Rationed?
Household Credit and Business Activity Under Usury Limits

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Abstract

Many states have capped consumer loan interest rates to protect households from high cost lenders. Using a triple-difference analysis, we investigate how usury limits affect household borrowing and business activity. We find that the riskiest borrowers are rationed yet they are not less likely to have delinquencies. As small businesses may use consumer loans, we also study business activity under usury limits. Business establishments decrease after usury limits, especially in counties with a substantial small business sector or high rates of self employment. We conclude that consumer usury limits constrain both household credit and real economic activity.

*Chakrabarti, Morgan, Seltzer are at the Federal Reserve Bank of New York, and Garcia is at Columbia University. Thank you to Matteo Crosignani, Daniel Mangrum, Siddhartha Biswa, Joelle Scally, Filipe Correia, Yongqiang Chu and participants at the Fed System Equitable Growth Conference, NYC Metro Real Estate Conference, the 2024 WEAI meetings and the Fed System Regional Conference, the 2024 CEAR-RSI Household Finance Workshop, the 2025 ASSA-AREUEA Annual meetings, and the Philly Fed's Supervision Research Forum Seminar for helpful comments. The authors thank Justin Fisk for discussions and Neel Lahiri, Martin Hiti, Thu Pham, and Gabriel Leonard for helpful assistance. Special thanks to Sarah Zebar for her close involvement in the early stages of this project. The views expressed herein are those of the authors and do not necessarily reflect the views of the Federal Reserve Bank of New York or the Federal Reserve System.

1 Introduction

Usury limits, an ancient type of financial regulation, are spreading across the United States. In 2007, Congress capped interest rates on loans to military personnel at 36 percent and many states since then have followed suit. A bill recently introduced in the Senate, "The Predatory Lending Elimination Act," would apply the 36 percent cap across country.

Advocates for usury limits, a diverse coalition including consumer groups, clergy, and the Bar, claim they protect vulnerable households from exploitative lenders or curb monopoly power. Opponents, drawing on standard price theory, counter that limiting interest rates will lead lenders to ration credit to riskier borrowers. There is also debate about the political economy of usury limits, whether they serve the public good or private interests (see, for example, (Benmelich and Moskowitz, 2010; Glaeser and Scheinkman, 1998; Smith, 1937)).

Our paper contributes to these debates in two ways. We first investigate how recent state usury limits affect household credit and delinquencies. While theory predicts rationing, that prediction may not hold in practice for various reasons. For example, if the affected lenders enjoy market power, usury limits may be able to reduce their interest rates without limiting credit. At the same time, lenders may evade usury limits by, for example, changing the product structure or partnering with parties exempt from usury caps. Finally, even if the affected lenders do contract lending, exempt lenders, namely banks and credit unions, may fill the gap.

We investigate the credit impact of usury limits using credit bureau data on nearly four million households.[1] We observe total debt balances, the number of accounts held, and delinquencies by quarter. We compare "high-risk" borrowers, defined as those in the bottom decile of credit scores, to other, less risky ones. High-risk borrowers' average credit score was 525, versus 748 for others. To identify effects, we compare households in five states that capped consumer loan rates between 2016 and 2021 to counterparts in five states that did not enact limits over that period. We estimate event study models revealing how outcomes for high-risk borrowers changed under usury laws relative to households in control states. The models include household and zip code-by-quarter fixed effects, allowing for us to control for time-invariant household characteristics, and macroeconomic changes occurring in each zip code over time.

[1] Our data are from Consumer Credit Panel (CCP) maintained by the Federal Reserve Bank of New York and Equifax Bureau.

Consistent with standard price theory, we find that credit to the riskiest borrowers contracts sharply under usury limits. Their total debt balances decline about 16.9% relative to the sample mean, when compared to similar borrowers in states without caps. The decrease in balances is primarily driven by reductions in borrowing on the extensive margin, as observed through a reduction in the number of loan accounts per borrower (20% of the sample mean). These results indicate that these borrowers are not substituting toward cheaper sources of credit from mainstream lenders. While less credit might be seen as evidence that usury laws are protecting vulnerable borrowers from high-risk credit, delinquencies among those borrowers do not improve under usury limits. The reduction in credit with no reduction in delinquencies imply that the most vulnerable borrowers lose access to credit after usury limits, with little benefits in terms of improved credit standing.

While credit rationing is the most predictable response to usury limits, in some circumstances, lenders may respond by reallocating credit to marginally safer borrowers (Blitz and Long, 1965). Indeed, Adam Smith saw this reallocation as potential benefit of usury limits (Smith, 1937). Consistent with this prediction, we find increased credit for borrowers in the third, fourth and fifth risk score deciles. This implies that rather than leaving the state, some lenders responded to the usury limit by reallocating credit to borrowers for whom the rate cap is not binding.

Our second question is whether *consumer* loan usury limits constrain small business activity. Small businesses sometimes use personal loans for business purposes (for example Herkenhoff et al. (2021), Bahadir et al. (2024). Robb and Robinson (2012)), so the consumer credit constraints created by usury limits could potentially constrain small business activity in turn.

We first estimate the effects of usury limits on business activity by comparing the dynamics of establishment counts, employment, and wages in counties following the passage of usury limits to those where such laws were never passed. Establishment counts and employment decrease by 1.8% and 2.3%, respectively, suggesting a slight constraint on business activity. Dynamic difference-in-difference regressions show that these effects persist more than a year following these laws. Second, we find evidence that these effects are stronger in counties with high small business activity, defined as the proportion of a county's employment in firms with under 500 employees. Establishment counts and aggregate wages decrease more in small business intensive counties, relative to both counties in the same state and small business intensive counties in untreated states. The latter falls

by more than a third of average wages in small business intensive counties. We find, however, that in a triple-difference specification using small business intensity that the effects on employment in these counties are insignificant. Similarly, we find that the effect is stronger in counties with high populations of self-employed workers. Our results are robust to alternative definitions of small business intensity and exclusion of certain states and/or the years after the COVID pandemic. These findings are consistent with a reduction in business activity, primarily since small business owners lost access to an important source of financing due to usury limits.

Our findings deepen our understanding of how usury limits affect consumers and businesses. First, consistent with much of the literature (see for example Greer (1974), Villegas (1982), Rigbi (2013), Melzer and Schroeder (2017), Cuesta and Sepúlveda (2021), Nelson (2018), Bolen et al. (2023), Jansen et al. (2024), and Bhutta et al. (2016)), we conclude that the recent wave of anti-predatory lending constrains credit to high-risk borrowers. Moreover, our evidence that delinquency does not improve after usury limits calls into question claims by consumer advocates and some researchers that usury limits can reduce borrowing costs (Agarwal et al., 2015). The most novel contribution is our evidence that usury limits on consumer loans also constrain business activity, indicating that usury limits can have real economic effects. To our knowledge, we our the first to find this.

Our findings also add fodder to the political economy debate around usury regulations. Advocates maintain that usury limits serve the public good by lowering borrowing costs, by predatory lenders from exploiting borrowers' behavioral biases, or by providing social insurance (Glaeser and Scheinkman, 1998). Benmelich and Moskowitz (2010) challenge the public good view. Studying 19th century usury limits, they find that states with stricter limits had slower business activity, particularly among smaller firms. Moreover, they find that the stringency of limits was driven more by entrenched political and business power than by borrower protection laws. Our study of 21st century usury limits seems more in line with the private interest view; we find riskier borrowers are rationed without any benefit in terms of lower delinquency and that small business activity slows. The only apparent benefit we find - more credit for safer borrowers - seems unintended and thus not strictly motivated by the public good.

Our paper proceeds as follows. Section 2 discusses the recent resurgence in consumer usury legislation, including the five states we study. Section 3 describes our credit data, empirical strategy,

and presents the results. Sections 4 present the results on business activity. Section 5 concludes and discusses possible avenues for future work.

2 Recent Usury Legislation

Interest rates are among the most commonly regulated of all prices (see, for example, Rockoff (2003) and Glaeser and Scheinkman (1998)). Usury limits also have ancient roots, where Aristotle, Plato, and the Medieval Christian Church all condemned lending at any rate as immoral. In India, during the Buddhist period, the maximum rate was 15 percent per year on secured loans and 60 percent per year on unsecured loans. Even Adam Smith advocated usury limits to advantage "sober" creditors over "prodigal" ones (Smith, 1937).

Usury limits experienced a resurgence in the United States. starting in the mid-1990s, which saw the aggressive entry of payday lenders into the small dollar loan market (Rockoff, 2003). The main motivation for these modern usury limits were typically either to play a redistributionary role, or to guard against lenders exploiting the behavioral biases of borrowers (Glaeser and Scheinkman, 1998; Bertrand and Morse, 2011). The triple digit rates on payday loans (when annualized), plus the fact that some fraction of borrowers rolled over their loans repeatedly, convinced many critics and legislators that payday loans should be banned, either expressly or by usury limits.

The first ever national usury limit in the U.S. was the Military Lending Act of 2007, which prohibits charging military staff interest rates above a thirty-six percent ceiling. This initial legislation frequently leads consumer advocates to view thirty-six percent as the ideal interest rate cap on consumer loans. Accordingly, the Predatory Loan Elimination Act was introduced to the Senate in 2023, which would implement a federal usury limit by extending the 36% cap across the U.S.[2]

Recent usury regulations tend to be more complex than simply a uniform cap applied across the board to all loans and lenders. For example, banks and credit unions are exempt from the usury laws we study, which are targeted at alternative lenders such as installment, payday, and title lenders.[3] The interest limit may also depend on loan size, with looser caps for larger loans. In

[2] See https://www.congress.gov/bill/118th-congress/senate-bill/3549

[3] Ohio's law (HB132) requires small-dollar lenders to state in boldface font on their contracts: "The cost of this loan is higher than the average cost charged by financial institutions, such as banks or credit unions, on substantially similar loans." (Please see this link for the text of the source legislation.)

view of the complexity of these usury laws, our analysis depends only on whether a state enacted major usury regulation in a given year, but not on the specific limit(s).

We study five states that enacted consumer usury limits between 2016 and 2022. Those states, and the effective dates of the usury laws, are South Dakota (2016:Q4), New Mexico (2018:Q1), Ohio (2019:Q2), North Dakota (2021:Q3) and Illinois (2021:Q1). Annual reports from state agencies show substantial declines in the number of small dollar lenders registered in these states after the caps. In Illinois, the number of small dollar licensees declined by more than 50 percent after usury limits were enacted there in 2021.[4] In Ohio, the number of licensee locations declined by 24 percent between 2018 and 2019 while the volume of loans originated fell by 25 percent.[5] In South Dakota, licensed "money lenders" (including installment, payday, and title lenders) declined from 490 to 397 between 2016 and 2017. By contrast, the number of licensed mortgage companies rose from 2806 to 3227, indicating the usury limit did not constrain those lenders.[6] New Mexico passed a usury limit (HB 347) of 175% in April 2017 that took effect in January 2018. Despite the relatively high cap, the number of licensed small loan declined by 11 percent between 2016 and 2018.[7] The 2017 annual reports attributes the decrease that year to the passage of House Bill 347, which led "Several small loan companies either consolidated existing storefront locations or ceased all New Mexico operations."[8] Of 673 small loan companies licensed in NM in 2017, 442 reported making loans at rates above 175%.[9]

While Ohio's cap of 28 percent was potentially the most binding, other features of the law provided possible loopholes. For example the law expressly banned loans for less than $1000 or for under one year, presumably to prohibit payday loans. In response, many former payday lenders repurposed themselves as installment lenders offering larger, longer-term loans.[10] In view of these possible offsets, we report findings excluding Ohio in the appendix.

[4] Illinois Trends Report for Selected Consumer Loan Products for 2021.

[5] Ohio's data covers small dollar lenders, general lenders, consumer installment lenders

[6] The figures for SD are from personal correspondence with SD Department of Labor and Regulation (10/10/2024). See also page 14 in SD Division of Banking Industry Overview.

[7] New Mexico Financial Institutions Division 2018 Annual Report, p. 50.

[8] New Mexico Financial Institutions Division 2018 Annual Report p. 41.

[9] New Mexico's Installment Loan Product Annual Report shows the number of lenders of different types that made loans above 175%. The report does not indicate how many such loans were made.

[10] The number of licensed small dollar lender locations in Ohio fell by 40 between 2019 and 2018 while the number of licensed installment locations rose by 33.

3 Usury Limits and Household Borrowing

This section investigates how household credit outcomes changed after these usury limits took effect. First we look for effects at the state-level, then we focus on effects for high-risk borrowers. We begin by describing our credit data and empirical strategy, and then present results.

3.1 Consumer Credit Panel Data

Our credit data are from the New York Fed/Equifax Consumer Credit Panel (CCP). The CCP provides detailed, quarterly credit profiles for a 5% random sample of American households tracked by the Equifax credit bureau.[11] Our observation period begins in 2015:Q1, 7 quarters before the first event and ends in 2022:Q4, 7 quarters after the final event. This period encompasses the COVID pandemic which might have confounded the effects of the usury limits (by changing credit demand) but we show our results hold if exclude the post-COVID events.

Summary statistics are shown in Table 1. We breakout households in the bottom decile of credit scores ("high-risk" from hereon) versus lower risk households.[12] To understanding household borrowing behavior, we study total debt balances (excluding mortgages and student loans), number of accounts, and average balance per account.[13] The mean for total debt and average loan size are lower for high-risk borrowers, while the number of accounts is higher. This indicates that higher risk borrowers may sometimes borrow many relatively small loans. We also consider whether households are 90 days or more delinquent on any of their debt to understand how usury limits affect households' credit standing. Unsurprisingly, households in the bottom risk decile are delinquent far more frequently than other borrowers, with 57% having some delinquent debt balance.

Figure 1 shows the distribution of risk scores by risk score deciles in the treated and control states. The distributions are broadly similar, implying borrowers living in states without usury limits have similar credit profiles to those in control states. This assures us that borrowers in the control states with no usury limits are a reasonable group of controls for which to compare borrowers in treated states that pass usury limits with.

[11] See Lee and Van der Klaauw (2010) for a detailed description of the data.

[12] The risk deciles are based on the distribution of households in the year before the usury limit, for treated households, and for all sample observations for control households, calculated across the entire sample period.

[13] We exclude student loans and mortgage debt in calculating these variables, as it is not clear why they should be affected by usury limits. However, results are similar if we include student loans and mortgage debt.

3.2 Difference-in-differences analyses

We identify the credit impact of usury limits by comparing changes in outcomes in five states that enacted usury limits, after the usury limit is passed, relative to a set of seven control states without such limits.[14] To that end, we estimate difference-in-difference models:

$$y_{izst} = \beta Treated_s \times Post_{st} + \alpha_i + \alpha_z + \alpha_t + \gamma Age_i + \epsilon_{ist}. \quad (1)$$

The dependent variable, Y_{izst}, equals some outcome for individual i in zip code z in state s at date t. To study changes in household borrowing, we study a borrower's total debt balance, the average balance on a borrower's loan and the number of accounts the borrower has open. To study changes in a household's overall credit profile, we study whether borrowers are more likely to have a delinquency of 90 days or more after usury limits. $Treated_s$ equals 1 for states that enacted a rate cap between 2015:Q1 and 2022:Q4, and zero for control states. $Post_{st}$ equals 1 after state s enacted laws capping rates and zero before. We use a five-quarter estimation window, both pre- and post-treatment. For the control states we keep all observations. The coefficient β captures how each outcome changes for borrowers in states with usury limits after the laws pass, relative to borrowers in other states. Our identifying assumption is that usury laws are not themselves driven by credit trends in the state. We include fixed effects for the individual α_i, their zip code, α_z and the date α_t. We also control for the age of the household head, an important life-cycle determinant of credit demand.[15] The standard errors are clustered by state-quarter.

The empirical design of this paper relies on the staggered rollout of usury limits, across different states. As shown in Goodman-Bacon (2021), if there are differences in treatment effects across time, the difference-in-differences estimator may be difficult to interpret. Note that subsequent methods to address this concern rely on the existence of never-treated units to serve as controls (see, for example, Callaway and Sant'Anna (2021); Sun and Abraham (2021)). As we include a set of control states that never have any usury limits in our test sample, our empirical design incorporates a never-treated group into the analysis. We can therefore use the standard difference-in-differences specification above.

[14] The control states are Alabama, Delaware, Idaho, Missouri, South Carolina, Utah and Wisconsin. We identified these states and the treated states based on annual reports on usury laws by the National Consumer Law Center.

[15] Age is the only personal characteristic available in the CCP.

Table 2 displays the results. For this state-level analysis, we see no significant change in total debt balances or the number of accounts after usury limits, but a marginal drop in the average loan size. Similarly, we do not see any change in delinquencies after the usury limits, indicating that at the state-level borrowers' credit standings do not change after these laws are enacted. This is intuitive, as only borrowers without access to traditional credit markets typically access high-cost credit. For this reason, the average borrower in the CCP likely did not borrow from high-cost lenders prior to the implementation of usury limits.

Next, we examine the dynamics the change in borrowing surrounding usury limits using an event-study analysis. Specifically, we estimate the following regression:

$$y_{izst} = \sum_{k=-m}^{m} \alpha_k Treated_s * \mathbb{1}_{t-k} + \alpha_i + \alpha_s + \alpha_t + \gamma Age_{it} + \epsilon_{izst}, \tag{2}$$

where $\mathbb{1}_{t-k}$ is an indicator variable that the event took place k periods away from the observation's calendar time.[16] To address the staggered nature of the treatment, we follow Borusyak and Jaravel (2016) in omitting both α_0 and α_{-5} from the estimating equation (i.e., the coefficient for the period before the usury limit passed and the coefficient for the first period of the test sample). Figure A.1 displays the corresponding event-study plots, which display the α_k's from these regressions for changes in borrowing and credit standing. The results are broadly similar to what is observed in the difference-in-differences tables, with no indication of a pre-trend.

The results in this subsection do not indicate substantial aggregate changes in borrowing after usury limits. This is not surprising because the limits in question, at 36 percent or higher, are unlikely to bind for lower risk borrowers. Usury limits should be most constraining for households that do not have access to traditional credit markets (i.e., those with lower risk scores). In the next subsection, we focus on understanding how the usury limits affect credit access and standing for the least creditworthy households.

[16] For observations in the control state, we assume all observations are the period before the event date.

3.3 Effects for high-risk borrowers

To study how the implementation of usury limits affect credit outcomes for the riskiest cohort of borrowers, we estimate the following triple-difference model:

$$y_{izst} = \alpha_i + \alpha_{zt} + \beta HighRisk_i \times Treated_s \times Post_{st} + \gamma Age_i + \epsilon_{ist}, \tag{3}$$

where $HighRisk_i$ is equal to one if borrower i was in the bottom decile of risk scores before the usury limit is enacted.[17] Note that since we are able to exploit individual-level variation in risk scores, these regressions include zip code-by-quarter fixed effects in place of zip code and quarter fixed effects. These regressions can therefore be interpreted as estimating how credit outcomes change for high-risk borrowers following usury limits, after controlling for time-invariant household characteristics and aggregate zip code-level changes over time.[18]

Table 3 displays the estimates from these triple-difference regressions. When focusing on borrowers that are especially likely to use high-cost credit, usury limits appear to have more consequential effects. Debt held by high-risk borrowers decreases significantly in treated states relative to other borrowers. The point estimates implies a decline of 16.9% of the sample mean for borrowers in the bottom risk score decile relative to other borrowers. Average debt balances per account increase by 12.7% of the sample mean, while the number of accounts decreases by 20.2% of the sample mean.

Note that a reduction in the number of accounts indicates borrowers are either closing existing debt accounts, or opening fewer debt accounts. That is, household debt is declining on the extensive margin. At the same time, the increase in average debt balances per account is consistent with lenders requiring larger debt balances in the presence of usury limits in order to cover fixed costs involved in originating loans. To some extent then, the reduction of debt on the extensive margin is offset by an increase in borrowing on the intensive margin.

While usury limits may reduce credit access, households could benefit from these laws in other ways. For instance, borrowers may become delinquent on debt less often, and their credit scores may improve. If this is the case, we may expect that in the long run these declines in credit access may

[17] Results are qualitatively similar when defining high-risk borrowers as either those in the bottom quintile of risk scores, or those with risk scores below 620.

[18] Similar to in the difference-in-differences design, we are able to use the standard triple-difference specification due to the presence of households from never-treated states in our control group.

reverse as borrowers gain access to lower cost debt options. To this end, we also study changes in incidences of household delinquencies for high-risk households after usury limits. Contrary to views that usury limits could improve household credit conditions, there is no change in delinquencies for households after usury limits. Together, these results are consistent with a contraction in credit for high-risk borrowers, with no improvement in credit standing.

Next, we study how the change in household borrowing and credit standing changes after usury limits tends to evolve over time using the following dynamic triple-difference regression:

$$y_{izst} = \alpha_i + \alpha_{zt} + \sum_{k=-m}^{m} \alpha_k HighRisk_i \times Treated_s \times \mathbb{1}_{t-k} + \gamma Age_i + \epsilon_{izst}. \tag{4}$$

This exercise allows us to plot how outcomes change after the usury law, period-by-period. If α_k is close to zero prior to the usury limit, this is consistent with the parallel trends assumption required for a triple-difference framework. Just as in the difference-in-differences event-study, we follow Borusyak and Jaravel (2016) in omitting α_0 and α_{-5} from the estimating equation.

Figure 2 plots the point estimates over time. Consistent with parallel trends, there is not a substantial difference in the trend of each borrowing outcome prior to the enactment of the usury limit. However, total debt balances decrease by 8.8% of the sample mean immediately after the law is enacted. This decrease in debt balances eventually doubles from this initial decrease in the five quarters after the usury limit. Interestingly, average debt balances increase slightly after the usury limit. This is consistent with lenders requiring a higher debt balance in order to make loans under the binding usury limit sufficiently profitable. On the other hand, the number of accounts decreases by over 20% of the sample mean in the five quarters after the usury limit, indicating the decrease in debt balances is primarily driven by changes in borrowing on the extensive margin. These results show that debt balances decrease for borrowers with low credit scores after usury limits are enacted. This is consistent with (Bolen et al., 2023) who finds that interest rate caps in Illinois reduced access to credit for less creditworthy borrowers. Additionally, as observed in the triple-difference regression tables, there does not appear to be any change in delinquencies surrounding usury limits, indicating no improvement in credit standing following these laws.

The results in this subsection show that credit to high-risk borrowers fell after usury limits were enacted. Given this finding, it may be surprising that the difference-in-differences regressions

do not reveal a corresponding decrease in aggregate borrowing. In the next subsection, we will reconcile this difference by studying whether lenders facing usury limits reallocated credit from borrowers with low credit scores, to borrowers with marginally higher credit scores.

3.4 Credit Reallocation

When bound by a usury limit, lenders may respond by lending more to marginally safer borrowers for which the rate cap is not binding. As Blitz and Long (1965) observe (p. 613): "It is the less risky borrowers ...who are most likely to derive the greatest benefit from usury control.[19] Bodenhorn (2007) finds that lenders favor borrowers with prior relationships in presence of usury limits.

To test for reallocation, we estimate the same triple-difference regressions as before, except here we compare debt balances for borrowers in the second, third, fourth, and fifth deciles of risk scores to those in higher deciles.[20] Specifically, if lenders are reallocating credit from high-risk borrowers to those with marginally safer credit profiles, we may expect borrowing to increase for those in the second, third, fourth and fifth risk score deciles.

The results from this analysis are shown in Table 4. Panel (a) shows how borrowing and credit standing changed for borrowers in the second risk score decile relative to borrowers in higher risk score deciles after usury limits. Aside from a small increase in the number of accounts, there was little change in debt balances and delinquencies for borrowers in this group after usury limits. The relatively inconsequential effect of usury limits on the second risk score decile could indicate that some borrowers in this group lost access to credit due to usury limits, while other marginal borrowers obtained new credit.

Panel (b) contains results examining how usury limits affected borrowers in the third risk score decile relative to others. Lending to borrowers in this decile increased significantly relative to those with higher credit scores. Specifically, borrowing increased by 9.1% of the sample mean for those in the third decile of credit scores relative to other borrowers. This increase in credit is consistent with a reallocation of lending from high-risk borrowers to relatively more creditworthy borrowers. Next,

[19] Lenders may increase credit to safer borrowers if, for example, funding constraints prevented them from making such loans before the usury limit.

[20] We exclude borrowers in lower risk score deciles because we have already found that credit to that cohort declined relative to higher risk score cohorts.

we separately examine changes in borrowing at the intensive and extensive margins for borrowers in the third risk score decile compared to relatively more creditworthy borrowers. There is no change in the average loan size; however, the number of accounts increases by about 14% of the sample mean. This is consistent with lenders either opening more new accounts, or closing fewer existing accounts, with borrowers in the third risk score decile relative to those in higher risk score deciles. Delinquencies for borrowers in this group did not change, showing that the relative increase in credit for borrowers in the third risk score decile is not accompanied by a change in credit standing.

Panels (c) and (d) display results comparing the fourth and fifth risk score deciles, respectively, to higher deciles of borrowers. For both of these groups, borrowing and the number of accounts increases, although we also observe tiny increases in average loan sizes and delinquencies for the fifth risk score decile. Interestingly, the magnitude of the change in total borrowing is largest for the fourth risk score decile. This is consistent with the reallocation of credit being most consequential for borrowers with below median risk scores, while the change in usury laws had relatively little effect on those in the top half of the risk score distribution. Overall, these results suggest that lenders reallocated credit toward more creditworthy borrowers after usury limits. This is consistent with lenders using their newly available funds to lend to borrowers for whom their breakeven rate is lower than the rate allowed under the usury limit.

The dynamics of these changes are displayed in Figure 3. Panels (a)–(d) display changes in total debt balances for the second, third, fourth and fifth decile borrowers relative to the bottom decile. For all of these groups, there was little difference in the path of debt balances prior to passage of usury limits. However, there is a large increase in debt balances for borrowers in the third, fourth and fifth risk score deciles relative to those in the bottom risk score decile immediately after usury limits are enacted. Panels (e)–(h) confirm that average loan sizes did not change significantly around usury limits. However, panels (i) through (l) show that there is a statistically significant increase in the number of accounts after usury limits for affected borrowers, providing further evidence that the change in debt balances primarily occurred on the extensive margin. Finally, panels (m) through (p) examine delinquency rates, which do not change surrounding usury limits.

Overall, the results in this subsection are consistent with lenders extending more credit to borrowers with higher risk scores, relative to those with the lowest risk scores, after usury limits. This is consistent with lenders reallocating credit to borrowers for which the usury limit is not

binding. Given that some lenders reallocate credit to relatively higher risk borrowers after usury limits, it appears that instead of leaving the state, some lenders chose to change their operations to abide by the usury limits. Moreover, the increase in credit for some borrowers following usury limits helps to explain why lending does not change in the aggregate following the implementation of usury limits.

3.5 Robustness

We conduct several tests to ensure the robustness of our results. First, we repeat the triple-difference analysis as a difference-in-differences test, examining how debt balances change for high-risk borrowers relative to others within the treated states in Table A.1, and the results are similar. Next, we note that Ohio's usury law also included some expansions of certain high-cost credit (Standaert, 2018). Given the nuances of the Ohio law, Table A.2 repeats the analysis excluding Ohio, and the results are similar. One additional concern is that the change in borrowing and credit conditions could be related to the COVID-19 pandemic, and the associated policy response. To this point, Table A.3 shows results excluding data from the COVID years, which are similar to the baseline results, indicating the findings are not driven by the COVID-19 pandemic. Finally, we note that usury laws are often complicated, and we may be including some control states that have usury laws not fully captured by the NCLC data. Fortunately, it is well documented that Wisconsin and Utah have no usury laws, making them particularly useful control states. Given this institutional feature, we include a version of the test excluding all control states except Utah and Wisconsin in Table A.4, and the results are similar to the baseline.

The results in this section show that borrowing decreased, with little change in delinquencies, for households with low risk scores after usury laws were enacted. This indicates that usury limits lead to credit contractions for borrowers in the lowest risk score deciles, without a clear improvement in credit standing. Next, we will examine whether the contraction in credit from usury limits corresponds with any change in real economic conditions.

4 Usury Limits and Small Firms

Previous literature has shown that in the 19th century, usury limits reduced economic activity, especially for small firms (Benmelich and Moskowitz, 2010). Similarly, we next examine whether we see reductions in business activity following usury limits in our setting, particularly for smaller firms. To do this, we focus on identifying small firms based on the density of small businesses in a region, as well as the prevalence of self-employment in a region.

4.1 Small Firm Data

We use the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW) to observe business activity at the county-level. The QCEW aggregates employment microdata from each state's unemployment insurance accounting system, supplemented by surveys to ensure accurate industry and location information. This dataset provide county-level employment statistics that covers roughly 95% of all private sector employment. These data allow us to observe county-level establishments, employment and wages.[21] We drop counties with total establishments above the 95th percentile from our sample as their outcomes are substantial outliers, predominantly as a function of their large populations.

We use two different approaches to identify variation in the importance of small firms in a county. First, we use the Census Bureau's Quarterly Workforce Indicators (QWI) to identify what portion of business establishments in a county are associated with small firms. The QWI sources its data from state-level administrative records linking employees and employers through the Census' Local Employment Dynamics (LED) program. Importantly, the QWI also provides a breakdown of establishments according to the size of the firm that owns the establishment. This can allow us to see how much of a county's business activity is reliant on relatively smaller firms.

We use the QWI to construct a Small Business Intensity (SBI) index. We define the SBI as the percentage of county's employees that are employed in a firm with less than five hundred employees across all establishments.[22] The SBI for counties in treated states is taken as the average of the

[21] We focus on private sector statistics and calculate employment as the average county-level private employment across the three months in each quarter.

[22] We identify small firms as those with less than five hundred employees based on the Small Business Administration's definition of a small business. However, results defining small businesses as those with under 20 employees are shown in the appendix.

four quarters prior to the passage of usury limit laws, while the measure for counties in control states is calculated as the average across the entire sample period. We characterize 143 "small business intensive" counties as those within the top quintile of SBI (i.e., that have SBI greater than 78.2%).[23] The median SBI is 62.0% and the bottom quintile is 49.2%, so more than half of the counties in our sample have a majority of workers employed in smaller firms. We identify counties in the top quintile of small business intensity as "small business intensive" counties for the purposes of our analysis.

Summary statistics according to the small business intensity are displayed in Table 5. The average small business intensive county has less than a sixth of the average number of establishments of other counties (286 vs. 1,183, respectively). Average employment is less than a fifth (2,054 vs. 15,550), indicating that such counties also have a lower average number of workers per establishment. This supports our use of SBI as a measure of small business activity, as it correlates well with establishment size as measured by number of employees. Average annual household income is roughly $2,600 less in high SBI counties, although the difference is insignificant. High SBI counties are noticeably more rural, with 79% of these counties classified as having urban populations of fewer than 5,000 by the USDA's Rural-Urban Continuum Codes (RUCC's), compared to 27% of lower SBI counties.

In an alternative approach to identify reliance on small firms, we use the American Community Survey (ACS) to identify what portion of a county is self-employed. The ACS includes a question for whether an individual is a self-employed worker. The definition of "self-employed" often includes independent contractors, such as "gig economy" workers. As these types of workers are especially unlikely to have access to capital often available to firms, self-employed workers may be especially likely to rely on consumer loans that could be affected by usury limits. We use the ACS to calculate the proportion of a county that is self-employed. The SEI for counties in treated states is taken as the average of the four quarters prior to the passage of usury limit laws, while the measure for counties in control states is calculated as the average across the entire time period. We identify counties in the top quintile of self-employment intensity as "self-employment intensive" counties for the purposes of our analysis. Summary statistics according to counties' SEIs are displayed in

[23] Results are similar when instead defining small business intensive counties as those within the top decile of SBI, or with above median SBI.

Table 6. Similar to when comparing counties according to SBI, high SEI countieds tend to have lower total wages, fewer establishments, and lower employment than other counties.

4.2 Effects on Business Activity

To estimate the effects on usury limits on county-wide business outcomes, we first use a difference-in-difference model akin to our analysis of household debt:

$$y_{cst} = \alpha_c + \alpha_t + \beta Treated_s \times Post_{st} + \epsilon_{cst}. \tag{5}$$

where y_{cst} is an outcome variable for county c in state s in quarter t. $Post_{st} = 1$ if a usury limit law was passed in state s before or in quarter t, 0 otherwise. County and quarter fixed effects are included to control for inherent differences in business outcomes across counties and for sample-wide time trends in economic activity. Standard errors are clustered at the state-quarter level.

Table 7 displays estimates from the above difference-in-difference specification. Number of establishments falls by 1.8% of the sample-wide average following usury limits, while employment decreases by 2.3%, indicative of a slight negative effect of these laws on overall business activity. Our results also suggest that the average employment per establishment also decreases slightly. Quarterly wages decrease by \$4.22 million, roughly 3% of the sample mean, but the insignificant coefficient on wages per employee implies that most of this decrease can be explained by the effects on employment.

We similarly analyze the dynamics on business outcomes in the quarters leading up to and following usury limits with the following regression:

$$y_{cst} = \sum_{k=-m}^{m} \alpha_k Treated_s * \mathbb{1}_{t-k} + \alpha_c + \alpha_s + \alpha_t + \epsilon_{cst}, \tag{6}$$

where $\mathbb{1}_{t-k}$ is an indicator variable denoting the passage of usury limits k quarters before or after quarter t. As in the CCP analysis, we omit α_0 and α_{-5} from the estimating equation. We display the results of this regression in Figure A.2. These graphs reflect the same patterns as the difference-in-difference tables. Establishments and employment decrease following the passage of usury limits, while the effects on wages are less significant. Notably, the negative impacts on establishment counts

and county-level employment appear to continue more than a year after the passage of these laws.

The findings in the difference-in-differences analysis show that business activity declined after usury limits. These findings provide important insights for policymakers, as they imply that consumer usury limits may have real economic effects. Next, we will consider whether business activity declines more in counties where small firms are more important, since those types of firms may be especially limited in terms of their access to capital.

4.3 Effects on Small Businesses Intensive Counties

The difference-in-differences analysis shows a decline in establishments following usury limits. Importantly, we expect this decline in establishments to be concentrated in counties with a high population of small firms, who we expect to be more likely to rely on high-cost consumer credit for financing. To this end, we estimate the effects of usury limits on small business activity using the following triple-difference model with our high-SBI measure:

$$y_{cst} = \alpha_c + \alpha_{st} + \beta HighSBI_c \times Treated_s \times Post_{st} + \epsilon_{cst}, \tag{7}$$

where y_{cst} is an outcome variable for county c in state s in quarter t. The outcomes of interest are number of establishments, total employment, quarterly wages (in millions), and quarterly wages per employee from the QCEW. County fixed effects are included to control for time-invariant differences between high- and low-SBI counties such as population and population-density, which could be correlated with small business outcomes. State-quarter fixed effects address time-varying trends in business activity due to unrelated state-level policies and business conditions. Standard errors are also clustered at the state-quarter level.

Table 8 provides estimates from the above specification. The number of establishments in small business intensive counties falls by 3.3% of the high-SBI mean following the passage of usury limit laws and aggregate quarterly wages decreased by nearly 39% of the average quarterly wages of small business intensive counties, with wages per employee decreasing as well. Although employment also appears to decrease, the coefficients are insignificant.

In a similar fashion to the analysis of household debt outcomes, we study the evolution of small

business outcomes after usury limits over time through a dynamic triple-difference regression:

$$y_{cst} = \alpha_c + \alpha_{st} + \sum_{k=-m}^{m} \alpha_k HighSBI_c \times Treated_s \times \mathbb{1}_{t-k} + \epsilon_{cst}. \tag{8}$$

where we again omit α_0 and α_{-5} and cluster standard errors at the state-quarter level.

Figure 4 displays the results of the event study specification. As in Table 8, establishments and wages decrease (and continue to do so) in the quarters following the passage of usury limits. These decreases appear to progress over the year following usury limits, with the effects flattening out afterwards. The magnitudes of these decreases mirror those observed in the triple-difference table.

4.4 Effects on Self-Employment Intensive Counties

The previous set of results examined changes in business activity after usury limits according to differences in SBI. Next, we will examine whether usury limits disproportionately affect counties according to the amount of self-employment in the county. Specifically, we implement the following regression:

$$y_{cst} = \alpha_c + \alpha_{st} + \beta HighSEI_c \times Treated_s \times Post_{st} + \epsilon_{cst}, \tag{9}$$

where $HighSEI_c$ is equal to one if county c is in the top-quintile of self-employment intensity. If counties with more reliance on self-employment are more affected by usury limits, then we should expect to see $\beta < 0$.

Results of our triple-difference regressions examining changes in business activity after usury limits according to SEI are displayed in Table 9. The first column shows changes in establishments after usury limits, which decline by about 1 percentage point relative to the sample mean in high-SEI counties located in treated states. This is consistent with a greater decline in business activity following usury limits in counties with significant self-employment. Moreover, while we do not see a differential change in employment, total wages and wages per employee decline in treated counties with high-SEI following usury limits.

Next, we examine the changes in business activity in high-SEI counties following usury limits

dynamically in event-study plots. These results are shown in Figure 5. For all outcomes variables except employment, there is not a differential change in the outcome for high-SEI treated counties prior to the usury limit, indicating that the parallel trends assumption is likely reasonable. However, we observe an decline in establishments immediately after the usury limits pass, which increases over time. Similar effects are observed for total wages and wages per employee.

Similar to when examining small business intensity, business activity appears to decline more after usury limits in counties that are especially reliant on self-employment. Together with the change in business activity for high-SBI counties, these results provide evidence that the change in business activity after usury limits was more severe in regions with significant economic activity by small firms. These results are intuitive, insofar as small firms may have limited access to funding in traditional lending markets, thereby requiring them to rely on funding from high-cost lenders.

4.5 Robustness

We conduct a number of additional tests to ensure the robustness of our results. First, we repeat the robustness analysis previously conducted in the CCP analysis using the small business activity sample. Table A.5 displays results for a difference-in-differences regression, comparing changes in business activity for high-SBI counties to others, within treated states. Table A.6 shows a similar analysis comparing high-SEI counties to others. Table A.7 and Table A.8 repeat the triple-difference analysis, excluding Ohio from the sample. Table A.9 and Table A.10 include results from the triple-difference analysis excluding the COVID pandemic period. Table A.11 and Table A.12 show results only using Utah and Wisconsin as control states. In all these tests, the results are qualitatively similar to those observed in Table 8 and Table 9.

Lastly, we define small business employment as employment in firms with under five hundred employees, a definition guided by the Small Business Administration. However, the effects of usury limits may be more salient for even *smaller* businesses. To this end, we adopt an alternative definition of small business intensity using employment in firms with under twenty employees, calculated again from the QWI. We characterize small business intensive counties under this new definition as the top quintile of counties ranked by proportion of employment in firms under twenty employees and present these results in Table A.13. The results with an alternative definition of small business intensity do not differ drastically from our main specification.

5 Conclusion

Usury limits, an ancient form of financial regulation, are resurging in the modern U.S. as many states cap rates to curb predatory payday and other non-bank small dollar lenders. Despite their antiquity, the efficacy of these laws is still debated.

This paper contributes to the debate by studying how these laws reshape credit markets and small business activity. We show that lending to the main clientele of these types of loans tend to decrease following implementation of usury limits without a corresponding improvement in credit conditions. At the same time though, we show that lending actually increases to relatively more creditworthy borrowers, implying usury limits can have spillover effects that affect those outside their main base of customers. Most surprisingly, we find that capping consumer loan rates stifles business activity, particularly in counties with more small business activity.

These findings call into question the potential benefits of usury limits. In particular, if usury limits lead to credit crunches for high-risk borrowers, without corresponding reductions in delinquencies for these borrowers, it is not clear how these laws benefit the most vulnerable borrowers. At the same time, declines in small business activity is especially concerning, as it indicates real economic consequences of usury limits. Overall, the findings present reasons for policymakers to be cautious in the implementation of usury limits.

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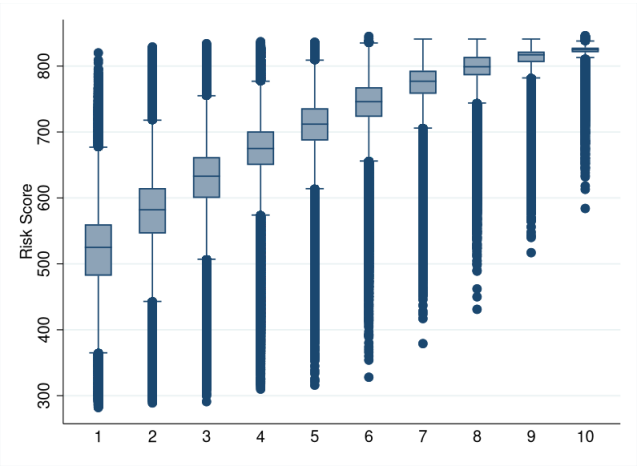
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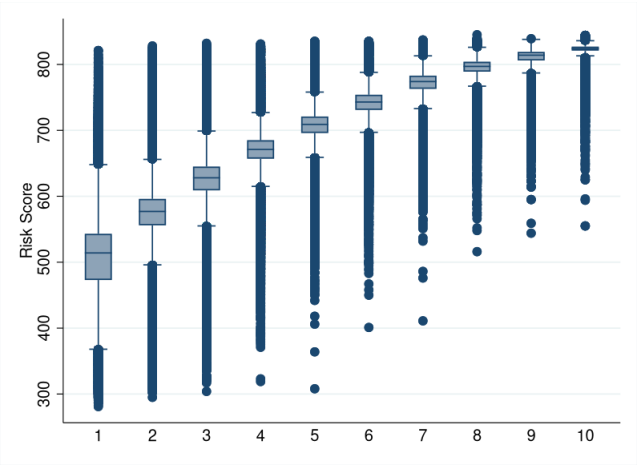
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Figure 1. Distributions of Risk Scores for Households in Treated and Control States

(a) Control States

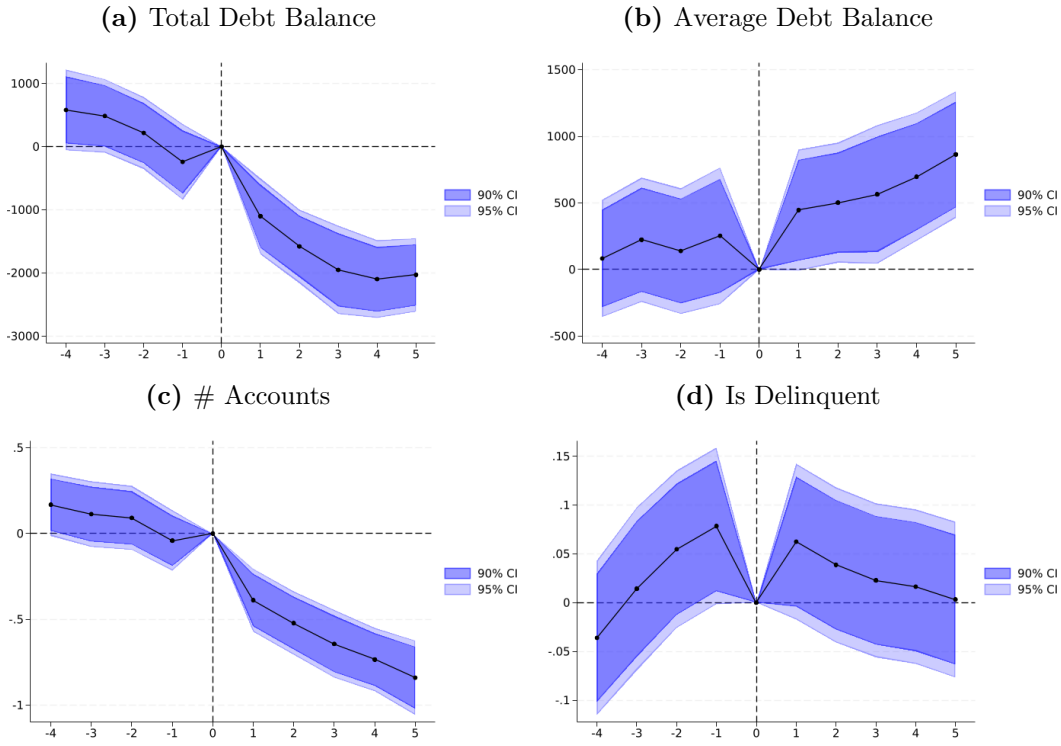


(b) Treated States



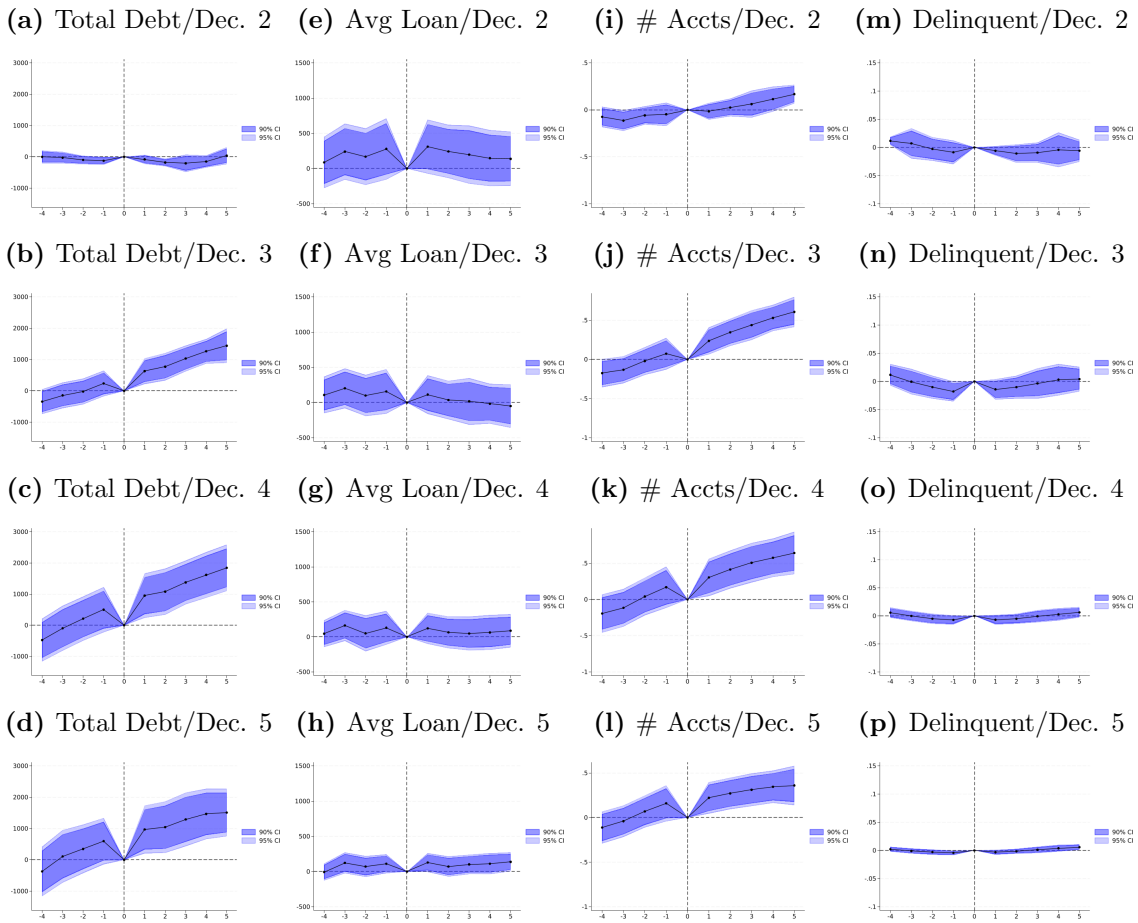
This figure shows the distribution of each risk score decile for control and treated states. Debt excludes mortgage debt and student loan debt. For control states, the deciles are defined based on household risk scores throughout the sample period, and for treated states the deciles are defined based on household risk scores in the four quarters leading up to the passage of the usury limit. Source: New York Fed Consumer Credit Panel/Equifax 2015:Q1–2022:Q4 and authors' calculations.

Figure 2. Changes in Borrowing and Credit Standing for High-Risk Borrowers Over Time



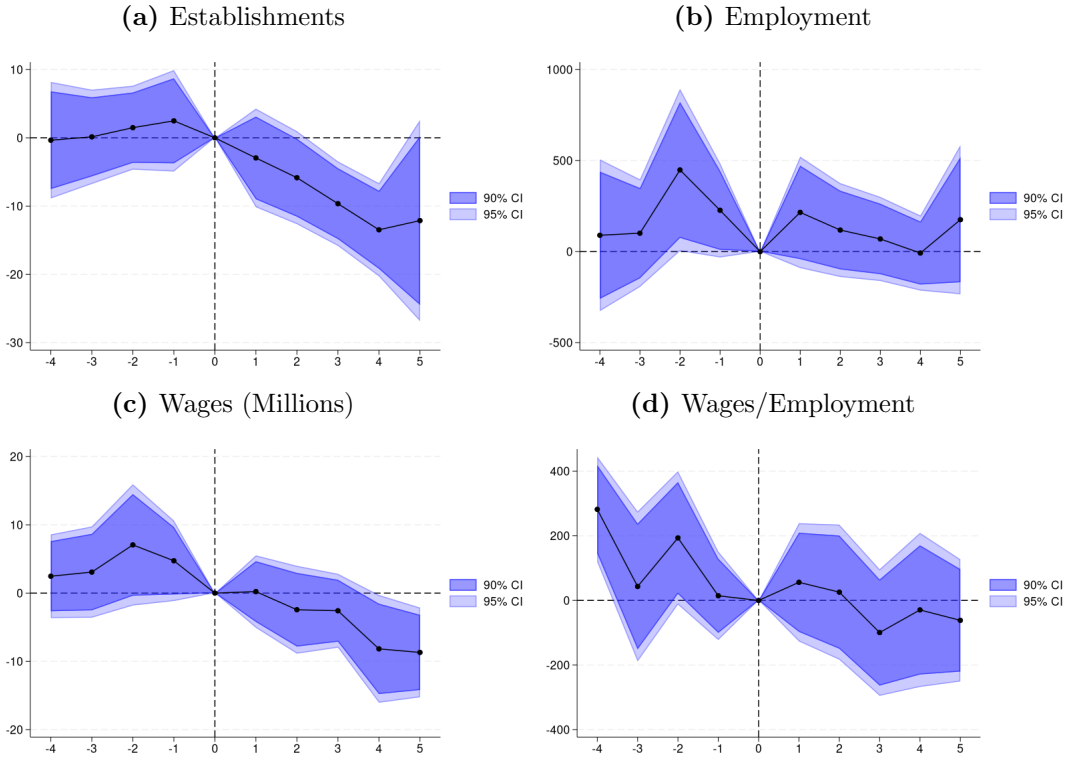
These figures show dynamic triple-differences plots examining the changes in borrowing and credit standing after usury limits for borrowers in the bottom risk decile, located in states passing the usury limit, relative to other borrowers. Debt excludes mortgage debt and student loan debt. Delinquencies are defined as any debt delinquency of 90 days or more. For control states, the deciles are defined based on household risk scores throughout the sample period, and for treated states the deciles are defined based on household risk scores in the four quarters leading up to the passage of the usury limit. Individual and zip code-by-quarter fixed effects are included in regressions and standard errors are clustered at the state-by-quarter level. Source: New York Fed Consumer Credit Panel/Equifax 2015:Q1–2022:Q4 and authors' calculations.

Figure 3. Changes for other deciles after usury limits



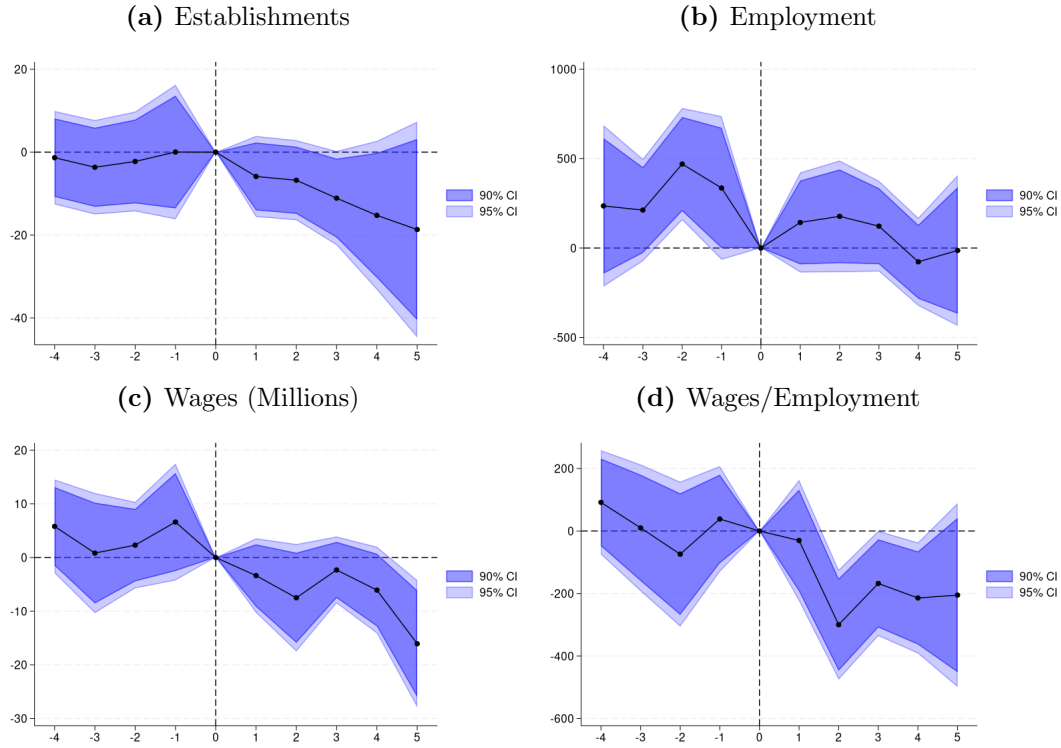
These figures show dynamic triple-difference plots examining the changes in credit standing and debt balance outcomes after usury limits for borrowers with risk scores in either the second, third, fourth or fifth decile, located in states passing the usury limit, relative to borrowers in all higher risk score deciles. Delinquencies are defined as any debt delinquency of 90 days or more. For control states, the deciles are defined based on household risk scores throughout the sample period, and for treated states the deciles are defined based on household risk scores in the four quarters leading up to the passage of the usury limit. Individual and zip code-by-quarter fixed effects are included in regressions and standard errors are clustered at the state-by-quarter level. Source: New York Fed Consumer Credit Panel/Equifax 2015:Q1–2022:Q4 and authors' calculations.

Figure 4. Changes in Business Activity by Small Business Intensity Over Time



These figures show dynamic triple-differences plots examining the changes in establishments, employment, and wages after usury limits for counties in the top quintile of small business intensity, located in states passing the usury limit, relative to other counties. Counties with establishments above the 95th percentile are dropped from all regressions. For control states, the small business intensity quintiles are defined based on small business employment throughout the sample period, and for treated states the quintiles are defined based on small business employment in the four quarters leading up to the passage of the usury limit. County and state-by-quarter fixed effects are included in regressions and standard errors are clustered at the state-by-quarter level. Source: BLS Quarterly Census of Employment and Wages 2015:Q1–2022:Q4, U.S. Census Bureau Quarterly Workforce Indicators 2015:Q1–2022:Q4, and authors' calculations.

Figure 5. Changes in Business Activity by Self-Employment Intensity Over Time



These figures show dynamic triple-differences plots examining the changes in establishments, employment, and wages after usury limits for counties in the top quintile of self-employment intensity, located in states passing the usury limit, relative to other counties. Counties with establishments above the 95th percentile are dropped from all regressions. For control states, the self-employment intensity quintiles are defined based on self-employment employment throughout the sample period, and for treated states the quintiles are defined based on self-employment employment in the four quarters leading up to the passage of the usury limit. County and state-by-quarter fixed effects are included in regressions and standard errors are clustered at the state-by-quarter level. Source: BLS Quarterly Census of Employment and Wages 2015:Q1–2022:Q4, American Community Survey 2015:Q1–2022:Q4, and authors' calculations.

Table 1: Consumer Credit Panel (CCP) Summary Statistics

	Bottom Risk Decile				All Other Deciles			
	Mean	Median	StDev	Count	Mean	Median	StDev	Count
Total Debt	9,652.31	4,545.00	14,066.00	4,017,068	11,492.05	4,201.00	21,172.26	36,148,655
Average Loan Size	3,487.89	1,658.23	5,558.52	3,306,108	3,907.47	1,851.71	8,870.25	30,988,897
Num. Accounts	3.85	2.50	4.27	4,017,068	3.31	2.50	3.28	36,148,655
Is Delinquent	0.57	1.00	0.50	4,017,068	0.07	0.00	0.25	36,148,655
Observations	4,017,068				36,148,655			

These tables show summary statistics for borrowers in the bottom risk decile and all others. For control states, the deciles are defined based on household risk scores throughout the sample period, and for treated states the deciles are defined based on household risk scores in the four quarters leading up to the passage of the usury limit. Debt excludes mortgage debt and student loan debt. Source: New York Fed Consumer Credit Panel/Equifax 2015:Q1–2022:Q4 and authors' calculations.

Table 2: Changes in Borrowing and Credit Standing After Usury Limits

	Total Debt	Average Loan Size	Num. Accounts	Is Delinquent
Treated × Post	-44.138	-73.987***	-0.010	0.000
	(34.448)	(16.121)	(0.006)	(0.000)
Mean	11,288.5	3,864.1	3.4	0.1
N	40,302,185	34,397,194	40,302,185	40,302,185
Household FE	Yes	Yes	Yes	Yes
Zip Code FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.686	0.469	0.796	0.623

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These tables show difference-in-differences regression results examining the changes in borrowing and credit standing outcomes after usury limits for borrowers located in states passing the usury limit, relative to other borrowers. Debt excludes mortgage debt and student loan debt. Delinquencies are defined as any debt delinquency of 90 days or more. All regressions control for borrower age. Standard errors are clustered at the state-by-quarter level. Source: New York Fed Consumer Credit Panel/Equifax 2015:Q1–2022:Q4 and authors' calculations.

Table 3: Changes in Borrowing and Credit Standing for High-Risk Borrowers After Usury Limits

	Total Debt	Average Loan Size	Num. Accounts	Is Delinquent
High-risk × Treated × Post	-1916.727***	489.608***	-0.678***	0.010
	(72.468)	(82.851)	(0.018)	(0.006)
High-risk × Treated	181.597	351.528*	-0.036	-0.028***
	(197.169)	(199.210)	(0.048)	(0.010)
Mean	11,307.6	3,866.9	3.4	0.1
N	40,142,498	34,257,537	40,142,498	40,142,498
Household FE	Yes	Yes	Yes	Yes
Zip Code-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.688	0.490	0.797	0.625

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table shows triple-differences regression results examining the changes in borrowing and credit standing outcomes after usury limits for borrowers in the bottom risk decile, located in states passing the usury limit, relative to other borrowers. Debt excludes mortgage debt and student loan debt. Delinquencies are defined as any debt delinquency of 90 days or more. For control states, the deciles are defined based on household risk scores throughout the sample period, and for treated states the deciles are defined based on household risk scores in the four quarters leading up to the passage of the usury limit. All regressions control for borrower age. Standard errors are clustered at the state-by-quarter level. Source: New York Fed Consumer Credit Panel/Equifax 2015:Q1–2022:Q4 and authors' calculations.

Table 4: Reallocation of Credit After Usury Limits

(a) Risk Score Decile 2

	Total Debt	Average Loan Size	Num. Accounts	Is Delinquent
Decile 2 × Treated × Post	-74.718	79.310	0.118***	-0.008
	(73.821)	(66.828)	(0.027)	(0.007)
Decile 2 × Treated	183.107*	300.389**	0.079	0.020**
	(97.209)	(147.627)	(0.047)	(0.010)
Mean	11,491.7	3,907.3	3.3	0.1
N	36,127,842	30,955,144	36,127,842	36,127,842
Household FE	Yes	Yes	Yes	Yes
Zip-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.688	0.492	0.806	0.562

(b) Risk Score Decile 3

	Total Debt	Average Loan Size	Num. Accounts	Is Delinquent
Decile 3 × Treated × Post	1061.823***	-73.603	0.468***	-0.001
	(54.531)	(56.303)	(0.022)	(0.005)
Decile 3 × Treated	345.062*	228.255**	0.010	0.024***
	(179.075)	(100.297)	(0.056)	(0.003)
Mean	11,650.1	3,886.0	3.3	0.0
N	32,113,144	27,897,648	32,113,144	32,113,144
Household FE	Yes	Yes	Yes	Yes
Zip-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.689	0.493	0.816	0.505

(c) Risk Score Decile 4

	Total Debt	Average Loan Size	Num. Accounts	Is Delinquent
Decile 4 × Treated × Post	1334.982***	11.921	0.499***	0.000
	(83.097)	(53.445)	(0.032)	(0.002)
Decile 4 × Treated	665.788**	365.566***	-0.087	0.016***
	(277.618)	(112.404)	(0.084)	(0.001)
Mean	11,457.6	3,824.7	3.3	0.0
N	28,099,556	24,623,921	28,099,556	28,099,556
Household FE	Yes	Yes	Yes	Yes
Zip-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.692	0.493	0.823	0.449

(d) Risk Score Decile 5

	Total Debt	Average Loan Size	Num. Accounts	Is Delinquent
Decile 5 × Treated × Post	1134.199***	59.303**	0.287***	0.002**
	(130.384)	(27.865)	(0.022)	(0.001)
Decile 5 × Treated	-261.957	356.859***	-0.104*	0.004***
	(323.645)	(101.836)	(0.062)	(0.001)
Mean	11,062.2	3,759.8	3.2	0.0
N	24,084,787	21,149,579	24,084,787	24,084,787
Household FE	Yes	Yes	Yes	Yes
Zip-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.687	0.492	0.826	0.385

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These tables show triple-differences regression results examining the changes in borrowing and credit standing outcomes after usury limits for borrowers in the second, third, fourth or fifth risk score decile, located in states passing the usury limit, relative to borrowers in the bottom risk score decile. Debt excludes mortgage debt and student loan debt. Delinquencies are defined as any debt delinquency of 90 days or more. For control states, the deciles are defined based on household risk scores throughout the sample period, and for treated states the deciles are defined based on household risk scores in the four quarters leading up to the passage of the usury limit. All regressions control for borrower age and use weights provided by Nielsen IQ. Standard errors are clustered at the state-by-quarter level. Source: New York Fed Consumer Credit Panel/Equifax 2015:Q1–2022:Q4 and authors' calculations.

Table 5: Summary Statistics by Small Business Intensity

	(1) Small business intensive counties				(2) All other counties			
	Mean	Median	StDev	Count	Mean	Median	StDev	Count
Establishments	285.94	214.00	245.22	2770	1182.68	674.00	1318.41	12077
Employment	2053.82	1396.83	1881.48	2770	15549.63	7697.33	18999.05	12077
Wages	18.53	11.85	19.94	2770	167.57	74.49	220.88	12077
Wages Per Employee	8540.17	8329.03	1771.18	2734	9914.38	9829.41	1778.42	12047
Observations	2770				12701			

These tables show summary statistics for counties in the top quintile of small business intensity as derived from the Quarterly Workforce Indicators (QWI). Outcomes are calculated from the Quarterly Census of Employment and Wages (QCEW). Counties with establishments above the 95th percentile are dropped. For control states, the small business intensity quintiles are defined based on small business employment throughout the sample period, and for treated states the quintiles are defined based on small business employment in the four quarters leading up to the passage of the usury limit. Sources: BLS Quarterly Census of Employment and Wages 2015:Q1–2022:Q4, U.S. Census Bureau Quarterly Workforce Indicators 2015:Q1–2022:Q4, and authors' calculations.

Table 6: Summary Statistics by Self-Employment Intensity

	(1) Self employment intensive counties				(2) All other counties			
	Mean	Median	StDev	Count	Mean	Median	StDev	Count
Establishments	299.96	189.00	449.09	2559	1164.37	654.50	1303.30	12288
Employment	2200.99	1237.67	3767.04	2559	15287.24	7243.83	18880.95	12288
Wages	20.68	10.93	39.67	2559	164.56	70.81	219.58	12288
Wages Per Employee	8612.06	8415.88	1930.95	2445	9867.94	9757.53	1767.83	12336
Observations	2559				12912			

These tables show summary statistics for counties in the top quintile of self-employment intensity as derived from the American Community Survey (ACS). Outcomes are calculated from the Quarterly Census of Employment and Wages (QCEW). Counties with establishments above the 95th percentile are dropped. For control states, the self-employment intensity quintiles are defined based on self-employment throughout the sample period, and for treated states the quintiles are defined based on small business employment in the four quarters leading up to the passage of the usury limit. Sources: BLS Quarterly Census of Employment and Wages 2015:Q1–2022:Q4, American Community Survey 2015:Q1–2022:Q4, and authors' calculations.

Table 7: Changes in Business Activity After Usury Limits

	Establishments	Employment	Wages	Wages Per Employee
Treated × Post	-18.55***	-300.5**	-4.222**	-26.97
	(4.233)	(91.77)	(1.378)	(20.72)
Mean	1015.2	13023.7	139.7	9659.4
N	14850	14849	14850	14778
County FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.995	0.997	0.985	0.945

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

These tables show difference-in-differences regression results examining the changes in business outcomes after usury limits for counties located in states passing the usury limit, relative to other counties. Counties with establishments above the 95th percentile are dropped from all regressions. Standard errors are clustered at the state-by-quarter level. Sources: BLS Quarterly Census of Employment and Wages 2015:Q1–2022:Q4 and authors' calculations.

Table 8: Changes in Business Activity by Small Business Intensity After Usury Limits

	Establishments	Employment	Wages	Wages Per Employee
Small business intensive × Treated × Post	-9.431***	-30.18	-7.221***	-108.5*
	(2.251)	(87.85)	(1.876)	(58.15)
Mean	1010.4	13026.3	139.8	9657.5
N	14825	14846	14847	14746
County FE	Yes	Yes	Yes	Yes
State-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.995	0.997	0.986	0.947

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table shows triple-differences regression results examining the changes in business outcomes after usury limits for counties in the top quintile of small business intensity, located in states passing the usury limit, relative to other counties. Counties with establishments above the 95th percentile are dropped from all regressions. For control states, the small business intensity quintiles are defined based on small business employment throughout the sample period, and for treated states the quintiles are defined based on small business employment in the four quarters leading up to the passage of the usury limit. County and state-by-quarter fixed effects are included in regressions and standard errors are clustered at the state-by-quarter level. Sources: BLS Quarterly Census of Employment and Wages 2015:Q1–2022:Q4, U.S. Census Bureau Quarterly Workforce Indicators 2015:Q1–2022:Q4, and authors' calculations.

Table 9: Changes in Business Activity by Self-Employment Intensity After Usury Limits

	Establishments	Employment	Wages	Wages Per Employee
Self-employment intensive × Treated × Post	-10.33**	-138.4	-9.657***	-192.5***
	(4.183)	(90.72)	(2.681)	(57.24)
Mean	1010.4	13026.3	139.8	9657.5
N	14825	14846	14847	14746
County FE	Yes	Yes	Yes	Yes
State-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.995	0.997	0.986	0.947

Standard errors in parentheses

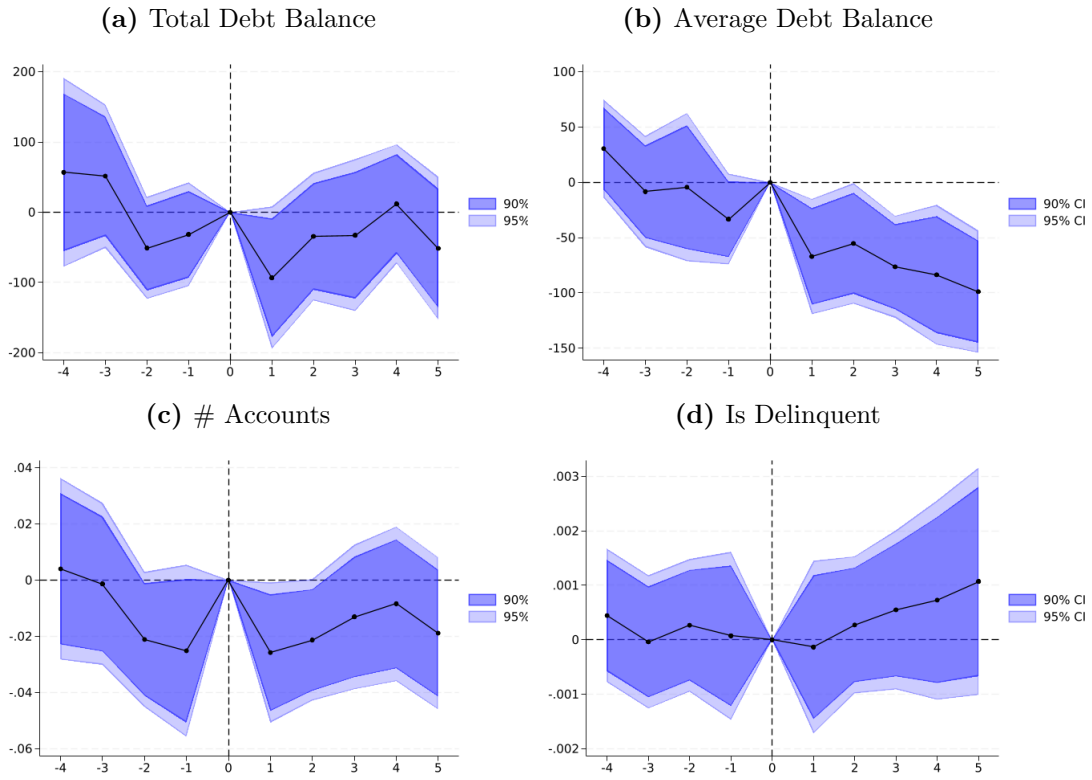
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table shows triple-differences regression results examining the changes in business outcomes after usury limits for counties in the top quintile of self-employment intensity intensity, located in states passing the usury limit, relative to other counties. Counties with establishments above the 95th percentile are dropped from all regressions. For control states, the self-employment intensity quintiles are defined based on self-employment intensity throughout the sample period, and for treated states the quintiles are defined based on self-employment in the four quarters leading up to the passage of the usury limit. County and state-by-quarter fixed effects are included in regressions and standard errors are clustered at the state-by-quarter level. Sources: BLS Quarterly Census of Employment and Wages 2015:Q1–2022:Q4, American Community Survey 2015:Q1–2022:Q4, and authors' calculations.

Appendix

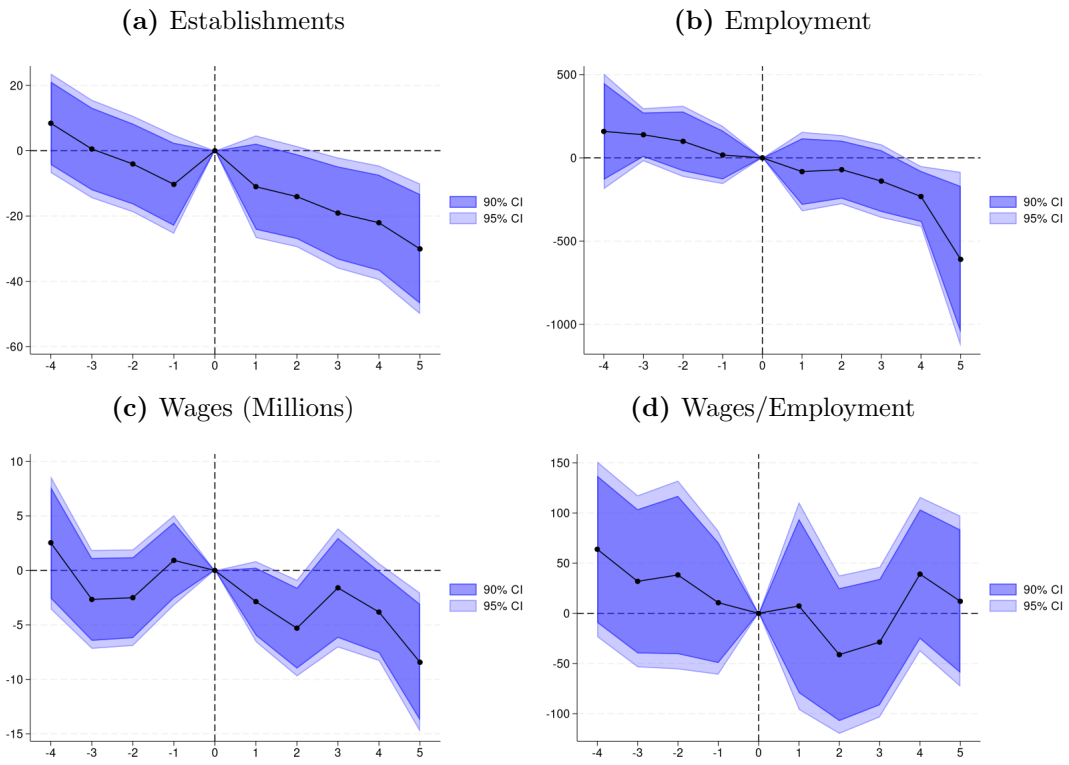
A.1 Appendix Results

Figure A.1. Changes in Debt Balance and Credit Standing Outcomes Over Time



These figures show dynamic difference-in-differences plots examining the changes in debt balance outcomes after usury limits for borrowers located in states passing the usury limit, relative to other borrowers. Delinquencies are defined as any debt delinquency of 90 days or more. Individual, zip code and quarter fixed effects are included in regressions, and standard errors are clustered at the state-by-quarter level. Source: New York Fed Consumer Credit Panel/Equifax and authors' calculations.

Figure A.2. Changes in Business Activity Over Time



These figures show dynamic difference-in-differences plots examining the changes in establishments, employment, and wages after usury limits for counties located in states passing the usury limit, relative to other counties. Counties with establishments above the 95th percentile are dropped from all regressions. County and quarter fixed effects are included in regressions and standard errors are clustered at the state-by-quarter level. Source: BLS Quarterly Census of Employment and Wages 2015:Q1–2022:Q4 and authors' calculations.

Table A.1: Changes in Borrowing and Credit Standing After Usury Limits - Within Treated States

	Total Debt	Average Loan Size	Num. Accounts	Is Delinquent
High-risk × Post	-1754.400***	481.194***	-0.630***	0.012*
	(87.407)	(92.500)	(0.020)	(0.007)
Mean	11,253.8	3,362.8	3.7	0.1
N	10,382,710	9,302,806	10,382,710	10,382,710
Household FE	Yes	Yes	Yes	Yes
Zip Code FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.830	0.686	0.907	0.758

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These tables show difference-in-differences regression results examining the changes in borrowing and credit standing outcomes after usury limits for high-risk borrowers in states passing the usury limits, relative to other borrowers. Delinquencies are defined as any debt delinquency of 90 days or more. Sample are limited to states that pass a usury limit. The deciles are defined based on household risk scores in the four quarters leading up to the passage of the usury limit. All regressions control for borrower age. Standard errors are clustered at the state-by-quarter level. Source: New York Fed Consumer Credit Panel/Equifax and authors' calculations.

Table A.2: Changes in Borrowing and Credit Standing for High-Risk Households - Excluding Ohio

	Total Debt	Average Loan Size	Num. Accounts	Is Delinquent
High-risk × Treated × Post	-1831.655***	646.681***	-0.659***	0.007
	(86.598)	(150.373)	(0.027)	(0.010)
High-risk × Treated	427.557*	350.117	-0.018	-0.018
	(216.861)	(248.944)	(0.058)	(0.011)
Mean	11,331.3	3,959.9	3.3	0.1
N	35,800,003	30,324,360	35,800,003	35,800,003
Household FE	Yes	Yes	Yes	Yes
Zip code-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.672	0.477	0.781	0.614

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These tables show triple-differences regression results examining the changes in borrowing and credit standing outcomes after usury limits for borrowers in the bottom risk decile, located in states passing the usury limit, relative to other borrowers. Delinquencies are defined as any debt delinquency of 90 days or more. For control states, the deciles are defined based on household risk scores throughout the sample period, and for treated states the deciles are defined based on household risk scores in the four quarters leading up to the passage of the usury limit. Borrowers from Ohio are excluded from the analysis. All regressions control for borrower age. Standard errors are clustered at the state-by-quarter level. Source: New York Fed Consumer Credit Panel/Equifax and authors' calculations.

Table A.3: Changes in Borrowing and Credit Standing for High-Risk Households - Excluding COVID Years

	Total Debt	Average Loan Size	Num. Accounts	Is Delinquent
High-risk × Treated × Post	-1918.707***	480.011**	-0.617***	0.021**
	(151.815)	(182.161)	(0.050)	(0.009)
High-risk × Treated	-903.337***	494.486	-0.178**	0.000
	(213.349)	(357.822)	(0.070)	(0.013)
Mean	10,804.6	3,969.9	3.2	0.1
N	31,756,004	25,993,304	31,756,004	31,756,004
Household FE	Yes	Yes	Yes	Yes
Zip code-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.752	0.499	0.815	0.644

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These tables show triple-differences regression results examining the changes in borrowing and credit standing outcomes after usury limits for borrowers in the bottom risk decile, located in states passing the usury limit, relative to other borrowers. Delinquencies are defined as any debt delinquency of 90 days or more. For control states, the deciles are defined based on household risk scores throughout the sample period, and for treated states the deciles are defined based on household risk scores in the four quarters leading up to the passage of the usury limit. Analysis excludes observations in 2020 or later. All regressions control for borrower age. Standard errors are clustered at the state-by-quarter level. Source: New York Fed Consumer Credit Panel/Equifax and authors' calculations.

Table A.4: Changes in Borrowing and Credit Standing for High-Risk Households - Excluding All Controls States Except Wisconsin and Utah

	Total Debt	Average Loan Size	Num. Accounts	Is Delinquent
High-risk × Treated × Post	-1918.6***	490.1***	-0.679***	0.00995
	(-26.46)	(5.83)	(-38.26)	(1.51)
High-risk × Treated	391.1	37.58	0.0849	-0.0131
	(1.53)	(0.15)	(1.02)	(-1.11)
Mean	10721.5	3499.4	3.436	0.0912
N	19,928,410	17,424,810	19,928,410	19,928,410
Household FE	Yes	Yes	Yes	Yes
Zip Code-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.745	0.547	0.845	0.676

t statistics in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These tables show triple-differences regression results examining the changes in borrowing and credit standing outcomes after usury limits for borrowers in the bottom risk decile, located in states passing the usury limit, relative to other borrowers. Delinquencies are defined as any debt delinquency of 90 days or more. For control states, the deciles are defined based on household risk scores throughout the sample period, and for treated states the deciles are defined based on household risk scores in the four quarters leading up to the passage of the usury limit. Analysis excludes all control states except Wisconsin and Utah. All regressions control for borrower age. Standard errors are clustered at the state-by-quarter level. Source: New York Fed Consumer Credit Panel/Equifax and authors' calculations.

Table A.5: Changes in Business Activity by Small Business Intensity After Usury Limits - Within Treated States

	Establishments	Employment	Wages	Wages Per Employee
Small business intensive × Post	-9.536***	-94.18	-8.860***	-108.5
	(2.460)	(92.92)	(2.085)	(59.96)
Mean	888.5	12329.0	133.3	10034.7
N	3575	3575	3548	3412
County FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	1.000	0.998	0.994	0.945

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

These tables show difference-in-differences regression results examining the changes in establishments, employment, and wages after usury limits for counties in the top quintile of small business intensity, located in states passing the usury limit, relative to other counties. Counties with establishments above the 95th percentile are dropped from all regressions. Sample are limited to states that pass a usury limit. Quintiles are defined based on small business employment in the four quarters leading up to the passage of the usury limit. County and quarter fixed effects are included in regressions and standard errors are clustered at the state-by-quarter level. Sources: BLS Quarterly Census of Employment and Wages 2015:Q1–2022:Q4, U.S. Census Bureau Quarterly Workforce Indicators 2015:Q1–2022:Q4, and authors' calculations.

Table A.6: Changes in Business Activity by Self Employment After Usury Limits - Within Treated States

	Establishments	Employment	Wages	Wages Per Employee
Self-employment intensive × Post	-8.230*	-275.9*	-12.51***	-164.8**
	(3.448)	(125.2)	(2.805)	(51.69)
Mean	888.5	12329.0	133.3	10034.7
N	3575	3575	3548	3412
County FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	1.000	0.998	0.994	0.945

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

These tables show difference-in-differences regression results examining the changes in establishments, employment, and wages after usury limits for counties in the top quintile of self employment intensity, located in states passing the usury limit, relative to other counties. Counties with establishments above the 95th percentile are dropped from all regressions. Sample are limited to states that pass a usury limit. Quintiles are defined based on self employment in the four quarters leading up to the passage of the usury limit. County and quarter fixed effects are included in regressions and standard errors are clustered at the state-by-quarter level. Sources: BLS Quarterly Census of Employment and Wages 2015:Q1–2022:Q4, American Community Survey 2015:Q1–2022:Q4, and authors' calculations.

Table A.7: Changes in Business Activity by Small Business Intensity After Usury Limits - Excluding Ohio

	Establishments	Employment	Wages	Wages Per Employee
Small business intensive × Treated × Post	-9.298***	-54.63	-7.576***	-122.3**
	(2.382)	(85.91)	(1.952)	(61.67)
Small business intensive × Treated	120.4***	2006.4***	21.37***	572.7***
	(27.87)	(660.6)	(7.587)	(83.53)
Mean	975.4	12360.9	132.9	9604.3
N	13945	13966	13976	13826
County FE	Yes	Yes	Yes	Yes
State-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.995	0.997	0.985	0.947

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These tables show triple-differences regression results examining the changes in establishments, employment, and wages after usury limits for counties in the top quintile of small business intensity, located in states passing the usury limit, relative to other counties. Counties with establishments above the 95th percentile are dropped from all regressions. For control states, the small business intensity quintiles are defined based on small business employment throughout the sample period, and for treated states the quintiles are defined based on small business employment in the four quarters leading up to the passage of the usury limit. Counties in Ohio are excluded from the analysis.County and quarter fixed effects are included in regressions and standard errors are clustered at the state-by-quarter level. Sources: BLS Quarterly Census of Employment and Wages 2015:Q1–2022:Q4, U.S. Census Bureau Quarterly Workforce Indicators 2015:Q1–2022:Q4, and authors' calculations.

Table A.8: Changes in Business Activity by Self Employment Intensity After Usury Limits - Excluding Ohio

	Establishments	Employment	Wages	Wages Per Employee
Self-employment intensive × Treated × Post	-11.42***	-158.2*	-10.13***	-202.4***
	(4.240)	(92.29)	(2.754)	(58.77)
Self-employment intensive × Treated	136.4***	1779.3***	20.68***	1373.3***
	(35.34)	(470.5)	(6.324)	(120.3)
Mean	975.4	12360.9	132.9	9604.3
N	13945	13966	13976	13826
County FE	Yes	Yes	Yes	Yes
State-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.995	0.997	0.985	0.947

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These tables show triple-differences regression results examining the changes in establishments, employment, and wages after usury limits for counties in the top quintile of self employment intensity, located in states passing the usury limit, relative to other counties. Counties with establishments above the 95th percentile are dropped from all regressions. For control states, the self employment intensity quintiles are defined based on self employment throughout the sample period, and for treated states the quintiles are defined based on small business employment in the four quarters leading up to the passage of the usury limit. Counties in Ohio are excluded from the analysis. County and quarter fixed effects are included in regressions and standard errors are clustered at the state-by-quarter level. Sources: BLS Quarterly Census of Employment and Wages 2015:Q1–2022:Q4, American Community Survey 2015:Q1–2022:Q4, and authors' calculations.

Table A.9: Changes in Business Activity by Small Business Intensity After Usury Limits - Excluding COVID Years

	Establishments	Employment	Wages	Wages Per Employee
Small business intensive × Treated × Post	-15.99***	-139.1**	-5.610***	-148.8
	(2.662)	(56.42)	(1.538)	(91.09)
Small business intensive × Treated	-238.4***	-3594.1***	-29.39**	79.10
	(65.98)	(1369.1)	(13.99)	(122.4)
Mean	923.2	12275.6	125.5	9603.8
N	11731	11751	11751	12427
County FE	Yes	Yes	Yes	Yes
State-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.998	0.993	0.986	0.953

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These tables show triple-differences regression results examining the changes in establishments, employment, and wages after usury limits for counties in the top quintile of small business intensity, located in states passing the usury limit, relative to other counties. Counties with establishments above the 95th percentile are dropped from all regressions. For control states, the small business intensity quintiles are defined based on small business employment throughout the sample period, and for treated states the quintiles are defined based on small business employment in the four quarters leading up to the passage of the usury limit. Analysis excludes observations in 2020 or later. County and quarter fixed effects are included in regressions and standard errors are clustered at the state-by-quarter level. Sources: BLS Quarterly Census of Employment and Wages 2015:Q1–2019:Q4, American Community Survey 2015:Q1–2022:Q4, and authors' calculations.

Table A.10: Changes in Business Activity by Self Employment Intensity After Usury Limits - Excluding COVID Years

	Establishments	Employment	Wages	Wages Per Employee
Self-employment intensive × Treated × Post	-2.131	-75.27	-5.507**	-133.8**
	(5.079)	(73.92)	(2.241)	(61.93)
Self-employment intensive × Treated	-75.64	-2164.7*	-15.63	-431.5*
	(83.69)	(1303.3)	(13.21)	(227.5)
Mean	923.2	12275.6	125.5	9603.8
N	11731	11751	11751	12427
County FE	Yes	Yes	Yes	Yes
State-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.998	0.993	0.986	0.953

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These tables show triple-differences regression results examining the changes in establishments, employment, and wages after usury limits for counties in the top quintile of self employment intensity, located in states passing the usury limit, relative to other counties. Counties with establishments above the 95th percentile are dropped from all regressions. For control states, the self employment intensity quintiles are defined based on self employment throughout the sample period, and for treated states the quintiles are defined based on self employment in the four quarters leading up to the passage of the usury limit. Analysis excludes observations in 2020 or later. County and quarter fixed effects are included in regressions and standard errors are clustered at the state-by-quarter level. Sources: BLS Quarterly Census of Employment and Wages 2015:Q1–2019:Q4, American Community Survey 2015:Q1–2022:Q4, and authors' calculations.

Table A.11: Changes in Business Activity by Small Business Intensity After Usury Limits - Excluding All Controls States Except Wisconsin and Utah

	Establishments	Employment	Wages	Wages Per Employee
Small business intensive × Treated × Post	-9.431***	-30.18	-7.221***	-108.5*
	(2.277)	(88.92)	(1.898)	(58.85)
Small business intensive × Treated	181.4***	3826.4***	49.45***	592.0***
	(42.57)	(769.9)	(8.588)	(107.1)
Mean	1058.0	14657.2	160.1	10032.5
N	6578	6567	6538	6480
County FE	Yes	Yes	Yes	Yes
State-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.998	0.998	0.988	0.942

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These tables show triple-differences regression results examining the changes in establishments, employment, and wages after usury limits for counties in the top quintile of small business intensity, located in states passing the usury limit, relative to other counties. Counties with establishments above the 95th percentile are dropped from all regressions. For control states, the small business intensity quintiles are defined based on small business employment throughout the sample period, and for treated states the quintiles are defined based on small business employment in the four quarters leading up to the passage of the usury limit. Analysis excludes observations in 2020 or later. Analysis excludes all control states except Wisconsin and Utah. County and quarter fixed effects are included in regressions and standard errors are clustered at the state-by-quarter level. Sources: BLS Quarterly Census of Employment and Wages 2015:Q1–2022:Q4, U.S. Census Bureau Quarterly Workforce Indicators 2015:Q1–2022:Q4, and authors' calculations.

Table A.12: Changes in Business Activity by Self Employment Intensity After Usury Limits - Excluding All Controls States Except Wisconsin and Utah

	Establishments	Employment	Wages	Wages Per Employee
Self-employment intensive × Treated × Post	-10.33**	-138.4	-9.657***	-192.5***
	(4.232)	(91.82)	(2.713)	(57.93)
Self-employment intensive × Treated	10.84	584.1	14.66*	301.9***
	(47.35)	(687.5)	(8.074)	(105.4)
Mean	1058.0	14657.2	160.1	10032.5
N	6578	6567	6538	6480
County FE	Yes	Yes	Yes	Yes
State-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.998	0.998	0.988	0.942

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These tables show triple-differences regression results examining the changes in establishments, employment, and wages after usury limits for counties in the top quintile of self employment intensity, located in states passing the usury limit, relative to other counties. Counties with establishments above the 95th percentile are dropped from all regressions. For control states, the self employment intensity quintiles are defined based on self employment throughout the sample period, and for treated states the quintiles are defined based on self employment in the four quarters leading up to the passage of the usury limit. Analysis excludes observations in 2020 or later. Analysis excludes all control states except Wisconsin and Utah. County and quarter fixed effects are included in regressions and standard errors are clustered at the state-by-quarter level. Sources: BLS Quarterly Census of Employment and Wages 2015:Q1–2022:Q4, American Community Survey 2015:Q1–2022:Q4, and authors' calculations.

Table A.13: Changes in Small Business Activity After Usury Limits - Under 20 Employees Definition

	Establishments	Employment	Wages	Wages Per Employee
Small business intensive × Treated × Post	-11.35***	-78.14	-7.984***	-147.7**
	(2.407)	(79.37)	(1.959)	(61.20)
Mean	1010.4	13026.3	139.8	9657.5
N	14825	14846	14847	14746
County FE	Yes	Yes	Yes	Yes
State-quarter FE	Yes	Yes	Yes	Yes
Err. Cluster	State-quarter	State-quarter	State-quarter	State-quarter
R-Squared	0.995	0.997	0.986	0.947

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table shows triple-differences regression results examining the changes in business outcomes after usury limits for counties in the top quintile of an alternate definition of small business intensity, located in states passing the usury limit, relative to other counties. We define small business intensity here as counties in the top quintile of proportion of private employment in firms with under 20 employees. Counties with establishments above the 95th percentile are dropped from all regressions. For control states, the small business intensity quintiles are defined based on small business employment throughout the sample period, and for treated states the quintiles are defined based on small business employment in the four quarters leading up to the passage of the usury limit. County and state-by-quarter fixed effects are included in regressions and standard errors are clustered at the state-by-quarter level. Sources: BLS Quarterly Census of Employment and Wages 2015:Q1–2022:Q4, U.S. Census Bureau Quarterly Workforce Indicators 2015:Q1–2022:Q4, and authors' calculations.

Table A.14: List of Usury Limit Laws

State	**Law**	**Law Name**	**Passed**	**Effective**	**Limit**
South Dakota	IM 21	South Dakota Payday Lending Initiative	11/8/2016	11/16/2016	36%
New Mexico	HB 347	Installment Loan Fee Limits & Literacy Fund	4/6/2017	1/1/2018	175%
Ohio	HB 123	Modify short-term, small, and mortgage loan laws	7/30/2018	4/27/2019	28%
Illinois	SB 1792	Predatory Loan Prevention Act	3/23/2021	3/23/2021	36%
North Dakota	SB 2103	Money Brokers Act	4/19/2021	8/1/2021	36%