Finance and Economics Discussion Series

Federal Reserve Board, Washington, D.C.
ISSN 1936-2854 (Print)
ISSN 2767-3898 (Online)

The Effect of Liquidity Constraints on Labor Supply: Evidence from Interest Rate Ceilings

Kabir Dasgupta, Brenden J. Mason

2025-110

Please cite this paper as:
Dasgupta, Kabir, and Brenden J. Mason (2025). "The Effect of Liquidity Constraints on Labor Supply: Evidence from Interest Rate Ceilings," Finance and Economics Discussion Series 2025-110. Washington: Board of Governors of the Federal Reserve System, https://doi.org/10.17016/FEDS.2025.110.

The Effect of Liquidity Constraints on Labor Supply: Evidence from Interest Rate Ceilings

Kabir Dasgupta* and Brenden J. Mason†‡

December 9, 2025

Abstract

We exploit the spatiotemporal variation in US states' interest rate ceilings on small-dollar loans to identify the effect of liquidity constraints on labor supply. Exogenously-capped interest rates lead to consumers being shut out of the market for cash loans. In response, labor supply increases by approximately 0.4 hours per week. We also find that the propensity to take personal leaves decreases. Labor supply, therefore, is used to overcome financial constraints, but is not the only method: the effect on earnings is less than many small-dollar loans, suggesting that borrowers employ multiple mechanisms to cope with tightened credit conditions.

JEL Classification: D15, G5, G23, J22

Keywords: Liquidity Constraints; Labor Supply; Usury; Payday Lending; Credit Rationing; Consumption Smoothing

---

*Senior Economist, Consumer and Community Affairs, Federal Reserve Board, Washington, DC, United States

†Associate Professor of Economics, North Central College, Naperville, IL, United States

‡Correspondence: Department of Economics and Finance, North Central College, 30 N. Brainard St, Naperville, IL, 60540, United States. E-mail address: bjmason@noctrl.edu

This research was conducted with restricted access to Bureau of Labor Statistics (BLS) data. The results and views expressed in this study are those of the authors and do not reflect the views of the BLS, the Federal Reserve Board, or the Federal Reserve System.

Acknowledgements: We would like to thank Jeff Larrimore, Alexander Plum, and participants at the 2023 Midwest Economics Association Conference and the 2023 Western Economic Association International Conference for helpful comments, discussion, and suggestions. All errors are our own. We do not have any conflicts of interest.

Section 1: Introduction and Overview

How do consumers cope with negative shocks to their income or expenditures? Neoclassical economic theory makes a clear prediction: consumers will draw down a buffer stock of savings. If the shock is novel or if, for whatever reason, there are no savings, then consumers will borrow, spreading the cost of debt over time. But what if consumers face liquidity constraints and are therefore unable to borrow? One potential mechanism available is increasing labor effort: an employed consumer could work more hours; workers who are 'hours constrained' may search for an additional job.

In this paper we empirically test whether workers increase their labor supply in the face of a liquidity constraint. Credibly establishing a causal relationship between labor supply and liquidity constraints is difficult on account of the endogeneity. An adverse macroeconomic shock could affect labor demand and, simultaneously, credit supplied by financial intermediaries. Moreover, obtaining credit is often a function of labor supply, e.g., as part of a screening mechanism to overcome asymmetric information.

We identify the causal effect of liquidity constraints on labor supply by exploiting the spatiotemporal variation in US states' interest rate ceilings, which typically apply to small-dollar (cash) loans. In particular, our research design is a quasi-experiment that compares two groups of workers within a state that has a prohibitively-low interest rate ceiling: those who live close to a neighboring state that has no such cap on interest rates and those who live close to a different neighboring state that initially has no such cap, but changes policy during the sample period.

As a stylized example, residents of Massachusetts who live close to Rhode Island, where small-dollar cash credit, e.g., payday lending, is legal, can be assumed to have easier access to payday loans. On the other hand, Massachusetts residents who live close to New Hampshire experienced a break in their access to payday loans. This is because in New Hampshire, small-dollar cash credit was legally available up to and including 2008. But in 2009 high-interest small-dollar lending became severely restricted in the state through the imposition and enforcement of an interest rate ceiling of 36% annual percentage rate (APR). Comparing labor market outcomes of the two groups of Massachusetts residents before- and after New Hampshire's policy change could generate the effect of small-dollar credit access on labor supply. Moreover, this design generates plausibly exogenous variation in liquidity constraints because it is the neighboring state that is generating the variation. To the best of our current knowledge, this identification strategy was first used by Melzer (2011) to estimate the real costs, e.g., difficulty paying bills, of access to payday loans. Interest rate ceilings always apply to payday loans, but they often affect the alternative financial services (AFS) industry more broadly, e.g., auto title loans.

Using self-reported monthly measures of labor supply from the county-level Current Population Survey (CPS) across the years 2002 to 2019, we find that 'hours worked' statistically increases by approximately 0.4 hours per week for workers aged between 25 and 64—0.55 hours per week for those with less than a bachelor's degree. The latter group are the workers who are most likely to use small-dollar cash loans. We find that the propensity to take sick/personal days decreases—a kind of increase in labor supply, but we find little evidence on the propensity to hold a second job. Regarding earnings, we find a short-term increase, which seems to come, in part, from a decrease in the propensity for a worker to remain with their same employer from a year ago, implying that some workers have switched jobs. Notably, our confidence interval on earnings rules out the size of a typical payday loan, lending credence to the notion that workers diversify their coping mechanisms in the face of an adverse shock to the liquidity constraint.

We corroborate the findings above by performing several robustness checks. We also run our analyses on annual 'hours worked' data from the American Community Survey (ACS). Our results are qualitatively, but not statistically, confirmed. Moreover, to get a continuous measure of the effect of the interest rate ceiling (rather than a binary dummy), we use the number of payday lenders locations from the US Census Bureau's County Business Patterns (CBP) data; the code is NAICS 522390. But this includes related subindustries as well. Hence, we instrument this number of establishments by the binary dummy for a prohibitively-low interest rate ceiling. The results corroborate the main CPS findings mentioned above.

We run a few placebo tests using our CPS data as well. Our findings are notably smaller as we increase the distance: our findings do not hold as strongly when we change the definition of 'live close to' from 25 miles to 40 miles. We also find no results for salaried workers since they are paid the same salary regardless of how many hours they work.

As a final set of analyses, we bring in two additional datasets and employ a slightly different identification strategy. Our CPS analysis uses county-level distance (and instrumented sellers), but all of our variation is generated by a few states on the East Coast—Pennsylvania, Washington DC, and New Hampshire. Furthermore, the CPS (and ACS) data are not panel data; respondents are not tracked across time, thereby precluding the possibility of controlling for individual-level fixed effects. To broaden the scope, therefore, we run a standard difference-in-differences (DID) regression on self-reported measures of labor supply using the National Longitudinal Survey of Youth (1997 cohort; NLSY97, henceforth). The NLSY97 has geographic indicators as well as self-reported measures labor supply.[1] With this NLSY97 data, we use the spatiotemporal variation in payday loan usage across all states. Hence, the variation comes from the domestic state, i.e., comparing New Hampshire residents who used payday loans to fellow New Hampshire residents who did not, before- and after the 2009 policy change relative to residents of states who were not exposed to a restrictive interest rate ceiling. Our results above largely hold: hours worked increases, but propensity to hold a second job is unchanged.

Finally, it has been well noted within the labor economics literature that self-reported measures of 'hours worked' can suffer from measurement error. To accommodate this critique, we run a DID analysis using monthly hours worked as reported at the state level by firms. The data come from the Current Employment Statistics (CES), the "establishment survey" conducted by the Bureau of Labor Statistics (BLS). We find slight evidence for an an increase in hours worked among the goods-producing supersector, but a decrease in the leisure and hospitality sector.

These findings build upon the previous literature within the nexus of borrowing constraints and labor supply, of which there are not many papers. Pijoan-Mas (2006) and Athreya (2008) estimate structural models of labor supply when a borrowing constraint is relaxed. Hours worked falls in their models. Empirically, Rossi and Trucchi (2016) examine labor supply data from credit-constrained Italian workers who suddenly receive an inheritance. These workers, on average, decrease their labor supply. The effect is especially strong on self-employed workers. Dao Bui and Ume (2020) use spatiotemporal intrastate bank branching deregulation to estimate the effect of exogenous credit availability on labor supply. They, too, find that hours of work fall. Nawaz, Koirala, and Butt (2025) analyze the 2022 wave of the Survey of Consumer Finances (SCF) to understand the links between credit and labor market outcomes. They instrument credit constraints with distance to a financial institution to identify the effect on labor supply. The authors find that credit-constrained individuals work more hours. Our findings are consistent with these previous studies. We build upon them insofar as interest rate ceilings have their most direct impact on the market for small-dollar loans, typically offered exclusively by

[1]While geographic information (e.g. county or state) of people's residence is not publicly available, we obtained separate access to the location information with the help of a confidential data access agreement with the Bureau of Labor Statistics.

alternative financial service (AFS) providers, e.g., payday lenders, refund anticipation lenders (RAL), etc. Unlike traditional bank loans, which are secured by the item being purchased, e.g., an automobile, a house, etc., small-dollar loans are *cash* loans; the money can be used for anything. This is an important distinction. Miller and Soo (2020) find that an exogenous increase in credit availability through credit cards does not reduce the demand for payday loans. The authors hypothesize that it is the cash nature of AFS loans that make them special. In some sense, then, our paper is truly concerned with an exogenous change in *liquidity* constraints rather than *credit* constraints.

Taken as a whole, our findings contribute to a number of strands of literature. Liquidity constraints play a vital role within the subfield of consumption economics—from households determining intertemporal wealth allocation to the effectiveness of fiscal policy (Attanasio & Weber, 2010; Jappelli & Pistaferri, 2010). In a relatively simple intertemporal consumption model, a binding liquidity constraint manifests itself as the consumer being at a kink in the budget constraint, forcing consumption to equal the first-period's income. One way to alleviate this constraint is to increase first-period income by working more hours, as noted by Jappelli and Pistaferri (2017). We provide quasi-experimental evidence that this is indeed what some consumers do.

Within labor economics, our results shed light on the importance of accounting for liquidity constraints. For example, cross-sectional estimates of the elasticity of labor supply will be incorrect if the econometrician fails to consider liquidity constraints. Suppose a liquidity-constrained worker is compared to one who is not. Now suppose that both of their wages increase. The constrained worker can cut down on hours, as the additional income helps to alleviate the constraint. Without accounting for liquidity constraints, this worker's labor supply could offset the substitution effect of someone else in the sample, leading to a negligible overall result. Indeed, estimates the elasticity of labor supply on the intensive margin are small.

Our results may help shed light on the specification of preferences within the life cycle model of labor supply. There is a debate about whether consumption and leisure are substitutes or complements—whether the two are additively separable in utility or not, respectively. Our main finding is that on the intensive margin liquidity constraints lead to an increase in labor supply. If liquidity constraints also (simultaneously) lower consumption, this would corroborate the results of Blundell, Pistaferri, and Saporta-Eksten (2016). These authors find that a temporary wage shock leads to a decrease in consumption, which they interpret as evidence of substitutability between hours and consumption. An alternative—actually, additional—explanation is the presence of liquidity constraints. The authors state that it "is important to understand the role played by liquidity constraints in affecting consumption and labor supply choices." Our findings present *prima facie* evidence toward that call.

From a policy perspective, our findings highlight a (likely) unintended consequence legislating interest rates. Many previous studies on payday lending and alternative financial services focus on consequences like credit scores, delinquencies, and bankruptcies; see Bolen, Elliehausen, and Miller Jr (2020) for a detailed overview of this literature. We add to this literature demonstrating yet another effect: people work more hours when they become credit rationed. Regarding welfare, on one hand, a revealed preference argument implies that these workers are worse off: if these workers had untapped labor earnings and if that choice was voluntary, then these workers must be worse off. On the other hand, if the additional hours translate into human capital through learning-by-doing, a widening of their professional network, or if they receive more from the earned income tax credit, then the additional labor supply could benefit them.

Section 2: Institutional Context and Preliminary Analyses

Interest rate ceilings on small-dollar loans typically have payday lending as the target of the intervention since it's the most salient sub-industry within the alternative financial services (AFS) industry. Accordingly, we take payday loans as the hallmark example of small-dollar (cash) loans, but it's important to note that interest rate ceiling legislation is often applied to other small-dollar markets, e.g., refund-anticipation loans, auto title loans, etc. In this section we lay out some of the details of the market for payday loans. For details on the rest of the AFS industry, see Bolen et al. (2020).

Payday loans are small short-term unsecured cash loans. The size of the loan is typically \$100-\$500, but could be more—although they typically aren't in the thousands. The loan is usually two weeks in duration. The borrower writes a postdated check or authorizes the lender to access their checking account to debit the account on the due date of the loan. The borrower applies, the lender underwrites, and the loan is disbursed, often within some short span such as 15 minutes. The loan is disbursed in the form of cash or a direct deposit to a checking account. Hence, payday loans are often called "cash advance loans" or "deferred deposit loans." The loans have no collateral insofar as they are unsecured by any tangible property like a pawn loan (secured by a durable good) or an auto title loan (secured by an automobile). Payday loans are 'secured' by future income and hence they are recourse loans: if the borrower defaults on the loan, the lender will cash the check or debit the account. If the funds are insufficient, the borrower will have to pay an overdraft fee to the bank and still owe the payday lender.[2] Payday lenders do not report to the three major credit-rating agencies, even in the case of a default. Instead, the payday lender may sell the debt to a collector, perhaps after repeated calls to the delinquent borrower. Defaulting on a payday loan will typically shut the consumer out of the storefront payday loan market—at least at that firm.[3]

Payday lenders typically lend a maximum of about 20-25% of a borrower's gross monthly income. Sometimes this amount is legislated by a US state; other times its firm-level or corporate policy. The amount charged is approximately \$15 per \$100 borrowed. The finance fee, therefore, is 15%, but on an annualized percentage rate (APR) basis it works out to approximately 390%. Most storefront payday lenders display both the finance charge, and in keeping with federal Truth In Lending Act (TILA) legislation, the APR. It is *this* interest rate that is capped at the state level, which is most often 36%, in alignment with the 36% ceiling put in place as a result of the 2006 Military Lending Act (MLA).[4] The MLA legislates that payday lenders cannot lend to members of the US military at a rate that is above 36% APR.

In Table 1 we show some basic demographics of payday borrowers using two sources: the NLSY97 and the Federal Reserve Board's Survey of Household Economics and Decisionmaking (SHED). The NLSY97 is a biennial panel survey; we use 2007 through 2017. The SHED is an annual survey with a panel element; we use the 2018 and 2019 waves.[5] These survey results will be useful for our regression analyses below because they tell us which variable(s) are relevant for conditioning.

Column 2 shows that of the self-reported users of payday loans, 42% were male, 58% were female—closely matching the results of column 5 of the SHED. Regarding income, there is wider

[2]See the following studies on the symbiotic relationship between payday loans and overdraft fees: Campbell, Martinez-Jerez, and Tufano (2012); Morgan, Strain, and Seblani (2012); Melzer and Morgan (2015); Di Maggio, Ma, and Williams (2025).

[3]Using administrative data from three large payday lenders between 2000 and 2004, Dobbie and Skiba (2013) find that about 19% of payday loans end in default. For qualitative information on the payday lending collections process see the book by Servon (2017).

[4]See Miller Jr (2019) for the source of the specific value of 36% as the most common cap.

[5]The NLSY97 is conducted through in-person interviews, while the SHED is fully online.

Table 1: Survey Demographics of Payday Loan Users

	NLSY97 (2007 - 2017)			SHED (2018 - 2019)		
	Did not use payday loan	Used payday loan	p-value	Did not use payday loan	Used payday loan	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Male	0.52	0.42	0.00	0.46	0.43	0.32
Female	0.48	0.58	0.00	0.54	0.57	0.32
Age category Under 25	0.08	0.07	0.33	-	-	-
Age category Above 25	0.92	0.93	0.33	-	-	-
Ages 18 - 24	-	-	-	0.07	0.09	0.43
Ages 25 - 54	-	-	-	0.51	0.70	0.00
Ages 55 - 64	-	-	-	0.20	0.14	0.00
Ages 65 and above	-	-	-	0.22	0.07	0.00
White	0.70	0.56	0.00	0.65	0.38	0.00
Black	0.15	0.26	0.00	0.11	0.32	0.00
Hispanic	0.14	0.17	0.01	0.16	0.25	0.00
Household size 1	0.13	0.09	0.00	0.20	0.21	0.47
Household size 2 - 4	0.69	0.67	0.17	0.69	0.58	0.00
Household size >= 5	0.19	0.24	0.00	0.11	0.20	0.00
HS or less	0.27	0.42	0.00	0.31	0.38	0.01
Some college	0.34	0.42	0.00	0.34	0.46	0.00
Bachelors or more	0.39	0.16	0.00	0.36	0.16	0.00
Employed	0.84	0.90	0.00	0.62	0.64	0.41
Income less than 40K	0.64	0.84	0.00	0.36	0.57	0.00
Income 40K-100K	0.32	0.15	0.00	0.35	0.33	0.59
Income above 100K	0.04	0.00	0.00	0.28	0.09	0.00
Sample	56,551	1,689	-	18,583	533	-

Notes: The above table presents person-level weighted estimates of different socio-economic and demographic variables of payday loan users and non-users for the NLSY97 and the SHED. The p-values denote the statistical significance of the differences between two the samples classified by payday loan users and non-users. The NLSY97 sample is comprised of individuals aged between 23 and 38 years since the longitudinal survey follows a birth cohort who were born between 1980 and 1984. The SHED sample is comprised of the most recent observations of those individuals who are longitudinally surveyed in both surveys.

disparity across the two surveys, just as there is in employment. A few points are worth noting. First, payday loan borrowers are typically not impoverished: they need a steady income, which need not come from labor earnings. They also need a bank account and therefore must have at least somewhat decent credit.

Regarding education, there is also close agreement between the two surveys: 16% have a bachelors degree, which is close to the 2000-2001 survey findings of Lawrence and Elliehausen (2008), who find that 19% of payday loan users have a bachelor's degree. Previous analyses of small-dollar loan and payday lending legislation have found that formal education is an important indicator.[6] Age is another variable upon which we condition: the overwhelming group of payday loan users are above the age of 25. We see this in both the NLSY97 and the SHED survey results. This also corroborates Bolen et al. (2020), who analyze the 2015 wave of the National Financial Capability Survey (NFCS) and find that 86% of payday loan users are 25 or above. In our main analyses of labor supply from the CPS, we condition on education as well as age being above 25. Not only does this help pin down the likely users of payday loans, but it also aids against conditioning on a post-treatment outcome since it's possible, in principle, that borrowers use these loans to invest in human capital.

2.1 Are Payday Borrowers Liquidity Constrained?

In this section we document that payday borrowers face binding borrowing constraints. We review the relevant literature and present novel findings using the SHED survey. The imposition of a binding interest rate ceiling would almost certainly ration these borrowers even more, tightening the already-tight constraints on liquidity. Table 2 uses pooled 2018 and 2019 SHED results to illustrate the borrowing constraints faced by the survey respondents. Several points are worth noting. First, a majority of respondents who have used payday loans also have a credit card, further demonstrating that these consumers have at least some credit at their disposal (although some may be maxed out on the card). Second, payday loan users seem to want more credit, evidenced by the fact that they apply for credit at a rate that is higher than non-users. Third, given the low confidence in their application, they seem to be aware of their constraints. Finally, regardless of how we measure credit rationing, payday loan users are much more likely to be denied credit, either in part- or in full.

[6] For instance, see Bolen et al. (2020) and the sources cited therein.

Table 2: Credit Outcomes and Rationing among Payday Loan Users

	Non-PDL user (a)	PDL user (b)	Sample size (a)	Sample size (b)
Has credit card	0.83	0.63	17,248	458
Applied for any credit last year	0.39	0.69	17,253	459
Confidence regarding credit application	0.85	0.44	16,576	428
Those who applied for credit last year:				
Fully rationed	0.22	0.71	6,574	317
Partially rationed	0.15	0.49	6,570	317
Discouraged borrower	0.16	0.57	6,571	316

Notes: This evidence comes from pooling the 2018 and 2019 SHED surveys. All of the differences are statistically significant at the 1% level. 'Fully rationed' means that the survey respondent chose 'turned down'. 'Partially rationed' means that the respondent chose 'approved, but received less than applied for'. 'Discouraged borrower' means that the respondent 'put off the application due to an anticipated rejection.'

Our survey results reinforce the existing research that document the credit constraints that payday loan borrowers face. Using administrative data from "three large payday lenders" between 2000 and 2004, Dobbie and Skiba (2013) find that payday loan customers borrow an additional 39 to 44 cents per each additional dollar of credit extended, which they compare to Gross and Souleles (2002), who find the number is 10 to 14 cents for credit card borrowers. Miller and Soo (2020) analyze administrative data for the years 2013 to 2017 from Clarity Services, a credit bureau for alternative financial services. They find that payday loan applicants have about $2,000 in revolving credit limit with a usage rate of over 80%; the average for a non-payday loan user is an $18,000 limit and a 54% usage rate. Bertrand and Morse (2009) find that payday loan usage falls when borrowers receive their tax rebate checks; Skiba (2014) confirms these results, but finds that it's short-lived. Cui (2017) finds that state-level supplements to the earned income tax credit (EITC) reduces online payday loan demand. Dettling and Hsu (2021) show evidence that minimum wage hikes lead to less payday loan demand. These latter four studies again demonstrate behavior consistent with a liquidity constrained population. See Karlan and Zinman (2010) and Carvalho, Olafsson, and Silverman (2024) for evidence from South Africa and Iceland, respectively, further demonstrating that individuals who seek such high cost credit are already liquidity constrained.

2.2 Are the Interest Rate Ceilings Binding?

States can pass an interest rate ceiling, e.g., 36% APR, but the effect will depend on lender response. Under a 36% APR ceiling, a lender can charge no more than $ 1.38 per $100 for a two week loan. This is not profitable for most payday lenders. If they cannot lend profitably at that

Table 3: Effect of an Interest Rate Ceiling

	Used Payday Loans		Employed	
	TWFE	ETWFE	TWFE	ETWFE
Interest Rate Cap	-0.020**	-0.018**	-0.007	0.004
	(0.009)	(0.008)	(0.012)	(0.012)
Individuals	5,966	5,924	5,967	5,925
Observations	30,429	30,303	30,443	30,317
Individual FE	Yes	Yes	Yes	Yes
Survey and State FE	Yes	Yes	Yes	Yes
Individual Characteristics	Yes	-	Yes	-
Sample Mean (Pre-Treatment)	0.034		0.940	

Notes: TWFE denotes two-way fixed effects. EWTFE stands for extended two-way fixed effects, which comes from Wooldridge (2021), estimated using the JWDID package in Stata. The linear TWFE regressions include controls for individual characteristics, which include age in years, household size, household income, binary indicator of whether a person is married, and categorical indicator of number of pre-school aged children (0 for no child, 1 for 1-2 children, and 2 for more than 2 children). The ETWFE regressions include a categorical control for race and ethnicity. The JWDID package only allows one time-invariant control. Robust standard errors are clustered on the individual level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

rate, then lenders must find a way to avoid the interest rate cap or stop offering the product, thereby reducing the supply of payday loans.

While it is true that lenders sometimes attempt to circumvent interest rate caps, there is evidence that the supply of loans decreases after a cap is imposed.[7] Using two variations of difference-in-differences regressions, we show in Table 3 that there is a reduction in payday loan usage after an interest rate cap is imposed.[8] This evidence corroborates evidence from other surveys: Dasgupta and Mason (2020) find that payday loan usage falls using various waves of both the Federal Deposit Insurance Corporation's (FDIC) unbanked/underbanked survey as well as the National Financial Capability Survey (NFCS). Furthermore, Bhutta (2014) finds that the quantity of payday lenders—measured as number of establishments using County Business Pattern data—falls relative to non-ceiling states, while Dasgupta and Mason (2020) find that the number of establishments fall relative to a synthetic control unit. What's more, Dasgupta and Mason (2020) find that after states pass interest rate ceilings, the 10-Ks of the publicly-traded payday lenders (such as Advance America) show that they no longer do business in those states.

[7]See Ramirez (2019) for details on the circumvention of Ohio's 2008 interest rate ceiling; see Elliehausen and Hannon (2024) for more recent evidence.

[8]We also check whether the interest rate ceilings affect employment (it shouldn't) since payday lending depends on consumers being employed, hence there don't appear to be any sample selection issues.

2.3 Do People Use Labor Supply as a Buffer?

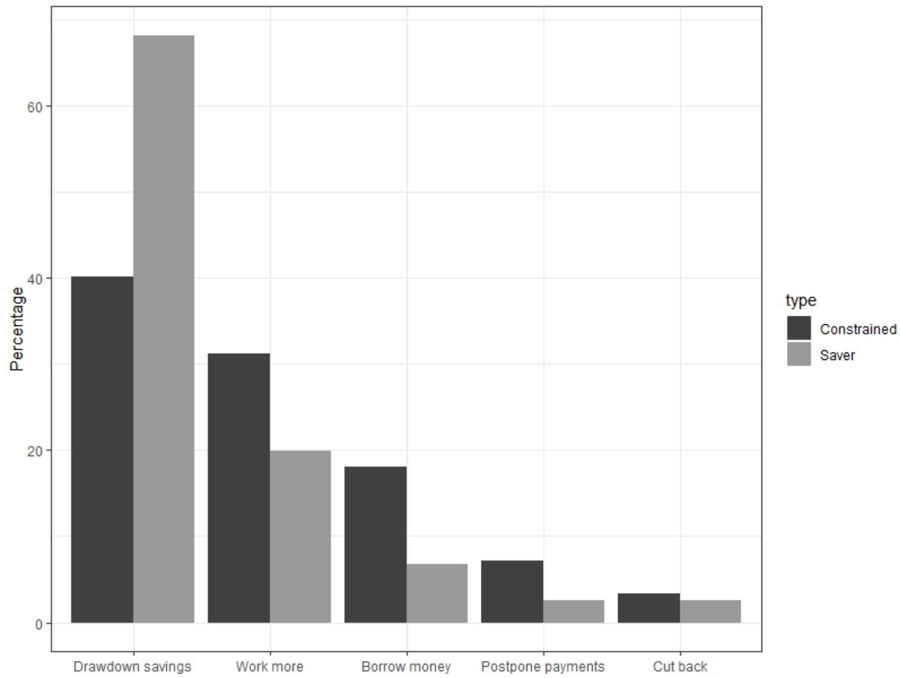
Within the labor economics literature, most theoretical models assume that workers can choose to change their hours of work at will. Empirically, however, it seems this may not be true. As evidence, many papers point to the low wage elasticity of labor supply on the intensive margin or workers' stated preferences that they would like to work more.[9] These workers are often known as 'hours constrained.' A comment by Adair Morse to Lusardi, Schneider, and Tufano (2011), which finds that survey evidence that many workers state that they would respond to a hypothetical shock by working more hours: "[n]ot many Americans can simply increase at will the number of hours they work at their current job or find short-term supplemental income. (pg. 135)"

In some sense, our paper addresses this question: credit can buffer shocks. All else equal, then, when the buffer is removed, the shock is felt by the consumer, and labor supply responds, as we show in the results below. In particular, we find that workers are less likely to take personal days- and sick leaves. This finding is noteworthy because even if it is true that workers cannot increase their labor hours at will, they should have additional control over whether to call in sick or cash out a personal day.

To further buttress these findings, we collect data from the 2022 Survey of Consumer Finances (SCF), produced by the Federal Reserve Board of Governors. The 2022 wave of this triennial survey asks the question: "If tomorrow you experienced a financial emergency that left you unable to pay all of your bills, how would you deal with it?" One of the potential responses is "work more." Importantly for our paper, the survey also asks: "[o]ver the past year, would you say that your (family's) spending exceeded your (family's) income, that it was about the same as your income, or that you spend less than your income?" A significant portion of those who answered that it was "about the same" can be viewed as liquidity constrained: in a two-period intertemporal framework, income equaling expenditure entails a solution at the kink of the budget curve. To the extent that the corner solution was not by choice, these survey respondents can be interpreted as liquidity constrained. Within the same framework, respondents who answered that 'income exceeded spending' could be interpreted as savers. We present simple descriptive evidence of answers to the first question, broken down by question 2. Figure 1 has the results.

[9] See Labanca and Pozzoli (2022) for a recent discussion of this work.

Figure 1: Coping with a "Financial Emergency"



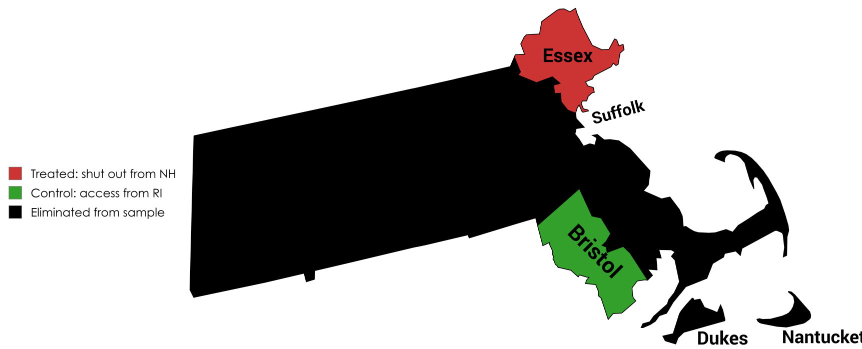
Notes The question comes from the 2022 wave of the Federal Reserve Board's Survey of Consumer Finances (SCF). Specifically, the question (x7775) asks: "If tomorrow you experienced a financial emergency that left you unable to pay all of your bills, how would you deal with it?" We disaggregated the responses to this question based on another question within the survey (x7510): "Over the past year, would you say that your (family's) income spending exceeded your (family's) income, that it was about the same as your income, or that you spent less than your income?" We denote a 'saver' as those who answered that their income exceeded their spending; the 'constrained' (liquidity constrained) are those who responded that income and spending were about equal. Note that 'savers' and 'constrained' refer to an intertemporal optimization framework: consumers who have spending equaling income are at a kink in the budget line, which, while it could happen by chance, we interpret as being liquidity constrained.

The findings are consistent with our definitions of 'savers' and 'liquidity constrained': savers list "drawdown savings" as the primary answer, while the proportion for liquidity constrained is notably lower. More saliently for our main research question, "work more" was chosen by 20% of savers, while it was over 30% for those who are liquidity constrained.

These descriptive statistics corroborate Lusardi et al. (2011), who pose a similar hypothetical question to survey respondents. Increasing labor supply is often just behind drawing down savings, followed by borrowing, as the most commonly-selected method for coping with shocks.[10]

[10] The *mechanism* is not asked by Lusardi et al. (2011). In other words, do workers plan to work more at their current (set of) job(s)? Or do they work more/longer hours after having switched jobs? Blundell, Brewer, and Francesconi (2008) and Benito and Saleheen (2013) empirically find that for wage increases the mechanism is primarily—nearly exclusively—through job-switching rather than through the current employer. We also find evidence of job-switching: the likelihood of having the same employer falls as a neighboring county has an interest rate ceiling put in place; see Table 10 below.

Figure 2: County-Level Example from Massachusetts: New Hampshire's 2009 Ceiling



Notes: The quasi-experimental research design is a difference-in-differences methodology. The treated group consists of Massachusetts workers who live "close to" New Hampshire, where 'close' is defined as having the geographic centroid of their county within 25 miles of the geographic centroid of the county of a state that has a restrictive interest rate ceiling in place; the ceiling is passed within the sample period, thereby exposing these workers to the treatment. The control group is Massachusetts workers who live "close to" a state that has no such restrictive interest rate ceiling in place. Essex County, Massachusetts has its geographic center within (approximately) 25 miles of a Rockingham County, New Hampshire. Bristol County, Massachusetts has its geographic center within 25 miles of Bristol County, Rhode Island. New Hampshire implemented (and enforced) a restrictive interest rate ceiling on small-dollar loans in 2009, while Rhode Island implemented so such restrictive legislation. All other Massachusetts counties are eliminated from our samples because they received the treatment—a binding interest rate ceiling—in the pre-sample period: Massachusetts's interest rate ceiling goes back to 1898. Map credit: www.mapchart.net

Section 3: County-Level Research Design

Figure 2 presents a stylized example of our main research design.[11] Massachusetts has enforced its usury law since 1898. Hence, the counties colored in black in Figure 2 have never had (legal) access to small-dollar credit. The people who live in the red county, Essex, along with the people who live in the green county, Bristol, have had access through the neighboring states of New Hampshire and Rhode Island, respectively; all of Massachusetts's other neighbors have restrictively-low interest rate ceilings in place. In January 2009, New Hampshire passed a binding 36% APR interest rate cap, effectively banning most small-dollar loans since it is not possible to make a profit on that low of an interest rate. Dasgupta and Mason (2020) show empirically that the number of payday lending establishments falls precipitously in New Hampshire relative to a synthetic New Hampshire after the passage of its cap. Therefore, workers in Essex County no longer have access to the small-dollar credit market. All the while, access to Rhode Island's

[11]This example is realistic, and we do have the affected counties in our analysis. Nonetheless, the example is mostly for illustrating the mechanics of the research design: most of the variation in our county-level analysis is generated by the interest rate ceiling implementations of Pennsylvania and Washington D.C.

credit markets remained unencumbered.[12] This situation sets up a quasi-experiment: people in the Massachusetts counties who live "close to" New Hampshire are completely shut out of the storefront market for small-dollar loans; this is the treatment group. The control group is comprised of Massachusetts residents who live close to Rhode Island. They are the control group because they still have feasible access to small-dollar loans. We compare the difference in average hours worked in the Rhode Island-close counties and the New Hampshire-close counties prior to New Hampshire's rate cap. We then make the same comparison after New Hampshire imposes the interest rate ceiling. The difference in these differences is therefore an estimate of the average treatment effect of liquidity constraints on labor supply.

We define 'close' using a county-level 25-mile centroid-to-centroid measure. In Figure 2, for example, Essex County is in our treated group because its geographic center is within (approximately) 25 miles of the geographic center of a New Hampshire county (Rockingham). Bristol County is in the control group because its geographic center is within 25 miles of a Rhode Island county (Bristol).[13] Other Massachusetts counties, e.g., Suffolk, Dukes, Nantucket, etc., all have their geographic centers outside of a 25-mile radius of any state that has no stringent restrictions on small-dollar loans.[14] The full set of our states, counties, and policy changes are in Table A1 in the Appendix.

This research design is a slight modification of that used first by Melzer (2011), and later by Bhutta (2014), Melzer and Morgan (2015), Melzer (2018), and Dobridge (2018).[15] The modification is that our distance measure is defined as 25 miles *centroid to centroid*, whereas the cited authors define their respective treatment groups to be within 25 miles *of the border* of a

[12] Rhode Island enacted some term- and licensing restrictions on small-dollar loans, but Fekrazad (2020) finds that there is no change in the number of suppliers; borrowers are not rationed (usage increases, but average income remains unchanged).

[13] O. E. Lukongo and Miller (2018) analyze spatial data from Arkansas, where residents have to travel out of state for small-dollar credit. The authors find that outstanding loans fall dramatically after about 40 miles.

[14] We use Stata's *geodist* package to calculate distance. See here: https://ideas.repec.org/c/boc/bocode/s457147.html; An alternative source of county-by-county distances comes from the National Bureau of Economic Research (NBER) database; see here: https://www.nber.org/research/data/county-distance-database. We opt for the former for simplicity in implementation, i.e., already built-in package. To be sure, the composition of the treatment- and control groups are mostly unchanged whether we use *geodist* or the NBER database.

[15] Melzer (2011) finds that access to payday loans causes people to various forms of financial distress; Bhutta (2014) shows that payday loan access has no meaningful impact on financial health, e.g., credit scores; Melzer and Morgan (2015) find that prohibitively-low interest rate ceilings lead to an increase in the cost of overdraft credit by mainstream financial institutions like banks and credit unions; Melzer (2018) finds that payday loans cause its users to rely more heavily on social safety nets; Dobridge (2018) finds that payday loan access mitigates the decline in extreme-weather-induced shocks to consumption.

neighboring state.[16] We provide evidence that centroid to centroid corresponds to more access (Table A3 in the Appendix).[17]

This research design is built on the premise that both groups of borrowers, treated- and control, travel at most 25 miles across state lines to obtain small-dollar cash loans. There is ample evidence in support of this assumption. Melzer (2011) cites anecdotal evidence that people residing in states with a binding interest rate ceiling will travel to a neighboring state to obtain small-dollar credit. In-depth focus group interviews corroborate Melzer's anecdotes.[18]

There is also empirical evidence from small-dollar credit providers. Prager (2014) regresses the number of payday lenders on various factors that might explain their location. The data encompasses all counties in the United States in 2006. The variable *BORDER* takes a 1 if the payday lender is located in a 'payday-permissive' state and borders a state that effectively prohibits the practice through an interest rate ceiling or a usury law; it is zero otherwise. At the time of his sample, the average urban county had nearly 84 locations per (million) capita. All else equal, that number is more than 50% higher for *BORDER*—an effect that is highly economically- and statistically significant. Ramirez and Harger (2020) corroborate Prager's results using monthly, state-level administrative data for payday branch locations at the county level during the 2005 to 2010 time period. The authors find large economic- and statistical effects: "border counties...have 83 percent more new branches and 14 percent more operating branches relative to interior counties."

Finally, there is indirect empirical evidence of borrowers' willingness to travel to obtain credit. Campbell et al. (2012) find that Georgia's 2004 interest rate ceiling led to more involuntary checking account closures; the effect is statistically nil near (within 30 miles) South Carolina, a state that places no prohibitive restrictions on small-dollar credit. The results are statistically significant within Georgia's interior counties, that is, those far (60+ miles) away from South Carolina. O. E. B. Lukongo and Miller Jr (2022) provide a detailed case study of Arkansas and its stringent usury ceiling; all six of Arkansas's neighbors (at the time, 2013) had no usury

[16] An alternative definition of distance is a population-weighted center, sometimes known as a county's "center of gravity." The principal reason we opt not to use this alternative definition is that our time span is quite long: 2002 to 2019, 18 years. Population, demographics, and degree of urbanization can all change over a generation. The geographic center, as a contrast, is fixed across time.

[17] To be sure, our main results are robust to the distinction between centroid to centroid and centroid to border; see Table A4 in the Appendix.

[18] See the following report from Pew Charitable Trusts: https://www.pewtrusts.org/en/research-and-analysis/reports/2012/07/19/who-borrows-where-they-borrow-and-why

restrictions. Using administrative data on outstanding small-dollar cash installment loans, the authors find that approximately 94% of these loans are held by residents in the Arkansas's 15 perimeter counties, which has approximately 44% of the population. The authors interpret this as strong evidence that these residents are obtaining such loans by crossing state lines.

3.1 Data, Specification, and Results

We use data on hours worked from the Current Population Survey (CPS), recorded at the monthly frequency. The CPS data are self-reported by individuals, as are the measures of location (county) and some basic demographic variables. Our sample starts in January 2002, which gives enough of a pre-treatment baseline: the policy changes that generate treatment status take place in 2007 (Pennsylvania), 2008 (Washington DC), and 2009 (New Hampshire).[19] See Table A1 in our Appendix for details.[20] We stop our analysis in 2019 to avoid any potential confounding effect of the COVID-19 pandemic, which dramatically altered the behavior of labor supply, and presumably, access to storefront alternative financial services.

In addition to location and demographics, the CPS provides a rich set of other labor market outcomes, which we use to assess some mechanisms of the effect in Section 5 below. These other labor outcomes are as follows: propensity to take a sick/personal leave; whether the respondent is with the same employer; the propensity to hold a second job; whether the worker is hourly- or salaried; and weekly earnings. In addition, the monthly frequency facilitates short-term analysis. The liquidity constraints studied in this paper rely on AFS, many of which are short-term—typically between two weeks to 90 days in duration.[21]

The relatively high frequency of our CPS data may entail some issues. The high-cost nature of AFS has the potential to ensnare borrowers into a sort of 'debt trap', the effects of implementing a cap may manifest after several months; the effect may be a bit long-lived. Furthermore, monthly

[19] Although these policy changes overlap with the Great Recession, we don't see this as a major threat to our research design. Agarwal, Gross, and Mazumder (2016) find that while survey evidence from the SCF shows an uptick in payday loan usage during the Great Recession, the profitability of publicly-traded payday lenders did not break its trend during the same time period; Google searches for "payday loan" showed only a minor uptick; and neither the Midwest, California, nor Nevada saw meaningful changes in payday lending activity. Moreover, 25 miles entails that the locales probably face symmetric business cycles and have the same degree of cointegration with the national economy.

[20] See Morgan et al. (2012) and Melzer and Morgan (2015) for details on payday lending in Pennsylvania. The state attempted to effectively ban the industry in 1998, but lenders were able to avoid the ceiling until November 2007.

[21] See Bolen et al. (2020) for details on AFS; see footnote 90 in their paper for auto title loans, which have the potentially longest duration, but likely not longer than six months.

frequency data could exhibit some noise. We therefore consider two additional sources of self-reported measures of hours worked measured at the annual frequency: the Annual Social and Economic Conditions (ASEC) of the CPS, also known as the "March Supplement" to the CPS; and the American Community Survey (ACS), which is administered by the Census Bureau. The CPS-ASEC data have the same sample time frame, 2002 to 2019, but the ACS period starts in 2005. The ACS coverage is much broader than the CPS and CPS-ASEC. Therefore, the sample of counties in our quasi-experiment increases. Table A1 in the Appendix lists the ACS counties by default; the CPS (and CPS-ASEC) counties are a subset of these counties; see column 3 of the table.

Our county-level analysis relies on a difference-in-differences identification strategy with the following specification:

$$LS_{ict} = \alpha_c + \lambda_t + \beta \cdot CAP_{c't} + \mathbf{X_{ict}^T} \cdot \gamma + e_{ict} \tag{3.1}$$

where LS is the measure of labor supply for individual i in county c in time period t; α and λ are county- and time fixed effects, respectively. β is a measure of the average treatment effect since $CAP_{c't}$ is a 1 for all counties exposed to a neighboring state's ban and a zero for counties that neighbor a state where such lending is permitted. The prime in the subscript on CAP illustrates that it is the 25-mile centroid neighbor that is generating the variation in CAP. X is a matrix of individual controls, including martial status, race, ethnicity, education, and family size; it also includes a county-level control, which is the state unemployment rate. Note that we limit our analysis to workers who are above the age of 25 to limit the possibility of conditioning on a post-treatment outcome, e.g., education, since it is conceptually possible that people use short-term credit to fund investments in education. Furthermore, as Table 1 shows, formal education is perhaps the single most important determinant of whether someone uses a payday loan.[22]

[22]Nearly every study that examines payday lending demand finds education to be a strong indicator. The demographics of auto title borrowers more closely matches the typical American; see Bolen et al. (2020) for an overview, footnote 39 in particular.

Table 4: Effect of small dollar loan interest rate cap on labor supply

Max. distance threshold – 25 miles	CPS Monthly – 2002-2019		CPS ASEC – 2002-2019			ACS – 2005-2019	
	Total hours/week	Main job hours/week	Usual hours/week last year	Hours last week	Weeks worked last year	Usual hours/week last year	Worked min. 50 weeks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A– Sample mean (Overall):	40.96	40.21	40.51	40.14	48.89	40.36	0.79
Interest rate cap	0.407***	0.349	0.787**	0.529	0.086	0.147	-0.003
	(0.203)	(0.225)	(0.376)	(0.527)	(0.482)	(0.100)	(0.009)
Observations	145,472	146,414	20,442	18,325	20,442	638,591	638,591
Panel B– Sample mean (< Bachelor's degree)	40.36	39.64	39.92	39.49	48.48	39.69	0.78
Interest rate cap	0.550***	0.521**	0.890**	0.437	0.112	0.133	-0.007
	(0.214)	(0.209)	(0.416)	(0.524)	(0.470)	(0.138)	(0.005)
Observations	82,612	83,083	11,946	10,555	11,946	379,436	379,436
Panel C– Sample mean ($\geq$ Bachelor's degree)	41.83	41.01	41.41	41.10	49.51	41.42	0.80
Interest rate cap	0.259	0.144	0.392	0.705	-0.029	0.092	0.000
	(0.495)	(0.505)	(0.413)	(0.778)	(0.646)	(0.123)	(0.005)
Observations	62,860	63,331	8,496	7,770	8,496	259,155	259,155

Notes: In the above regressions, the treatment status is determined by whether a county from a state where payday lending is banned is within 25-mile radius (centroid-based distance) of a county from another state where payday lending industry existed at least for a portion of the analysis period. Columns (1)-(2) are based on CPS monthly data (January 2002 - December 2019) and include total hours and usual hours (at main job) worked per week as outcome variables. Columns (3)-(5) are based on CPS ASEC (2002-2019) data and include usual hours per week worked in the prior year, hours worked in the week prior to the survey and weeks worked in the prior year. Columns (6)-(7) are based on ACS samples (2005-2019) and include usual hours worked per week in the prior year and a binary indicator of whether an individual worked at least 50 weeks in the preceding year. The regression sample is based on employed individuals aged 25-64. We perform Gardner's (2022) two-stage differences-in-differences (2-stage DID) for the above analysis. As additional robustness tests, we report point estimates from alternative DID specifications in Appendix; see Table A1. All our regressions are weighted by survey-based sample weights. We also report pre-treatment sample means of the outcome variables above. The regressions include control for race, ethnicity, age and age-squared, education, marital status, state-level unemployment rates along with county and time (or year for annual surveys) fixed effects. Robust standard errors are reported in parentheses and are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1.

We estimate β from Equation 3.1 using the two-stage differences-in-differences method of Gardner (2022). The results are in Table 4. For robustness and completeness, we also estimate β using the standard two-way fixed effects (TWFE) estimator, as well as the staggered differences-in-difference estimators proposed by Callaway and Sant'Anna (2021) and Wooldridge (2021). See Table A2 of the Appendix for these results.

Columns 1 and 2 of Table 4 contain our main results. Consistent with a relatively simple Neoclassical model of intertemporal choice with flexible labor supply, hours worked shows an increase. For instance, Rossi and Trucchi (2016) and Jappelli and Pistaferri (2017) formally show that labor supply will unambiguously increase. The simulations of the models of Pijoan-Mas (2006) and Athreya (2008) are also consistent with our findings.[23] Our findings are consistent with the survey evidence of Lusardi et al. (2011), who find that "work more" is a commonly-chosen response as a way to cope with a hypothetical adverse expenditure shock.

The magnitude of our main findings, 0.407, implies an additional 1.6 additional hours worked per month. This is the total treatment effect of small-dollar credit access. To put these results into context, Rossi and Trucchi (2016) find that the relaxation of credit constraints, an unexpected inheritance, reduces Italian male labor supply by approximately four hours per month; Dao Bui and Ume (2020) estimate the effect on hours worked (CPS-ASEC) of a credit expansion through exogenous increases in bank branches to be approximately 0.3 hours per week, 1.2 per month. Nawaz et al. (2025) analyze the 2022 SCF and find that the distance-induced relaxation of credit constraints reduces hours worked by approximately 0.8 hours per week or 3.2 per month. Our estimated average treatment effect, therefore, is roughly in line with these studies.

Our identification strategy relies on AFS *access* rather than *usage*, and hence our main result is an estimate of the average treatment effect. To get a sense of the average treatment effect on the treated, we condition the sample on those survey respondents who do not have a bachelor's degree since education is one of the principal indicators for AFS take-up, e.g., payday loan usage. See Table 1 for evidence and see Bolen et al. (2020) and the sources therein for additional evidence.[24] Panel B demonstrates that the effect is stronger for those with less than a bachelor's degree: 0.55 additional hours per week (2.2 per month) relative to the control group, an increase

[23] In a multiperiod model, ambiguity is possible. Kumar and Liang (2024) extend a model of home-equity credit to three periods and find that labor supply unambiguously increases the next period when the collateral constraint is tightened, but the effect is ambiguous for the subsequent period.

[24] Income is another demographic variable that predicts AFS and payday loan usage, but we do not condition on income because labor supply affects income—and, through the income effect, is affected by income.

of approximately 1.36% of the pre-treatment sample mean. If we assume that 3% of Americans have used a payday loan (see the last line in Table 1), then the treatment effect for users would be quite substantial.[25]

To be sure, it is not completely unreasonable to think that the treatment effect for the treated population would be substantial. Stegman (2007) cites a study that notes that payday lenders and check cashers (which often offer payday loans) outnumbered McDonald's, Burger King, Sears, JC Penney, and Target combined. Journalist Gary Rivlin's book (2010) documents how the AFS industry is larger than the liquor industry, at least, as of about 2008. The direct regulatory effect of interest rate ceilings on small-dollar loans is not limited to payday lending. These caps can affect users of auto title loans, subprime credit cards, and refund anticipation loans, with strong indirect effects on the pawnshop industry; see Bhutta, Goldin, and Homonoff (2016) and Carter (2015).

Our identification strategy highlights small-dollar credit access, which affects a broader segment of the population than solely the users of AFS. One such spillover channel is through the mainstream credit suppliers such as banks and credit cards. Even though banks and credit card companies do not, on the surface, compete with payday loans, they do offer short-term credit: overdraft protection, non-sufficient funds (NSF) charges, and credit card late fees.[26] There is evidence that these markets are affected by interest rate ceilings, albeit indirectly. Melzer and Morgan (2015) find that the price of bank overdraft credit falls, but so, too, does the limit. The overall price—price per unit of credit—increases in geographies that have their access to payday loans cut off.[27] The authors also find some evidence that the profitability of credit unions increase. Morgan et al. (2012) find that shutting down access to payday loans leads to higher overdraft fees and more bounced checks. Zinman (2010) and Bhutta et al. (2016) corroborate these findings using survey evidence.[28] Bhutta et al. (2016) follow up their survey evidence with administrative data on credit card usage (consumer credit panel, CCP) and find that credit

[25] Any self-reported measure of payday loan usage may entail some underreporting due to stigma; see Apostolidis, Brown, and Farquhar (2023), for instance.

[26] If measured on an APR basis, overdraft credit would often be in the thousands of percentage points; see footnote 10 in the report here: https://www.consumerfinance.gov/rules-policy/final-rules/overdraft-lending-very-large-financial-institutions-final-rule/

[27] Di Maggio et al. (2025) demonstrate that the relationship between banks and payday lenders is symbiotic: aggressive bank practices like 'high to low' ordering of NSF fees creates part of the demand for payday loans.

[28] But cf. Campbell et al. (2012) who find that involuntary checking-account closures *falls* in Georgia counties that have less feasible access to payday loans.

usage falls among individuals with a low credit score. The authors hypothesize that the shutting down of payday lending access, which leads to checking account closures, could, in turn, ripple through to the credit card market. Our findings from Table 4 demonstrate that the ripple effect also makes its way into the labor market. With the price of overdraft credit increasing, non-payday loan users may substitute towards more labor; other workers may increase their working hours to service this now-pricier debt.

Another possibility is that workers who may have never used AFS may become aware that there is no longer short-term credit available and they begin working more—a kind of labor-market precautionary buffer. An analogy may be a consumer who has a credit card but never uses it; the account is kept open "just in case." What would the workers do with the additional buffer income? Allcott, Kim, Taubinsky, and Zinman (2022) investigate the possibility that people save this additional income (see their online appendix) using the Panel Survey of Income Dynamics (PSID) and find that 'holdings of liquid assets' does not change in states that ban access to payday loans. Nevertheless, the possibility of a labor-buffer remains open for several reasons. First, these authors lump together holdings of all assets: checking, savings, time deposits, money market funds, and US Treasuries. Second, the authors control for income, potentially nullifying the labor-to-savings channel that we are highlighting here. Third, for households that have zero financial assets, the authors take the log of 1 plus zero, thereby obscuring the interpretation as an average treatment effect (Chen & Roth, 2024). Finally, the study only considers *financial* assets. It's possible that workers increase labor supply and "save" it through the purchase of a durable good, which provide consumption services across time. Indeed, Adams, Einav, and Levin (2009) find strong tax-rebate-induced sensitivity of auto loan purchases, a finding corroborated by Zhang (2017). We therefore believe this to be an open question for future research.

Our annual measures show seemingly-conflicting results. The CPS-ASEC shows a relatively large, statistical increase, while the ACS does not, despite both being annual surveys. There may be several reasons for the discrepancy in findings. First, the coverage for the ACS starts in 2005, reducing the pre-treatment baseline period. Second, the comprehensiveness of the ACS coverage expands our analysis relative to our CPS analyses. Our ACS analysis incorporates many more counties, especially North Carolina, and a couple of new control states (New York and West Virginia), and a few states that have no restrictions on small-dollar loans (Tennessee and Ohio); see Table A1 in the Appendix for details.[29] A final possibility may stem from the administration of the surveys. The ACS is done on a rolling basis, while the CPS-ASEC is done every March. It is therefore more difficult to line up the timing of the policy change with the timing of the survey. As one example, New Hampshire's policy took effect January 1, 2009. The rolling-nature of the ACS implies that some of the respondents may have answered the survey in June, some September, and some in January. This latter point entails that the ACS would indeed be biased downward, as some of the responses cover a time span far away from the timing of the treatment, thereby mitigating the effect. Nonetheless, our CPS-ASEC- and ACS results corroborate our CPS monthly findings, at least qualitatively.[30] The signs of the coefficient in each analysis is positive. The magnitudes are stronger for those with no bachelor's degree.

3.2 Placebo Test: Salaried Workers

If it's true that shutting down access to small-dollar credit leads to an increase in labor supply, we would not expect to see this effect for salaried workers; they are paid an annual, fixed amount rather than by the hour. Hence, when running our analysis on salaried workers, we expect to find no effect. In other words, we should expect hourly wage earners to be driving the results in Table 4. Table 5 contains the estimates, itself further broken down by education (less than bachelor's degree). This analysis, like the others, is conditioned on prime-age workers (25 - 64), male and female.

The findings confirm the idea that it is hourly workers rather than salaried workers that are driving the results in Table 4. The magnitude of the effect increases to about 0.74 hours per week (3 per month). Note that this effect encompasses users of AFS such as payday loans, but could also extend to non-users as well, e.g., through the reduction of overdraft credit limits (Melzer & Morgan, 2015).

[29]Ohio passed a 36% APR cap in 2008, but payday lenders were able to reclassify their business and continue to make loans; see Ramirez (2019) for details. In 2018, Ohio once again restricted payday lending, but allowed installment loans. We therefore classify Ohio as a state where there is still access, albeit in a modified form. In any case, the status of payday lending, and AFS more broadly, in Ohio has no bearing on our CPS results.

[30]Our instrumental variable findings in Section 3.4 show a close match quantitatively between the CPS-ASEC and the ACS.

3.3 Placebo Test: Distance as 40 Miles

The key to our identification strategy in this analysis is that borrowers are willing to travel (and cross state lines) to obtain credit. Traveling entails costs: time, gas, automobile depreciation, possibly tolls, etc. Therefore, we should expect there to be a weaker effect on labor supply as we increase the distance of county centroid to county centroid, despite the increase in sample size.

As a falsification exercise, therefore, we rerun our monthly CPS analysis in Table 4 using 40 miles rather than 25. As we expand the centroid-to-centroid distance, the number of counties included in the sample increases. New counties changes the treatment-control comparison. Therefore, we we cut any new county that the 40-mile expansion adds. Note that the number of total counties are the same, and hence the sample size remains unchanged. This placebo analysis changes some of the treated counties to control counties, i.e., some that did not have access using a 25-mile radius now have access with a 40-mile distance. The results for this placebo test are in columns 5 and 6 of Table 5.

The results corroborate the hypothesized link between liquidity constraints and labor supply. The coefficient for the overall sample as well as for those with less than a bachelor's degree is smaller with a 40-mile maximum distance threshold. Moreover, none of the results are statistically distinguishable from zero, but still positive in sign. The results confirm the idea that while people are willing to travel to obtain credit, they are not so willing as to increase the length and duration of their trip by 60%, i.e., going the additional 15 miles from "within 25" to "within 40."

Table 5: Effect of small dollar loan interest rate cap on labor supply by wage payment types and alternative distance threshold

	Paid hourly basis		Not paid hourly basis		Max distance: 40 miles	
	Total hours last week	Usual hours last week	Total hours last week	Usual hours last week	Total hours last week	Usual hours last week
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A– Sample mean (Overall)	38.94	38.18	42.26	41.66	41.04	40.28
Interest rate cap	0.562	0.496	0.236	0.198	0.272	0.329
	(0.481)	(0.379)	(0.389)	(0.507)	(0.217)	(0.219)
Observations	15,721	15,826	18,104	18,198	145,472	146,414
Panel B– Sample mean (< Bachelor's degree)	39.07	38.41	41.59	41.02	40.44	39.72
Interest rate cap	0.743*	0.736**	0.009	-0.231	0.332	0.360
	(0.427)	(0.311)	(0.414)	(0.425)	(0.277)	(0.241)
Observations	12,018	12,089	7,208	7,240	82,612	83,083
Panel C– Sample mean (≥ Bachelor's degree)	38.48	37.37	42.71	42.10	41.84	41.04
Interest rate cap	-0.220	-0.130	0.359	0.577	0.278	0.339
	(0.998)	(0.855)	(0.553)	(0.667)	(0.557)	(0.549)
Observations	3,703	3,737	10,896	10,958	62,860	63,331

Notes: The CPS monthly (January 2002-December 2019) samples in columns (1)-(4) in above table are classified by employed individuals who are paid on an hourly basis and those who are not. The individuals in the analyzed samples are interviewed in the CPS monthly "earner study" questions. Therefore, we use the CPS person-level weight (called 'earnwt') that are designed for analysis using individuals included in that series. In columns (5)-(6), for comparability reasons, we test the effects of the interest rate caps by expanding the maximum access distance threshold (to 40 miles) by using the counties included in the analysis reported in columns (1) and (2) of Table 4. The regression specifications in the above table are similar to the models used in Table 4. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

3.4 Robustness Check: Credit Access as a Continuous Measure

One critique of our findings thus far is that it's not certain that there even are payday lenders located in the neighboring counties that are giving the treated counties the exposure. In other words, appealing to Figure 2, is it even true that there are payday loan suppliers located in Rockingham County, New Hampshire, which borders Essex County, Massachusetts (and has its centroid within 25 miles of that of Essex's)? This critique is put forth by Caskey (2012) of Melzer (2011), and, implicitly, the other studies that rely on the same identification strategy, e.g., Bhutta (2014), Melzer and Morgan (2015), Melzer (2018), and Dobridge (2018)—and, by extension, our modified version.

We address the concern in this subsection. We regress hours worked on the number of payday lending establishments, essentially replacing $CAP_{c't}$ in Equation 3.1 with the *number* of payday lenders. Our measure of payday lending establishments is imperfect: it is the number of establishments in a given county, grouped within the NAICS code 522390, which, in addition to payday lending, includes other alternative financial services, e.g., check-cashing services, etc. The number of establishments is available from two sources in two frequencies: quarterly from the Quarterly Census of Employment and Wages (QCEW) from the BLS; and annual from the County Business Patterns (CBP) from the US Census Bureau. For the CPS monthly data, we use the quarterly QCEW series. For the annual CPS-ASEC and ACS data, we use the annual CBP series. See Bhutta (2014) and Dasgupta and Mason (2020) for uses of this series (CBP) as a proxy for payday lending establishments.[31] Our ordinary least squares (OLS) regression results are in panel A of Table 6.

One problem with a simple OLS specification in this context is that payday lending location decisions are not random. Their location decisions are based on the maximization of expected profit. In fact, Prager (2014) and Ramirez and Harger (2020) show empirically that, all else equal, payday lenders *purposefully* move toward the borders of restrictive states in order to capture the neighboring market, i.e., Advance America setting up locations in Rockingham County, New Hampshire factoring in the ability to service Essex County, Massachusetts borrowers.

To address this concern, we instrument the number of payday lenders with the interest rate

[31] See Barth, Hilliard, Jahera, and Sun (2016) for a critique of this series as a proxy for payday lenders.

ceiling and run the following two-stage least squares (2SLS) regression:

$$\text{First Stage: } PDL_{c't} = \alpha_c + \lambda_t + \delta \cdot CAP_{c't} + \mathbf{X}^{\mathbf{T}}_{\mathbf{ict}} \cdot \gamma + \epsilon_{ic't} \tag{3.2}$$

$$\text{Second Stage: } LS_{ict} = \alpha_c + \lambda_t + \rho \cdot \widehat{PDL}_{c't} + \mathbf{X}^{\mathbf{T}}_{\mathbf{ict}} \cdot \iota + \varepsilon_{ict} \tag{3.3}$$

where $PDL_{c't}$ is the number of 'NAICS 522390' establishments in county c' in time period t (annual) and $\widehat{PDL}_{c't}$ is the fitted values from the first stage. The prime in the notation represents the fact that it is the *neighboring* county. Keeping with the stylized example shown in Figure 2, the first stage has the number of establishments on the left-hand side for Rockingham County, New Hampshire. This series is regressed on fixed effects and the binary variable denoting the implementation of New Hampshire's interest rate ceiling in 2009. The matrix of individual controls are kept in the specification for notational felicity vis-a-vis the second-stage regression. The fitted values of this first-stage regression represent an exogenous change in the number of establishments in Rockingham County, New Hampshire—the county to which Essex County, Massachusetts residents commute for storefront payday loans. The second stage regresses hours worked for individuals in Essex County, Massachusetts on fixed effects, demographic controls, and the exogenously-pure number establishments from the first stage. The results of this 2SLS regression are found in panel B of Table 6.

The results corroborate our main findings in Table 4. Credit access decreases working hours. Note that in our main DID results in Table 4 the intervention is a *restriction* in cash loans; hours worked increases. In the 2SLS results in Table 6, the coefficient estimate pertains to the marginal effect of *the number of establishments* on labor supply. Hence, the interpretation is that as cash credit suppliers increases (decreases), labor supply decreases (increases). Further note that in contrast to Table 4, the 2SLS results demonstrate similar findings for the annual data. The CPS-ASEC and ACS findings are very similar, even in magnitude (panel B). The findings in Table 6 demonstrate the robustness of our main analysis another way: the IV analysis relies on 2SLS regression for its implementation rather than any particular choice regarding DID estimator.[32]

[32] See Baker, Callaway, Cunningham, Goodman-Bacon, and Sant'Anna (in press) for detailed explanations of the relatively new DID estimators.

Table 6: Effect of access to credit intermediation facilities on labor market outcomes

	CPS Monthly – 2002-2019 (Col. 1-4)				CPS ASEC – 2002-2019 (Col. 5-8)				ACS – 2005-2019 (Col. 9-10)	
	Total hours/week	Main job hours/week	Multiple jobs	Absent from work	Usual hours/week last year	Hours last week	Weeks worked last week	Absent from work	Usual hours/week last year	Absent from work
Panel A – Least squares regression	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Establishments (NAICS 522390)	-0.010***	-0.014***	0.000***	0.000	-0.001	0.002	0.008	0.000*	0.003**	0.000
	(0.003)	(0.003)	(0.000)	(0.000)	(0.006)	(0.008)	(0.006)	(0.000)	(0.002)	(0.000)
Panel B – Two-staged least squares (2-SLS)										
Establishments (NAICS 522390)	-0.086***	-0.074***	0.001**	0.001***	-0.054***	-0.036	0.022	0.001**	-0.059*	0.000
	(0.020)	(0.019)	(0.000)	(0.000)	(0.021)	(0.026)	(0.020)	(0.000)	(0.031)	(0.001)
First stage										
Interest rate cap (neighboring state)	-5.783***	-5.794***	-5.785***	-5.785***	-16.070***	-16.220***	-16.070***	-16.070***	-2.699***	-2.699***
	(0.118)	(0.117)	(0.116)	(0.116)	(0.508)	(0.538)	(0.508)	(0.508)	(0.102)	(0.102)
Observations	145,472	146,414	153,623	153,623	20,442	18,325	20,442	20,442	638,391	638,391

Notes: In Panel A, we perform ordinary least squares regressions and in Panel B, we perform two-stage least squares (2-SLS) estimation in the above table to analyze outcomes included in Tables 4 and 10. The instrumental variable used in the above analysis is indicator of interest rate cap imposed by a neighboring state. Columns (1)-(4) are based on CPS monthly data (January 2002 - December 2019) and include total hours per week and usual hours per week worked, indicator of having multiple jobs, and indicator of being absent from work in the prior week (not laid off). Columns (5)-(8) are based on CPS ASEC (2002-2019) data and include usual hours per week worked in the prior year, hours worked in the prior week, number of weeks worked in the prior year, and indicator of being absent from work in the prior week. Columns (9)-(10) are based on ACS samples (2005-2019) and include usual hours per week worked in the prior year and indicator of being absent from work in the prior week. The regression sample is based on employed individuals aged 25-64. All our regressions are weighted by survey-based person-level sample weights. The regressions include control for race, ethnicity, age and age-squared, education, marital status, state-level unemployment rates along with county and time (or year for annual surveys) fixed effects. Robust standard errors are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 7: States that Passed Interest Rate Ceilings between 2007 - 2019

Treated State	Policy Date
Arizona	July, 2010
Arkansas	March, 2011
Colorado	February, 2019
District of Columbia	May, 2008
Montana	January, 2011
New Hampshire	January, 2009
Oregon	July, 2007
South Dakota	November, 2016

[a] *Notes*: For details on each particular state and control states, see Dasgupta and Mason (2020), especially the online appendix. States that have interest rate ceilings for the entirety of the sample period, 2007 - 2019, i.e., the "always treated" are dropped from the analyses. Ohio passed an interest rate ceiling that took effect in early 2019, but the legislation allowed small-dollar credit lenders to offer installment loans. Some payday lenders shut down operations while others did not. We therefore do not consider Ohio as one of the treated states. For the state-level analyses, we drop Pennsylvania from the sample.

3.5 Robustness Check: Centroid to Border

In this subsection, we alter the research design to be consistent with the previous studies that rely on neighboring states' policies. Rather than defining 'close' as 25-miles centroid-to-centroid, we instead define it as 25- (and then 15-) miles centroid-to-border. This latter design dramatically increases our sample size, but implicitly assumes that the AFS suppliers locate exactly on the border, which we argue would bias our results downward.[33] We re-estimate the effects of the neighboring interest rate ceiling on various labor outcomes using the 25-mile centroid-to-border definition, and then again using a 15-mile definition. At a 25-mile radius, the signs are similar to the coefficients in Table 4, but the magnitudes are indeed smaller. Taking advantage of the increased sample size, we run the centroid-to-border analysis using a 15-mile radius. The results confirm the identification from our centroid-to-centroid design (the exception being likelihood of holding multiple jobs). The detailed values are in Table A4 in the Appendix.

Section 4: Further Robustness and Supplemental Analyses

In this section, we use an alternative identification strategy and two new datasets. The change in identification strategy is exploiting the spatiotemporal variation in state-level interest rate

[33] We find that payday loan usage is lower using the definition of 25-mile radius centroid-to-centroid rather than 25-mile centroid-to-border. See our regression results using SHED data in Table A3 in the Appendix.

ceilings. In other words, in this section, we compare 'hours worked' for *New Hampshire* residents as opposed to Massachusetts residents before- and after its own interest rate ceiling; the control group is workers in states that faced no such cap throughout the sample time period. We also change the sample period to states that passed prohibitively-low interest rate ceilings between 2007 and 2019. Table 7 lists the analyzed states and time periods of the policy change.[34] We start these analyses in 2007 because of data limitations (see below); we end the sample in 2019 to avoid any influence of COVID-19. There are more states in this analysis compared to that of the county level because the additional states are located in more geographically expansive parts of the country, i.e., counties' centroids are generally farther than 25 miles from a neighboring state's county's centroid. More importantly, none of the neighbors of the additional states had restrictive interest rate ceilings in place during the sample period. Arkansas, for example, shares a border with six states, none of which had a restrictive interest rate ceiling in effect during our sample period.

To be sure, in these state-level analyses, endogeneity could be a concern. States may pass such legislation due to unobserved reasons that are correlated with the labor market. Benmelech and Moskowitz (2010), for instance, find that state usury laws were passed to appease special interest groups to limit competition.

We use two alternative data sets: one more granular, the NLSY97, and the other is aggregated, the Current Employment Statistics (CES) from the BLS, the latter is sometimes known as "the establishment survey." The NLSY97 is a biennial longitudinal survey (2007 - 2017). The longitudinal aspect of the survey allow us to control for individual fixed effects, thereby allowing us to account for time-invariant factors not tied to the demographic controls in our main analysis, i.e., race, ethnicity, education, and age. One such factor could be early personal experience with, say, financial literacy, which could vary with geography and state of residence. An individual fixed effect would capture any potential impact of such influences.

Our CES data are aggregated and measured from firms at the monthly frequency. The data begin in 2007 and allow for a select analysis of some sub-industries, e.g., manufacturing. The CES provides a check on the other results since the data come from firms rather than workers.

Our identification strategy relies on difference-in-differences with staggered intervention pe-

[34]See the online appendix to Dasgupta and Mason (2020) for some details on the legislation of these states and also the states in the control group, i.e., states that did not have interest rate ceilings throughout the sample period.

riods, estimated using the combination of the within- and between estimators. Formally, we estimate the following regression with the CES data:

$$LS_{st} = \alpha_s + \lambda_t + \beta \cdot CAP_{st} + e_{st} \tag{4.1}$$

where LS is labor supply and CAP is the treatment, namely, the dummy variable for whether there is a restrictively-low interest ceiling—a rate cap—in place at time (month) t for state s. Alpha is the state fixed effect, while lambda is the time (month) fixed effect; e_{st} is the estimated residual. We do not include any covariates, in part, because of the lack of relevant, non-confounding state-level data at the monthly frequency. For the NLSY97 data, we essentially run Equation 3.1, but swapping out c' (county) with s (state).

4.1 Individual Level Analysis from the NLSY97

We run two DID regressions using the NLSY97 data. First, we estimate a standard difference-in-differences design via a two-way fixed effects (TWFE) regression. Second, we use the Extended Two Way Fixed Effects Estimator (EWTFE), based on Wooldridge (2021).[35]

The DID regression results are in Table 8. The findings largely corroborate the county analysis. 'Hours worked' is positive and statistically significant at conventional levels in the TWFE analysis; the effect is just about as strong for employees who are paid hourly. The ETWFE analyses are not statistically significant at conventional levels, but they have a positive sign. Here, too, the hourly workers demonstrate a slightly stronger magnitude. We also checked whether workers hold multiple jobs (columns 4 through 8). Although the signs of the coefficient are positive, consistent with an increase in labor supply, they are not statistically discernible from zero at conventional levels.

4.2 Aggregated Data: Hours Worked Measured from Firms

Our CES results can be found in Table 9. Most of the coefficient estimates have a positive sign. The "supersector" (BLS terminology) of 'goods-producing' and its sub-supersector, manufacturing, have the largest magnitudes. Table 9 is consistent with our main county-level findings

[35]The ETWFE is run in Stata using the JWDID package. We contemplated using the CS-DID estimator of Callaway and Sant'Anna (2021), but we opted not to because it works best with longer time panels. Furthermore, there aren't many treated states, i.e., states that implement an interest rate ceiling between 2007 and 2017.

Table 8: Effect of Interest Rate Cap on Labor Supply - NLSY97

	Hours Worked per Week All Jobs		Hours Worked per Week Hourly Employees		Number of Jobs All Jobs		Number of Jobs Hourly Employees	
	TWFE	ETWFE	TWFE	ETWFE	TWFE	ETWFE	TWFE	ETWFE
Interest Rate Cap	1.614**	0.817	1.611***	0.934	0.051	0.054	0.021	0.042
	(0.638)	(0.581)	(0.609)	(0.583)	(0.039)	(0.039)	(0.035)	(0.034)
Individuals	5,755	5,705	5,652	5,596	5,778	5,731	5,778	5,731
Observations	28,055	27,911	26,950	26,701	28,606	28,476	28,606	28,476
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey and State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Characteristics	Yes	-	Yes	-	Yes	-	Yes	-
Sample Mean (Pre-Treatment)	42.11		41.81		1.432		1.303	

Notes: TWFE denotes two-way fixed effects. ETWFE stands for extended two-way fixed effects, which comes from Wooldridge (2021), estimated using the Stata package JWDID. The linear two-way fixed effects regressions include control for individual characteristics, which include age in years, household size, household income, binary indicator of whether a person is married, and categorical indicator of number of pre-school aged children (0 for no child, 1 for 1-2 children, and 2 for more than 2 children). The JW-DID regressions include a categorical control for race and ethnicity. The JWDID package only allows one time-invariant control. Robust standard errors are clustered on the individuals-level and reported in parentheses. ***p $\leq 0.01, **p \leq 0.05, *p \leq 0.10$.

in Table 4: tighter liquidity constraints lead to an increase in hours worked.

It's worth noting some essential differences between the CPS measure of hours worked and the CES measure. The CPS is self-identified, which is more likely to suffer from measurement error. Nonetheless, the CPS has the advantage of including work performed in the informal sector ("shadow economy"), unregistered firms, and the self-employed. The CES, conversely, draws its sample population from firms that partake in states' unemployment insurance. In this regard, the CPS is a broader measure and perhaps better suited to the population most directly affected by interest rate ceilings on small-dollar cash loans.

A consistent finding in Table 9 is (other than the CS-DID implementation) is that 'hours worked' in the leisure and hospitality industry falls.[36] A tentative hypothesis is that this decrease may be the result of the link between small-dollar cash loans and consumption expenditures. Pew Research conducted focus group interviews with payday loan customers. When asked (paraphrasing) "what would you do if you were short on cash and these loans were no longer available," the overwhelming response (81%) noted 'cut back on expenditures'.[37] Presumably, the first cut would be to discretionary spending. We leave this hypothesis as an avenue for future research.

[36]The two-digit supersector code is 70. We also did a preliminary analysis on the "employee to population ratio" for this supersector. The estimated coefficient was positive, but statistically insignificant.

[37]See page 16 of the 2012 report *Payday Lending in America: Who Borrows and Why* here: https://www.pew.org/en/research-and-analysis/reports/2012/07/19/who-borrows-where-they-borrow-and-why

Table 9: Effect of Interest Rate Ceiling on Hours Worked - Measured from Firms

	TWFE	ETWFE	DID2S	CS-DID
Total Private	0.027	0.109	0.101	0.331
	(0.138)	(0.132)	(0.165)	(0.240)
Total Goods-Producing	0.684	0.978***	0.967	0.852
	(0.435)	(0.202)	(0.899)	(1.048)
Total Private Service-Producing	-0.040	0.119	-0.111	0.364
	(0.172)	(0.142)	(0.213)	(0.254)
Total Manufacturing	0.699*	0.295	0.257	0.641
	(0.365)	(0.273)	(0.487)	(0.733)
Total Leisure and Hospitality	-0.886***	-0.683***	-0.681**	0.130
	(0.008)	(0.182)	(0.341)	(0.705)

Notes: TWFE stands for two-way fixed effects; ETWFE is the extended two-way fixed effects estimator of Wooldridge (2021), implemented using Stata's user-written package JWDID; DID2S is the two-stage difference-in-differences estimator of Gardner (2022); CS-DID is the estimator of Callaway and Sant'Anna (2021). CES is the Current Employment Statistics from the US Bureau of Labor Statistics (BLS). Data are weekly hours worked from establishments participating in the unemployment insurance program. The frequency is monthly. The "total" represents 'hours worked' from both types of employees, supervisory- as well as non-supervisory; 'total' also entails urban- and rural areas. States that are "always treated," i.e., states that had a prohibitively-low interest rate ceiling throughout the sample, e.g., Massachusetts, have been eliminated. We also eliminate Pennsylvania from this sample; Ohio is a control state and Oregon is a treated state. The total sample size is for all of the regressions is 6,396 with the exception of "total manufacturing," which has a reduced number of states available with a full sample. Standard errors are cluster-robust, being clustered at the state level.*** p<0.01, ** p<0.05, * p<0.1.

Section 5: Mechanisms and Additional Consequences

The preceding analyses, when taken together, demonstrate that labor supply increases as a result of a binding interest rate ceiling that affects an already-liquidity-constrained population. But what is the mechanism(s) at work? In other words, in what way do interest rate restrictions on small-dollar loans cause an increase in labor supply on the intensive margin? One possibility is that workers simply work more hours at their existing job. This potential mechanism seems to conflict with the bulk of findings within empirical labor economics: workers who generally want to work more hours—regardless of their credit availability—find it difficult to do so. Rather, these workers generally need to switch jobs in order to work more hours.[38]

To test the job-switching mechanism, we analyze CPS data on whether workers who face the neighboring state's binding interest rate ceiling are more likely to switch jobs. Technically, the question asks if the respondent is still with the same employer as the year prior. Although an on-the-job search is costly in terms of time and effort, it may be worthwhile if an important source of cash credit somewhat-suddenly becomes unavailable. Blundell et al. (2008) and Benito and Saleheen (2013) find that workers switch jobs to take advantage of wage increases, e.g., the elasticity of labor supply is nearly nil on the intensive margin for job-stayers, but it is positive for job-switchers. It's possible that the same thing could be happening with the "credit elasticity of labor supply."

If some people are willing to undertake the search for a new job, others may also search for an additional job. There is some reason to believe this to be the case. For example, He and le Maire (2023) and Kumar and Liang (2024) find that credit expansion through mortgage reforms, e.g., relaxing the restrictions on home-equity loans in Denmark and Texas, respectively, reduces labor supply at the aggregated level; the labor force participation rate falls. Column 1 of Table 10 finds that the propensity to hold multiple jobs actually decreases (but not statistically) as the ceilings are put in place—a finding that corroborates the (statistically significant) increase from Table 6. On the other hand, in Tables 4 and 5 show that 'hours at all jobs' has slightly higher effect than 'hours at main job'. One way to reconcile these seemingly conflicting findings is that as cash loans are restricted, some people work more hours or perhaps seek a second job. But this effect may be partially offset by a countervailing force: for some borrowers, the interest

[38]This phenomenon of switching jobs is prevalent in the literature on the elasticity of labor supply. See, for example: Blundell et al. (2008) and Benito and Saleheen (2013) and the sources therein.

rate ceiling relieves them of having to work a second job to service the costly debt payments. When that credit is no longer available, people quit the second job (or gig).

Column 3 of Table 10 contains our results. We do indeed see some evidence. There is a decrease in the likelihood that the worker is still with the same employer. This is consistent with workers changing jobs, perhaps to bargain for more hours or increased flexibility with a new employer. The decrease, while statistically significant, is economically modest, as might be expected given the costs required in finding a new job. Nevertheless, Nawaz et al. (2025) find evidence that credit-rationed consumers do indeed search more for jobs than their non-rationed counterparts.

Even for workers who are unwilling or unable to find a second job or switch jobs or cannot pick up extra shifts at will, there may be another mechanism at their disposal: not taking a leave of absence, e.g., calling in sick to work, cashing out a personal day, etc. This mechanism does not depend on a new would-be employer, job-search technology, or the will of the current employer. In other words, the decision to take a personal leave is more likely to be fully under the control of the worker. Depending on the job and particulars of the employer, this may also be one of the mechanisms available to salaried employees, e.g., cashing out personal time.

Column 2 of Table 10 has the main results for this variable. The sign is positive and statistically significant at the 10% level for the overall sample. Hence, on average, people are less likely to take a personal leave as small-dollar credit is restricted. When broken down by education level, the sign is still positive, but not statistically indistinguishable from zero at conventional levels. These results corroborate the results for this variable in our IV analysis: see columns 4 (CPS monthly) and 8 (CPS-ASEC annual) of Table 6. In this latter analysis, the signs are negative, which means that, all else equal, as the number of small-dollar lenders increases, the propensity to take a personal leave falls. With more credit available, workers no longer need to call into work for additional income. In other words, credit availability allows workers to consume more leisure.

If workers are working more hours and/or switching jobs and calling out of work less frequently, we would expect earnings to increase. We examine this possibility in column 4 of Table 10. Our finding shows an approximate 9% increase in earnings for those with less than a bachelor's degree. Our marginal effect on hours worked from Table 4 shows an increase of approximately 1.36% relative to the sample mean, which is outside the 95% confidence interval on earnings. The implication is that workers are employing multiple facets of their labor market decisions, including not missing shifts, changing jobs, and possibly adding an additional job. In earnings levels, a 95% confidence interval around the marginal effect in allows us to rule out a weekly earnings impact of any amount greater than \$78.65 per week or \$157.30 every two weeks. This is less than the average payday loan. Thus, we can conclude that people cope with a liquidity constraint through the labor market, but not *exclusively* through the labor market; they must be relying on some other coping mechanism.[39]

These earnings results may appear to be rather large, but it is important to note that it is not just credit *per se*, but rather small-dollar credit, which is unique in the market for credit insofar as it is a *cash* loan. Within certain pockets of the economy, credit cards cannot be used. Cash, on the other hand, can be used nearly everywhere. If we are correct about this mechanism—and its interpretation—then this result synthesizes with the findings of Miller and Soo (2020), who find that when formal credit supply expands, e.g., credit card limits increase, payday loan demand remains relatively unchanged. Why? The authors speculate that it is the special cash nature of payday loans that make them imperfect substitutes to formal credit. Our evidence here provides some corroboration—albeit quite indirectly—of that speculation.

[39]This has some economic intuition. If a borrower's utility is a function of consumption, leisure, and, say, familial ties, then when hit with a liquidity constraint, they diversify their coping strategies through cutting back on expenditures, working more, and borrowing from family or friends, respectively.

Table 10: Effect of small dollar loan interest rate cap on additional labor market outcomes

	CPS Monthly – 2002-2019 (Col 1-4)				CPS ASEC – 2002-2019 (Col 5-6)		ACS – 2005-2019 (Col 7-8)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Max. distance threshold = 25 miles	Multiple jobs	Absent from work	Same employer	Log weekly earnings	Absent from work	Log yearly earnings	Absent from work	Log yearly earnings
Panel A– Sample mean (Overall):	0.056	0.036	0.978	1226.04	0.029	42292.68	0.026	39973.56
Interest rate cap	-0.005	-0.006*	-0.004	0.052*	-0.015***	0.018	-0.001	0.006
	(0.004)	(0.003)	(0.003)	(0.031)	(0.005)	(0.036)	(0.002)	(0.009)
Observations	153,623	153,623	99,043	35,209	20,442	18,743	638,391	603,986
Panel B– Sample mean (< Bachelor's degree)	0.048	0.034	0.978	937.49	0.029	30642.43	0.027	28368.30
Interest rate cap	-0.004	-0.005	-0.008**	0.089**	-0.008	0.019	-0.001	0.001
	(0.005)	(0.004)	(0.003)	(0.034)	(0.006)	(0.050)	(0.003)	(0.013)
Observations	87,612	87,612	55,381	20,085	11,946	10,840	379,436	356,704
Panel C– Sample mean (≥ Bachelor's degree)	0.067	0.040	0.977	1644.03	0.030	60046.95	0.024	58466.76
Interest rate cap	-0.006	-0.005	0.002	0.019	-0.030**	0.018	-0.001	0.001
	(0.007)	(0.003)	(0.004)	(0.044)	(0.015)	(0.027)	(0.002)	(0.015)
Observations	66,011	66,011	43,662	15,124	8496	7903	259,155	247,282

Notes: In the above regressions, the treatment status is determined by whether a county from a state where payday lending is banned is within 25-mile radius (centroid-based distance) of a county from another state where payday lending industry existed at least for a portion of the analysis period. Columns (1)-(4) are based on CPS monthly data (January 2002 - December 2019) and include indicators of whether an individual had multiple job holdings, whether they were absent from work in the prior week (not laid off), whether they were employed with the same employer from last month, and log weekly earnings (in 2020$). Columns (5)-(6) are based on CPS ASEC (2002-2019) data and include indicator of whether an individual was absent from work in the prior week and log yearly earnings (in 2020$). Columns (7)-(8) are based on ACS samples (2005-2019) and include similar indicators as CPS ASEC analysis in columns (5)-(6). The regression sample is based on employed individuals aged 25-64. We perform Gardner's ((2022)) two-stage differences-in-difference (2-stage DID) for the above analysis. All our regressions are weighted by survey-based sample weights. For earnings measure from the CPS monthly data, we use person-level weight that is recommended to be used for the sample used in "earner study" questions (called 'earnwt'). The regressions include control for race, ethnicity, age and age-squared, education, marital status, state-level unemployment rates along with county and time (or year for annual surveys) fixed effects. We also report pre-treatment sample means of the outcome variables above. For measures of earnings, we report the sample mean estimates in 2020$ values. Robust standard errors are reported in parentheses and are clustered at the county level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

There are a couple of other potential mechanisms that we are unable to test with our data. One such mechanism is workers borrowing money from their employer and then paying it back in kind through working more hours, picking up additional shifts, etc. There is at least some evidence of this: 17% of Pew (2012) respondents said that they would borrow from their employer if payday loans were no longer available. Note that this mechanism, if it exists, would likely be pretty narrow: it would almost certainly work exclusively through small firms rather than medium- or large ones. Another potential mechanism is the self-employed. Sole proprietors can increase labor supply, at least in principle, or, at the very least they can produce "sweat equity" (Bhandari & McGrattan, 2021). This kind of labor supply, e.g., effort exerted or intensity of work doesn't show up in the formal statistics. We are unaware of any data, administrative, survey, or otherwise, that links payday lending to the self-employed, although Bolen et al. (2020) highlight some sources (footnote 36) showing that auto title loan borrowers have higher rates of self-employment.

Section 6: Conclusion, Policy Implications, and Welfare

Standard economic theory implies that consumers smooth consumption. Credit markets are often the medium through which such smoothing takes place. When credit markets are rendered inaccessible, consumption is predicted to fall. We also document that this holds for the consumption of leisure as well: when costly credit markets are shut down—particularly through prohibitively low APR interest rate ceilings—labor supply increases through multiple avenues. It seems consumers are maintaining the marginal rate of substitution of the consumption of goods and services and the consumption of leisure, as Rossi and Trucchi (2016) note- and find in their paper, theoretically and empirically.

From a policy perspective, the debate about high-cost loans and how best to regulate them comes up from time to time. Interest rate ceilings are often proposed at the state- and federal levels. Previous studies examine outcomes such as bankruptcies, delinquencies, credit scores, and material well-being (consumption). Our paper demonstrates the labor market outcomes of such regulation. In response to what people will do in Minnesota once payday loans are effectively, banned the director of non-profit lender Exodus Lending said "work more hours, take on a second job, sell your plasma—just the things that people do who don't go to payday lenders, and that's most people."[40] Dooley and Gallagher (2024) find a link between plasma selling and payday loans. We find that hours work do indeed increase, but the propensity to take on a second job largely does not.

Regarding welfare implications, a revealed preference argument suggests that if workers could have worked more but chose not to, then any policy change that induces an increase in hours worked would be a welfare-reducing one. Furthermore, if workers are going to work when they are sick, then their productivity may suffer. However, if the worker is in an industry where there are productivity gains from learning-by-doing, then working more may ultimately, in the long-run, boost wages or other forms of compensation. If the worker is unaware of these learning-by-doing gains or heavily discounts the long-run wage increases, then the policy change could be welfare enhancing.

Section 7: Appendix

[40] See: https://www.seattletimes.com/business/are-state-interest-rate-caps-an-automatic-win-for-borrowers/

Table A1: County-Level Analysis: Treated- and Control Counties - ACS and CPS

State	County	CPS	Treated	Access from	Shut out from
Connecticut	New London County	No	0	Rhode Island	-
Connecticut	Windham County	Yes	0	Rhode Island	-
Maryland	Anne Arundel County	Yes	1	-	Washington DC
Maryland	Caroline County	No	0	Delaware	-
Maryland	Carroll County	Yes	1	-	Pennsylvania
Maryland	Cecil County	Yes	0	Delaware	-
Maryland	Charles County	Yes	0	Virginia	-
Maryland	Howard County	Yes	1	-	Washington DC
Maryland	Montgomery County	Yes	0	Virginia	-
Maryland	Prince George's County	Yes	0	Virginia	-
Maryland	Washington County	Yes	1	-	Pennsylvania
Massachusetts	Bristol County	Yes	0	Rhode Island	-
Massachusetts	Essex County	Yes	1	-	New Hampshire
New Jersey	Camden County	Yes	1	-	Pennsylvania
New Jersey	Gloucester County	No	1	-	Pennsylvania
New Jersey	Hunterdon County	Yes	1	-	Pennsylvania
New Jersey	Mercer County	Yes	1	-	Pennsylvania
New Jersey	Sussex County	Yes	1	-	Pennsylvania
New Jersey	Warren County	Yes	1	-	Pennsylvania
New York	Broome County	No	1	-	Pennsylvania
North Carolina	Alleghany County	No	0	Virginia	-
North Carolina	Anson County	No	0	South Carolina	-
North Carolina	Ashe County	No	0	Tennessee	-
North Carolina	Camden County	No	0	Virginia	-
North Carolina	Caswell County	No	0	Virginia	-
North Carolina	Cherokee County	No	0	Tennessee	-
North Carolina	Cleveland County	No	0	South Carolina	-
North Carolina	Gaston County	No	0	South Carolina	-
North Carolina	Graham County	No	0	Tennessee	-
North Carolina	Robeson County	Yes	0	South Carolina	-
North Carolina	Rockingham County	No	0	Virginia	-
North Carolina	Union County	Yes	0	South Carolina	-
West Virginia	Berkeley County	No	0	Virginia	-
West Virginia	Brooke County	No	0	Ohio	-
West Virginia	Cabell County	No	0	Ohio	-
West Virginia	McDowell County	No	0	Virginia	-
West Virginia	Monongalia County	No	1	-	Pennsylvania
West Virginia	Monroe County	No	0	Virginia	-
West Virginia	Pleasants County	No	0	Ohio	-

Table A2: County-Level Analysis: Alternative DID methodologies

Max. distance threshold = 25 miles	(1)	(2)	(3)
Panel A: Total hours/ week (CPS Monthly)	TWFE	ETWFE	CS-DID
Interest rate cap	0.500**	0.365	2.915*
	(0.217)	(0.234)	(1.619)
Observations	145,472	145,472	145,472
Panel B – Usual hours/ week (CPS Monthly)			
Interest rate cap	0.428*	0.368*	3.470**
	(0.214)	(0.218)	(1.670)
Observations	146,414	146,414	146,414
Panel C – Usual hours/ week (CPS ASEC)			
Interest rate cap	0.871***	0.503*	0.696
	(0.239)	(0.293)	(0.895)
Observations	20,442	20,442	20,442
Panel D – Usual hours/ week (ACS)			
Interest rate cap	0.160*	0.147	0.414
	(0.950)	(0.099)	(0.266)
Observations	638,591	638,591	638,591

Notes: TWFE: Two-way fixed effects model; ETWFE - Extended TWFE model proposed by Wooldridge (2021); CSDID - DID methodology proposed by Callaway and Sant'Anna (2021). In the above table, we estimate alternative DID specifications to analyze the effect of small dollar loans interest rate cap on labor supply (hours worked) using individual-level samples used in Table 4. *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Measures of access to payday lending locations on payday loan use

	(1)	(2)	(3)	(4)
	Centroid to Border: 25-mile radius	Centroid to Border: 15-mile radius	Centroid to Centroid: 40-mile radius	Centroid to Centroid: 25-mile radius
Restriction on payday access	-0.006	-0.011	-0.005	-0.018*
	(0.008)	(0.008)	(0.008)	(0.009)
Observations	1656	1147	1835	798

Notes: The above table reports regression results based on the 2018-2019 SHED to analyze the link between restrictions on access to payday loans and consumers' payday loan use. We use SHED's confidential ZIP code information to identify counties using Housing and Urban Development's (HUD) ZIP crosswalk files. The regression is based on counties from always-banning states, similar to the empirical approach in the analysis in Table 4. Based on the various distance-based specifications used above to define proximity to counties from neighboring states, the regressions compare counties from always-banning states that have proximity access to payday permissive jurisdictions to counties from always-banning states that once had access to a payday permissive jurisdiction, but that access got restricted due to the implementation of interest rate cap on small dollar loans. In columns (1)-(2), we analyze centroid-to-border specifications (25-mile and 15-mile radii). In columns (3)-(4), we analyze centroid-to-centroid specification (40-mile and 25-mile radii). *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A4: Effect of small dollar loan interest rate cap on labor market outcomes: Centroid to Border specification

	Total hours/week	Main job hours/week	Multiple jobs	Absent from work	Same employer	Log weekly earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A - Maximum distance 25 miles						
Sample mean	41.122	40.366	0.057	0.037	0.979	1228.335
Interest rate cap	0.118	0.089	-0.001	-0.002	-0.003	0.037
	(0.248)	(0.297)	(0.005)	(0.002)	(0.003)	(0.029)
Observations	270,570	272,277	285,250	285,250	184,722	65,173
Panel B - Maximum distance 15 miles						
Sample mean	41.212	40.421	0.059	0.035	0.979	1227.518
Interest rate cap	0.578***	0.683***	-0.012***	-0.002	-0.005*	0.084***
	(0.226)	(0.272)	(0.005)	(0.004)	(0.003)	(0.033)
Observations	169,627	170,684	179,019	179,019	114,218	40,849

Notes: The above regressions look at the effect of small dollar loan interest rate caps using an alternative specification in which access to a payday lending jurisdiction is measured using centroid-to-border distance. While in the primary analysis, we measure payday loan access using centroid-to-centroid distance based thresholds between counties, in the above table we consider the distance from an 'always-banning' county's centroid to any point on the border of another proximate county across the state border. We use the 2002-2019 CPS monthly samples for the above regressions. In Panel A, we show the specification based on a maximum distance threshold of 25 miles (centroid-to-border). In Panel B, we present results for a maximum threshold of 15 miles. *** p<0.01, ** p<0.05, * p<0.1.

References

Adams, W., Einav, L., & Levin, J. (2009). Liquidity constraints and imperfect information in subprime lending. *American Economic Review*, *99*(1), 49–84.

Agarwal, S., Gross, T., & Mazumder, B. (2016). How did the great recession affect payday loans. *Economic Perspectives*, *40*(2), 1–12.

Allcott, H., Kim, J., Taubinsky, D., & Zinman, J. (2022). Are high-interest loans predatory? theory and evidence from payday lending. *The Review of Economic Studies*, *89*(3), 1041–1084.

Apostolidis, C., Brown, J., & Farquhar, J. (2023). Stigma in payday borrowing: a service ecosystems approach. *European Journal of Marketing*, *57*(10), 2737–2764.

Athreya, K. (2008). Credit access, labor supply, and consumer welfare. *FRB Richmond Economic Quarterly*, *94*(1), 17–44.

Attanasio, O. P., & Weber, G. (2010). Consumption and saving: models of intertemporal allocation and their implications for public policy. *Journal of Economic literature*, *48*(3), 693–751.

Baker, A., Callaway, B., Cunningham, S., Goodman-Bacon, A., & Sant'Anna, P. H. (in press). Difference-in-differences designs: A practitioner's guide. *Journal of Economic Literature*.

Barth, J. R., Hilliard, J., Jahera, J. S., & Sun, Y. (2016). Do state regulations affect payday lender concentration? *Journal of Economics and Business*, *84*, 14–29.

Benito, A., & Saleheen, J. (2013). Labour supply as a buffer: evidence from uk households. *Economica*, *80*, 698–720.

Benmelech, E., & Moskowitz, T. J. (2010). The political economy of financial regulation: Evidence from us state usury laws in the 19th century. *The journal of finance*, *65*(3), 1029–1073.

Bertrand, M., & Morse, A. (2009). What do high-interest borrowers do with their tax rebate? *American Economic Review*, *99*(2), 418–423.

Bhandari, A., & McGrattan, E. R. (2021). Sweat equity in us private business. *The Quarterly Journal of Economics*, *136*(2), 727–781.

Bhutta, N. (2014). Payday loans and consumer financial health. *Journal of Banking & Finance*, *47*, 230–242.

Bhutta, N., Goldin, J., & Homonoff, T. (2016). Consumer borrowing after payday loan bans. *The Journal of Law and Economics*, *59*(1), 225–259.

Blundell, R., Brewer, M., & Francesconi, M. (2008). Job changes and hours changes: understanding the path of labor supply adjustment. *Journal of Labor Economics*, *26*(3), 421–453.

Blundell, R., Pistaferri, L., & Saporta-Eksten, I. (2016). Consumption inequality and family labor supply. *American Economic Review*, *106*(2), 387–435.

Bolen, J. B., Elliehausen, G., & Miller Jr, T. W. (2020). Do consumers need more protection from small-dollar lenders? historical evidence and a roadmap for future research. *Economic Inquiry*, *58*(4), 1577–1613.

Callaway, B., & Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of econometrics*, *225*(2), 200–230.

Campbell, D., Martinez-Jerez, F. A., & Tufano, P. (2012). Bouncing out of the banking system: An empirical analysis of involuntary bank account closures. *Journal of Banking & Finance*, *36*(4), 1224–1235.

Carter, S. P. (2015). Payday loan and pawnshop usage: The impact of allowing payday loan rollovers. *Journal of Consumer Affairs*, *49*(2), 436–456.

Carvalho, L., Olafsson, A., & Silverman, D. (2024). Misfortune and mistake: The financial conditions and decision-making ability of high-cost loan borrowers. *Journal of Political Economy*, *132*(9), 3173–3213.

Caskey, J. P. (2012). Payday lending: New research and the big question. In P. N. Jefferson (Ed.), *The oxford handbook of the economics of poverty.* Oxford University Press.

Chen, J., & Roth, J. (2024). Logs with zeros? some problems and solutions. *The Quarterly Journal of Economics*, *139*(2), 891–936.

Cui, C. (2017). Cash-on-hand and demand for credit. *Empirical Economics*, *52*(3), 1007–1039.

Dao Bui, K., & Ume, E. S. (2020). Credit constraints and labor supply: Evidence from bank branching deregulation. *Economic Inquiry*, *58*(1), 335–360.

Dasgupta, K., & Mason, B. J. (2020). The effect of interest rate caps on bankruptcy: Synthetic control evidence from recent payday lending bans. *Journal of Banking & Finance*, *119*, 105917.

Dettling, L. J., & Hsu, J. W. (2021). Minimum wages and consumer credit: Effects on access and borrowing. *The Review of Financial Studies*, *34*(5), 2549–2579.

Di Maggio, M., Ma, A., & Williams, E. (2025). In the red: Overdrafts, payday lending, and the underbanked. *The Journal of Finance*, *80*(3), 1691–1738.

Dobbie, W., & Skiba, P. M. (2013). Information asymmetries in consumer credit markets: Evidence from payday lending. *American Economic Journal: Applied Economics*, *5*(4), 256–282.

Dobridge, C. L. (2018). High-cost credit and consumption smoothing. *Journal of Money, Credit and Banking*, *50*(2-3), 407–433.

Dooley, J. M., & Gallagher, E. A. (2024). Blood money: Selling plasma to avoid high-interest loans. *The Review of Financial Studies*, *37*(9), 2779–2816.

Elliehausen, G., & Hannon, S. M. (2024). Fintech and banks: Strategic partnerships that circumvent state usury laws. *Finance Research Letters*, *64*, 105387.

Fekrazad, A. (2020). Impacts of interest rate caps on the payday loan market: Evidence from rhode island. *Journal of banking & finance*, *113*, 105750.

Gardner, J. (2022). Two-stage differences in differences. *arXiv preprint arXiv:2207.05943*.

Gross, D. B., & Souleles, N. S. (2002). Do liquidity constraints and interest rates matter for consumer behavior? evidence from credit card data. *The Quarterly journal of economics*, *117*(1), 149–185.

He, A. X., & le Maire, D. (2023). Household liquidity constraints and labor market outcomes: Evidence from a danish mortgage reform. *The Journal of Finance*, *78*(6), 3251–3298.

Jappelli, T., & Pistaferri, L. (2010). The consumption response to income changes. *Annu. Rev. Econ.*, *2*(1), 479–506.

Jappelli, T., & Pistaferri, L. (2017). *The economics of consumption: theory and evidence.* Oxford University Press.

Karlan, D., & Zinman, J. (2010). Expanding credit access: Using randomized supply decisions to estimate the impacts. *The Review of Financial Studies*, *23*(1), 433–464.

Kumar, A., & Liang, C.-Y. (2024). Labor market effects of credit constraints: Evidence from a natural experiment. *American Economic Journal: Economic Policy*, *16*(3), 1–26.

Labanca, C., & Pozzoli, D. (2022). Constraints on hours within the firm. *Journal of Labor Economics*, *40*(2), 473–503.

Lawrence, E. C., & Elliehausen, G. (2008). A comparative analysis of payday loan customers.

Contemporary Economic Policy, *26*(2), 299–316.

Lukongo, O. E., & Miller, T. (2018). Evaluating the spatial consequence of interest rate ceiling using a spatial regime change approach. *The American Economist*, *63*(2), 166–186.

Lukongo, O. E. B., & Miller Jr, T. W. (2022). The cost of rate caps: Evidence from arkansas. *Journal of Financial Research*, *45*(4), 881–909.

Lusardi, A., Schneider, D., & Tufano, P. (2011). Financially fragile households: Evidence and implications. *Brookings Papers on Economic Activity*, *2011*(1), 83–134.

Melzer, B. T. (2011). The real costs of credit access: Evidence from the payday lending market. *The Quarterly Journal of Economics*, *126*(1), 517–555.

Melzer, B. T. (2018). Spillovers from costly credit. *The Review of Financial Studies*, *31*(9), 3568–3594.

Melzer, B. T., & Morgan, D. P. (2015). Competition in a consumer loan market: Payday loans and overdraft credit. *Journal of Financial Intermediation*, *24*(1), 25–44.

Miller, S., & Soo, C. K. (2020). *Does increasing access to formal credit reduce payday borrowing?* (Tech. Rep.). National Bureau of Economic Research.

Miller Jr, T. W. (2019). How do small-dollar, nonbank loans work? *Mercatus Research Paper*.

Morgan, D. P., Strain, M. R., & Seblani, I. (2012). How payday credit access affects overdrafts and other outcomes. *Journal of money, Credit and Banking*, *44*(2-3), 519–531.

Nawaz, M., Koirala, N. P., & Butt, H. (2025). Labor supply as a buffer: The implication of credit constraints in the us. *Journal of Risk and Financial Management*, *18*(6), 299.

Pijoan-Mas, J. (2006). Precautionary savings or working longer hours? *Review of Economic dynamics*, *9*(2), 326–352.

Prager, R. A. (2014). Determinants of the locations of alternative financial service providers. *Review of Industrial Organization*, *45*, 21–38.

Ramirez, S. R. (2019). An examination of firm licensing behaviour after a payday-loan ban. *Applied Economics*, *51*(46), 5090–5103.

Ramirez, S. R., & Harger, K. (2020). Identifying border effects of payday-lending regulations. *Journal of Applied Economics*, *23*(1), 539–559.

Rivlin, G. (2010). Broke, usa: from pawnshops to poverty, inc.: How the working poor became big business. *(No Title)*.

Rossi, M., & Trucchi, S. (2016). Liquidity constraints and labor supply. *European Economic*

Review, *87*, 176–193.

Servon, L. (2017). *The unbanking of america: How the new middle class survives.* Houghton Mifflin Harcourt.

Skiba, P. M. (2014). Tax rebates and the cycle of payday borrowing. *American Law and Economics Review*, *16*(2), 550–576.

Stegman, M. A. (2007). Payday lending. *Journal of Economic Perspectives*, *21*(1), 169–190.

Wooldridge, J. M. (2021). Two-way fixed effects, the two-way mundlak regression, and difference-in-differences estimators. *Available at SSRN 3906345*.

Zhang, C. Y. (2017). Consumption responses to pay frequency: Evidence from 'extra'paychecks. *ACR North American Advances*, *45*, 170–74.

Zinman, J. (2010). Restricting consumer credit access: Household survey evidence on effects around the oregon rate cap. *Journal of banking & finance*, *34*(3), 546–556.