



Restricting consumer credit access: Household survey evidence on effects around the Oregon rate cap

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ABSTRACT

Many policymakers and some theories hold that restricting access to expensive credit helps consumers by preventing overborrowing. I examine some effects of restricting access, using household panel survey data on payday loan users collected around the introduction of binding restrictions on payday loan terms in Oregon. Borrowing fell in Oregon relative to Washington, with former payday borrowers shifting partially into plausibly inferior substitutes: bank overdrafts and late bill payment. Additional evidence suggests that restricting access caused deterioration in the overall financial condition of Oregon households. Overall the results are consistent with restricted access harming, not helping, consumers on average.

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1. 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 US; as others have noted, there are now more outlets offering small, two-week “payday loans” at 400% APR than McDonald’s and Starbucks outlets combined.¹ Revealed preference logic says

that this growth should be welfare-improving: a consumer borrows only if she will benefit (weakly, in expectation). In contrast a growing body of work on psychological biases in household finance suggests that many consumers overborrow relative to an unbiased benchmark.² This work 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

² One psychological bias that can produce overborrowing is present-biased (time-inconsistent) preferences; see, e.g., Laibson’s (1997) model and contrast to the neoclassical (exponential discounting) case where revealed preference reveals the consumer’s welfare-maximizing choice. Empirically, Skiba and Tobacman (2008b) find that payday borrowing patterns are most consistent with partially naïve quasi-hyperbolic discounting, and Laibson et al. (forthcoming) find that consumers with present-biased preferences would commit \$2000 to not borrow on credit cards. Other explanations for overborrowing include biased expectations (see, e.g., Ausubel (1991) on over-optimism producing excess credit card borrowing), and exponential growth bias that produces underestimation of borrowing costs (Stango and Zinman, 2009, forthcoming).

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 credit rationing that is too severe (Glaeser and Scheinkman, 1998).

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¹ Payday loans typically extend a few hundred dollars in return for a check post-dated to the borrower’s next pay date in the amount of the loan principal + a finance charge of at least \$15 per \$100. See Section 2 for details on the product and the market.

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 President Barack 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 (Campbell et al., 2008; Carrell and Zinman, 2008; Skiba and Tobacman, 2008a; Melzer, 2009). 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 make productive investments, broadly defined: smoothing negative expenditure shocks (Wilson et al., 2008; Morse, 2009), preventing negative income shocks (Karlan and Zinman, forthcoming), or otherwise managing liquidity to alleviate financial distress (Morgan and Strain, 2008).⁵ These findings suggest that restricting access will harm borrowers by preventing them from financing valuable consumption smoothing and investment opportunities (e.g., in job retention).⁶

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).⁷ 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 US. 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 (employment status), and two subjective respondent assessments of their financial condition over the last 6 months, and of their expected trend for the future.⁸ Employ-

ment 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.⁹

The data and methodology have some disadvantages as well. Although the sample does seem to be representative of payday borrowers in most respects, my sample is considerably older than average, raising a question of external validity. With regards to internal validity, 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, however, 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 (as in Morgan and Strain, 2008). 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 7–9 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 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.¹⁰ As such the results suggest that restricting access harmed Oregon respondents, at least over the short-term, by hindering productive consumption smoothing and/or investment (e.g., in job retention).

The paper proceeds with a brief overview of the payday loan market. Section 3 then details the Oregon policy change and subsequent lender exit. Section 4 describes the sample frame and survey data. Section 5 details my approaches to estimating treatment effects and related threats to identification. Section 6 presents the main results: estimates of the five-month impacts of the Cap on credit access, credit use, and financial condition. Section 7 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

⁴ <http://www.barackobama.com/issues/economy/>.

⁵ Other related studies in developing country settings focus on the effects of access to “productive” credit (targeted to microentrepreneurs) rather than consumer credit; see, e.g., Coleman (1999), Kaboski and Townsend (2005), McKernan (2002), Pitt et al. (2003), and Pitt and Khandker (1998). There may be little economic distinction between small, closely-held businesses and the households that run them, and there is evidence that microentrepreneurial loans are often used for income smoothing or household investment rather than business investment (Morduch 1998; Menon, 2003; Karlan and Zinman, 2009). See also Burgess and Pande (2005) and Burgess et al. (2005), which find that state-led bank branch expansion increased lending to the poor and decreased rural poverty in India.

⁶ Karlan and Zinman (forthcoming) find that access to a four-month loan at 200% APR significantly increased the likelihood that a borrower was employed 6–12 months after taking a loan. The mechanism seems to be that many borrowers use the loans to smooth shocks (to household health, or especially to transportation) that would otherwise lead to absences from work and eventual firing.

⁷ 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.

⁸ The survey questions are: “In general, how would you describe your financial situation in the last 6 months” [getting better/getting worse/about the same], and “Thinking about the future, do you expect your financial situation to:” [get better/get worse/stay the same]. Karlan and Zinman (forthcoming) find that treatment effects on quantitative and qualitative measures of household well-being are positively correlated; see Section 6 for details. See Kahneman and Krueger (2006) for a more general discussion of subjective well-being measures and their uses.

⁹ 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.

¹⁰ The impact studies cited above also find evidence consistent with large treatment-on-the-treated effects.

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