The real effects of financial technology: Marketplace lending and personal bankruptcy

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ABSTRACT

We examine how financial technology affects households in terms of personal bankruptcy by leveraging exogenous variation in marketplace credit supply to Connecticut and New York residents. We document a persistent rise in bankruptcies in the affected states following sharp decreases in marketplace lending, particularly among low-income households and in areas where marketplace loans for financing medical bills are severely rationed. Borrowers' indebtedness or local economic conditions do not explain the results. The supply of other consumer credit by banks and finance companies remains unaffected, suggesting that the observed increase in bankruptcies arises principally from reversing access to marketplace credit.

1. Introduction

The start of the 21st century has been marked by the rise of new financial technology (fintech), ranging from online banking and mobile payments to distributed ledger technology and marketplace lending. Technological advancements make it easier to control finances, provide alternative payment instruments and enhance access to funding. However, little is known about the potential risks and benefits of these new technologies in terms of affecting household financial health. In this paper, we investigate the effect of new financial technology on personal bankruptcy, focusing on a relatively novel type of consumer credit, marketplace lending.

A marketplace loan is a fixed-rate, unsecured consumer debt issued by an online lending platform connecting borrowers with investors. Investors supply funds directly to borrowers via the platform, or alternatively, marketplace lenders may partner with a bank to originate loans. In 2017, marketplace platforms originated 38% of all personal loans, predominantly requested for debt consolidation, small businesses financing, and covering medical expenses.[1]

Fintech lending introduces several innovations to traditional underwriting. Most importantly, online lenders use sophisticated statistical techniques for processing large datasets to inform algorithmic tools and allow borrowers to be screened (Vallee and Zeng, 2019) and provided with credit quickly (Wang, 2018). In screening borrowers, in addition to risk assessments provided by credit agencies, marketplace lenders use relatively more private data such as utility payments, health insurance claims, or borrowers' purchasing history (Jagtiani and Lemieux, 2019). Better and more data reduce asymmetric information and allow fintech platforms to more accurately assess borrowers' risk and extend loans to individuals who would otherwise be credit rationed by traditional lenders, without exposing investors to greater relative default risk (Croux et al., 2020; Schweitzer and Barkley, 2017; Tang, 2019). In addition, the lack of traditional financial intermediaries involved in the loan origination process may result in lower overheads and provide marketplace borrowers with credit at interest rates lower than they could obtain from conventional lenders (De Roure et al., 2018).

To the extent that individuals prefer to avoid bankruptcy rather than default strategically to discharge debt, marketplace lending can potentially reduce the incidence of personal bankruptcy. It may allow households to lower their debt refinancing costs (Balyuk, 2018; De Roure et al., 2018; Jagtiani and Lemieux, 2019) and provide them with timely liquidity in the face of income shocks, such as unforeseen medical costs. Both credit card debt and medical costs are among the main determinants of personal bankruptcy (Domowitz and Sartain, 1999;

White, 2007; Gross and Notowidigdo, 2011; Dobkin et al., 2018). On the other hand, the rapid expansion of marketplace lending may raise the number of personal bankruptcies by providing credit to less credit-worthy individuals, increasing household debt, and possibly throwing borrowers into a debt-trap of over-borrowing (Gross and Souleles, 2002; Fay et al., 2002; Livshits et al., 2016).

In this paper, we empirically test the ex-ante ambiguous relationship between the availability of marketplace credit and personal bankruptcy. We exploit a decision by the U.S. Second Circuit Court of Appeals that resulted in an exogenous reduction in the supply of marketplace credit. In May 2015, the court, whose jurisdiction covers Connecticut, Vermont, and New York, ruled that loans originated to borrowers in those states with an interest rate above the borrower's state usury limit are null and void if these loans are held by non-bank financial institutions.[2] While unrelated to marketplace lending, the ruling cast doubt on the enforceability of marketplace loans, in particular above-usury marketplace loans issued to residents of Connecticut and New York.[3] This is due to the fact that although fronting banks issue the majority of marketplace loans, they sell them outright to marketplace platforms, which are non-bank financial institutions.[4]

We identify the effect of marketplace lending rationing on bankruptcy filing using difference-in-difference estimations. We compare changes in bankruptcy filings and marketplace lending in the treatment group (3-digit zip codes in Connecticut and New York) and the control group (3-digit zip codes in all other states) before and after the treatment event.

Using monthly data from the U.S. Federal Judicial Centre, we show that following the court ruling, personal bankruptcy filings rise by 6% more in Connecticut and New York zip codes relative to the control group zip codes. This is driven by an increase in Chapter 7 and 13 bankruptcies.

We attribute the observed *relative* increase in the incidence of personal bankruptcy in the regions affected by the treatment event to the reduction in marketplace lending. Consistent with classical price theory, the interest rate controls imposed by the court verdict result in credit rationing. The three largest U.S. marketplace lenders significantly reduce lending in the affected zip codes. Relative to the control group of zip codes, the total volume and number of newly originated marketplace loans in NY and CT decline by 30% and 22%, respectively. This effect is driven by a significant decline in marketplace loans for payment of medical costs and debt refinancing, including for refinancing credit card debt.[5]

The hypothesis linking the *relative* rise in bankruptcy filings in NY and CT, compared with other untreated states, to the rationing of marketplace credit following the treatment event is supported by further

tests. First, we show that personal bankruptcy rises in proportion to the reduction in marketplace lending across different income groups. Households in the highest income group neither experience marketplace credit rationing nor a hike in bankruptcies, while households in the lowest income group experience both the most severe marketplace credit rationing and the largest rise in bankruptcies following the verdict. Second, we show that the intensity of the treatment event effect on personal bankruptcy filings is more potent in areas where the reduction in marketplace lending is significantly higher, particularly in areas with severe cutbacks in marketplace loans to finance personal expenses related to health care.

Our finding that marketplace lending, particularly loans for medical costs, may help households avoid bankruptcy is in line with the fact that many personal bankruptcies are related to the inability of individuals to cover their medical expenses (Gross and Notowidigdo, 2011). In addition, individuals who request loans to cover health care costs are, on average, significantly less indebted than those who request loans for debt refinancing and other purposes.[6] This fact, suggests these individuals are more likely to be credit rationed but also less likely to default on their debt obligations due to overindebtedness once they obtain marketplace loan. Rationing of marketplace credit may deprive individuals of an important source of external funding in crisis situations.

We reject several other plausible alternative explanations behind the rise in personal bankruptcy following the treatment event. First, we rule out the possibility that marketplace borrowers' indebtedness drives personal bankruptcy filings. Second, we show that the treatment event's effect is limited to the enforceability of marketplace loans. The verdict leaves unaffected the volume of other types of consumer credit, including card credit, auto loans, and student loans extended by banks and finance companies. Third, we present results indicating that the increase in bankruptcy is unlikely to be caused by borrowers switching to forms of high-interest credit, such as payday loans. Fourth, we rule out that the rise in bankruptcy could result from increased defaults by marketplace borrowers in the affected states. Finally, we show that prior bankruptcy filings of marketplace borrowers cannot solely explain our results.

Our empirical model controls for factors affecting marketplace lending and personal bankruptcy filings, including demand for marketplace loans, the value of filers' dischargeable debt, and their total liabilities. Our results are robust across an array of econometric specifications and treatment group definitions, alternative standard error clustering, and matching treatment and control group states.

In summary, we find that reversing access to new lending technology in the form of restricting marketplace lending affects households by precipitating a rise in personal bankruptcies. The effect on bankruptcies grows and persists over time, in line with the notion that rather than being used to merely postpone and delay filing, marketplace loans help households avoid bankruptcy.

These results matter because bankruptcy is associated with large micro- and macroeconomic effects. While filing may help some individuals discharge or restructure debt (White and Zhu, 2010), bankruptcy also imposes substantial negative externalities. The per capita fiscal costs of bankruptcy exceed the costs of unemployment programs (Lefgren et al., 2010) and federal health insurance (Mahoney, 2015; Fisher, 2017). Bankruptcy imposes credit rationing (Lin and White,

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[2] The verdict applies if a bank issues and assigns a loan in a debt sale to a non-bank and the loan's interest rate exceeds the borrower's state usury limit and, importantly, if the bank retains no ongoing economic interest in the loan.

[3] Due to automation of the investment process (Balyuk, 2019) some marketplace lending investors may not be able to fully control which loans they fund. This may discourage them from completely investing in this segment of unsecured consumer credit and as such we may observe a negative effect of this court verdict also on loans carrying interest rates below usury limits.

[4] Above-usury loans extended to borrowers in Vermont, where only the interest in excess of the state usury limit is void, are treated differently from loans to borrowers in Connecticut and New York, where the complete interest and loan principal are void.

[5] While our estimates on the effects of marketplace credit supply are smaller when compared to related studies on the impact of personal bankruptcies resulting from credit card lending, they represent an economically meaningful magnitude. In our study, the 6% rise in personal bankruptcies follows a 30% reduction in marketplace lending. Dick and Lehnert (2010) finds that credit card lending has a much larger effect as they find that a small 4% increase in the growth of credit card lending is associated with a 10-16% rise in personal bankruptcies.

[6] Panel A of Table 1 provides a comparison of several loan and borrower characteristics for loans requested for medical bill payments, debt refinancing and other purposes. Mean values of loan volume, interest rates, loan maturities are similar for all these loan categories. Likewise, borrowers' FICO scores, annual income and job length do not exhibit significant differences. The average debt-to-income (DTI) ratio of individuals who request loans for health related expenses is 10%. DTI ratios of borrowers requesting loans for debt consolidation and other purposes exceeds 20%.

2001) and higher interest rates (Gropp et al., 1997) on other borrowers and non-filers. Bankruptcy filers also suffer from a tarnished credit record and difficulties finding housing and employment (Han and Li, 2011), ten times higher delinquency rates on new debt (Cohen-Cole et al., 2013), and the risk of becoming a target for onerous and pernicious credit offers (Han et al., 2011) after filing for bankruptcy.[7]

Our findings add to the literature on credit provision and bankruptcy filing. Skiba and Tobacman (2019) show that payday loan credit access worsens households' cash flows positions, thereby increasing the probability of default and bankruptcy, while Gross, Notowidigo, and Wang (2014) show that additional non-credit financing in the form of tax rebates eases liquidity constraints and prompts more filings. We show, in contrast, that access to marketplace credit has the potential to reduce bankruptcies. The technology behind marketplace lending allows for the collection and use of non-standard personal information on borrowers, which lends further credence to the finding by Iyer et al. (2015) that new lending technology can enhance lending efficiency.

Chava et al. (2019) suggest that marketplace loan take-up increases borrowers' credit rating in the short-run, while over time, marketplace borrowers exhibit a higher number of delinquencies. Our finding that marketplace lending rationing increases the incidence of personal bankruptcy can be reconciled with their results. A borrower becoming delinquent on a single debt obligation does not mechanically lead to a bankruptcy filing (Drozd and Serrano-Padial, 2017).[8] Braggion et al. (2022) document that marketplace lending platforms allow Chinese residents to engage in regulatory arbitrage. Faced with tighter residential mortgage loan to value regulation in China, individuals cover additional down payment with funds raised through marketplace lending platforms. Such practice subsequently leads to higher delinquencies and defaults among these individuals who become over-indebted. However, unlike in China, information on debt raised by individuals through online lending platforms in the United States is shared with major credit bureaus. As such, both banks and marketplace platforms in the U.S. have a more accurate overview of individuals' financial situation.[9]

Our paper also contributes to the literature on technological progress in financial markets and its effect on households by documenting that, in contrast to other forms of credit, the provision of marketplace credit is negatively related to the incidence of personal bankruptcy, particularly among low-income households. The existing literature generally finds that default, bankruptcy, and the availability of traditional credit exhibit a positive relationship (Domowitz and Sartain, 1999; Gross and Souleles, 2002; Fay et al., 2002, Dick and Lehnert, 2010; Gathergood et al., 2019; Skiba and Tobacman, 2019). Moreover, Livshits et al. (2016) document that improvements in traditional lending technology in the context of credit card debt raise the number of personal bankruptcies. This positive relationship is likely a result of increasing reliance on hard information in screening potential borrowers by traditional lenders (Dick and Lehnert, 2010).

Our findings provide a novel result but can be reconciled with the existing literature because the innovative technology behind marketplace lending allows for the fast collection and use of new personal information on borrowers, which improves screening and the efficiency of financial intermediation. Our paper thereby lends further support to the results by Jagtiani and Lemieux (2019) and Vallee and Zeng (2019). Our

result that restricting access to this new lending technology may lead to more personal bankruptcies is also related to the idea in Allen et al. (2016) that shocks inhibiting information collection on borrowers can lead to a rise in personal bankruptcies.[10]

2. Background: Personal bankruptcy, usury limits, marketplace lending, and the treatment event

We discuss the institutional background covering the bankruptcy code (Section 2.1), usury laws (Section 2.2), the U.S. marketplace lending industry (Section 2.3), and the treatment event (Section 2.4).

2.1. Personal bankruptcy in the U.S.

Filing for bankruptcy allows a household to discharge debt immediately or over time with a repayment plan. A debtor starts the process by filing with a bankruptcy court.

Different chapters (7, 11, 12, or 13) can be filed for in the U.S.[11] Chapter 7 wipes out the dischargeable debt after any non-exempt assets have been sold. Many creditors filing under this chapter however have little or any non-exempt property. Under Chapter 13, the borrower agrees with the debtor to a repayment plan that restructures the debt, typically over three to five years. Chapter 13 wipes out more debt than a Chapter 7 filing. Similar to Chapter 13, Chapter 11 allows for debt restructuring, but debtors do not need to turn over their disposable income as under Chapter 13. The cases under Chapter 11 are typically filed by relatively wealthier households given that these cases are significantly more complex and costly compared to cases filed under Chapter 7 and Chapter 13.[12] Finally, Chapter 12 allows agricultural businesses, such as farmers and commercial fishermen, to file for personal bankruptcy.

Personal bankruptcy filings in the U.S. have been in decline in recent years. The vast majority (97%) of cases are consumer filings, and, prior to 2014, there were generally over 1 million consumer bankruptcies per year, two-thirds of which filed are under Chapter 7. Since 2014, the number of filings has steadily fallen to about 750,000 per year by the end of 2017, a low last seen in 1994. Personal business bankruptcies have also fallen to about 25,000 business filings per year, down from 45,000 filings per year before 2014.[13]

2.2. Usury laws in the U.S

The U.S. Code of Laws states that for national banks, the interest rate on loans deemed usurious is forfeited. If some of the interest has already been paid, the borrower can recover up to twice the amount of the above-usury interest. According to U.S. Code 12 §86, the usury limit for loans originated by national banks is determined by the "interest at the rate allowed by the laws of the State, Territory, or District where the

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[7] The distress remains after bankruptcy flag removal (Dobbie et al., 2019).

[8] While delinquency is the point of entry to default, Athreya et al. (2018) show that cessation of debt repayments is not highly persistent, given that less than 1% of borrowers that are behind on payments in one quarter will go into bankruptcy the next quarter, while the vast majority (85%) resume payments the next quarter.

[9] Braggion et al. (2022) also acknowledge that the collection of information on marketplace lending by credit bureau(s) could significantly mitigate regulatory arbitrage and its adverse effects they consider in their study.

[10] The alternative question of how the court verdict we study affects marketplace loans on the *secondary* market in terms of trading prices, borrower quality, loan default probability and marketplace loan size has been addressed by Honigsberg et al. (2018). Our paper instead focuses on the *primary* market impact and goes beyond that by addressing the impact on household financial health. In further contrast, we document how usury laws for fintech loans affect borrowers across different credit scores, incomes, levels of indebtedness and usages of marketplace loans, which allows us to address the different focus of our paper, namely how marketplace lending affects personal bankruptcy.

[11] US Courts Basics: www.uscourts.gov/services-forms/bankruptcy/bankruptcy-basics/process-bankruptcy-basics.

[12] The filing fee of Chapter 11 bankruptcies is five times higher than other chapters. US Courts, "Bankruptcy Court Miscellaneous Fee Schedule": https://www.uscourts.gov/services-forms/fees/bankruptcy-court-miscellaneous-fee-schedule.

[13] American Bankruptcy Institute (2019): https://www.abi.org/newsroom/bankruptcy-statistics.

bank is located."[15]

Usury limits and penalties vary by state, borrower type, and loan term.[15] Some states, like Utah, have no usury limit, while others have high-interest caps and harsh penalties.[16] Usury laws in the U.S. have evolved over time. Starting in 1833, the idea was established that a loan is valid when made, i.e., a non-usurious loan cannot be made usurious by a subsequent transaction. In addition, the 1863 National Bank Act (NBA) included the federal pre-emption doctrine meaning that federal laws trump state usury laws for state-chartered and national banks. Subsequently, in the first half of the 20th century, the Russell Sage Foundation engaged in an effort to improve credit conditions for poorer households and advocated the adoption of Uniform Small Loan Laws (USLL), which allows lenders to charge interest rates exceeding the state usury limit if the lenders obtain relevant state licenses. The USLL are credited with establishing the focal 36% as the maximum APR still found on many types of loans today (Saunders, 2013). Subsequently, a momentous decision by the Supreme Court in *Marquette National Bank v. First of Omaha Serv. Corp* in 1978 confirmed that national banks could charge interest up to the rate in which the bank is headquartered, irrespective of the borrower's state of residence. In 1980, Congress passed Depository Institutions Deregulation and Monetary Control Act (DIDMCA) expanding federal pre-emption under the NBA to all federally insured banks. Combined with advances in information technology and credit scoring models, this proved to be a fillip for the emergence of a nationwide credit card industry and secondary debt markets in the 1980s (Staten, 2008).

In the 21st century, the permissive legal environment combined with the Internet and ever more widespread internet and communication technology (ICT) adoption among U.S. households in the 2000s paved the way for the rise of new financial technologies, including marketplace lending. In the early stage of the industry, online lenders were observing the usury laws of borrowers' states of residence. But platforms thereafter decided to let the overall interest rate cap for marketplace loans approach 36%, irrespective of a borrower's home state usury limit (Rigbi, 2013). Some marketplace lenders achieved this by partnering with WebBank, an FDIC-insured bank chartered in Utah, a state with no usury ceiling. When the partnering bank receives a loan application from an online platform, it originates the loan and sells it to the platform. The platform then sells notes to investors pledging to fund the loan. This model allows marketplace lending platforms to 'export' the no-usury limit of Utah to borrowers residing in virtually any state in the U.S. by relying on the aforementioned federal pre-emption of state usury laws and the valid-when-made doctrine.

However, in May 2015, the verdict in *Madden vs. Midland Funding LLC* court case precipitously cast doubt upon the enforceability of above-usury marketplace loans issued to borrowers in Connecticut and New York, thereby threatening the loan origination model of marketplace lenders.

2.3. Marketplace lending in the U.S

The growth of the marketplace lending industry has been rapid. In

2017, marketplace lenders originated 38% of all personal loans, up from 1% in 2010.[17] The industry has evolved from peer-to-peer lending into what is now described as 'marketplace lending.' Recently, self-directed retail investors play a small role in providing funds for these platforms relative to institutional investors such as banks, asset managers, insurance companies, hedge funds, and other large non-bank investors.[18]

To obtain a marketplace loan, a borrower makes a proposal for a loan by posting a listing, indicating the purpose and amount of the loan, besides providing other application information to the platform. Investors choose which proposals to fund and whether to fund a portion or the whole requested amount. Once sufficiently funded, the loan is originated.

When lending through marketplace platforms takes the form of a traditional peer-to-peer (P2P) transaction, the investors directly supply the funds to borrowers via the lending platform. However, the common model of the largest platforms is to co-operate with a fronting bank in facilitating loans. The bank issues the loan to the borrower but immediately sells and assigns the loan to the lending platform, which permanently retains ownership of the debt. The price is the loan's principal amount. In a separate second transaction, the marketplace platform receives the principal of the loan from the investors that selected to fund the loan (Mason, 2016). Investors financing the loan become creditors of the marketplace platform. The fronting bank has no obligation to the loan's investors. In case of delinquency or default, as the owner of the loan, the marketplace platform is responsible for any necessary debt collection (Verstein, 2012).

2.4. Treatment event

The marketplace lending model came under scrutiny when the treatment event, the *Madden vs. Midland Funding LLC case,* suddenly raised the question of whether the marketplace platform, instead of the fronting bank, is the 'true lender.' The verdict poses the issue of whether, by partnering with a bank in a state with no usury laws, marketplace lenders may rely on the federal preemption of state usury laws, which the *National Bank Act* and *Depository Institutions Deregulation and Monetary Control Act* reserve for national and state-chartered banks, including their agents and subsidiaries.

The following describes the key aspects of the *Madden vs. Midland* case, our treatment event.[19]

In 2005, Ms. Saliha Madden, a New York resident, opened a credit card account with Bank of America (BoA). Ms. Madden accrued debt using the card for purchases. In the following year, BoA, a national bank headquartered in North Dakota, sold its credit-card program to FIA Card Services N.A. (FIA), a national bank in Delaware. In 2008 Ms. Madden became delinquent on loan payments. FIA considered the debt to be uncollectable. It charged off Madden's debt and sold it to Midland Funding LLC (Midland), one of the US's largest purchases of unresolved consumer debt.

Midland is not a chartered national bank, unlike Bank of America and FIA. In November 2010, Midland attempted to collect payments from Ms. Madden at 27% interest as permitted by Delaware usury law. In response, Ms. Madden filed a lawsuit against Midland, alleging in the ensuing 2011 class-action suit that the debt collector violated New York's criminal usury law prohibiting interest rates exceeding 25%. Midland objected, maintaining that 27% can be charged as the loan was obtained from a national bank (FIA) in Delaware which permits such an interest rate. In September 2013, the *District Court for Southern New York* ruled in favor of Midland based on the National Bank Act's preemption of federal law over state usury laws for national banks. The court held

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[14] US Code (2019) http://uscode.house.gov/browse/prelim@title12/chapter2/subchapter4&edition=prelim.

[15] The discussion is based on Marvin (2016).

[16] In Connecticut, the general usury limit is 12% (see Connecticut General Statute §37.4). However, there are wide range of exemptions including loans by state or federal banks, savings and loans associations, credit unions, student loans, and others (see Statute §37.9). Source: https://www.cga.ct.gov/current/pub/chap_673.htm. In New York, any loan carrying an interest exceeding 16% constitutes civil usury, and loans surpassing 25% of interest are considered criminal usury, a class E felony (New York Banking Law, Section 14-A). The owner of a usurious loan in New York forfeits any interest and the complete principal of the loan (see N.Y. Penal Law 190.40). Source: https://www.nysenate.gov/legislation/laws/BNK/14-A.

[17] See TransUnion (2019) data.

[18] Lending Club, ibid.

[19] The exposition is based on Mason (2016), Marvin (2017), and Honigsberg et al. (2017).

that 27% was permitted as the loan was governed by the usury laws in Delaware, the state where the bank from which Midland bought the loan is chartered.

In May 2015, however, after Ms. Madden filed an appeal against the initial decision by the lower New York district court, the U.S. *Court of Appeals for the Second Circuit,* which covers all of New York, Connecticut, and Vermont, ruled in favor of Ms. Madden. The ruling reversed the earlier decision by the lower court. The court held that the borrower's state usury laws cannot be circumvented in this case because Midland, the debt collector:

> *"neither is a national bank nor a subsidiary or agent of a national bank or is otherwise acting on behalf of a national bank, and because application of [New York's] state law on which Madden's claim relies would not significantly interfere with any national bank's ability to exercise its powers under the National Bank Act."*[20]

In other words, the *Madden* ruling indicates that exemption from state usury laws enjoyed by national banks and their subsidiaries no longer applies to loans once they are sold to non-bank financial institutions. Interest and principal of such loans are null and void in New York and Connecticut, while in Vermont, only the interest above the usury level is to be considered null. While *Madden* did not relate to marketplace lending directly, the decision has created legal uncertainty about the enforceability of any marketplace loans whose interest rate exceeds the usury limit in New York, Connecticut, and Vermont. That is because the loan origination model behind marketplace platforms consists of loans being facilitated by a bank but then sold outright to marketplace platforms, which are currently designated as non-bank financial institutions by the OCC.

In reaching its verdict, the Second Circuit court noted the scope of its decision by distinguishing its case from three previous legal precedents.[21] First, any revolving loans, such as credit cards, in which the bank retains an interest are left unaffected by *Madden* (see *Krispin v May*). Second, *Madden* does not apply to closed-end loans, such as mortgages, if the bank charges the interest rate (see *Phipps v FDIC*). Third, *Madden* does not affect any loans where the non-bank acts as the agent or subsidiary of a national or state-chartered bank (see *FDIC. v. Lattimore Land Corp*). In other words, *Madden* only applies if a bank issues and assigns a loan to a non-bank and if the bank retains no ongoing economic interest in the loan, and when the loan's interest rate is raised beyond the usury limit of the borrower *ex-post* loan assignment. In the view of an expert legal opinion by Horn and Hall (2017), "*Madden* should have no material relevance to [...] banks and loan originators and servicers that work in cooperation with one another on loan origination and servicing activities." This is also reflected in the response by rating agencies, industry reports, and legal briefs, which have singularly concentrated on the verdict's effect on marketplace lending.[22]

Marketplace lenders have attempted to cushion the verdict's impact by letting the fronting bank originating loans retain an interest in the loan after it was sold to marketplace platform. As a result, marketplace platform could be considered as a 'subsidiary' or 'agent' of the national bank and, allowing them to circumvent the borrower's state usury laws. Despite restructuring their origination model the regulatory uncertainty remains. Several marketplace platforms continue to point out in their investment prospectus, as filed with the Securities and Exchange Commission (SEC), that *Madden* casts a significant doubt on fintech lending, particularly enforceability of marketplace loans.[23]

3. Hypotheses Development

3.1. The effect of the treatment event on marketplace lending

Economic theory on the effects of usury laws and interest rate controls informs our prior expectations about how the treatment event affects marketplace loan availability. As early as Locke (1691), it was recognized that usury limits could trigger credit rationing.[24] Our treatment event provides a quasi-natural experiment that allows us to derive novel insights into how interest rate controls affect credit supply in modern financial markets augmented by new lending technology.

A price ceiling set below the equilibrium level leads to rationing, with the fall in quantity supplied depending on the price-elasticities of demand and supply as well as the structure of the credit market. Distinguishing credit from other types of goods is the presence of asymmetric information in the form of moral hazard (hidden action) and adverse selection (concealed information). The seminal models by Jaffee and Russell (1976), Stiglitz and Weiss (1981), and Bester (1985) suggest, first, that there are several segments to the credit market based on the risk rating of the borrower and, second, that supply is non-monotonic in that only above the risk-adjusted profit maximizing level will interest rate reductions raise credit supply. At the equilibrium interest rate, however, reductions in the price of credit will be offset by credit rationing, especially when the loan supply is elastic.

The supply of marketplace credit is likely to be particularly elastic due to the use of sophisticated computer-based credit score and risk models, which allow marketplace lenders to separate their customers into finer market segments and tailor loan terms more specifically to borrower characteristics (Hynes and Posner, 2002; Edelberg, 2006; Staten, 2008). Marketplace lenders can reduce lending to borrowers, in particular high-risk borrowers, which would have been offered above-usury interest loans and, instead, supply the capital to other risk buckets or divert the funds to altogether other investment opportunities in a different part of the credit market. We formulate the following two hypotheses related to the treatment event's effect on marketplace lending:

Hypothesis 1. *Following the treatment event, the volume and number of newly originated marketplace loans decreases.*

Hypothesis 2. *The marketplace credit rationing effects of the treatment event are more severe for borrowers with a poor credit rating.*

3.2. The effect of marketplace lending on bankruptcy filing

The financial technology associated with marketplace lending is an improvement in screening technology. The innovation consists in the collection and use of alternative data that feed into sophisticated statistical tools used to analyze highly dimensional datasets quickly at a low cost. Theoretical work by Vallee and Zeng (2019) suggests that marketplace lending technology constitutes a major deviation from the traditional banking model in that screening and information production are done jointly by the lending platform and its investors, resulting in superior screening outcomes.[25]

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[20] Case at https://cases.justia.com/federal/appellate-courts/ca2/14-2131/14-2131-2015-05-22.pdf?ts=1432305005.

[21] Jones Day, "Secondary Loan Markets Post-Madden" (November 1, 2016).

[22] Fitch, "Challenges Linger as U.S. Marketplace Lending ABS Rises," Reuters, (September 10, 2015).

[23] For instance, see Prosper Marketplace prospectus: https://prosper.com/Downloads/Legal/Prosper_Prospectus_2018-03-12.pdf.

[24] The first formal model of the effects of usury ceilings was proposed by Blitz and Long (1965) and there are many empirical studies of how usury laws affect the volume, risk and price of credit. See Greer (1975), Wolkin and Navratil (1981), Villegas (1982), Peterson (1983), and more recently Temin and Voth (2007) and Benmelech and Moskowitz (2010).

[25] Traditionally, the role of information production on behalf of investors has been exclusively reserved for banks (Gorton and Pennacchi, 1990; Dang et al., 2017). Aside from unsecured consumer lending, the new underwriting technology has also been shown to offer benefits in the context of fintech mortgage lending, including faster funding (Fuster et al., 2019) and superior credit risk assessment (Buchak et al., 2018; Fuster et al., 2018).

A wide variety of models show that improvements in screening technology reduce information asymmetries and can improve credit access along the intensive and extensive margin.[26]

Economic theory, however, is ambiguous about whether improved screening technology raises or lowers bankruptcies. Better technology may reduce Type I errors of mistakenly classifying a borrower as riskier than his actual risk type. As new borrowers get access to loans, after previously being excluded from credit markets by an older technology unable to correctly price loans to borrowers that are relatively riskier than existing borrowers' average risk profile, the number of bankruptcies may rise ('better screening raises bankruptcies'). However, better technology also reduces Type II errors of mistakenly classifying a borrower as less risky than his actual risk type ('better screening reduces bankruptcies'). A model making this intuition explicit is found in Livshits et al. (2016), who provide closed-form solutions suggesting that the net effect of better screening technology on bankruptcy features such a tradeoff.

Therefore, access to new financial technology associated with marketplace lending may, on the one hand, have a beneficial effect on personal bankruptcies when better screening improves risk-based pricing for existing borrowers (Livshits et al., 2016). Empirically, marketplace platforms have been documented to provide quickly accessible consumer loans (Fuster et al., 2019), which are cheaper than credit cards (De Roure et al., 2018) and serve previously underserved borrowers (Jagtiani and Lemieux, 2019; Schweitzer and Barkley, 2017; Tang, 2019). By allowing borrowers to refinance existing debt at cheaper interest rates as well as smooth adverse shocks to income or expenses, marketplace loans may ease households' financial distress.[27] By restricting marketplace credit, the treatment event may thus increase bankruptcy filings:

Hypothesis 3.1. *Restricting marketplace lending increases personal bankruptcy filings.*

Access to new financial technology associated with marketplace lending may, on the other hand, have an adverse effect on personal bankruptcies. Better screening could worsen the incidence of bankruptcies by expanding credit access to new borrowers with riskier observable characteristics (Livshits et al., 2016). Such borrowers may be more likely to overestimate their ability to repay loans due to behavioral biases (Ausubel, 1991). Additionally, better screening technology has the potential to raise bankruptcies by an intensive margin shift of increasing the amount of debt per household.[28] By restricting marketplace credit access, the treatment event, therefore, could potentially lower the number of bankruptcies:

Hypothesis 3.2. *Restricting marketplace lending decreases personal bankruptcy filings.*

4. Data and identification strategy

4.1. Data

The marketplace lending data were obtained from three of the leading and largest online platforms offering marketplace loans to U.S. residents. Our proprietary dataset includes detailed information on loan requests placed on each platform since January 2013. We identify the borrower's location of residence at the 3-digit zip code level of granularity as well as the loan listing start date, origination date, loan purpose, the amount of money requested, the amount of funds granted as well as the borrower's FICO score and debt-to-income ratio. The loan-level data also allow us to calculate the monthly volume and number of delinquent and charged-off loans at the 3-digit zip code level.

About 70 marketplace loans are newly originated in the average 3-digit zip code and month. The average marketplace borrower in our sample applies for a loan of $12,818. The average interest rate on marketplace loans is 15%, and their principal can range up to $40,000. The average annual income of a marketplace borrower is $65,000. Differentiating borrowers by credit risk, these figures range from an average loan size of $6,550 at 21.75% interest for the riskiest borrowers to an average loan size of $16,100 at 7.81% interest for the least risky marketplace borrower group. On average, in our sample, many loans are requested for debt refinancing (60%), small personal business loans (4%), and medical expenses (13%).[29]

Bankruptcy filing data were obtained from the U.S. Federal Judicial Centre (FDJ). The FDJ provides us with information on each bankruptcy case filed until September 2018, differentiated by the various chapters under which petitions were filed. The dataset distinguishes between personal business and consumer bankruptcies and provides the annual income of each filer and the total amount of filers' liabilities and dischargeable debt. The dataset, moreover, allows us to identify the residence of filers up to the 5-digit zip code and, in some cases, even up to the 9-digit zip code. To match the geographic granularity of the marketplace lending data, we aggregate bankruptcy filings per month at the 3-digit zip code level. However, Table A.1 shows that our baseline results for changes in personal bankruptcy continue to hold when performed at the 5-digit zip code level.

The average 3-digit zip code area exhibits 86 individuals filings for personal bankruptcy, of which 57 cases are Chapter 7 and 28 cases are Chapter 13 filings. Of the total number of bankruptcy filings, the share of consumer bankruptcy and personal business bankruptcies is, respectively, 98.8% and 1.2%. Filers have an average income of $42,618, with income for Chapter 7 filers ($38,355) being lower than Chapter.13 filers ($50,872). Households filing for consumer bankruptcy tend to have less income ($42,533) relative to those filing for personal business bankruptcy ($65,315).[30]

The New York Federal Reserve Center for Microeconomic Data provides us with information on the annual volume of consumer lending in each U.S. state differentiated by credit card lending (revolving accounts from banks, bankcard companies, national credit card companies, credit unions, and savings & loan associations), student loans (from banks, credit unions, and other financial institutions as well as federal and state governments) and auto loans (from banks, credit unions, savings and loan associations, as well as automobile dealers and

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[26] See Drozd and Nosal (2008), Narajabad (2012); Athreya et al. (2012); Livshits (2015); Livshits et al. (2016); Drozd and Serrano-Padial (2017); Sanchez (2018); Lester et al. (2019).

[27] This is supported by the fact that marketplace loans are predominantly used for debt refinancing, especially credit card debt, or paying medical bills. Credit card debt and medical costs are one of the key determinants of personal bankruptcy (Domowitz and Sartain, 1999; White, 2007; Gross and Notowidigdo, 2011).

[28] See the theoretical models in Narajabad (2012); Sanchez (2012); and Athreya et al. (2012). Empirical evidence on the effect of a wide variety of household debt on default and personal bankruptcy is provided by Domowitz and Sartain (1999); Gross and Souleles (2002); Fay et al. (2002); Dick and Lehnert (2010); Skiba and Tobacman (2019); Livshits et al. (2007, 2010); and Gathergood et al. (2019).

[29] Other popular uses of credit are: financing cars, RVs, motorcycles, boats, vacation, engagement rings, weddings or cosmetic procedures (not included in the medical expenses category).

[30] Chapter 12 bankruptcy is available to family farmers and family fishermen and is classified as business bankruptcy. Therefore, we are not able to use Ch. 12 for non-business bankruptcies in our analysis.

automobile financing companies).[31]

The sample period covers the 69-month period from January 2013 to September 2018 for 795 3-digit zip codes. In total, there are 929 3-digit zip code areas in the U.S. On average, a 3-digit zip code area covers a population of 350,000 people. We remove from the sample locations in states which legally restrict or restricted residents from raising funds via online platforms during our sample period. These states include Iowa, Maine, Mississippi, Nebraska, North Dakota, and West Virginia. We further exclude zip codes in unincorporated territories of the U.S. and IRS zip codes and zip codes of the U.S. Armed Forces outside the U.S. Our final sample includes 54,885 observations. Table 1, Panel B presents summary statistics for the variables used in our regressions.

4.2. Main outcome variables

The number of bankruptcy filings and newly originated marketplace loans (number and volume) per month in a 3-digit zip code are the main outcome variables of interest.

To test the effect of the treatment event on bankruptcy filing rates, we calculate the total number of bankruptcies filed per month and track the total number of filings differentiated into personal business and consumer bankruptcy filings in each zip code per month and by the chapter of the filing. We also calculate the number of all different chapter filings and personal business and consumer filings separately.

To examine how the treatment event affects the intensive and extensive margin of marketplace credit supply, we first analyze the verdict's effect on the dollar volume and number of newly originated marketplace loans. We also calculate the number and volume of marketplace loans whose interest rate exceeds a borrower's state usury limit, as we expect that these loans are likely to be most affected by the verdict. Second, we estimate how the treatment event affects marketplace borrowers across different risk profiles. We calculate the volume and number of marketplace loans for borrowers differentiated by credit risk categories which are defined by which quantile of the distribution of FICO scores they belong to. Third, to measure how the court case verdict affects marketplace credit supply across loans for different purposes, we calculate the dollar amount and number of marketplace loans requested for debt refinancing, medical bills, and small business expenses, all of which are loans that ought to help households avoid filing for bankruptcy. We estimate the effect of the treatment event on the total volume of these loan categories and the volume of loans borrowed for all other purposes.

All our dependent variables (denoting marketplace lending and bankruptcy filings) enter the regressions as a log of one plus the value of the variable.

4.3. Identification strategy

To formally test the hypotheses linking marketplace lending restrictions to personal bankruptcy, we use difference-in-differences estimations. We exploit the court verdict as an exogenous source of variation in the supply of marketplace lending. We compare the evolution of the volume and the number of newly originated marketplace loans and bankruptcy filings between the treatment (all 3-digit zip codes in Connecticut and New York) and control group (3-digit zip codes in all other states) before and after the verdict. We estimate the specifications of the following form:

$$ln(Y)_{zm} = \beta_1 Post_m * Treated_z + \beta_2 Treated_z + \beta_3 * Post_m + \varepsilon_{zm}. \quad (1)$$

Y denotes our outcome variables for 3-digit zip code z in month m. $Post$ is a dummy variable equal to 1 for all months following the

decision by the U.S. Court of Appeals for the Second Circuit in the case of *Madden vs. Midland Funding LLC* in May 2015, and zero for months preceding the verdict. *Treated* is a dummy variable equal to 1 for all 3-digit zip codes located in Connecticut and New York, and zero for zip codes located in all other U.S. states. The main coefficient of interest, β_1, measures the effect of the treatment event on our dependent variables. It captures the change in the volume or number of marketplace loans and bankruptcy filings in 3-digit zip codes in Connecticut and New York relative to the change in those variables in all other 3-digit zip codes.[32,33]

We augment the baseline specification (Eq. (1)) with a set of control variables, 3-digit zip code- and month-fixed effects (α_z and γ_m), which absorb the terms *Treated* and *Post*. The resulting auxiliary specification takes the form:

$$ln(Y)_{zm} = \alpha_z + \beta Post_m * Treated_z + \delta Controls_{zm} + \gamma_m + \varepsilon_{zm}. \quad (2)$$

To control for factors affecting changes in bankruptcy filings and marketplace loan origination, we also include the following control variables, aggregated at the 3-digit zip code and month level and measured in logs: the total dollar amount of funds requested through online platforms (*Requested funds*), the average dollar amount of debt discharged by bankruptcy filers (*Dischargeable debt*), as well as the average total liabilities of residents filing for bankruptcy (*Total liabilities*).[34] In all specifications, we cluster heteroscedasticity-adjusted standard errors at the 3-digit zip code-level to account for serial correlation following Bertrand et al. (2004).[35]

5. Results

In the following, we discuss the effect of the treatment event on marketplace lending (Section 1) and personal bankruptcy filing (Section 2). We analyze these effects across different income groups (Section 3) and present evidence to reject plausible alternative mechanisms for the observed rise in bankruptcy filings following the verdict (Section 4). Finally, we analyze the persistence of the effects from marketplace lending restrictions on precipitating personal bankruptcy (Section 5).

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[31] The Federal Reserve Bank of New York provides household debt statistics by state based on a nationally representative random sample from Equifax. See https://www.newyorkfed.org/microeconomics/databank.html.

[32] To ensure that the logarithm specification does not lead to an over-estimation, which could occur when including 3-digit zip codes that did not record any marketplace loan originations in individual months during the sample period, we remove all observations for zip codes in which no marketplace loans were originated in at least one month. This restricts our sample period to 593 3-digit zip codes. The results of this test, presented in Table A.2, support our baseline estimates.

[33] The treatment group only includes Connecticut and New York because borrowers in these two states are relieved from paying the principal amount and interest of above-usury marketplace loans. In contrast, borrowers in Vermont are only relieved from paying the interest above the borrower's state usury limit. We only include Connecticut and New York to preserve homogeneity within the treatment group. However, our results are robust to including 3-digit zip codes from Vermont in the treatment group or removing Vermont's zip codes from the sample entirely. The tests are presented in Appendix A, Table A.3.

[34] In Table A.4 in an online appendix, we present the results obtained using two alternative specifications. In Panel A, we display results estimated by saturating specification 2 with state-month level unemployment rates and state-quarter Gross State Product and personal income growth rates. Including these variables helps us determine if the economic conditions in the affected states are likely to drive changes in bankruptcy filings and the availability of marketplace loans. In Panel B, we show results estimated using specifications that include zip code and month-fixed effects and exclude all control variables.

[35] Our findings are also robust to clustering standard errors at the level at which the treatment event varies, i.e., at the level of the eleven U.S. Court of Appeals Districts, and in addition at the 3-digit zip code and month, state, and county level. These results are presented in Appendix A, Table A.5.

Table 1
Summary Statistics.

Panel A: Comparison of loan and borrower characteristics by marketplace loan purpose			
	Mean values		
Loans requested for:	Medical expenses	Debt refinancing	All other purposes
Loan characteristics			
Volume	14,172	14,239	13,711
Interest rate	14.74	14.97	15.98
Maturity	40.87	42.69	39.76
Borrower characteristics			
FICO score	695	692	689
Annual income	75,374	73,900	71,999
Current employment length	3.9	3.8	4
Debt-to-income ratio	10%	20.40%	20.20%

Panel B: Summary statistics						
Variable	N	Mean	St Dev	Min	Median	Max
Dependent variables						
Marketplace loans						
Volume - above usury limit	54,855	289,740.299	2,524,042.263	0	0	164,645,696
Volume - above usury limit (ln)	54,855	6.060	6.231	0	0	18.919
Number - above usury limit	54,855	23.736	195.761	0	0	12,818
Number - above usury limit (ln)	54,855	1.508	1.745	0	0	9.459
Volume – total	54,855	869,307.657	7,613,453.490	0	300,050	390,174,112
Volume - total (ln)	54,855	12.351	2.236	0	12.612	19.782
Number – total	54,855	69.928	564.656	0	24	28,449
Number - total (ln)	54,855	3.199	1.291	0	3.219	10.256
Volume - Borrower Rating 1	54,855	65,779.241	711,567.676	0	12,100	43,548,236
Volume - Borrower Rating 1 (ln)	54,855	6.913	4.984	0	9.401	17.589
Volume - Borrower Rating 2	54,855	137,802.792	1,306,567.790	0	45,000	70,833,536
Volume - Borrower Rating 2 (ln)	54,855	9.395	4.002	0	10.714	18.076
Volume - Borrower Rating 3	54,855	394,507.911	3,025,784.564	0	145,500	162,295,584
Volume - Borrower Rating 3 (ln)	54,855	11.380	2.822	0	11.888	18.905
Volume - Borrower Rating 4	54,855	73,564.472	113,777.328	0	34,500	1,659,400
Volume - Borrower Rating 4 (ln)	54,855	8.417	4.638	0	10.449	14.322
Volume - Borrower Rating 5	54,855	99,926.242	1,161,614.693	0	28,000	57,683,052
Volume - Borrower Rating 5 (ln)	54,855	7.892	4.880	0	10.240	17.870
Number - Borrower Rating 1	54,855	11.838	72.746	0	2	4,402
Number - Borrower Rating 1 (ln)	54,855	1.355	1.253	0	1.099	8.390
Number - Borrower Rating 2	54,855	15.257	113.983	0	4	6,034
Number - Borrower Rating 2 (ln)	54,855	1.783	1.115	0	1.609	8.705
Number - Borrower Rating 3	54,855	27.873	213.708	0	11	10,931
Number - Borrower Rating 3 (ln)	54,855	2.460	1.190	0	2.485	9.299
Number - Borrower Rating 4	54,855	6.543	6.767	0	2	86
Number - Borrower Rating 4 (ln)	54,855	1.232	0.945	0	1.099	4.466
Number - Borrower Rating 5	54,855	6.213	73.740	0	2	3,600
Number - Borrower Rating 5 (ln)	54,855	1.125	0.956	0	1.099	8.189
Volume - relevant loans	54,855	715,344.704	6,272,861.871	0	250,225	332,709,216
Volume - relevant loans (ln)	54,855	12.115	2.391	0	12.430	19.623
Volume - debt refinancing loans	54,855	607,369.317	6,019,028.442	0	206,125	317,587,264
Volume - debt refinancing loans (ln)	54,855	11.590	2.411	0	12.414	19.576
Volume - medical expenses loans	54,855	89,773.048	181,550.268	0	28,433	117,783,170
Volume - medical expenses loans (ln)	54,855	9.760	4.262	0	9.356	16.694
Volume - personal business loans	54,855	18,835.090	111,685.118	0	6,078	7,333,081.5
Volume - personal business loans (ln)	54,855	2.242	4.157	0	5.232	15.808
Volume - other loans	54,855	153,962.954	1,393,770.805	0	46,650	71,119,360
Volume - other loans (ln)	54,855	9.583	3.843	0	10.75	18.08
Number - relevant loans	54,855	51.011	451.255	0	18	23,836
Number - relevant loans (ln)	54,855	2.949	1.255	0	2.944	10.079
Number - debt refinancing loans	54,855	40.496	428.068	0	14	22,472
Number - debt refinancing loans (ln)	54,855	2.929	1.253	0	2.944	10.02
Number - medical expenses loans	54,855	9.039	18.052	0	4	4,536
Number - medical expenses loans (ln)	54,855	1.384	0.504	0	0.954	7.338
Number - personal business loans	54,855	1.568	8.135	0	0	533
Number - personal business loans (ln)	54,855	0.407	0.433	0	0	6.28
Number - other loans	54,855	15.917	116.359	0	6	5,714
Number - other loans (ln)	54,855	1.901	1.209	0	1.946	8.651
Personal bankruptcies – Number						
All personal bankruptcies	54,855	86.522	124.078	0	49	2,174
All personal bankruptcies (ln)	54,855	3.792	1.259	0	3.912	7.685
Chapter 7 bankruptcies	54,855	57.583	80.330	0	31	1,345
Chapter 7 bankruptcies (ln)	54,855	3.412	1.214	0	3.466	7.205
Chapter 11 bankruptcies	54,855	0.633	2.805	0	0	280
Chapter 11 bankruptcies (ln)	54,855	0.268	0.520	0	0	5.638

Table 1 (*continued*)

Chapter 12 bankruptcies	54,855	0.035	0.217	0	0	5
Chapter 12 bankruptcies (ln)	54,855	0.023	0.132	0	0	1.792
Chapter 13 bankruptcies	54,855	28.271	54.225	0	12	1,152
Chapter 13 bankruptcies (ln)	54,855	2.513	1.361	0	2.565	7.05
Consumer bankruptcies	54,855	85.544	122.932	0	48	2,155
Consumer bankruptcies (ln)	54,855	3.780	1.260	0	3.892	7.676
Chapter 7 consumer bankruptcies	54,855	57.077	79.711	0	31	1,330
Chapter 7 consumer bankruptcies (ln)	54,855	3.402	1.215	0	3.466	7.194
Chapter 11 consumer bankruptcies	54,855	0.170	0.552	0	0	10
Chapter 11 consumer bankruptcies (ln)	54,855	0.100	0.287	0	0	2.398
Chapter 13 consumer bankruptcies	54,855	28.268	54.223	0	12	1,152
Chapter 13 consumer bankruptcies (ln)	54,855	2.512	1.361	0	2.565	7.05
Personal business bankruptcies	54,855	0.978	3.216	0	0	280
Personal business bankruptcies (ln)	54,855	0.398	0.618	0	0	5.638
Chapter 7 personal business bankruptcies	54,855	0.506	1.331	0	0	96
Chapter 7 personal business bankruptcies (ln)	54,855	0.254	0.474	0	0	4.575
Chapter 11 personal business bankruptcies	54,855	0.463	2.652	0	0	280
Chapter 11 personal business bankruptcies (ln)	54,855	0.197	0.452	0	0	5.638
Chapter 12 personal business bankruptcies	54,855	0.007	0.089	0	0	3
Chapter 12 personal business bankruptcies (ln)	54,855	0.005	0.058	0	0	1.386
Chapter 13 personal business bankruptcies	54,855	0.003	0.054	0	0	2
Chapter 13 personal business bankruptcies (ln)	54,855	0.002	0.036	0	0	1.099
Non-performing loans – Number						
Delinquent loans late 16-29 days/Volume of all loans	54,855	0.249	0.536	0	0.179	100
Delinquent loans late 16-29 days/Volume of all loans (ln)	54,855	0.196	0.215	0	0.165	4.615
Delinquent loans late 30-59 days/Volume of all loans	54,855	0.793	0.816	0	0.786	50
Delinquent loans late 30-59 days/Volume of all loans (ln)	54,855	0.515	0.365	0	0.580	3.932
Delinquent loans late 60-89 days/Volume of all loans	54,855	0.622	0.688	0	0.585	50
Delinquent loans late 60-89 days/Volume of all loans (ln)	54,855	0.424	0.336	0	0.460	3.932
Delinquent loans late 90+ days/Volume of all loans	54,855	0.524	0.546	0	0.471	20
Delinquent loans late 90+ days/Volume of all loans (ln)	54,855	0.369	0.314	0	0.386	3.045
Charged off loans /Volume of all loans	54,855	0.501	0.569	0	0.445	50
Charged off loans /Volume of all loans)	54,855	0.355	0.311	0	0.368	3.932
Non-performing loans originated prior to the treatment event						
Delinquent loans late 16-29 days/Volume of all loans	54,855	0.282	0.767	0	0	100
Delinquent loans late 16-29 days/Volume of all loans (ln)	54,855	0.190	0.298	0	0	4.615
Delinquent loans late 30-59 days/Volume of all loans	54,855	0.928	1.338	0	0.717	100
Delinquent loans late 30-59 days/Volume of all loans (ln)	54,855	0.530	0.475	0	0.541	4.615
Delinquent loans late 60-89 days/Volume of all loans	54,855	0.775	1.247	0	0.514	100
Delinquent loans late 60-89 days/Volume of all loans (ln)	54,855	0.453	0.455	0	0.415	4.615
Delinquent loans late 90+ days/Volume of all loans	54,855	0.688	1.182	0	0.394	100
Delinquent loans late 90+ days/Volume of all loans (ln)	54,855	0.408	0.441	0	0.332	4.615
Charged off loans /Volume of all loans	54,855	0.695	1.133	0	0.370	50
Charged off loans /Volume of all loans (ln)	54,855	0.409	0.448	0	0.315	3.932
Main explanatory variables						
Post*Treated	54,855	0.044	0.205	0	0	1
Treated	54,855	0.075	0.264	0	0	1
Post	54,855	0.580	0.494	0	1	1
Control variables						
Requested funds	54,855	6,876,656.619	12,530,492.617	0	3,164,175	490,868,000
Requested funds (ln)	54,855	14.881	1.481	0	14.967	21.784
Average dischargeable debt	54,855	387,929.490	26,866,270.265	0	134,974.078	5,729,379,840
Average dischargeable debt (ln)	54,855	11.744	1.480	0	11.813	22.469
Average total liabilities	54,855	593,264.719	26,291,499.465	0	147,225.578	5,188,903,424
Average total liabilities (ln)	54,855	11.870	1.494	0	11.9	22.37

Notes. This table presents summary statistics for all dependent and explanatory variables.

5.1. Does the treatment event affect marketplace loan origination?

First, we present the treatment event's effect on marketplace lending. Table 2 reports the estimates obtained using Eqs. (1) and (2). To preview the findings, our results support Hypotheses 1 and 2, suggesting that the treatment event leads to marketplace credit rationing, particularly for less credit-worthy borrowers that are typically in greater need of funds to overcome financial hardship.

Table 2, Panel A shows the marketplace credit rationing following the court ruling on the intensive and extensive margin of marketplace lending. The total volume and number of marketplace loans in 3-digit zip codes in Connecticut and New York declines, respectively, by 30%

and 22% relative to zip codes in all other states after the treatment event.[36] The documented reductions in marketplace lending supply are driven mainly by a fall in the volume and number of marketplace loans carrying an interest rate exceeding the borrower's state usury limit. The volume of these loans in the treatment group zip codes falls by 81% after the ruling, relative to the control group zip codes. In terms of the extensive margin, the treatment is associated with a 44% reduction in the number of above-usury limit marketplace loans in 3-digit zip codes in Connecticut and New York relative to other states.

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[36] To calculate the % change in the dependent variable we use the following formula: $\Delta y = 100*(\exp^{\beta} - 1)$. For instance, a coefficient of -0.363 on the interaction term between *Post* and *Treated* (Panel A of Table 2) suggests that, following the court ruling, marketplace lending dropped in zip codes in Connecticut and New York by 30.44%.

Table 2

Marketplace credit rationing around the treatment event.

PANEL A: Intensive and extensive margin

Dependent variable:	Above usury limit marketplace loans				All marketplace loans			
	Volume (ln)		Number (ln)		Volume (ln)		Number (ln)	
Post*Treated	-1.660***	-1.634***	-0.573***	-0.568***	-0.376***	-0.363***	-0.266***	-0.246***
	(0.196)	(0.198)	(0.053)	(0.053)	(0.053)	(0.052)	(0.013)	(0.012)
Treated	-0.801**		-0.299***		0.566***		0.257*	
	(0.331)		(0.101)		(0.182)		(0.133)	
Post	0.572***		0.452***		1.291***		1.002***	
	(0.044)		(0.017)		(0.023)		(0.007)	
Requested funds (ln)		0.184***		0.039*		0.749***		0.149***
		(0.061)		(0.020)		(0.136)		(0.043)
Dischargeable debt (ln)		-0.004		0.001		-0.008		0.004
		(0.038)		(0.009)		(0.015)		(0.004)
Total liabilities (ln)		-0.013		-0.000		0.006		-0.003
		(0.039)		(0.009)		(0.014)		(0.004)
3-Digit Zip Code FE	no	yes	no	yes	no	yes	no	yes
Year-Month FE	no	yes	no	yes	no	yes	no	yes
R-squared	0.008	0.597	0.022	0.633	0.087	0.738	0.135	0.955
Observations	54,855	54,855	54,855	54,855	54,855	54,855	54,855	54,855

PANEL B: Intensive and extensive margin – Matched sample

Dependent variable:	Above usury limit marketplace loans				All marketplace loans			
	Volume (ln)		Number (ln)		Volume (ln)		Number (ln)	
Post*Treated	-1.689***	-1.671***	-0.504***	-0.504***	-0.522***	-0.412***	-0.238***	-0.221***
	(0.212)	(0.213)	(0.056)	(0.056)	(0.147)	(0.106)	(0.028)	(0.027)
Controls	no	yes	no	yes	no	yes	no	yes
3-Digit Zip Code FE	no	yes	no	yes	no	yes	no	yes
Year-Month FE	no	yes	no	yes	no	yes	no	yes
R-squared	0.008	0.597	0.087	0.738	0.022	0.633	0.135	0.955

PANEL C: Intensive and extensive margin by borrowers' rating

Borrower rating:	1	1	2	2	3	3	4	4	5	5
Dependent variable:	Volume (ln)									
Post*Treated	-4.274***	-4.269***	-0.646***	-0.590***	-0.341***	-0.246***	-0.159	-0.091	0.162*	0.116
	(0.123)	(0.123)	(0.118)	(0.118)	(0.082)	(0.082)	(0.136)	(0.136)	(0.144)	(0.145)
R-squared	0.219	0.682	0.072	0.592	0.056	0.638	0.065	0.504	0.096	0.548
Dependent variable:	Number (ln)									
Post*Treated	-1.122***	-1.118***	-0.319***	-0.311***	-0.142***	-0.125***	0.049	0.056*	0.084*	0.092**
	(0.025)	(0.025)	(0.023)	(0.023)	(0.017)	(0.016)	(0.031)	(0.031)	(0.037)	(0.037)
R-squared	0.232	0.802	0.095	0.869	0.089	0.924	0.095	0.776	0.119	0.766
Controls	no	yes	no	yes	no	yes	no	yes	no	yes
3-Digit Zip Code FE	no	yes	no	yes	no	yes	no	yes	no	yes
Year-Month FE	no	yes	no	yes	no	yes	no	yes	no	yes

PANEL D: Intensive and extensive margin by loan purpose

Loan purpose:	Relevant loans		Debt refinancing loans		Medical expenses loans		Personal business loans		Other loans	
Dependent variable:	Volume (ln)									
Post*Treated	-0.369***	-0.368***	-0.372***	-0.311***	-0.699***	-0.687***	-0.103***	-0.088***	-0.194***	-0.151***
	(0.059)	(0.057)	(0.062)	(0.061)	(0.166)	(0.166)	(0.023)	(0.022)	(0.038)	(0.036)
R-squared	0.073	0.707	0.072	0.702	0.065	0.426	0.012	0.370	0.135	0.621
Dependent variable:	Number (ln)									
Post*Treated	-0.235***	-0.216***	-0.235***	-0.216***	-0.375***	-0.373***	-0.036***	-0.034***	-0.174***	-0.168***
	(0.014)	(0.013)	(0.014)	(0.013)	(0.051)	(0.051)	(0.011)	(0.010)	(0.027)	(0.026)
R-squared	0.116	0.947	0.115	0.946	0.065	0.560	0.013	0.488	0.183	0.884
Controls	no	yes	no	yes	no	yes	no	yes	no	yes
3-Digit Zip Code FE	no	yes	no	yes	no	yes	no	yes	no	yes
Year-Month FE	no	yes	no	yes	no	yes	no	yes	no	yes

Notes. This table reports the coefficients and standard errors clustered at the 3-digit zip code level (in parentheses) obtained using Eqs. (1) and (2). Panel A documents the effect of the treatment event on the amount and number of above usury limit and total marketplace loans obtained by borrowers from online platforms. Panel B replicates results in Panel A using a sample of treatment and control group 3-digit zip codes matched on pre-treatment characteristics. Panel C documents the effect of treatment event on the amount and number of loans by borrowers credit score, where the rating of 1 denotes the bottom 20 percentile of the FICO score and the rating of 5 denotes the top 20 percentile. Panels D reports changes in the volume and number of marketplace loans by the loan purpose. The main explanatory variable is an interaction term between the variable *Post* (equal to 1 for months after the announcement of the verdict in May 2015, and zero otherwise) and *Treated* (equal to 1 for 3-digit zip codes in the affected states Connecticut and New York, and zero otherwise). Control variables include: the logarithm of the dollar amount of funds requested through online platforms by residents in each 3-digit zip code and month (*Requested funds*), the logarithm of the dollar amount of debt discharged by bankruptcy filers 3-digit zip code and month (*Dischargeable debt*), the logarithm of average total liabilities of residents filing for bankruptcy 3-digit zip code and month (*Total liabilities*).

3-digit zip code and month fixed effects are included ("yes") or not included ("no"). The number of observations in Panel A, C and D is 54,855. In Panel B the number of observations is 20,700. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Table 3
Personal bankruptcies around the treatment event.

PANEL A: Total bankruptcies *(ln)*										
Chapter:	All chapters		Chapter7		Chapter 11		Chapter 12		Chapter 13	
Post*Treated	0.059***	0.054***	0.032***	0.027**	0.012	0.015	-0.002	-0.003	0.211***	0.209***
	(0.017)	(0.017)	(0.012)	(0.012)	(0.018)	(0.018)	(0.005)	(0.005)	(0.045)	(0.045)
Treated	-0.304**		-0.146		0.094		-0.006		-0.677***	
	(0.132)		(0.132)		(0.059)		(0.004)		(0.118)	
Post	-0.173***		-0.231***		-0.042***		0.005***		-0.009	
	(0.006)		(0.006)		(0.005)		(0.002)		(0.007)	
Controls	no	yes	no	yes	no	yes	no	yes	no	yes
3-Digit Zip Code FE	no	yes	no	yes	no	yes	no	yes	no	yes
Year-Month FE	no	yes	no	yes	no	yes	no	yes	no	yes
R-squared	0.008	0.965	0.009	0.951	0.004	0.503	0.001	0.109	0.012	0.931

PANEL B: Total bankruptcies *(ln)* – Matched sample										
Chapter	All chapters		Chapter7		Chapter 11		Chapter 12		Chapter 13	
Post*Treated	0.060***	0.060***	0.042**	0.041**	0.018	0.024	0.003	0.003	0.153***	0.151***
	(0.023)	(0.022)	(0.019)	(0.019)	(0.019)	(0.019)	(0.005)	(0.005)	(0.049)	(0.050)
Controls	no	yes	no	yes	no	yes	no	yes	no	yes
3-Digit Zip Code FE	no	yes	no	yes	no	yes	no	yes	no	yes
Year-Month FE	no	yes	no	yes	no	yes	no	yes	no	yes
R-squared	0.005	0.960	0.008	0.947	0.004	0.533	0.000	0.104	0.019	0.926

PANEL C: Consumer bankruptcies *(ln)*								
Chapter:	All chapters		Chapter7		Chapter 11		Chapter 13	
Post*Treated	0.063***	0.057***	0.031***	0.026**	0.046***	0.046***	0.212***	0.210***
	(0.017)	(0.017)	(0.012)	(0.012)	(0.012)	(0.012)	(0.045)	(0.045)
Controls	no	yes	no	yes	no	yes	no	yes
3-Digit Zip Code FE	no	yes	no	yes	no	yes	no	yes
Year-Month FE	no	yes	no	yes	no	yes	no	yes
R-squared	0.008	0.965	0.009	0.951	0.002	0.374	0.012	0.931

PANEL D: Business bankruptcies *(ln)*										
Chapter:	All chapters		Chapter7		Chapter 11		Chapter 12		Chapter 13	
Post*Treated	0.035	0.038*	0.070***	0.070***	-0.015	-0.011	-0.001	-0.001	-0.004	-0.004
	(0.022)	(0.023)	(0.017)	(0.018)	(0.018)	(0.017)	(0.002)	(0.002)	(0.004)	(0.004)
Controls	no	yes	no	yes	no	yes	no	yes	no	yes
3-Digit Zip Code FE	no	yes	no	yes	no	yes	no	yes	no	yes
Year-Month FE	no	yes	no	yes	no	yes	no	yes	no	yes
R-squared	0.003	0.567	0.002	0.487	0.006	0.425	0.001	0.032	0.003	0.041

Notes. This table reports the coefficients and standard errors clustered at the 3-digit zip code level (in parentheses) obtained using Eqs. (1) and (2). Panels A, C and D document the effect of the treatment event on the number of total, consumer and personal business bankruptcy filings, respectively. Panel B replicates results in Panel A using a sample of treatment and control group 3-digit zip codes matched on pre-treatment characteristics. The main explanatory variable is an interaction term between the variable Post (equal to 1 for months after the announcement of the verdict in May 2015, and zero otherwise) and *Treated* (equal to 1 for 3-digit zip codes in the affected states Connecticut and New York, and zero otherwise). Control variables include: the logarithm of the dollar amount of funds requested through online platforms by residents in each 3-digit zip code and month (*Requested funds*), the logarithm of the dollar amount of debt discharged by bankruptcy filers 3-digit zip code and month (*Dischargeable debt*), the logarithm of average total liabilities of residents filing for bankruptcy 3-digit zip code and month (*Total liabilities*). 3-digit zip code and month fixed effects are included ("yes") or not included ("no"). The number of observations in Panel A, C and D is 54,855. In Panel B the number of observations is 20,700. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.
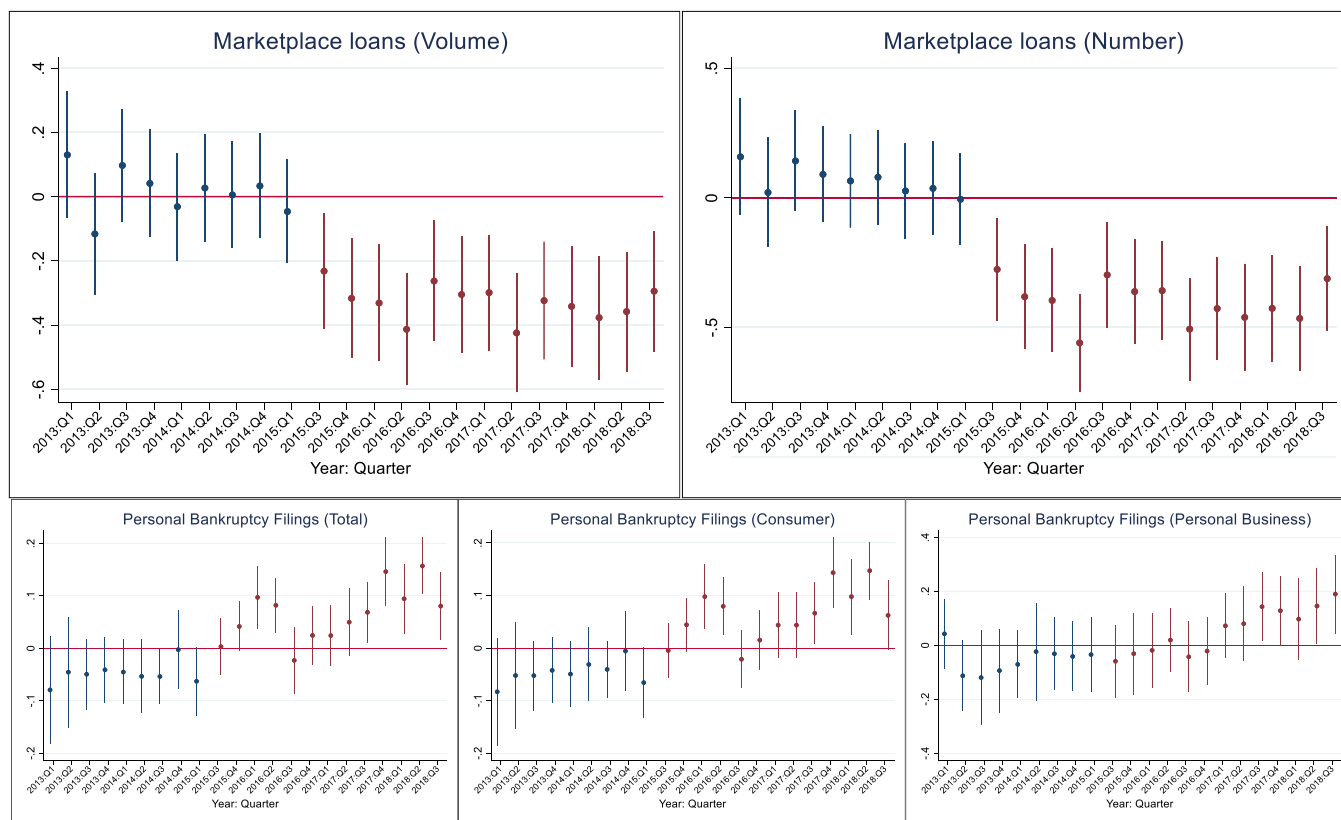
Fig. 1. Dynamic Results.
Notes. This figure presents the results of regressions estimated using specification 3 in which the main explanatory variable, an interaction term between the variable Post and Treated is further interacted with a dummy variable equal to 1 for each pre- and post-treatment period (and zero otherwise). The figure shows the estimated coefficient on the triple interaction term together with the 95% confidence interval.

In Panel B we replicate our results using a matched sample, obtained using Lemmon and Roberts (2010) nearest neighbour matching procedure. Matching each treatment group zip code with four control group zip codes provides almost exactly the same results as those presented in Panel A, documenting that our baseline results are robust to using alternative control groups and a number of observations.[37]

The credit rationing across marketplace borrowers with different FICO scores is documented in Table 2, Panel C. There is significant heterogeneity in the magnitude of rationing across different risk-classes of borrowers. Using the FICO scores of borrowers, we construct five borrower credit risk rating quintiles. The lowest quintile (Rating 1) denotes the riskiest borrowers with a FICO score in the bottom quintile (20th percentile) of FICO scores. The highest quintile (Rating 5) denotes the least risky borrowers whose FICO score is in the top 20th percentile of FICO scores.

We find a statistically significant reduction in the lending provided to borrowers in the three lowest FICO rating quintiles. Marketplace lending volume falls by 22% for borrowers with a FICO score in in the 3rd quintile, 45% for borrowers with a FICO score in the 2nd quintile, and 98% for borrowers with a FICO score in the lowest 1st quintile with the poorest credit scores. In contrast, for most borrowers with an already high credit score, marketplace lending increases, although this increase is not statistically significant at any conventional level.

On the extensive margin, borrowers in the lowest, i.e., 1st quintile of FICO scores, face a 67% fall in the number of marketplace loans following treatment. Borrowers in the 2nd and 3rd quantiles experience a reduction of 27% and 12%, respectively. In sharp contrast, the provision of marketplace loans experiences a statistically significant 9% increase for borrowers with a FICO score in the highest quintile.

We document the marketplace credit rationing effect differentiated by loan purpose in Table 2, Panel D. We are particularly concerned with loans that may help individuals avoid filing for bankruptcy. Out-of-pocket medical bills cause up to one quarter of personal bankruptcies, particularly among low-income households (Gross and Notowidigdo, 2011). High credit card debt is another important factor contributing to bankruptcy among households in the U.S. (Domowitz and Sartain, 1999). Thus, the inability to obtain marketplace funds for either (i) debt financing or (ii) paying medical bills may significantly increase the probability of filing for bankruptcy.[38]

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[37] We use a *probit* model to estimate the effect of the average pre-treatment growth rates of marketplace lending and personal bankruptcy filings, average pre-treatment: debt-to-income ratio of marketplace borrowers, funds requested through online platforms by residents, the average dollar amount of debt discharged by bankruptcy filers and the average total liabilities of residents filing for bankruptcy in each 3-digit zip code area on the probability of an area being in the treatment group. We then compute propensity scores using the estimates obtained from the *probit* regressions. 3-digit zip code areas' nearest neighbors are zip codes with the most similar propensity score. For each treated region we choose one or four nearest neighbor areas from the control group. The estimates, obtained with a sample matching each treatment group area with one control group area, presented in Table A.6, are again almost identical to our main estimates.

[38] In addition, loans for small personal businesses might be relevant for bankruptcy as such loans are often requested for financing equipment purchases or covering unexpected expenses required for continuing to operate a personal business. Significant reductions in this type of marketplace lending may help to explain the observed changes in personal business bankruptcy filings.

Table 4

Personal bankruptcies and marketplace credit rationing across income groups.

PANEL A: Marketplace lending: intensive and extensive margins

Income range:	<$25,000		$25,000-$49,999		$50,000-$74,999		$75,000-$99,999		>$100,000	
Dependent variable:	Volume (ln)	Number (ln)	Volume (ln)	Number (ln)	Volume (ln)	Number (ln)	Volume (ln)	Number (ln)	Volume (ln)	Number (ln)
Post*Treated	-1.088***	-0.392***	-0.516***	-0.380***	-0.411***	-0.215***	-0.020*	-0.043	0.082	0.091
	(0.140)	(0.030)	(0.110)	(0.023)	(0.078)	(0.018)	(0.011)	(0.029)	(0.062)	(0.073)
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
3-Digit Zip Code FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Month FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
R-squared	0.553	0.738	0.587	0.879	0.615	0.914	0.548	0.845	0.575	0.862

PANEL B: Bankruptcy rates

Income range:	<$25,000			$25,000-$49,999			$50,000-$74,999			$75,000-$99,999			>$100,000		
Bankruptcy type:	Total (ln)	Consumer (ln)	Business (ln)	Total (ln)	Consumer (ln)	Business (ln)	Total (ln)	Consumer (ln)	Business (ln)	Total (ln)	Consumer (ln)	Business (ln)	Total (ln)	Consumer (ln)	Business (ln)
Post*Treated	0.075***	0.081***	0.023	0.066***	0.065***	0.032**	0.041***	0.041***	0.017*	0.002	0.002	0.004	-0.015	-0.015	-0.001
	(0.020)	(0.021)	(0.015)	(0.022)	(0.022)	(0.015)	(0.016)	(0.015)	(0.009)	(0.019)	(0.018)	(0.010)	(0.016)	(0.016)	(0.001)
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
3-Digit Zip Code FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Month FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
R-squared	0.926	0.926	0.489	0.929	0.929	0.259	0.890	0.890	0.161	0.823	0.822	0.270	0.804	0.804	0.029

Notes. This table reports the coefficients and standard errors clustered at the 3-Digit Zip Code FE level (in parentheses). The results in Panel A explain the effect of the treatment event on the amount and number of marketplace loans. Panel B documents the effect of the treatment event on the number of total, business and consumer bankruptcy filings. The sample is split by the income of marketplace borrowers and the income of people filing for bankruptcy. The main explanatory variable is an interaction term between the variable *Post* (equal to 1 for months after the announcement of the verdict in May 2015, and zero otherwise) and *Treated* (equal to 1 for 3-digit zip codes in the affected states Connecticut and New York, and zero otherwise). Control variables include: the logarithm of the dollar amount of funds requested through online platforms by residents in each 3-digit zip code and month (*Requested funds*), the logarithm of the dollar amount of debt discharged by bankruptcy filers 3-digit zip code and month (*Dischargeable debt*), the logarithm of average total liabilities of residents filing for bankruptcy 3-digit zip code and month (*Total liabilities*). 3-digit zip code and month fixed effects are included. The number of observations in all regressions is 54,855. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Table 5

The rise in personal bankruptcy following reduction in MPL by loan type.

PANEL A: Sample split based on change in the volume of all marketplace loans						
	Below median change in MPL volume			*Above* median change in MPL volume		
Bankruptcy type:	Total	Consumer	Business	Total	Consumer	Business
Post*Treated	0.049***	0.052***	0.027	0.069**	0.073***	0.038
	(0.016)	(0.017)	(0.027)	(0.031)	(0.024)	(0.044)
Controls	yes	yes	yes	yes	yes	yes
3-Digit Zip Code FE	yes	yes	yes	yes	yes	yes
Year-Month FE	yes	yes	yes	yes	yes	yes
Observations	27,393	27,393	27,393	27,462	27,462	27,462
R-squared	0.961	0.961	0.562	0.966	0.966	0.571
PANEL B: Sample split based on change in the volume of marketplace loans for debt refinancing						
	Below median change in loans for debt refinancing			*Above* median change in loans for debt refinancing		
Bankruptcy type:	Total	Consumer	Business	Total	Consumer	Business
Post*Treated	0.057**	0.057**	0.063*	0.051**	0.059***	0.041
	(0.025)	(0.025)	(0.038)	(0.022)	(0.022)	(0.034)
Controls	yes	yes	yes	yes	yes	yes
3-Digit Zip Code FE	yes	yes	yes	yes	yes	yes
Year-Month FE	yes	yes	yes	yes	yes	yes
Observations	27,393	27,393	27,393	27,462	27,462	27,462
R-squared	0.960	0.960	0.554	0.967	0.967	0.576
PANEL C: Sample split based on change in the volume of marketplace loans for medical bills						
	Below median change in loans for medical bills			*Above* median change in loans for medical bills		
Bankruptcy type:	Total	Consumer	Business	Total	Consumer	Business
Post*Treated	0.042***	0.046***	0.027	0.094***	0.094***	0.056
	(0.011)	(0.012)	(0.024)	(0.024)	(0.027)	(0.036)
Controls	yes	yes	yes	yes	yes	yes
3-Digit Zip Code FE	yes	yes	yes	yes	yes	yes
Year-Month FE	yes	yes	yes	yes	yes	yes
Observations	26,013	26,013	26,013	28,842	28,842	28,842
R-squared	0.963	0.963	0.592	0.966	0.966	0.539

Notes. This table reports the coefficients and standard errors clustered at the 3-digit zip code level (in parentheses) obtained using Eq. (2). Panels A, B and C document the effect of *the treatment event* on the number of total, consumer and personal business bankruptcy filings. In Panel A the sample is split by the changes in total volume of marketplace loans. In Panel B and C the sample is split by the changes in marketplace loans requested for debt refinancing or paying medical bills, respectively. The sample "*Below* median change in MPL volume" includes 3-digit zip codes where the drop in the volume of marketplace loans is *less* severe. The sample "*Above* median change in MPL volume" includes 3-digit zip codes where the drop in the volume of marketplace loans is *more* severe. The main explanatory variable is an interaction term between the variable *Post* (equal to 1 for months after the announcement of the verdict in May 2015, and zero otherwise) and *Treated* (equal to 1 for 3-digit zip codes in the affected states Connecticut and New York, and zero otherwise). Control variables include: the logarithm of the dollar amount of funds requested through online platforms by residents in each 3-digit zip code and month (*Requested funds*), the logarithm of the dollar amount of debt discharged by bankruptcy filers 3-digit zip code and month (*Dischargeable debt*), the logarithm of average total liabilities of residents filing for bankruptcy 3-digit zip code and month (*Total liabilities*). 3-digit zip code and month fixed effects are included. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Following the treatment event, the total volume of bankruptcy-relevant types of loans together (*Relevant loans*) falls by 30% in zip codes in Connecticut and New York relative to zip codes in the control group. We observe a drop in the volume of marketplace loans for small business loans (9%), debt refinancing (27%), and, in particular, loans for medical costs (50%). The volume of loans acquired for all other purposes declines by 14%.[39] The pattern is similar for the marketplace credit rationing effect along the extensive margin. The number of marketplace loans for small business purposes, debt refinancing, and medical costs fall by 3%, 20%, and 31%, respectively.

In sum, we document a significant reduction in the volume and number of marketplace loans following the court ruling, particularly to those individuals who may be in greater need of external funding to sustain income shocks or unexpected expenses, particularly medical bills, and to refinance their existing debt at cheaper rates.

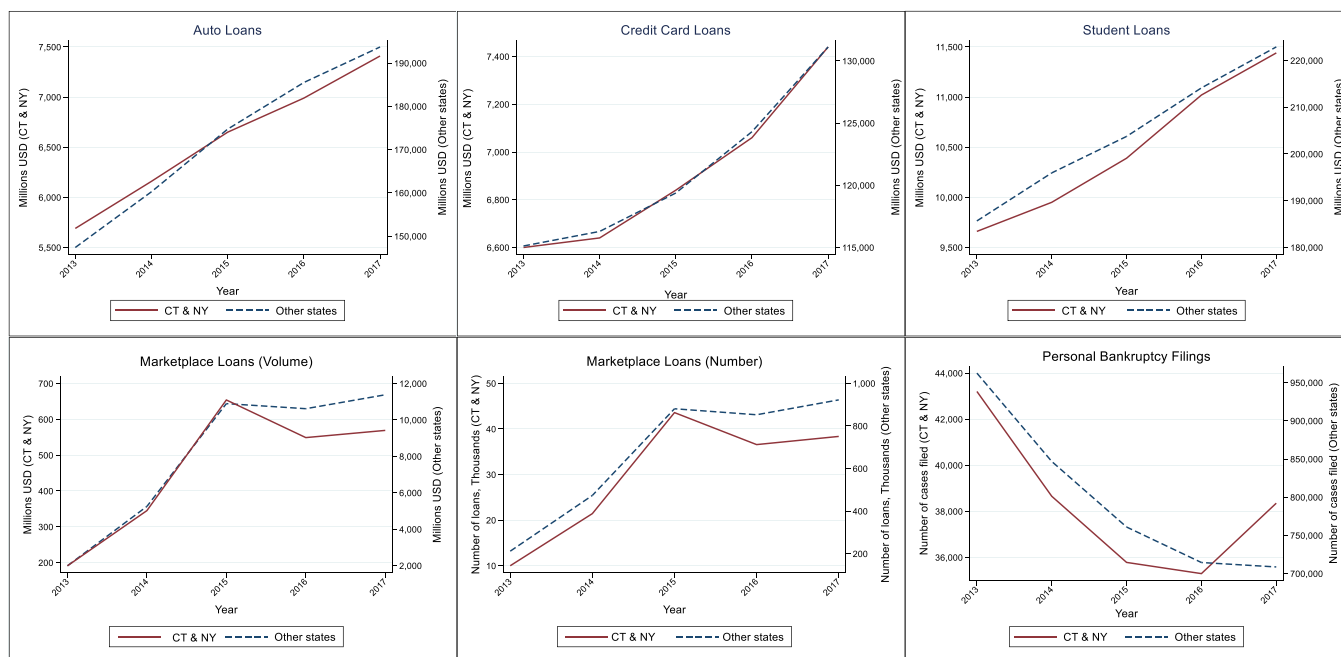### 5.2. Does restricting marketplace lending affect bankruptcy rates?

We now analyze how restrictions on marketplace lending affect the number of individuals filing for bankruptcy. We continue using

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[39] Other loans category includes loans acquired for home improvements, student use, auto purchase, baby & adoption expenses, boat purchase, cosmetic procedures, engagement ring and wedding financing, and vacations.

Table 6
Borrowers indebtedness channel.

Dependent variable:	Marketplace loans *(ln)*		Bankruptcies *(ln)*		
	Volume	Number	Total	Consumer	Personal business
PANEL A: Borrowers' debt-to-income ratio					
Debt-to-income ratio below median DTI					
Post*Treated	-0.341***	-0.211***	0.096***	0.093***	0.053
	(0.067)	(0.034)	(0.024)	(0.023)	(0.033)
Controls	yes	yes	yes	yes	yes
3-Digit Zip Code FE	yes	yes	yes	yes	yes
Year-Month FE	yes	yes	yes	yes	yes
Observations	27,393	27,393	27,393	27,393	27,393
R-squared	0.778	0.964	0.974	0.974	0.603
Debt-to-income ratio above median DTI					
Post*Treated	-0.419***	-0.278***	0.038***	0.038***	0.031
	(0.095)	(0.018)	(0.014)	(0.014)	(0.019)
Controls	yes	yes	yes	yes	yes
3-Digit Zip Code FE	yes	yes	yes	yes	yes
Year-Month FE	yes	yes	yes	yes	yes
Observations	27,462	27,462	27,462	27,462	27,462
R-squared	0.667	0.939	0.950	0.950	0.360
PANEL B: Borrowers' number of open credit lines below median					
Number of open credit lines below median					
Post*Treated	-0.361***	-0.224***	0.060***	0.060**	0.078
	(0.074)	(0.026)	(0.023)	(0.025)	(0.047)
Controls	yes	yes	yes	yes	yes
3-Digit Zip Code FE	yes	yes	yes	yes	yes
Year-Month FE	yes	yes	yes	yes	yes
Observations	26,910	26,910	26,910	26,910	26,910
R-squared	0.701	0.946	0.962	0.962	0.575
Number of open credit lines above median					
Post*Treated	-0.239***	-0.305***	0.042**	0.046**	0.031
	(0.042)	(0.053)	(0.020)	(0.019)	(0.025)
Controls	yes	yes	yes	yes	yes
3-Digit Zip Code FE	yes	yes	yes	yes	yes
Year-Month FE	yes	yes	yes	yes	yes
Observations	27,945	27,945	27,945	27,945	27,945
R-squared	0.809	0.966	0.962	0.963	0.564

Notes. This table reports the coefficients and standard errors clustered at the 3-digit zip code level (in parentheses) obtained using Eq. (2). Panels A and B document the effect of *the treatment event* on the volume and number of marketplace loans, and the number of total, consumer and personal business bankruptcy filings. The sample is split on the average, pre-treatment debt-to-income ratio (DTI) of marketplace borrowers and the median number of open credit lines of marketplace borrowers. In Panel A the sample includes 3-digit zip codes where the average, pre-treatment DTI is below or above the median of this ratio for all 3-digit zip codes. In Panel B the sample includes zip codes where marketplace borrowers have below or above median number of open credit lines. The main explanatory variable is an interaction term between the variable *Post* (equal to 1 for months after the announcement of the verdict in May 2015, and zero otherwise) and *Treated* (equal to 1 for 3-digit zip codes in the affected states Connecticut and New York, and zero otherwise). Control variables include: the logarithm of the dollar amount of funds requested through online platforms by residents in each 3-digit zip code and month (*Requested funds*), the logarithm of the dollar amount of debt discharged by bankruptcy filers 3-digit zip code and month (*Dischargeable debt*), the logarithm of average total liabilities of residents filing for bankruptcy 3-digit zip code and month (*Total liabilities*). 3-digit zip code and month fixed effects are included. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

Fig. 2. Other Consumer Lending Around the Treatment Event.
Notes. This figure presents the trends in the evolution of annual volume of credit card loans, auto loans, student loans, marketplace lending and total number of personal bankruptcy filings in the treatment and control group 3-digit zip codes between 2013 and 2017.

estimations in the form of specifications 1 and 2. We let the dependent variable represent the number of bankruptcy cases filed per month in each zip code.

The treatment event's effect on the total number of bankruptcies, including personal business and consumer (non-business) bankruptcies, is presented in Table 3, Panel A. Following the verdict, the change in the total number of bankruptcy filings, irrespective of the chapter under which bankruptcy is filed, is 6% higher in 3-digit zip codes in Connecticut and New York relative to the 3-digit zip codes in the control group. The estimated coefficient on the interaction term between *Post* and *Treated* is positive and statistically significant in regressions where the dependent variable denotes Chapter 7 and Chapter 13 bankruptcy filings. Chapter 7 filings increase by 3.2%, and Chapter 13 cases jump by 20%. Chapter 11 and Chapter 12 bankruptcy filings are unaffected.[40] Panel B documents that these inferences remain unaffected when a matched sample is used to obtain the results.

Table 3, Panels C and D present, respectively, the treatment event effect on the number of consumer and personal business bankruptcy filings separately. Consumer bankruptcy petitions surge by 6.5%, and personal business bankruptcy cases increase by 3.5%. Table 3, Panel C indicates that the rise in consumer bankruptcy filings following the treatment event is driven by a statistically significant 3.1% increase in

Chapter 7 filings and a 23% rise in Chapter 13 filings.[41] Table 3, Panel D shows that, among personal business bankruptcies, only Chapter 7 filings record a statistically significant increase of 7.2%.

Overall, the results in Table 3 suggest that restricting marketplace lending increases personal bankruptcy filings, in particular Chapter 7 and 13 cases. These results support Hypothesis 3.1.

5.3. Dynamic results

We next trace out the dynamic effect of our treatment event. We modify specification 2 by interacting variable *Treated* with a set of quarter dummy variables and estimate:

$$ln(Y)_{zq} = \alpha_z + \beta_1 2013q1_q * Treated_z + \beta_2 2013q2_q * Treated_z + \dots$$
$$+ \beta_{23} 2018q3_q * Treated_z + \delta Controls_{zq} + \gamma_q + \varepsilon_{zq}, \quad (3)$$

where quarter dummies (2013q1 – 2018q3) = 1 for observations in the respective quarter, 0 otherwise.[42] The dependent variables aggregated at the 3-digit zip code z and quarter q include the volume and number of marketplace loans, the number of all, consumer, and personal business bankruptcy filings. These tests allows us to examine validity of parallel trends assumption, one of the main difference-in-difference identifying assumptions, and shed light on the persistency of the results presented in Tables 2 and 3.

Fig. 1 plots the estimated coefficients and the 95% confidence intervals of our coefficients of interest. Graphs in all panels document lack of statistically significant differences in marketplace lending and personal bankruptcy filings between the treatment and control group

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[40] Recall that Chapter 11 bankruptcy cases are usually filed by wealthy households that are left unaffected by credit rationing. Bankruptcy under Chapter 12 is available to farmers and commercial fishermen.

[41] Table 3, Panel C does not include estimations for Ch. 12 bankruptcies since these are only personal business bankruptcies.
[42] This analysis is performed on a matched sample of treatment and control group zip codes, as in Table 2 Panel B and Table 3 Panel B.

Table 7

The effect of treatment by affected state.

PANEL A: Treatment group includes only Connecticut state zip codes					
Dependent variable:	Marketplace loans *(ln)*		Bankruptcies *(ln)*		
	Volume	Number	Total	Consumer	Personal business
Post*Treated	-0.357***	-0.204***	0.015**	0.015***	-0.051
	(0.032)	(0.012)	(0.007)	(0.005)	(0.052)
Controls	yes	Yes	yes	yes	yes
3-Digit Zip Code FE	yes	Yes	yes	yes	yes
Year-Month FE	yes	Yes	yes	yes	yes
Observations	51,405	51,405	51,405	51,405	51,405
R-squared	0.737	0.955	0.966	0.965	0.560
PANEL B: Treatment group includes only New York state zip codes					
Dependent variable:	Marketplace loans *(ln)*		Bankruptcies *(ln)*		
	Volume	Number	Total	Consumer	Personal business
Post*Treated	-0.358***	-0.274***	0.061***	0.066***	0.055**
	(0.061)	(0.014)	(0.018)	(0.018)	(0.024)
Controls	yes	yes	yes	yes	yes
3-Digit Zip Code FE	yes	yes	yes	yes	yes
Year-Month FE	yes	yes	yes	yes	yes
Observations	54,165	54,165	54,165	54,165	54,165
R-squared	0.737	0.955	0.965	0.965	0.570

Notes. This table reports the coefficients and standard errors clustered at the 3-digit zip code level (in parentheses) obtained using Eq. (2). The results in Panel A and B document the effect of *the treatment event* on the volume and number of marketplace loans, and the number of total, business and consumer bankruptcy filings. The results in Panel A are obtained with sample excluding observations for New York and Panel B presents the results obtained using sample excluding observations for Connecticut. The main explanatory variable is an interaction term between the variable *Post* (equal to 1 for months after the announcement of the verdict in May 2015, and zero otherwise) and *Treated* (equal to 1 for 3-digit zip codes in the affected states Connecticut and New York, and zero otherwise). Control variables include: the logarithm of the dollar amount of funds requested through online platforms by residents in each 3-digit zip code and month (*Requested funds*), the logarithm of the dollar amount of debt discharged by bankruptcy filers 3-digit zip code and month (*Dischargeable debt*), the logarithm of average total liabilities of residents filing for bankruptcy 3-digit zip code and month (*Total liabilities*). 3-digit zip code and month fixed effects are included ("YES") or not included ("NO"). ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

regions prior to the court verdict taking place, yielding support to validity of our control group regions as a counterfactual for the treatment group areas.[43]

Following the verdict, the volume and number of marketplace lending is significantly lower in affected areas in each quarter of our sample period (Panels A and B). We also observe a statistically significant increase in the number of all and consumer bankruptcy filings in most of the post-treatment quarters (Panel C and D). We also find an increase in personal business bankruptcies (Panel E), although here the effect is significantly lagged, appearing statistically significant only in the last quarters of our sample period.

Our finding that the number of personal bankruptcies rises after the treatment event and continues to rise more strongly over time is to be expected and explained as follows. Firstly, some households may have already been considering filing prior to the verdict but could avoid it to the extent that marketplace lending helped these households to, for example, settle their medical bills or refinance and consolidate debt. When the verdict suddenly restricts marketplace credit, the treatment event can exert a relatively immediate impact by triggering households already considering filing to finally place their documents with the bankruptcy court. Secondly, other households that did not previously consider filing prior to the treatment event may be prompted to start considering filing after access to marketplace lending dries up. Such households may need time to compile all the information necessary to fill out the various bankruptcy forms, schedule a consultation with an attorney, and find a government-approved credit counselling agency to complete the mandatory pre-filing credit-counseling course.[44] Therefore, marketplace credit rationing can be expected to have both almost immediate and sustained impact on bankruptcy filings.

5.4. The effect of the treatment event across income groups

In order to corroborate the link between marketplace credit rationing

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[43] To reinforce these results we perform three additional tests to examine parallel trends and validity of our control group. First, following Roberts and Whited (2013) we compare the trends in the evolution of the key outcome variables. Table A.7 presents differences in the growth rates in our main dependent variables for the entire period preceding the treatment event. We also report t-statistics suggesting that in all cases, these differences are not statistically significant. Next, we conduct two falsification tests where we assign the treatment to zip codes in states unaffected by the court verdict, and perform Monte Carlo simulations. We randomly assign placebo treatment status to unaffected 3-digit zip codes in the post-treatment period (Panel A) or in the pre-treatment period (Panel B). We construct the variable Placebo which is equal to 1 for randomly chosen 3-digit zip codes and post-(pre-)treatment periods, and 0 otherwise. We then estimate the regression using Eq. (2) replacing the interaction term *Post*Treat* with Placebo variable and save the p-value on the Placebo coefficient. We repeat this process 1,000 times. We compute the rejection rates of the null hypothesis at the 1%, 5%, and 10% statistical significance levels. We also report the mean coefficient and the average t-statistic for the Placebo treatment variable. The results of these tests are presented in Appendix A, Table A.8 and show that unaffected regions exhibit neither statistically nor economically significant changes in the volume and number of marketplace lending or changes in bankruptcy rates before or after the treatment event. Taken together, the set of diagnostic tests suggest the treatment event was not anticipated and that 3-digit zip codes in the control group states constitute a valid counterfactual for the 3-digit zip code areas located in Connecticut and New York.

[44] See FTC, *Choosing a Credit Counselor*, https://www.consumer.ftc.gov/articles/0153-choosing-credit-counselor. Also to avoid monetary sanctions, the debtor and the debtor's attorney need to spend reasonable time to ensure the accuracy of the filing information before filing. (See *Federal Rule of Bankruptcy Procedure* Rule 9011 providing for the imposition of sanctions for any parties, including law firms and attorneys, that violate FRBP 9011(B).

Table 8

The effect of treatment on marketplace loan defaults.

PANEL A: Default rates: All originations										
Dependent variable: Number of loans in delinquency or default/Total outstanding loans *(ln)*										
	16-29 days delinquent		*30-59 days delinquent*		*60-89 days delinquent*		*90+ days delinquent*		*Charged off loans*	
Post*Treated	-0.111***	-0.111***	-0.052***	-0.053***	-0.047***	-0.048***	-0.038***	-0.039***	-0.047***	-0.047***
	(0.006)	(0.006)	(0.012)	(0.012)	(0.009)	(0.009)	(0.009)	(0.010)	(0.008)	(0.008)
Controls	no	yes	no	yes	no	yes	no	yes	no	yes
3-Digit Zip Code FE	no	yes	no	yes	no	yes	no	yes	no	yes
Year-Month FE	no	yes	no	yes	no	yes	no	yes	no	yes
R-squared	0.304	0.424	0.290	0.437	0.336	0.481	0.346	0.496	0.374	0.527
PANEL B: Default rates: Pre-Madden originated loans										
Dependent variable: Number of loans in delinquency or default/Total outstanding loans *(ln)*										
	16-29 days delinquent		*30-59 days delinquent*		*60-89 days delinquent*		*90+ days delinquent*		*Charged off loans*	
Post*Treated	-0.017	-0.017	-0.004	-0.006	-0.012	-0.012	-0.016	-0.014	-0.006	-0.007
	(0.013)	(0.013)	(0.016)	(0.016)	(0.017)	(0.018)	(0.016)	(0.016)	(0.013)	(0.013)
Controls	no	yes	no	yes	no	yes	no	yes	no	yes
3-Digit Zip Code FE	no	yes	no	yes	no	yes	no	yes	no	yes
Year-Month FE	no	yes	no	yes	no	yes	no	yes	no	yes
R-squared	0.139	0.235	0.194	0.305	0.232	0.326	0.244	0.334	0.278	0.370

Notes. This table reports the coefficients and standard errors clustered at the state level (in parentheses). The presented results document the effect of *the treatment event* on the number of marketplace loan delinquencies and charge offs. Panel A presents the results for all loans originated prior to and after *the treatment event*. Panel B presents the results only for a sample of loans originated prior to *Madden*. The main explanatory variable is an interaction term between the variable *Post* (equal to 1 for months after the announcement of the verdict in May 2015, and zero otherwise) and *Treated* (equal to 1 for 3-digit zip codes in the affected states Connecticut and New York, and zero otherwise). Control variables include: monthly state unemployment rates (*Unemployment*), the logarithm of average total assets of residents filing for bankruptcy in each state and month (*Total assets*), and the logarithm of the dollar amount of funds requested through marketplace platforms by residents in each state per month (*Requested funds*). State and month fixed effects are included ("yes") or not included ("no"). The number of observations in all regressions is 54,855. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

and the observed surge in personal bankruptcy in the zip codes affected by the treatment event, we analyze the effect of the verdict across different income brackets. We use data on the annual income provided by individuals in their bankruptcy filings and self-reported incomes of marketplace borrowers, which are verified by lending platforms. We re-estimate the auxiliary specification 2 for different income ranges. We split borrowers and bankruptcy filers into five income brackets: with an annual income <$25,000 (bracket 1), $25,000-$49,999 (bracket 2), $50,000-$74,999 (bracket 3), $75,000-$100,000 (bracket 4), and finally with an annual income >$100,000 (bracket 5).[45] Table 5 shows the effect of the treatment event on the volume and number of marketplace loans (Panel A) and bankruptcy filings (panel B) across different income brackets.

Low-income borrowers experience more severe credit rationing following the treatment event. Table 4, Panel A shows that borrowers in the treatment group with an income of less than $25,000 (income bracket 1) experience a decline in the volume of marketplace credit of 66% relative to the residents of control group zip codes in the same income bracket (coefficient -1.088). This rationing of marketplace credit recedes for borrowers with higher annual incomes. Relatively high-income borrowers (bracket 4) observe only a small fall in marketplace lending volume of 2%. No differential credit rationing effect of the treatment event can be observed for borrowers with the highest annual income (bracket 5). Similarly, in terms of credit rationing along the extensive margin, we observe significant reductions in the number of marketplace lending for individuals in the three lowest income brackets.

Table 4, Panel B shows a complementary pattern for bankruptcy filings. Low-income residents of Connecticut and New York zip codes file significantly more for bankruptcy compared to low-income residents of zip codes in other states following the treatment event. The incidence of

bankruptcy increases by 7.8%, 6.8% and 4.2% among individuals in the lowest three income brackets respectively. We observe no differential effect of the treatment event increasing personal bankruptcy among individuals with the highest income.

In sum, households in Connecticut and New York are more likely to experience personal bankruptcy the larger the contraction in marketplace lending to that income group. Households in the affected zip codes, which experience no reduction in marketplace lending, also do not exhibit increases in bankruptcy filings following the verdict. These results further corroborate Hypothesis 3.1.

5.5. The effect of the treatment event across different regions

To zoom in on how the treatment event affects households across various regions in New York and Connecticut, we split the sample according to the intensity of the change in marketplace lending volume. For each 3-digit zip code, we calculate the change in the total volume of marketplace lending before and after the treatment event and split the sample on the median value of marketplace lending volume changes in all zip codes. We classify all 3-digit zip codes experiencing a change in marketplace lending volume above (below) the median change in MPL observed in all 3-digit zip codes as being 'more severely affected' ('less severely affected') by the court verdict. Using Eq. (2), we estimate the effect of the treatment event on the number of all bankruptcy filings, as well as for consumer and personal business bankruptcy filings separately, for both sub-samples.

Table 5 presents the results of these tests. In Panel A, we split the sample according to a zip code's total marketplace lending volume. As expected, the rise in personal bankruptcies following the treatment event is much stronger in those zip codes where the treatment created more severe rationing of marketplace credit.

To shed further light on the mechanism via which the rationing of marketplace credit contributes to a rise in personal bankruptcies, we investigate if the rise in personal bankruptcy is higher when marketplace

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[45] Specification (1) yields materially equivalent results. We report tests only for specification (2) to preserve space.

Table 9
Borrowers past bankruptcy filings.

PANEL A: Borrowers with past bankruptcy filing below median

Dependent variable:	Marketplace loans *(ln)*		Bankruptcies *(ln)*		
	Volume	Number	Total	Consumer	Business
Post*Treated	-0.385***	-0.204***	0.058**	0.061**	0.024
	(0.102)	(0.030)	(0.026)	(0.031)	(0.049)
Controls	yes	yes	yes	yes	yes
3-Digit Zip Code FE	yes	yes	yes	yes	yes
Year-Month FE	yes	yes	yes	yes	yes
Observations	27,393	27,393	27,393	27,393	27,393
R-squared	0.699	0.945	0.963	0.963	0.596

PANEL B: Borrowers with past bankruptcy filing above median

Dependent variable:	Marketplace loans *(ln)*		Bankruptcies *(ln)*		
	Volume	Number	Total	Consumer	Business
Post*Treated	-0.351***	-0.327***	0.068***	0.062***	0.036**
	(0.053)	(0.046)	(0.018)	(0.018)	(0.021)
Controls	yes	yes	yes	yes	yes
3-Digit Zip Code FE	yes	yes	yes	yes	yes
Year-Month FE	yes	yes	yes	yes	yes
Observations	27,462	27,462	27,462	27,462	27,462
R-squared	0.813	0.966	0.961	0.961	0.552

Notes. This table reports the coefficients and standard errors clustered at the 3-digit zip code level (in parentheses) obtained using Eq. (2). Panels A and B document the effect of *the treatment event* on the volume and number of marketplace loans, and the number of total, consumer and personal business bankruptcy filings. The sample of zip codes is split on the median, pre-treatment number of marketplace borrowers with previous bankruptcy filing. In Panel A the sample includes 3-digit zip codes where the number of marketplace borrowers who previous filed for bankruptcy is below the median of this number for all 3-digit zip codes. In Panel B the sample includes 3-digit zip codes where prior to the treatment event the number of marketplace borrowers with prior bankruptcy filing is higher (above the median). The main explanatory variable is an interaction term between the variable *Post* (equal to 1 for months after the announcement of the verdict in May 2015, and zero otherwise) and *Treated* (equal to 1 for 3-digit zip codes in the affected states Connecticut and New York, and zero otherwise). Control variables include: the logarithm of the dollar amount of funds requested through online platforms by residents in each 3-digit zip code and month (*Requested funds*), the logarithm of the dollar amount of debt discharged by bankruptcy filers 3-digit zip code and month (*Dischargeable debt*), the logarithm of average total liabilities of residents filing for bankruptcy 3-digit zip code and month (*Total liabilities*). 3-digit zip code and month fixed effects are included. ***, **, and * indicate significance at the 1 percent, 5 percent, and 10 percent statistical level, respectively.

credit supply for a particular type of loan purpose dries up. The literature suggests that debt refinancing costs, as well as personal health expenditures and medical bills, are important contributing factors behind personal bankruptcy filings. In Panel B, we split the sample according to a zip code's change in marketplace credit obtained for debt refinancing. We observe a rise in personal bankruptcy of similar magnitude across both the more and less severely credit-rationed regions. In Panel C, we split the sample according to a zip code's change in marketplace credit obtained for medical expenses. We find that 3-digit zip codes experience a much higher surge in personal bankruptcy filings if they experience more severe rationing of marketplace credit for financing personal medical bills.

In sum, these results not only lend support to Hypothesis 3.1 but also suggest that marketplace lending requested for financing medical expenses may play a significant role in helping households avoid personal bankruptcy relative to fintech loans requested for debt refinancing.[46]

6. Ruling out alternative explanations

In this section, we test and reject several plausible alternative explanations and interpretations of our baseline results.

6.1. The effect of the treatment event and borrowers' indebtedness

One potential alternative interpretation of our results is that marketplace lending helps to delay bankruptcy filings of highly-indebted individuals. To investigate this claim, we perform an additional test bysplitting all 3-digit zip codes according to the debt-to-income ratio (DTI) of marketplace borrowers. For each zip code, we calculate the median DTI of marketplace borrowers. Areas with a median DTI above the median across all areas are classified as highly-indebted, while the remaining zip codes are considered less-indebted.

Table 6 presents the results of this test. In both less-indebted and highly-indebted (Panel A) areas, we observe rationing of marketplace credit which is similar in magnitude. However, the increase in bankruptcy cases is much higher in less indebted areas. These results suggest that marketplace lending is unlikely to delay bankruptcy and, second, that bankruptcy cases rise in the treatment group 3-digit zip codes due to marketplace credit rationed individuals filing for bankruptcy.

Panel B presents the results of the analysis when splitting zip codes at the level of the median number of credit lines that marketplace borrowers have open at the time of the origination of a marketplace loan. We perform this test in order to further rule out that borrowers' level of indebtedness explains the surge in bankruptcy filings documented in Table 3. If this was the case, one should observe a statistically significant effect of the treatment event on bankruptcy filings only in zip codes where marketplace borrowers have a relatively high number of open

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[46] In addition to our preferred Difference-in-Differences estimations, our results continue to hold using 2-stage IV regressions. We instrument the volume (number) of marketplace loans in the first-stage with the interaction term *Post*Treated*. The results of these tests, presented in Table A.9, further support the results presented in Table 2 and 3.

lines of credit. However, we find that in areas where individuals are limited by a smaller number of open credit lines, the effect of the treatment event on bankruptcy filings is stronger, while in areas where marketplace borrowers have access to a larger number of open credit lines, the effect of the treatment event on raising personal bankruptcy is smaller (Panel D). These results support the findings presented in Table 6, Panels A.

6.2. The treatment event and other consumer credit

Another alternative mechanism we reject is that the court ruling could affect the availability of other consumer credit. To rule out that changes in the availability of credit card lending or other consumer credit explain the rise in bankruptcy cases and to test whether the treatment event affects consumer credit other than marketplace loans, we turn to data obtained from the New York Federal Reserve's Consumer Credit Panel. These data provide us with the year-end volume of credit card, auto, and student loans originated in each U.S. state by bank and non-bank institutions. Fig. 2 illustrates the evolution in the availability of these loans from 2013 through to 2017, which suggest that the treatment event does not affect the volume of non-marketplace consumer credit.[47]

The finding that the treatment event does not have a statistically significant effect on other consumer lending, including credit card lending, should not be surprising. The verdict should not be expected to impact the credit card market because the vast majority of credit card lending occurs via general-purpose credit cards that are issued by members of credit card associations (Visa, Master Card, Discover, and American Express) that are banks with federal deposit insurance.[48] When these banks issue and then assign credit card debt to non-banks, the debt is exempted from state usury limits as the originating bank retains an ongoing economic interest in the loans, which distinguishes the credit card market from the origination model of marketplace lenders.

The verdict could potentially affect only that segment of the credit card market where card debt is sold outright to non-banks without the bank retaining an ongoing economic interest in the loan. We find, however, that this is a negligible part of overall credit card lending volume because banks sell outright their credit card debt only once it is charged-off, with the rate of charged-off credit card debt being merely 2-3% per year of which only 10% is on average sold off.[49,50]

6.3. The treatment event and payday loans

Alternatively, the increase in bankruptcy may be due to credit-rationed high-risk borrowers switching from marketplace platforms to high-interest credit such as payday loans, which are well-known predictors of household hardship. If consumers switching to other non-bank lending, such as payday lending, were responsible for the rise in bankruptcy following the treatment event, one would observe a more potent effect of the verdict on bankruptcy filings in 3-digit zip codes in Connecticut, where unlike in the New York state payday lending although restricted is legally available.[51]

To test this hypothesis, we repeat our baseline tests by separately including New York and Connecticut 3-digit zip code areas in the treatment group. We first compare areas in Connecticut to all other states, excluding New York regions from the analysis, and, secondly, exclude zip codes in Connecticut from our sample in order to compare New York zip codes to all other states. Table 7 presents the results.

We find that despite the treatment event having a similarly-sized negative effect on marketplace lending in both New York and Connecticut, the bankruptcy filings are significantly more affected by the verdict in zip codes in New York (Panel B) than in Connecticut (Panel A). This finding refutes the idea that borrowers switching to payday lending is responsible for the increase in bankruptcy rates after the treatment event.

The results also suggest that marketplace lending can significantly affect bankruptcy rates across zip codes in states with different utilization levels of marketplace loans. This finding that the treatment event raises personal bankruptcies in both New York and Connecticut, despite their macroeconomic and structural differences, also provides a good measure of the external validity of our results.[52]

6.4. Marketplace loan defaults around the treatment event

The increase in bankruptcy could be due to borrowers defaulting on their marketplace loans. The premise behind this alternative mechanism, which is not supported by the data, is that high-risk marketplace borrowers find themselves in a debt trap and default after being denied additional marketplace loans that would have staved off eventually filing for bankruptcy. To test this, we replace the dependent variable with the number of delinquent (16-29 days, 30-59 days, 60-89 days, and more than 90 days) and charged-off loans, expressed as a percent of total loans outstanding.

Table 8, Panel A shows that the coefficients on the interaction term between *Post* and *Treated* are negative and statistically significant, suggesting that the verdict results in a lower number of non-performing marketplace loans. The number of delinquent loans in Connecticut and New York zip codes falls by 3.7% to 10%, and the number of charged-off loans falls by about 4.6%. In Panel B, we restrict our sample to outstanding loans originated before the treatment event. We find again that the verdict reduces the number of delinquencies and charge-offs on these loans, although the magnitude of the effect is neither economically nor statistically significant. These results imply that the

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[47] To provide a formal test of these findings, we modify specification 2 and let the dependent variable be, respectively, the volume of credit card, auto, and student loans. For comparison, we also annualize and aggregate the volume and number of marketplace loans at the state level. We replace month-fixed effects with year-fixed effects and replace zip code-fixed effects with state-fixed effects. The results are presented in Table A.10. Apart from marketplace loans, the court verdict does not affect any other type of consumer credit.

[48] There is little (<3%) private label non-bank card debt origination (see CFPB, *The Credit Card Market*, 2017). Non-banks cooperate with banks under the 'rent-a-BIN' scheme. The card receivables are issued by the bank but sold and held by the non-bank. Banks receive a fee in return for renting their bank identification number (BIN). See FDIC (2007).

[49] See CFPB, *ibid*, and Federal Reserve data at https://www.federalreserve.gov/releases/chargeoff/chgallsa.htm.

[50] Another concern is that our results are driven by the effect the treatment event may have on the ability of financial institutions to sell non-performing loans to debt-collecting companies. However, most of the delinquent debt is recovered by third-party assignment, warehousing, internal recovery, or direct litigation, and not by debt sale. Further, Cheng, Severino, and Townsend (2019) show that consumers who negotiate with debt collectors are prone to strike bad deals that cause increased financial distress. Thus, if the treatment event would limit the ability of banks to sell charged-off loans to debt collectors, who evidently impose greater financial distress on borrowers, we should observe a lower, not higher, number of bankruptcy filings.

[51] Connecticut does not restrict payday lending. Although Connecticut's small loan law and check casher law (CGS §§ 36a-563, -565, and -581, Conn. Agencies Reg. § 36a-585-1) and usury limits (CGS §§ 37-4 and -9) put a restriction on payday loans, any person or entity with a license at the Connecticut Banking Department can make small consumer loans up to $15k with interest rates exceeding the usury limit. See: https://www.cga.ct.gov/2013/rpt/2013-R-0084.htm.

[52] Credit rationed borrowers in need of funding could also try to obtain funding from illegal sources (e.g. loan sharks). Information on the intensity of such activity is not publicly available. However, our results would suggest that greater availability of marketplace lending is likely to reduce the demand for such funding, potentially reducing the households financial distress.

observed reduction in delinquency rates stems from the improved average performance of loans originated after the court ruling and suggest that it is unlikely that existing marketplace borrowers are contributing to the rise in bankruptcy induced by the treatment event.[53]

6.5. Marketplace borrowers' prior bankruptcy filings

Our final analysis aims to address the concern that the treatment event increases filings primarily among borrowers who have a prior history of financial stress to the extent that they have filed for bankruptcy in the past. This alternative hypothesis would suggest that borrowers who filed in the past would be likely to file again, regardless of whether marketplace credit was fully available or being rationed. This could be the result of individuals suffering from the negative consequences of previous filings. To test this alternative hypothesis, we split our sample of 3-digit zip codes by the number of marketplace borrowers who previously filed for personal bankruptcy. We observe that the treatment event's effect has a similar magnitude in terms of raising personal bankruptcies in areas with either a high number (Table 9, Panel A) or a lower number (Panel B) of marketplace borrowers with a prior history of bankruptcy. In other words, Table 9 shows that the level of prior bankruptcy filings cannot solely explain the documented rise in bankruptcy filings in the zip codes affected by the treatment event.

7. Concluding remarks

We assess the real effects of financial technology in terms of its impact on households. We find that a pullback of marketplace lending is associated with a rise in personal bankruptcy. Withdrawing access to new lending technology leads to a persistent rise in personal bankruptcy filings, particularly among low-income households.

The empirical result that marketplace lending is inversely related to personal bankruptcy suggests that marketplace loans may affect households' financial conditions differently than other forms of costly credit, such as payday loans and credit card debt, which are positively related to the incidence of default and bankruptcy.

These findings have urgent policy implications. While this paper does not imply that marketplace lending or the fintech industry is void of risks and should be left unregulated, it suggests that improving fintech lending regulations may improve access to marketplace funding and help households avoid filing for personal bankruptcy.[54] Our results moreover suggest that, in the absence of a clear regulatory framework for fintech lending, the verdict also had the unintended consequence of persistently raising personal bankruptcies, particularly among low-income households in Connecticut and New York. Understanding the real effects of financial technology, therefore, also informs the intense regulatory deliberations on the wider fintech industry currently taking place at the federal and international level.

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[53] Although we do not observe an increase in delinquencies on non-marketplace loans following the verdict, we do observe an increase in the percent of other types of consumer credit 90+ days delinquent (see Appendix A, Table A.11). We find an increase in delinquencies on credit card debt and auto loans. Economically, these results are significant, implying respectively a 1% and 6% increase in delinquencies. However, only the effect on auto loans is statistically significant. These results are intuitive since marketplace credit rationed individuals who file for bankruptcy are likely to default on other existing type of debt.

[54] There exist a number of concerns regarding marketplace lending relating to consumer protection and market conduct as well as implications for macro-economic and financial stability, including falling lending standards and increasing pro-cyclicality of credit provision to the economy, in addition to moral hazard problems, leverage, liquidity and operational risks, as pointed out by the Financial Stability Board (2017).

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jbankfin.2023.106986.

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