172	-0.016	0.141	0.167	-0.026	0.209	0.148	0.060*
			0.024			*0.034*			*0.035*
highest education: no high school	0.104	0.083	0.021	0.109	0.095	0.014	0.090	0.078	0.012
			0.018			*0.028*			*0.025*
highest education: high school	0.440	0.385	0.055*	0.457	0.431	0.026	0.340	0.415	-0.075*
			0.031			*0.045*			*0.044*
highest education: some college	0.305	0.289	0.017	0.302	0.309	-0.007	0.310	0.275	0.035
			0.029			*0.042*			*0.041*
highest education: college+	0.151	0.243	-0.092***	0.146	0.151	-0.005	0.260	0.232	0.028
			0.025			*0.032*			*0.039*
income < $20,000	0.340	0.274	0.066**	0.370	0.320	0.051	0.281	0.269	0.012
			0.030			*0.045*			*0.042*
income $20,000-$50,000	0.512	0.459	0.053	0.492	0.526	-0.034	0.454	0.462	-0.008
			0.033			*0.047*			*0.047*
income > $50,000	0.148	0.267	-0.119***	0.138	0.155	-0.017	0.265	0.269	-0.004
			0.026			*0.034*			*0.042*
age	46.872	45.931	0.941	48.827	45.579	3.248**	47.209	45.097	2.211*
			0.913			*1.354*			*1.289*
female	0.623	0.608	0.015	0.638	0.613	0.026	0.630	0.594	0.036
			0.030			*0.044*			*0.044*
married	0.479	0.479	0.000	0.459	0.494	-0.035	0.482	0.477	0.005
			0.032			*0.046*			*0.046*
never married	0.165	0.219	-0.054**	0.144	0.179	-0.035	0.193	0.235	-0.042
			0.025			*0.033*			*0.037*
dependents	1.135	1.106	0.029	1.085	1.169	-0.083	1.135	1.088	0.048
			0.093			*0.139*			*0.132*
internet access	0.645	0.722	-0.077***	0.653	0.638	0.015	0.774	0.689	0.085**
			0.029			*0.043*			*0.040*
harder get short-term loan last 3 months	0.165	0.059	0.106***	0.158	0.170	-0.012	0.052	0.064	-0.012
			0.020			*0.035*			*0.022*
any auto title loan in last 3 months	0.115	0.083	0.033*	0.085	0.134	-0.049*	0.085	0.081	0.004
			0.019			*0.027*			*0.025*
any credit card cash advance in last 3 months	0.152	0.157	-0.005	0.180	0.135	0.045	0.176	0.146	0.030
			0.023			*0.033*			*0.034*
has overdraft line of credit or bounce protection	0.506	0.457	0.049	0.543	0.484	0.059	0.428	0.475	-0.047
			0.032			*0.046*			*0.046*
bounced a check in last 3 months	0.524	0.489	0.035	0.533	0.519	0.014	0.472	0.500	-0.028
			0.031			*0.045*			*0.046*
bounced 2 or more checks in last 3 months	0.299	0.299	0.000	0.289	0.306	-0.016	0.254	0.328	-0.074*
			0.029			*0.042*			*0.041*
any late bill in last 3 months	0.827	0.804	0.023	0.859	0.807	0.052	0.759	0.833	-0.074**
			0.024			*0.033*			*0.037*
frequently paid bills late in last 3 months	0.280	0.241	0.039	0.293	0.272	0.021	0.226	0.251	-0.025
			0.027			*0.041*			*0.039*
unemployed	0.125	0.116	0.009	0.125	0.125	0.000	0.131	0.106	0.026
			0.020			*0.030*			*0.029*
not working	0.338	0.278	0.060**	0.380	0.313	0.068	0.288	0.272	0.015
			0.029			*0.043*			*0.041*
unemployed or part-time work	0.219	0.182	0.037	0.235	0.209	0.026	0.222	0.157	0.065*
			0.025			*0.038*			*0.036*
retired	0.204	0.151	0.053**	0.235	0.184	0.051	0.152	0.151	0.001
			0.024			*0.037*			*0.033*
financial situation gotten worse last 6 months	0.186	0.192	-0.006	0.171	0.196	-0.025	0.181	0.199	-0.018
			0.024			*0.035*			*0.036*
expect financial situation to get worse in future	0.045	0.031	0.014	0.046	0.045	0.001	0.061	0.013	0.048***
			0.012			*0.019*			*0.018*
financial situation gotten worse, or expect to get worse	0.209	0.209	0.000	0.196	0.217	-0.021	0.219	0.202	0.017
			0.025			*0.037*			*0.037*
expect financial situation to get better in future	0.768	0.833	-0.065***	0.716	0.801	-0.086**	0.821	0.840	-0.018
			0.025			*0.039*			*0.034*
phone disconnected	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
			0.000			*0.000*			*0.000*
number of observations	520	520	1040	200	320		200	320	

Baseline (June/July 2007) survey responses only.
Cells report proportions or means, with standard error on difference in italics. Standard errors allow variance to differ across state or survey wave.
Observation counts for some variables are lower than reported in the bottom row, due to nonresponse.

Table 2. Borrowing: Analysis Sample Means and Estimates of Average Five-Month Treatment Effects

| | | Oregon | | Washington | | Difference-in-Differences | | |
| | | baseline | follow-up | baseline | follow-up | Unweighted | Weighted | |
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
any payday loan last 3 months		1	0.505	1	0.789	-0.284***	-0.292***	-0.256***
		(0)	(0.035)	(0)	(.029)	(0.046)	(0.051)	(0.064)
	N	200	200	199	199	399	395	399
any auto title loan in last 3 months		0.085	0.075	0.085	0.075	0.000	-0.042	-0.022
		(0.020)	(0.019)	(0.020)	(0.019)	(0.036)	(0.045)	(0.046)
	N	200	200	199	199	399	395	399
any credit card cash advance in last 3 months		0.180	0.130	0.176	0.146	-0.020	0.015	0.014
		(0.027)	(0.024)	(0.027)	(0.025)	(0.048)	(0.052)	(0.049)
	N	200	200	199	199	399	395	399
has bank overdraft line of credit or bounce protection		0.541	0.519	0.432	0.400	0.011	0.063	0.059
		(0.037)	(0.037)	(0.037)	(0.036)	(0.044)	(0.050)	(0.048)
	N	183	183	185	185	368	364	368
bounced a check, overdrafted, or insufficient funds in last 3 months		0.533	0.498	0.474	0.428	0.010	0.017	0.014
		(0.036)	(0.036)	(0.036)	(0.036)	(0.053)	(0.060)	(0.057)
	N	195	195	194	194	389	385	389
bounced or overdrafted twice or more in last 3 months		0.287	0.313	0.258	0.247	0.036	0.033	-0.021
		(0.032)	(0.033)	(0.031)	(0.031)	(0.049)	(0.061)	(0.077)
	N	195	195	194	194	389	385	389
any late bill in last 3 months		0.857	0.745	0.756	0.761	-0.117**	-0.028	-0.045
		(0.025)	(0.031)	(0.031)	(0.030)	(0.052)	(0.057)	(0.057)
	N	196	196	197	197	393	390	393
frequently paid bills late in last 3 months		0.296	0.224	0.218	0.193	-0.046	-0.053	-0.036
		(0.033)	(0.030)	(0.030)	(0.028)	(0.048)	(0.064)	(0.063)
	N	196	196	197	197	393	390	393
used any short-term credit, "loans" only in last 3 months		1	0.570	1	0.830	-0.260***	-0.267***	-0.281***
		(0)	(0.035)	(0)	(0.027)	(0.044)	(0.047)	(0.047)
	N	200	200	200	200	400	396	400
used any short-term credit, including bounced checks/ overdrafts/insufficient funds, in last 3 months		1	0.755	1	0.880	-0.125***	-0.110***	-0.111***
		(0)	(0.030)	(0)	(0.023)	(0.038)	(0.040)	(0.039)
	N	200	200	200	200	400	396	400
used any short-term credit, including late bills, in last 3 months		1	0.835	1	0.950	-0.115***	-0.090***	-0.105***
		(0)	(0.026)	(0)	(0.015)	(0.031)	(0.029)	(0.031)
	N	200	200	200	200	400	396	400
used any short-term credit, including bounced checks, etc. and late bills, in last 3 months		1	0.870	1	0.960	-0.090***	-0.071***	-0.070***
		(0)	(0.024)	(0)	(0.014)	(0.028)	(0.026)	(0.025)
	N	200	200	200	200	400	396	400
harder get short-term loan last 3 months		0.158	0.388	0.045	0.090	0.185***	0.173**	0.207***
		(0.030)	(0.040)	(0.016)	(0.021)	(0.056)	(0.068)	(0.069)
	N	152	152	178	178	330	326	330

* p<0.10, ** p<0.05, *** p<0.01.

Standard errors, in parentheses, allow variance to differ across state.

Sample for each outcome includes only those who responded to the question in both rounds of the survey.

Columns 1-5 do not attempt to correct for attrition or for dissimilarity across OR and WA.

Columns 6 and 7 weight to correct for attrition and dissimilarity (see Section V of text for details), and Column 6 drops observations in the top 1 percentile of weights.

Table 3. Other Indicators of Financial Condition: Analysis Sample Means and Estimates of Average Five-Month Treatment Effects

		Oregon		Washington		Difference-in-Differences		
		baseline	follow-up	baseline	follow-up	Unweighted	Weighted	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
unemployed		0.121	0.151	0.131	0.131	0.030	0.036	0.035
		(0.023)	(0.025)	(0.024)	(0.024)	(0.038)	(0.043)	(0.041)
	N	199	199	198	198	397	393	397
unemployed, or part-time work		0.231	0.256	0.222	0.182	0.066	0.059	0.057
		(0.030)	(0.031)	(0.030)	(0.027)	(0.041)	(0.048)	(0.045)
	N	199	199	198	198	397	393	397
"...describe your financial situation in last 6 months:" getting worse		0.172	0.207	0.181	0.156	0.060	0.080	0.073
		(0.027)	(0.029)	(0.027)	(0.026)	(0.047)	(0.061)	(0.062)
	N	198	198	199	199	397	393	397
"Thinking about the future, do you expect your financial situation to:" get worse		0.046	0.066	0.061	0.036	0.046*	0.058**	0.055**
		(0.015)	(0.018)	(0.017)	(0.013)	(0.027)	(0.029)	(0.028)
	N	196	196	196	196	392	388	392
any adverse: unemployed, financial situation worse last 6 months, or expect worse in the future		0.279	0.345	0.313	0.262	0.117**	0.151**	0.141**
		(0.032)	(0.034)	(0.033)	(0.032)	(0.055)	(0.064)	(0.065)
	N	197	197	195	195	392	388	392

* $p<0.10$, ** $p<0.05$, *** $p<0.01$.
Standard errors, in parentheses, allow variance to differ across state.
Sample for each outcome includes only those who responded to the question in both rounds of the survey.
Columns 1-5 do not attempt to correct for attrition or for dissimilarity across OR and WA.
Columns 6 and 7 weight to correct for attrition and dissimilarity (see Section V of text for details), and Column 6 drops observations in the top 1 percentile of weights.

Table 4. Predicted-Rationed in Oregon vs. Washington Borrowers

| | | Oregon | | Washington | | Difference-in-Differences | | |
| | | baseline | follow-up | baseline | follow-up | Unweighted | Weighted | |
Panel A. Borrowing		(1)	(2)	(3)	(4)	(5)	(6)	(7)
used any short-term credit, "loans" only		1	0.132	1	1	-0.867***	-0.861***	-0.866***
in last 3 months		(0)	(0.039)	(0)	(0)	(0.039)	(0.044)	(0.043)
	N	75	75	157	157	232	229	232
used any short-term credit, including bounced checks/		1	0.474	1	1	-0.520***	-0.481***	-0.466***
overdrafts/insufficient funds, in last 3 months		(0)	(0.058)	(0)	(0)	(0.058)	(0.062)	(0.062)
	N	75	75	157	157	232	229	232
used any short-term credit, including late bills,		1	0.658	1	1	-0.333***	-0.299***	-0.289***
in last 3 months		(0)	(0.055)	(0)	(0)	(0.055)	(0.054)	(0.053)
	N	75	75	157	157	232	229	232
used any short-term credit, including bounced checks,		1	0.711	1	1	-0.280***	-0.243***	-0.235***
etc. and late bills, in last 3 months		(0)	(0.052)	(0)	(0)	(0.052)	(0.050)	(0.048)
	N	75	75	157	157	232	229	232
Panel B. Other Indicators of Financial Condition								
unemployed		0.133	0.147	0.129	0.129	0.014	0.004	0.004
		(0.040)	(0.041)	(0.027)	(0.027)	(0.054)	(0.058)	(0.056)
	N	75	75	155	155	229	226	229
unemployed, or part-time work		0.187	0.187	0.206	0.174	0.032	0.027	0.000
		(0.045)	(0.045)	(0.033)	(0.031)	(0.054)	(0.061)	(0.063)
	N	75	75	155	155	229	226	229
"...describe your financial situation in last 6 months":		0.133	0.227	0.160	0.154	0.101	0.113	0.110
getting worse		(0.040)	(0.049)	(0.029)	(0.029)	(0.065)	(0.086)	(0.082)
	N	75	75	156	156	230	227	230
"Thinking about the future, do you expect your financial		0.040	0.067	0.058	0.045	0.040	0.036	0.034
situation to:" get worse		(0.023)	(0.029)	(0.019)	(0.017)	(0.043)	(0.042)	(0.041)
	N	75	75	156	156	230	227	230
any adverse: unemployed, financial situation worse		0.257	0.365	0.299	0.247	0.162**	0.169*	0.162*
last 6 months, or expect worse in the future		(0.051)	(0.056)	(0.037)	(0.035)	(0.081)	(0.093)	(0.089)
	N	74	74	154	154	227	224	227

* $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Standard errors, in parentheses, allow variance to differ across state.

Sample for each outcome includes only those who responded to the question in both rounds of the survey.

Columns 1-5 do not attempt to correct for attrition or for dissimilarity across OR and WA.

Columns 6 and 7 weight to correct for attrition and dissimilarity (see Section V of text for details), and Column 6 drops observations in the top 1 percentile of weights.

Oregon rationed households are those who are *predicted* to have a payday loan in the follow-up (based on baseline survey characteristics) but who do *not* actually have one (see Section V of text for details). There are 75 rationed Oregon households in the follow-up survey, and 157 Washington borrowers.