CBA | CONSUMER BANKERS ASSOCIATION
The Voice of the Retail Banking Industry

UTAH BANKERS ASSOCIATION
- SINCE 1908 -

NAIB
National Association of Industrial Bankers

February 4, 2020

BY ELECTRONIC MAIL

Robert E. Feldman
Executive Secretary
Attention: Comments
Federal Deposit Insurance Corporation
550 17th Street, NW
Washington, DC 20429

       Re: <u>Federal Interest Rate Authority, RIN 3064-AF21</u>

To Whom It May Concern:

       The Consumer Bankers Association ("CBA"), the Utah Bankers Association ("UBA"), and the National Association of Industrial Bankers ("NAIB" and collectively, the "Associations")[1] appreciate the opportunity to comment on the Federal Deposit Insurance Corporation's ("FDIC") Notice of Proposed Rulemaking ("NPR") to clarify state banks' federal interest rate authority.[2]

       We applaud the FDIC's efforts to clarify that the power of state banks to charge interest and make loans under Section 27 of the Federal Deposit Insurance Act ("FDIA") includes the power to transfer loans, including the agreed-upon interest-rate term of such loans, to bank and non-bank transferees.[3] In light of the significant disruption to the lending market and the reduction to consumers' access to credit caused by the Second Circuit

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[1] The CBA is the only member-driven trade association focused exclusively on retail banking. CBA's Corporate Members include the nation's largest retail banks, with 85% holding over $10 billion in assets. CBA's Associate Members represent the premier providers of goods and services to banks, including state banks.

The UBA is the professional and trade association for Utah's commercial banks, savings banks and industrial banks, including national banks. Established in 1908, the UBA serves, represents and advocates the interests of its members, enhancing their ability to be preeminent providers of financial services.

The NAIB is the primary trade association representing America's state-chartered industrial loan corporations. Among its initiatives, NAIB sponsors programs and opportunities for industrial banks, national banks, state banks and consumer lending organizations to interact.

[2] "Federal Interest Rate Authority," 84 Fed. Reg. 66845 (Dec. 6, 2019).

[3] Although not the focus of this comment letter, we also generally support the FDIC's proposed regulations in the NPR implementing section 24(j) of the FDIA.

decision in *Madden v. Midland Funding, LLC* ("*Madden*"),[4] it is imperative that the FDIC finalize a rulemaking that reaffirms the longstanding "valid-when-made" rule and rejects *Madden*.

I. FDIC's NPR Would Clarify Long-Standing Law

The valid-when-made rule has been a cardinal rule of lending for the past 200 years[5] that provides that if the interest rate of a bank's original loan was non-usurious at the time the loan is made, the loan does not become usurious upon transfer, such that the transferee may lawfully charge interest on the loan at the original rate. *Madden* rejected this fundamental precept of lending based on flawed reasoning in a decision that has been roundly criticized by the current and past Administrations, federal banking agencies, legal scholars, and industry participants.[6]

Madden also conflicts with the long-standing authority of state banks. State banks' authority to charge interest rates is patterned on the authority provided to national banks under the National Bank Act ("NBA"). Under the NBA, national banks have the power to make loans, to charge interest on such loans at the rate allowed by the laws of the state where the banking organization is located, and to subsequently sell, assign, or otherwise transfer such loans to banks and non-banking entities. As the Solicitor General and the OCC noted in their amicus brief regarding the petition for a writ of certiorari in *Madden*, "[a] national bank's power to charge the interest rate authorized by Section 85 [of the NBA] includes the power to transfer a loan, including the agreed-upon interest-rate term, to an entity other than a national bank."[7] Section 27 of the Federal Deposit Insurance Act, which sets the interest rate authority for state banks, was patterned on section 85 of the NBA.[8] The FDIC and courts have interpreted section 27 of the FDIA in *pari materia* with section 85 of the NBA and, accordingly, *Madden* raises uncertainty about the interest rate authority of state banks.[9] The FDIC rightly acknowledges in the NPR that, notwithstanding *Madden*, federal law, as well as long-standing common law, provide that "a bank's power to make loans

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[4] Madden v. Midland Funding, LLC, 786 F.3d 246 (May 22, 2015).

[5] *See* Gaither v. Farmers & Mech. Bank of Georgetown, 26 U.S. 37, 43 (1828); Nichols v. Fearson, 32 U.S. 103, 106 (1833).

[6] *See, e.g.,* U.S. Department of Treasury Report on Nonbank Financials, Fintech, and Innovation (July 2018); Brief for the United States as Amici Curiae Supporting Respondent, Madden, 786 F.3d 246 (No. 15-610) 2016 WL 2997343; Brookings Report "Can fintech increase lending? How courts are undermining financial inclusion," Peter Conti-Brown (April 16, 2019).

[7] *See* Brief for the United States as Amici Curiae Supporting Respondent, Madden, 786 F.3d 246 (No. 15-610) 2016 WL 2997343.

[8] *See* 12 U.S.C. § 1831d.

[9] *See, e.g.*, Mamot Feed Lot and Trucking v. Hobson, 539 F.3d 898 (2008); Greenwood Trust Co. v. Com. of Mass. 971 F.2d 818 (1st Cir. 1992); FDIC General Counsel's Opinion No. 11, Interest Charges by Interstate State Banks, 63 FR 27282 (May 18, 1998).

implicitly carries with it the power to assign loans, and thus, a State bank's statutory authority to make loans at [a permissible] rate necessarily includes the power to assign loans at the same rate."[10]

II. Negative Effects of *Madden*

Madden has caused, and continues to cause, significant harm to the safety and soundness of state banks by restricting these institutions' core lending and risk management functions. We agree with the FDIC that the ability of a state bank to sell or assign a loan is a key tool for proper risk management. Banks regularly sell, assign, or securitize loans to manage their balance sheets, improve their capital and liquidity positions, and meet consumers' needs for access to funding. As the FDIC notes in the NPR, loan sales also enable banks to meet unusual deposit withdrawal demands, pay unexpected debts, make additional loans and reduce asset concentrations.[11] The FDIC also rightly points out that state banks generally use loan sales and securitizations to diversify their funding sources and address interest-rate risk.[12] However, since *Madden*, banks have been restricted in their ability to make and sell loans in accordance with sound risk management principles. In particular, *Madden* has negatively affected market liquidity for secondary loans, including in the securitization market where market participants have been limited in their ability to include loans made in the Second Circuit in securitizations.[13]

Madden also has had a significant, adverse effect on consumers' access to credit in states in the Second Circuit, particularly low-to-moderate income consumers for whom access to credit is vital for their financial health. For example, studies have found that following *Madden*, lenders offered relatively less credit to borrowers in Connecticut and New York – making smaller loans and declining to issue loans to higher-risk borrowers – and that loans to borrowers in these states with interest rates above state limits trade lower in secondary markets as borrowers become late on their payments.[14] Certain borrower segments have seen a 52% decline in credit availability in Second Circuit states following

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[10] 84 Fed. Reg. at 66845.

[11] *Id.*

[12] 84 Fed. Reg. at 66851.

[13] We note that the importance of securitizations to the banking industry has been acknowledged by the Center for Responsible Lending and the National Consumer Law Center, which stated in their comment letter on the Office of the Comptroller of the Currency's ("OCC") proposal to clarify the interest rate authority of national banks and federal savings associations, that "[s]ecuritization of loans and revolving credit agreements can be a tool that banks use in the furtherance of their own legitimate bank lending programs…" The undersigned are concerned that unless the FDIC and OCC adopt rules that clarify banks' interest rate authority, there is a risk that additional courts will follow *Madden's* incorrect holding and further weaken the securitization market and threaten the safety and soundness of the banking industry.

[14] *See* Honigsberg et al., "How Does Legal Enforceability Affect Consumer Lending?" (October 2017)(included in Appendix A); Honigsberg et al., "What Happens when Loans Become Legally Void? Evidence from a Natural Experiment" (December 2016)(included in Appendix B).

Madden.[15] Another study, which was presented at the Federal Reserve, found that *Madden* reduced the volumes of certain lending segments by 10% in states in the Second Circuit and caused an 8% increase in personal bankruptcy filings in Connecticut and New York relative to other states following *Madden.*[16]

The adoption of the FDIC's NPR, with the suggestions we make below, would reduce significant legal uncertainty resulting from *Madden* that has inhibited banks from conducting their lending activities in accordance with sound risk management practice and hurt consumer access to credit.

III. FDIC Should Clarify the Subsequent Events that Would Not Affect a Permissible Interest Rate

We recommend that the FDIC revise the examples included in proposed section §331.4(e) of the subsequent events that would not affect the permissible interest rate of a loan to be more focused on the issues raised by *Madden* and avoid overbroad examples that could cause confusion and uncertainty in the future. In particular, the FDIC should remove references to a change in state law and the relevant commercial paper rate and revise 12 C.F.R. § 331.4(e) to read as follows:

> "Determination of interest permissible under section 27. Whether interest on a loan is permissible under section 27 of the Federal Deposit Insurance Act is determined as of the date the loan was made. The **interest on a loan shall not be made impermissible** ~~permissibility~~ under section 27 of the Federal Deposit Insurance Act ~~of interest on a loan shall not be affected by any subsequent events, including a change in State law, a change in the relevant commercial paper rate after the loan was made, or~~ **by** the **subsequent** sale, assignment, or other transfer of the loan."

The uncertainty caused by *Madden* has related to the effect of subsequent sales, assignments and other transfers of loans (e.g., participations and sales of receivables) by banks to non-bank entities. Accordingly, the FDIC's proposed regulation should clarify that those subsequent events would not cause a permissible interest rate to become impermissible. The inclusion of references to a subsequent change in state law and the relevant commercial paper rate could cause undue confusion with respect to the effect of such events on permissible variable interest rates terms that may be impacted by subsequent changes in law or the relevant commercial paper rate. For this reason, we recommend that the FDIC adopt a more streamlined regulation that focuses directly on the issues raised in *Madden.*

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[15] *Id.*

[16] *See* Piotr Danisewicz and Ilaf Elard "The Real Effects of Financial Technology: Marketplace Lending and Personal Bankruptcy"(included in Appendix C).

IV. FDIC Should Encourage the OCC to Revise its Proposed Regulation to be Interpreted i*n Pari Materia* with the FDIC's Proposed Rulemaking

While we applaud the OCC's proposal to clarify the interest rate authority of national banks and federal savings associations ("OCC Proposal"),[17] we urge the FDIC to coordinate with the OCC, and we encourage both agencies to adopt regulatory text that more closely matches the text proposed the FDIC (with the revisions suggested above). As the FDIC noted in the NPR, the intent of Congress when it originally enacted section 85 of the NBA in 1864 was to ensure parity between national and state banks in their ability to charge interest.[18] Congress reaffirmed this goal in 1980, when it enacted Section 27 of the Federal Deposit Insurance Act ("FDIA"), which was patterned on section 85 of the NBA.[19] Courts and the FDIC have noted that Congress made a conscious choice to pattern section 27 of the FDIA after section 85 of the NBA to achieve competitive equality in the area of interest charges between state and national banks and that such provisions are "'virtually identical' in substance, policy, and internal logic."[20] Accordingly, courts have construed the meaning of "charging interest" for purposes of section 27 of the FDIA with reference to 12 C.F.R. § 7.4001 and other OCC interpretations.[21]

By having OCC regulatory language setting the scope of permissible interest rates for national banks and federal savings associations that is at variance with the FDIC's regulation applicable to state banks, the OCC Proposal, if finalized as proposed, risks different judicial constructions of the interest rate regimes applicable to federal and state chartered banks, notwithstanding Congress's contrary intent. Accordingly, the FDIC should encourage the OCC to finalize regulatory text that closely matches the FDIC's regulation on permissible interest rates (with the revisions suggested above).

The FDIC's proposed regulatory language has several advantages over the regulatory text included in the OCC's Proposal. First, the FDIC's proposed language more clearly codifies the valid-when-made rule for purposes of federal banking law, by stating that, "whether interest on a loan is permissible" for purposes of federal law, "is determined as of the date the loan was made." In contrast, the proposed language in the OCC's Proposal is less

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[17] "Permissible Interest on Loans That Are Sold, Assigned, or Otherwise Transferred," 84 Fed. Reg. 64229 (Nov. 21, 2019).

[18] *See* 84 Fed. Reg. at 66846; Cong. Globe, 38th Cong., 1st Sess., 2123–27 (1864); *see* Roper v. Consurve, Inc., 578 F.2d 1106 (5th Cir. 1978), affirmed 445 U.S. 326 (1980).

[19] 12 U.S.C. § 1831d.

[20] *See*, e.g., Sawyer v. Bill Me Later, Inc., 23 F. Supp. 3d 1359 (D. Utah 2014); 63 Fed. Reg. 27282 (May 18, 1998).

[21] *See*, *e.g.*, Mamot Feed Lot and Trucking v. Hobson, 539 F.3d 898 (2008); Greenwood Trust Co. v. Com. of Mass. 971 F.3d 818 (1st Cir. 1992).

clear with respect to the point in time that the permissible interest rate for a loan is established. Lenders, borrowers, and participants in the secondary loan market would all benefit from clarity and transparency from FDIC and OCC regulations that expressly codify the valid-when-made rule.

Second, the FDIC's proposed language clearly indicates that a non-usurious loan made by a bank continues to be non-usurious upon a transfer to a non-bank transferee. The FDIC's proposed language states that "[t]he permissibility under section 27 of the Federal Deposit Insurance Act of interest shall not be affected by any subsequent events...," which necessarily includes the sale, assignment, or transfer to any bank *or non-bank transferee*. The FDIC's formulation is consistent with the intent of Congress and market participants' long-standing understanding of the law prior to *Madden*. In contrast, the OCC's proposed regulatory language potentially could be construed more narrowly, notwithstanding that the OCC clearly explained in the preamble in the OCC Proposal that its proposed rule is intended to apply to non-bank transferees.

The FDIC's proposed language, if adopted by the FDIC and OCC, would represent a clear interpretation as to the meaning and scope of allowable interest permitted to be charged under Section 27 of the FDIA and section 85 of the NBA. Such interpretations would likely receive deference by a reviewing court, consistent with the deference previously afforded by the Supreme Court to the OCC when interpreting Section 85 of the NBA.[22] For the reasons stated above, the FDIC should encourage the OCC to finalize regulatory text that closely matches the regulatory language that is ultimately adopted by the FDIC (with the revisions suggested above).

V. The FDIC Should Finalize the NPR Expeditiously

We strongly support the FDIC's finalization of the NPR as a targeted rulemaking that quickly remediates the significant and on-going harm to the economy and consumers resulting from *Madden*. The disruption and uncertainty caused by *Madden* has persisted for far too long. For almost five years, *Madden* has prevented banks from being able to manage their lending activities with the full range of tools normally used by financial institutions to operate prudently and in a safe and sound manner. Consumers living in states in the Second Circuit have and continue to be noticeably harmed through reduced access to credit, which for many consumers has had cascading negative implications for their long-term financial condition. The FDIC is well-positioned to remediate these harms by codifying the "valid-when-made" rule. We applaud the FDIC's leadership in taking the first step necessary to address this important issue by issuing the NPR, and now we urge the FDIC finalize the rulemaking, with the changes proposed herein, as expeditiously as possible.[23]

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[22] *See* Smiley v. Citibank (S.D.), N.A., 517 U.S. 735, 740-745 (1996).

[23] For these reasons, we also support that the NPR remain targeted and streamlined, without specifically addressing which entity is the true lender when a bank makes a loan and assigns it to a third party.

Adoption of the NPR, with the revisions suggested herein, would expand access to consumers' access to credit, improve liquidity in the secondary market for loans, and buttress the FDIC's other regulatory priorities. We support the agency's expeditious finalization of this important rulemaking.

Sincerely,

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APPENDIX A

How Does Legal Enforceability Affect Consumer Lending?
Evidence from a Natural Experiment

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October 2017

ABSTRACT

We use a natural experiment—an unexpected judicial decision—to study how the legal enforceability of debt contracts affects consumer lending. In May 2015, a federal court unexpectedly held that the usury statutes of three states—Connecticut, New York, and Vermont— applied to certain loans that market participants had assumed were exempt from those statutes. The case introduced substantial uncertainty about whether borrowers affected by the decision were under any legal obligation to repay principal or interest on their loans. Using proprietary data from three marketplace lending platforms, we use a difference-in-differences design to study the decision's effects. We find no evidence that borrowers defaulted strategically as a result of the decision. However, the decision reduced credit availability for higher-risk borrowers in affected states. And secondary-market data indicate that the price of notes backed by above-usury loans issued to borrowers in affected states declined, particularly when those borrowers were late on their payments.

Keywords: usury law; strategic default; consumer lending; National Bank Act; marketplace lending; *Madden v. Midland*

1. Introduction

Most US states have usury statutes that cap interest rates lenders may charge. Yet these statutes have only a marginal impact on consumer lending because federal banking law has long been understood to allow national banks to issue debt that is exempt from these limits. This understanding changed on May 22, 2015, when a federal appeals court with jurisdiction over three states ruled that the state usury exemption provided to national banks is lost if the national bank sells the debt to a nonbank before maturity. This unexpected judicial decision, *Madden v. Midland Funding LLC*, has great disruptive potential, as a large proportion of consumer debt issued by national banks is resold to nonbank investors before coming due.

The decision is particularly important in two of the states under the court's jurisdiction, Connecticut and New York. The usury statutes of these states treat usurious loans as void, meaning that borrowers have no legal obligation to repay any outstanding principal or interest. *Madden* therefore creates a natural experiment that allows us to study how market participants react to a large increase in the possibility that billions of dollars in outstanding consumer loans are no longer legally enforceable. Moreover, because the decision applies in only a few states, it provides a setting with a natural treatment group, allowing us to run difference-in-differences tests comparing loans issued to borrowers in New York and Connecticut to loans issued to borrowers in states unaffected by *Madden.*

To measure *Madden's* impact, we use proprietary data from three of the largest marketplace lending platforms. These platforms, which provide a growing source of nonbank consumer credit, enable prospective borrowers and lenders to find each other quickly and efficiently. Loans arranged through the platforms are issued by an affiliated bank but sold promptly to nonbank investors, making them vulnerable to *Madden*'s holding that loans transferred to

nonbanks are no longer exempt from state usury law. Although *Madden* applies to a wide range of loans and likely has effects beyond the marketplace-lending context, we focus on this relatively narrow setting because we obtain high-quality data from marketplace lending platforms that allow us to trace the loan process through different points in time.

During the period for which we have data – 2015 – there was significant uncertainty about the decision's ultimate implications. Possibilities remained that the Supreme Court would reverse the decision or that the nonbank defendant in the case would ultimately prevail on other theories of enforceability. Therefore, our study is of how market participants respond to a significant increase in the level of legal uncertainty rather than to an unambiguous change in the law.

Our study analyzes the effect of the decision on lenders and borrowers separately and provides clear evidence that the decision changed the behavior of some market participants. Beginning with lenders, we find that they were aware of the decision and modified their behavior in two ways. First, secondary market trading data show that *Madden* significantly reduced the price of notes backed by above-usury loans to borrowers in Connecticut and New York. Although we find statistically significant discounts for both non-current and current loans, the discount is highly economically meaningful for notes backed by non-current loans but close to zero for current loans. These findings indicate that debtholders were aware of *Madden* and its potential to harm their ability to collect on the loans, but were not especially concerned unless borrowers were already late on their payments. In other words, they did not expect widespread strategic default.

Second, lenders responded to the decision by extending relatively less credit to borrowers in Connecticut and New York. Not only did lenders make smaller loans in these states post-*Madden*, but they also declined to issue loans to the higher-risk borrowers most likely to borrow above usury rates. Our sample contains hundreds of loans issued to borrowers with FICO scores

below 640 in Connecticut and New York in the first half of 2015, but no such loans after July 2015. These findings are consistent with basic economic intuition, as well as with prior literature showing a negative association between credit availability and usury law (e.g., Benmelech and Moskowitz 2010).

With respect to borrower behavior, we find no evidence that the decision caused borrowers to default strategically on above-usury loans. Strategic default is a growing topic in the finance and economics literature, particularly since the financial crisis, during which many homeowners faced incentives to walk away from underwater mortgages (e.g., Foote, Gerardi and Willen 2008; Guiso, Sapienza and Zingales 2013; Mayer et al. 2014). Although the incentive to default on an unsecured and potentially unenforceable consumer loan seems stronger than the incentive to default on an underwater mortgage, there are many possible reasons why we find no evidence of such behavior. Some borrowers may have been unaware of the decision, and others may have worried that *Madden*'s uncertain future could subject them to lawsuits whose costs could easily outweigh the benefits of defaulting.[1]

Our study contributes to literature on the influence of legal institutions on behavior. Legal theorists have long debated whether legal enforcement mechanisms are necessary to ensure contractual performance, or whether reputational sanctions, the parties' taste for fairness, and other factors can be effective substitutes (e.g., Schwartz and Scott 2003; Rabin 1993). Recent work has tested these questions empirically by studying strategic default in the context of mortgages (e.g.,

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[1] As noted earlier, both lenders and consumers could view the case as creating legal ambiguity regarding the enforceability of the loans rather than truly voiding the loans. It is also possible that borrowers chose not to default due to non-pecuniary factors such as morality (Guiso et al. 2013) or that they were concerned with reputational risk. However, it is far from clear whether borrowers who strategically defaulted on consumer loans after *Madden* would suffer reputational harm. To date, credit-reporting agencies have yet to decide whether they can reduce a borrower's credit score for defaulting on a loan that, according to *Madden*, the borrower has no legal obligation to repay. Indeed, some consumer advocates object to use of the word default in this context, arguing that borrowers cannot "default" on a loan that is legally void.

Foote et al. 2008; Guiso et al. 2013; Mayer et al. 2014). We extend these studies by examining strategic default in a new setting: consumer lending—a market that, despite its very significant size, has been difficult to study due to data limitations (Tufano 2009; Campbell 2006).

We also contribute to the literature on the effects of legal uncertainty. Prior theoretical work has noted that uncertainty can distort incentives and cause markets to function inefficiently. To avoid violating an uncertain legal rule, market participants are incentivized to "over-comply" with the uncertainty, modifying their behavior so that it is no longer socially optimal (Calfee and Craswell 1984). For example, as applied to our setting, lenders who supplied socially optimal levels of credit prior to *Madden* were incentivized to "over-comply" with the decision and reduce lending beyond optimal levels. Our empirical evidence seems consistent with this argument, as loans to the highest-risk borrowers in Connecticut and New York disappeared entirely from our sample—even though similar borrowers in other states continued to receive funding. In this regard, legal uncertainty may be worse than a bad rule that allows for bargaining.

Finally, our findings contribute to the literature on law and debt contracting more generally. A large body of prior literature has studied how legal institutions are related to corporate debt contracts and loan syndication (e.g., Qian and Strahan 2007; Lerner and Schoar 2005). Although these papers encompass a broad range of subject areas, from corporate law (Wald and Long 2007) to bankruptcy law (Davydenko and Franks 2008), they focus almost exclusively on statutory law (one exception is Honigsberg, Katz and Sadka (2014), which incorporates both statutory law and judicial decisions). By contrast with most previous papers on law and debt contracts, our paper examines the effects of a decision by a significant federal court. Judicial decisions are critical for debt contracting in the United States, but they are difficult to study empirically because economically meaningful changes in the law governing debt contracts are rare. *Madden* provides

a unique opportunity to understand how parties incorporate judicial opinions into the contracting process. For example, as we discuss below, we find that marketplace-lending platforms took roughly two months to adjust their lending practices to the decision. From a methodological perspective, this finding suggests that researchers should be cautious when running event studies to evaluate the effects of unexpected court decisions and should set the event window carefully.

The remainder of the Article proceeds as follows. Part 2 reviews the legal and institutional setting and its application to marketplace lending platforms. Part 3 describes our data and methodology. Part 4 describes our results, and Part 5 concludes.

2.   Legal and Institutional Background

A.   State Usury Statutes and Federal Preemption

Dating back to the Old Testament, usury laws cap the interest rate that lenders may charge on loans. The policy merits of such caps have been debated for generations (e.g., Holy Bible: New International Version 1984, Leviticus 25-37; Shanks 1967; Homer and Sylla 2005). Opponents argue that usury limits exclude riskier borrowers from legitimate lending arrangements—or, worse, require them to resort to more expensive, and even black-market, sources of credit (Bentham 1787; Ryan 1924). Proponents counter that usury caps constrain lender market power and prevent naive borrowers from incurring debts they have little chance of repaying (National Consumer Law Center 2016).

Whatever the merits of this debate, most American states have adopted usury statutes that expressly cap interest rates. Penalties vary. Most statutes require lenders to return interest paid above the limit; some reward borrowers three times this amount.[2] Perhaps most severe are the laws

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[2] See, e.g., CAL. CIV. CODE § 1916-3 (providing for treble damages of usurious interest in California).

of states such as Connecticut and New York, which declare usurious loans null and void: the borrower is entitled to keep the principal as a gift and need not pay any fees associated with the loan.[3] Rate caps also differ across states. Although usury laws are frequently associated with payday lending, usury limits are often low enough to capture a significant portion of consumer lending—some states set limits as low as 5 percent for consumer loans.[4]

Despite their pervasiveness, usury laws have relatively little effect on modern American lending markets. The reason is that federal law preempts state usury limits, rendering these caps inoperable for most loans. For loans made by national banks, the National Bank Act ("NBA") establishes a usury limit equal to the limit of the state in which the bank is "located."[5] Loans made by state-chartered banks can preempt usury limits through a similar provision in the Federal Deposit Insurance Act.[6] This is why many banks, and particularly those that engage in significant consumer lending, are located in states such as South Dakota and Utah, which have no usury limit. Banks in those states can charge whatever the market will bear, even if the borrower lives in a state whose laws deem the rate usurious (Smith 2009).

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[3] See N.Y. GEN. OBL. L. § 5-501(1). As Stein (2001) explains, in New York, "[i]f a loan is usurious, it becomes wholly void": the "lender forfeits all principal and interest (the loan becomes a gift)"; see also *Seidel v. 18 East 17th Street Owners*, 598 N.E. 2d 7, 9 (N.Y. 1992) ("The consequences to the lender of a usurious loan [in New York] can be harsh: the borrower is relieved of all further payment—not only interest but also outstanding principal . . . New York usury laws historically have been severe in comparison to the majority of States."); *Ferrigno v. Cromwell Development Assoc.*, 44 Conn. App. 439, 439 (App. Ct. Conn. 1997) ("Loans with interest rates in excess of [the usury cap in Connecticut] are prohibited [by statute] and as a penalty no action may be brought to collect principal or interest on any such prohibited loan.").

[4] See Ga. Code Ann. § 7-4-18 (West 2016). See also, e.g., Ala. Code § 8-8-1, Minn. Stat. Ann. § 334.01 (West), 41 Pa. Stat. Ann. § 201 (West) (establishing a usury limit of 6% for loans below $50,000).

[5] The National Bank Act of 1864 expressly allows national banks to "charge on any loan . . . interest at the rate allowed by the laws of the State, Territory, or District where the bank is located, or at a rate of 1 per centum in excess of the discount on ninety-day commercial paper in effect at the Federal reserve bank in the Federal reserve district where the bank is located, whichever may be the greater." 12 U.S.C. § 85 (2016).

[6] Section 27 of the Federal Deposit Insurance Act ("FDIA"), 12 U.S.C. § 1831d. *Madden* did not explicitly address the federal-law provision addressing usury preemption for state-chartered banks. Nevertheless, the FDIA's preemption is sufficiently similar to the NBA's preemption provision that market participants have assumed loans initiated through state-chartered banks would be similarly affected.

Federal preemption in this area invites legal inquiries because banks that originate consumer loans often do not hold them until maturity. Rather, they sell much of the debt to nonbank investors such as hedge funds (Buhayar 2016). Further, consumer loans are often securitized (i.e., converted to marketable securities and resold to other investors). Such practices present the legal question whether a loan issued by a national bank continues to be exempt from the usury laws of the borrower's state after the loan is sold to a nonbank. The traditional rule under usury law is that a loan is "valid when made," meaning that a change in the identity of the lender or residence of the borrower does not alter its enforceability. Sometimes called the "cardinal law of usury," the valid-when-made rule is well-established, and before 2015 courts followed it consistently when determining the NBA's preemptive scope.[7] For example, in the 2000 case *Krispin* v. *May Department Stores Co.,* the US Court of Appeals for the Eighth Circuit held that debt owed on credit cards issued by a national bank continued to be exempt from the usury laws of the borrowers' state even though the bank had sold the receivables to a department store.[8]

B.   **The Second Circuit's *Madden* Decision**

Madden stunned markets by calling the cardinal law of usury into question. The plaintiff in the case, Saliha Madden, is a New Yorker who defaulted on her credit card debt. Her card was

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[7] The cases brought by the Consumer Finance Protection Bureau (CFPB) against CashCall in California and West Virginia are arguably exceptions to this rule. In those cases, the CFPB alleged that CashCall, a California financial institution, violated usury laws by purchasing loans issued by state-chartered banks and Native-American lending institutions (which, like national banks, also enjoy preemption of state usury laws) and immediately reselling those loans to consumers. In 2014, the Supreme Court of Appeals of West Virginia decided in *CashCall Inc. v. Morrisey* that Section 27 of the FDIA did not preempt claims against the defendant for violations of the West Virginia Consumer Credit Protection Act. And in 2016, the United States District Court for the Central District of California held in *Consumer Financial Protection Bureau v. CashCall, Inc., et al.* that the usury laws of the borrowers' home states should be applied. However, commentators have opined that these cases may not be reflective of current law. Indeed, in the California case the defendants have taken the relatively rare step of petitioning the Ninth Circuit Court of Appeals for interlocutory review of the trial court's decision. *See* Petition for Permission to Appeal *in Consumer Financial Protection Bureau v. CashCall, Inc.*, No. 17-8006 (9th Cir. Jan. 13, 2017).

[8] *Krispin v. May Department Stores Co*., 218 F.3d 939 (2000). Five years later, the Eighth Circuit again applied the valid-when-made rule to dismiss state-law usury claims based on loans issued by a national bank. *Phipps* v. *FDIC*, 417 F.3d 1006 (8th Cir. 2005). The Supreme Court first recognized the valid-when-made rule (though outside the context of the NBA) in 1833. *Nichols v. Fearson*, 32 U.S. (7 Pet.) 103, 109.

issued by Bank of America, and her account was originally serviced by FIA Card Services, a national bank based in Delaware, a state that permits banks to charge rates that would be usurious in New York. After Madden defaulted, FIA sold the receivable to Midland Funding, a debt collector. Midland sent Madden a collection notice seeking repayment of a balance calculated at 27% annual interest, the rate specified in her cardholder agreement. Madden declined to pay and sued Midland in federal court on behalf of herself and other New Yorkers. She claimed that the interest rate violated New York's usury laws, which set a civil cap of 16% and a criminal cap of 25%. In September 2013, the district court ruled for Midland, holding that the loan was valid when issued and remained so after its transfer to a nonbank.[9]

Madden appealed, and on May 22, 2015, the US Court of Appeals for the Second Circuit reversed, holding that the NBA's preemptive scope no longer applied to Madden's debt once it was sold to an entity that was not a national bank.[10] The NBA only preempts state laws whose application might "significantly interfere" with the exercise of the national banking power, and the court found that this requirement was not met in Madden's case. The court thus held that Madden's debt was subject to New York's usury laws. Because New York law renders usurious loans void, the holding would seemingly cancel Madden's outstanding credit-card balance.

C.   Subsequent Legal Developments

In response to the Second Circuit's decision, Midland petitioned the Second Circuit to rehear the case; when the petition was denied, Midland asked the Supreme Court to review the decision. Upon receipt of Midland's petition, the Supreme Court requested the Solicitor General's

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[9] See Stipulation for Entry of Judgment for Defendants for Purpose of Appeal, Madden v. Midland Funding LLC, No. 11-CV-8149 (May 30, 2014) ("preemption of New York's usury laws applies to non-bank assignees of national banks, regardless of whether the national bank retains any interest in or control over the assigned accounts."). We note that Madden's claims actually focused on New York's *criminal* usury statute, which makes it a Class E felony to charge interest of more than 25%. N.Y. PENAL LAW § 190.40.

[10] *Madden v. Midland Funding, LLC*, 786 F.23d 246, 250 (2d Cir. 2015).

view of the case. Although the Solicitor General's brief stated that the Second Circuit had "erred" and that the *Madden* "decision is incorrect," the brief counseled the Supreme Court that review was premature, as Midland could still prevail in the lower courts on other theories of enforceability (Solicitor General of the United States 2016).

The ensuing legal developments in Second Circuit have not been favorable for Midland or other nonbank debtholders. First, in April 2016, a proposed class-action lawsuit seeking damages for usurious lending was filed on behalf of consumers who borrowed through the Lending Club platform, an event that may lead to more widespread consumer knowledge of *Madden*.[11] Second, in June 2016, the Supreme Court followed the Solicitor General's advice and declined to hear *Madden*. Third, state financial regulators, including New York's Department of Financial Services, have successfully negotiated settlements with several nonbank lenders who, according to these regulators, have attempted to charge usurious interest in violation of state law.[12] Finally, in February 2017, the lower courts rejected Midland's argument that the agreement should be governed by Delaware law[13] and agreed to certify a class of plaintiffs (a crucial step in class action

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[11] See *Bethune v. Lending Club Corp. et al.*, No. 1:16-cv-02578-NRB (S.D.N.Y. April 6, 2016) (In a recent win for Lending Club, the court in that case granted Lending Club's motion to compel arbitration in January 2017.) The Second Circuit's *Madden* ruling could influence the ultimate outcome of other class-action lawsuits challenging the valid-when-made rule in other jurisdictions. Perhaps the most well-known of these cases is *Blyden v. Navient Corp.* Filed in California federal court in 2014, the plaintiff has alleged that the interest charged on her student loan is usurious under California state law. Her loan was issued by a national bank but assigned to several nonbanks, the defendants in the case. The case remains at the pleading stage, and the court has yet to reach the NBA preemption question. *See Blyden v. Navient Corp.*, No. 5:14-CV-2456, 2015 WL 4508069 (C.D. Ca. July 23, 2015) (dismissing plaintiff's complaint but giving her leave to amend); *see also MacDonald v. CashCall Corp.*, No. 16-2781, 2017 WL 1536427 (D.N.J. April 28, 2017) (declining to dismiss a similar suit raising claims under New Jersey's usury laws).

[12] For example, in May 2016 the New York State Department of Financial Services entered into such a settlement with National Credit Adjusters on the basis of findings including that the lender "pursued and collected payments made on thousands of usurious payday loan accounts of New York consumers." *In re National Credit Adjusters, LLC*, Consent Order (May 16, 2016). The company agreed, on the basis of the Department's allegations, to discharge in full more than $2 million in consumer debts, provide interest refunds of more than $700,000, and pay a civil penalty of $200,000. *See id.* ¶¶ 15, 16, 32.

[13] Because choice-of-law provisions in the agreement at issue in *Madden* stated that the agreement was to be governed under Delaware law, Midland argued that these provisions should be given effect. Had this argument prevailed, Madden's case would have been dismissed because the loan was not usurious under Delaware law.

litigation that is often not met). The case has now been cleared for discovery and seems destined for trial or, more likely, settlement.

Although these recent developments in the Second Circuit have not been favorable to nonbank lenders, two new avenues have opened that may ultimately overturn *Madden*. First, the Financial CHOICE Act proposed by the House Financial Services Committee includes language overturning the decision.[14] However, it is unclear whether the Act will pass and, if so, whether the language will be included in the final version. Second, government officials in two states have sued nonbank lenders over usury-related charges, and either case could end up in the Supreme Court. In a case that has attracted national attention, the Administrator of the Colorado Uniform Commercial Code sued Avant, Inc., a marketplace lending platform, for collecting usurious charges on past-due loans in violation of Colorado's usury cap.[15] And in Pennsylvania, the Attorney General sued a group of online, nonbank lenders for lending at interest in excess of the state's usury cap.[16] The Supreme Court's refusal to hear *Madden* does not necessarily signify that the justices consider the NBA issue in the case unimportant or believe that it was decided correctly,

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[14] Proposed Section 581 of the Financial CHOICE Act would amend the National Bank Act to say that a "loan that is valid when made as to its maximum rate of interest in accordance with this section shall remain valid with respect to such rate regardless of whether the loan is subsequently sold, assigned, or otherwise transferred to a third party, and may be enforced by such third party notwithstanding any State law to the contrary."

[15] The likelihood that this case will end up in the Supreme Court depends on the resolution of certain procedural issues. The case was filed in state court, but Avant attempted to remove it to federal court, asserting that it raises a federal question—namely, that the claims against Avant are preempted by the NBA. The question now confronting the state-court judge is whether the claims are so completely preempted that the lawsuit should be returned to federal court, where the claims would probably be dismissed as preempted, or whether the claims are at most partially preempted, permitting the state court to maintain jurisdiction. The Colorado judge has accepted several amicus briefs on this question, including one by the Clearing House Association and American Bankers Association that cites an earlier draft of this paper.

[16] In arguing that the case against them should be dismissed, the nonbank lenders argued that the claims were preempted because the loans were issued by a national bank. In response, the Attorney General derisively referred to this as a "rent-a-bank" scheme. In January 2016, the federal district court, citing *Madden,* denied the motion, reasoning that the preemption defense is available to national banks but not to nonbank defendants. *Pennsylvania v. Think Fin., Inc.,* No. 14-CV-7139, 2016 WL 183289 (E.D. Pa. Jan. 14, 2016). The case has yet to reach a final judgment.

so the Court may decide to hear either of these cases. A ruling by the Supreme Court for the nonbank lenders in either case could effectively overturn the Second Circuit's *Madden* decision.

D.   Marketplace-Lending Platforms and State Usury Law

Madden casts a shadow on debt markets in which originators do not hold loans to maturity but rather follow an originate-to-distribute business model. Marketplace lending is one such market (United States Department of the Treasury 2016). The industry has grown quickly as consumers have sought new sources of credit in the years following the financial crisis. While marketplace-lending platforms originated $5.5 billion in loans in 2014 (Small Business Association Office of Advocacy 2015), the three platforms we study here—which represent less than the full market—originated more than $12 billion in loans in 2015. The overall industry is expected to reach $150 billion in annual loan originations over the next decade (PricewaterhouseCoopers 2015).

While details vary across platforms, the general framework for marketplace lending is as follows. A borrower submits an application with standard information, including her credit information, employment history, and the purpose of the loan. The platform uses a proprietary algorithm to assign a risk grade to the proposed loan and then posts the loan request on the platform's website, where investors can search for specific loans that meet their desired risk characteristics. Upon finding a match, investors have the option of offering to fund the loan in full or in part. When one or more investors have offered to fund a proposed loan in full, the loan is issued by an affiliated bank pursuant to an agreement between that bank and the marketplace platform. The bank used by a number of marketplace platforms, WebBank, is located in Utah—a state with no usury limit (United States Department of the Treasury 2016). The originating bank promptly transfers its interest in the loan to the investors who have agreed to fund it. The platform

generally receives an origination fee upon the initiation of the loan and a servicing fee over its lifetime.

Several commentators have celebrated the emergence of marketplace lending as a means of providing additional competition for consumer credit (e.g., Economist 2014). These platforms can save borrowers money, as most loans are used to repay higher-interest forms of debt such as credit cards (Economist 2014; Vermont Department of Financial Regulation 2015; PricewaterhouseCoopers 2015). [17] Especially for higher-risk, lower-quality borrowers, the difference in rates can be significant.

These marketplace lending platforms rely on federal banking law to avoid the application of state usury laws. For example, because these loans are immediately sold to nonbank investors, platforms rely on the valid-when-made doctrine to shield their loans from usury caps. Further, marketplace loans, like other forms of consumer credit, are often securitized—according to one estimate, some $5 billion in notes based upon marketplace consumer loans was issued in 2015 alone (Iyvengar and Reed 2015). Investors in these notes, too, rely upon NBA preemption to ensure that the loans underlying the notes are not subject to state usury laws. Accordingly, the *Madden* decision is disclosed as a risk factor in prospectuses for notes backed by platform-originated loans (e.g., Prosper Funding LLC 2016).

E.   *Madden*'s Implications for Borrowers and Lenders

Madden was a surprise to market participants and has significant implications for a wide range of loans. However, although *Madden* cast doubt on the legal enforceability of certain consumer loans, the case's ultimate disposition and practical significance were uncertain during

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[17] This generalization may not apply to small-business lending. Some recent work suggests that small businesses can, and often do, borrow at lower rates from banks than they can through online debt-marketplace platforms (Federal Reserve Board, 2014; SBA, 2015).

the period we study and many questions remain unanswered even today. As noted above, it still was possible at the end of 2015 that the Supreme Court would ultimately reverse the decision or that the defendant-debtholder would prevail on other theories of enforceability. And the possibilities remain today that Congress will overturn the decision or that the Supreme Court will overrule it while reviewing a different case.

From a debtholder's perspective, there are two straightforward predictions. First, observers anticipated that *Madden* would disrupt secondary-market trading of above-usury loans issued to borrowers in affected states because investors would be reluctant to invest in loans that were potentially uncollectible. Indeed, in the flurry of law-firm memoranda that followed *Madden*, counsel warned investors that the Second Circuit's decision "could significantly disrupt the secondary market for bank loans originated by national banks" (Ropes & Gray LLP 2015).[18] Similarly, Midland's petition for certiorari in the Supreme Court argued that the Second Circuit's decision "threatens to inflict catastrophic consequences on secondary markets that are essential to the operation of the national banking system and the availability of consumer credit."[19]

Second, consistent with prior literature on the effects of usury laws, another prediction is that *Madden* would, within the affected states, reduce credit availability for higher-risk borrowers likely to borrow above usury rates (e.g., Goudzwaard 1968; Shay 1970; Greer 1974; Rigbi 2013; Melzer and Schroeder 2017). If lenders cannot legally charge rates sufficient to compensate for

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[18] Another large New York law firm remarked: "Perhaps most troubling about the opinion . . . is a cursory statement, which was made without explanation or supporting data, indicating that application of state usury laws to third-party assignees of bank-originated loans would not prevent or 'significantly interfere' with the exercise of national bank powers … Inexplicably, the court failed to realize the significance that its ruling would have on the ability of banks to sell their loans in the secondary market. Given that non-bank purchasers will be unable to enforce the terms of a loan according to the original agreement between the bank and borrower, [the decision] will undoubtedly chill the market for … securitizations and bank loan programs with third parties." (Paul Hastings LLP 2015).
[19] Pet. for Cert. in Midland Funding LLC et. al v. Saliha Madden, No. 15-610 (Nov. 10, 2015).

the default risk indicated by prospective borrowers' risk profiles, they will naturally lend less. The decline in credit availability could manifest as reductions in loan volume and/or loan size.

In terms of borrower impact, the effect of *Madden* is not as clear. Although *Madden* provides borrowers in Connecticut and New York with incentives to default on their above-usury loans, there are many reasons to expect that borrowers will not engage in such action. First, they may be unaware of the ruling. We think the two most plausible channels through which borrowers would learn of the case are plaintiffs' attorneys, who might publicize the case to search for clients, and bankruptcy attorneys, who might advise clients considering a bankruptcy filing to default on loans affected by the decision while continuing to pay their other debts. Although we searched for evidence that the case has been publicized through these channels, we have yet to find any. However, we anticipate that awareness of the case will increase if any *Madden*-related class action lawsuits are resolved favorably for the borrowers or their attorneys.

Second, borrowers might refrain from defaulting strategically for non-pecuniary reasons such as moral compunction. In a survey by Guiso et al. (2013), 82.3% of respondents indicated that it is morally wrong to walk away from a house when one can afford to pay the monthly mortgage. Finally, borrowers may be concerned that their reputation (i.e., credit score) would suffer, despite the fact that it is unclear whether borrowers may be penalized by credit agencies for defaulting on a loan that is, according to *Madden*, legally void.

Finally, and perhaps most importantly, legal uncertainty around *Madden* might reduce strategic defaults. Borrowers might have expected that the Supreme Court would overturn the decision, that Midland (the debt-collector) would prevail on other theories of enforceability, or that lenders would find ways to evade the decision. For example, it is unclear whether an above-

usury loan held by a nonbank investor can regain its enforceability if resold to a national bank.[20] If so, this would negate the benefits of strategic default. Such uncertainty likely increases the expected costs of defaulting strategically, as borrowers may fear that they will become defendants in potentially costly lawsuits if they default.

3.   Methodology and Descriptive Statistics

A. Research Design

For two reasons, the *Madden* decision offers a unique empirical setting in which to examine how law affects consumer lending. First, the decision was by all accounts a surprise, offering a plausibly exogenous shock to market expectations about the state of the law. Second, the decision applies in only a subset of the country: Connecticut, New York and Vermont, the states subject to the Second Circuit's jurisdiction. *Madden*'s limited geographic impact permits us to create plausible treatment and control groups to analyze the effects of the decision. Our analysis therefore utilizes a difference-in-differences approach. Although we considered a regression-discontinuity design comparing loans just above and below the usury threshold, we did not have enough loans with interest rates close to the threshold to use this approach.

First, we consider the proper treatment group. Our most obvious treatment group would be borrowers in the three Second Circuit states. However, that group would have a heterogeneity problem, as the states differ in their treatment of usurious loans. While usurious loans are void in Connecticut and New York, they remain valid in Vermont, where the borrower is excused only

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[20] We have questioned several bank managers on this point. If buying the loans would make them enforceable, we asked, why wouldn't a national bank buy these loans at a discount from nonbank investors? Are any banks already doing so? The managers answered that they were not sufficiently confident that the loans would be enforceable that they wanted to take the risk. They also worried that holding a significant portfolio of above-usury loans could harm their banks' reputations and invite regulatory scrutiny.

from paying interest above the permissible rate, and in a lawsuit against the lender can recover any such interest already paid, interest thereon, and reasonable attorney's fees.[21] Because the laws of the three states award very different damages, we are hesitant to group these three states for empirical purposes. Hence, we use only Connecticut and New York in our treatment group, and our Vermont loans are dropped from the tests. As a practical matter, including Vermont makes very little difference in our results, as we have relatively few observations in that state.

Second, we consider the proper control group. Our primary control group contains all loans whose borrowers live outside the Second Circuit, as such loans are not directly affected by the *Madden* decision. However, this group also has a heterogeneity problem. The heterogeneity results from uncertainty about the ultimate disposition of the *Madden* case during our sample period. In 2015, it was unclear whether the Supreme Court would affirm, reverse, or refuse to review the decision. In states outside the Second Circuit that have their own usury laws, the mere possibility that the Supreme Court would affirm *Madden*—making it applicable nationwide—could affect lender willingness to issue loans at above-usury rates. Further, even if the Supreme Court denied review, lenders might fear that courts in their state would find *Madden*'s logic persuasive and adopt it. However, states without usury laws should not be affected by this uncertainty—whether federal law preempts state usury law with respect to borrowers in those states is irrelevant because there are no usury laws to preempt. For this reason, we build a second control group consisting solely of loans to borrowers in states without usury caps.[22]

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[21] Vt. Stat. Ann. tit. IX, § 50(a)(2016).

[22] The states that have no statutory usury limits are Mississippi, New Hampshire, New Mexico, South Dakota, Virginia, and Utah. We note that the usury laws of some other states might not apply to some or all of the loans in our sample (e.g., some states impose usury limits only on loans below a certain dollar amount or exempt loans made to or from certain legal entities or for certain purposes). However, to be consistent and avoid ambiguity, we limit our no-usury sample only to those states that lack usury limits entirely.

When appropriate, we also include a third control group created using propensity score matching (PSM), a statistical technique that allows us to match the loans made to borrowers in Connecticut and New York with a comparable set of loans made to borrowers outside the Second Circuit. Our PSM sample is created using nearest-neighbor matching without replacement, meaning that we match each treatment loan-borrower pair with the most similarly situated control loan-borrower, and we do not reuse observations. However, as we describe below, the type of borrowers changed significantly in Connecticut and New York after *Madden* was decided, making it difficult to create a matched set of observations. Because of this, we are unable to use the PSM sample in some tables and the sample is not well-balanced across the control variables even when we do use it. While we include the PSM sample for completeness, we note the limitations of the analysis and include a robustness section with additional tests.

B. Descriptive Statistics

Studying *Madden*'s impact requires data on loans that were originated by banks in accordance with federal preemption of state usury laws but were sold to nonbank investors. Because loans issued through marketplace-lending platforms fit this description, we targeted these platforms. We were able to execute agreements with three of the largest marketplace lending platforms in the United States, pursuant to which the platforms agreed to share loan-level data with us for purposes of this study. Our nondisclosure agreements prohibit us from identifying the firms by name, but we note that all three are among the largest—if not the largest—marketplace-lending platforms in the United States (Federal Reserve Board 2014). The firms provided two types of data: (1) information on loans arranged through their platforms ("primary lending dataset"), and (2) information on secondary-market trading of notes backed by loans arranged on the platforms ("secondary-market dataset"). We use the aggregated data from all three platforms for our analysis.

Although other studies have examined aspects of marketplace lending using data from one lender (e.g., Rigbi 2013), we are unaware of any other papers that use the private data we examine here.

Our primary-lending dataset contains data on almost 950,000 loans, with a total principal amount of nearly $12 billion.[23] All loans were issued in 2015. They range from $1,000 to $35,000 in principal amount, with a mean (median) principal amount of about $12,500 ($10,500). The interest rates range from 5% to 66%, with a mean (median) value of 18% (15%). Figure 1 presents the total value of loans in this dataset for each month of 2015. The trend line included in the figure shows the overall growth of the market.

In addition to loan characteristics such as interest rate, principal amount, and term, our primary-lending dataset also includes the following characteristics for each borrower in our sample: annual income, debt-to-income ratio, number of recent delinquencies, total credit availability, months of employment in the borrower's current position, and an estimate of each borrower's FICO score. For privacy reasons, the platforms gave us only a four-point FICO range for each borrower (e.g., 660 to 664). In the analyses using FICO scores, we use the midpoint of these ranges.

Overall, the borrowers in the primary-lending dataset tend to be in the same credit range as the average American borrower. The mean (median) FICO score is 684 (681.5). By comparison, the mean FICO score in the United States is 695 (Fair Isaac Credit Organization 2015). (As a general rule, a score between 670 and 739 is considered "good" (Experian 2015).) Our borrowers—like the majority of marketplace borrowers—cite debt consolidation and repayment of credit card balances as the most common reasons for borrowing through a marketplace platform. Other listed reasons range from home improvements to special events such as weddings

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[23] One of the three marketplace platforms included in our study offers both a "market-based" program, in which investors can select the loan they wish to fund, and a smaller "take it or leave it" program, in which investors must accept a full package of loans on an all-or-nothing basis. Because only one of the marketplace platforms we worked with offers this "take it or leave it" program, we omit the loans from this program from our analysis.

Tables 1 and 2 provide summary statistics for our primary-lending dataset. Table 1 compares loan and borrower characteristics for the treatment and control groups, while Table 2 breaks down each group to show characteristics for loans issued before and after *Madden*. Term represents the loan's duration and is expressed in months. Debt-to-Income reflects the borrower's total monthly debt payments, excluding the requested loan and any mortgage payments, divided by the borrower's monthly income and is expressed in percentage terms. Delinquencies reflects the number of recent delinquencies in the borrower's credit file. Available Credit reflects the borrower's total revolving credit balance. Employment represents the number of years the borrower has been employed at her current position. FICO Score reflects the midpoint of the borrower's four-point FICO range. All values are presented at the mean.

The data in Table 2 suggest that borrower quality increased post *Madden* in Connecticut and New York but not outside the Second Circuit. For example, average borrower annual income rose significantly in Connecticut and New York but not elsewhere. We also see a much larger increase in average FICO scores in Connecticut and New York than in either of the control groups in the table.

Tables 3 to 5 present descriptive statistics for our secondary-market dataset. Two of the marketplace platforms in our sample not only initiate loans directly but also allow investors to trade notes based on those loans—or an increment thereof—on a secondary-market trading platform. Our secondary-market dataset contains data provided by these two platforms and includes more than 1.3 million trades, in sizes ranging from $25 to $12,000. Each note traded is backed by a single loan (only loans originated through that specific platform may be traded).[24]

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[24] Although some marketplace lenders sell notes based on bundled loans, we analyze only the trading of notes backed by individual loans. The investors in these notes, which primarily are institutions such as hedge funds, are able to identify the underlying borrower's state of residence.

Approximately 93% of the trades in this dataset are for notes backed by current loans; the other 7% are for notes backed by non-current loans.

Table 3 compares our treatment group with the non-Second Circuit control group, Table 4 compares the treatment group with the no-usury control group, and Table 5 compares the treatment group with the PSM sample. Because the change in law may have disparate effects on notes backed by non-current and current loans, we analyze each population separately. In Tables 3-5, Panel A of each table considers notes backed by non-current loans and panel B considers notes backed by current loans. We create the PSM samples by estimating the probability that the note traded will be based on a loan made to a borrower in New York or Connecticut, where the prediction model includes the variables included in Tables 3 to 5. As noted, we match the observations using nearest-neighbor matching without replacement. Principal Outstanding reflects the outstanding principal on the note at the time of the trade. Loan Amount is the total value of the loan underlying each note. Ask Price reflects the amount the purchaser paid for the note. Loan Age reflects the number of months between the loan's issue date and the trading date. Fifteen is a dummy variable reflecting whether the loan underlying the note was issued within fifteen months of the trading date. All other variables are as defined previously. As before, all values are presented at the mean.

4.   **Empirical Results**

This section presents our empirical results. As described below, we separately analyze *Madden*'s impact on lenders and on borrowers. We find evidence that debtholders are aware of the decision, and that they respond to the legal limbo in two ways. First, by analyzing secondary-market trading, we see that investors discount notes backed by above-usury loans to borrowers in Connecticut and New York. Second, we show that lenders reduced the flow of credit for the higher-

risk Connecticut and New York borrowers most likely to have loans above usury caps. However, we find no evidence that the decision induced borrowers to default strategically.

A.   Secondary-Market Trading

We begin with our analysis of whether *Madden* affected secondary-market trading of notes backed by marketplace loans to Connecticut and New York borrowers. As noted previously, notes traded on secondary markets can be backed either by non-current loans, where the borrower is late on her payments but has not yet defaulted, or by current loans, where the borrower is current on her payments. We expect that the effect of *Madden* will be most prominent for notes backed by non-current loans, where the risk of nonpayment is especially high. Using the trading data we collected, we calculate the discount that investors apply to each note based upon the difference between the price paid for the note and the value of the underlying loans if paid in full. Following investors in this field, we refer to that difference as the spread.[25] After controlling for other relevant variables, higher spreads indicate greater discounts, as higher values reflect the market's perception that the projected payout is insufficient to compensate for the time value of money plus the perceived nonpayment risk.

Because of the risk that the underlying loans may be uncollectible in Connecticut and New York after *Madden*, we expect that the spread on notes backed by above-usury loans increased after the decision. Table 6 presents the results of a series of triple difference regressions testing

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[25] We calculate the spread as yield to maturity minus the loan's interest rate. The yield to maturity is calculated based on the investor's purchase price; that is, yield to maturity reflects the yield that will be earned if the note is paid in full. For example, if the amount an investor paid for a note would yield a return of 10.30% if the note was repaid in full, and the interest rate on the underling loan was 12%, then the spread would be -1.70%. The spread on current loans is usually negative, reflecting that the investor expects to receive greater dollar value over the life of the loan than she is willing to pay for that loan today. By contrast, the spread on non-current loans is usually positive; the investors demand very high yield to maturity rates because they know that the loans are likely to default. For example, an investor might require a note backed by a non-current loan bearing an interest rate of 12% to have a yield of 20% (if paid in full). The spread in such an instance would be 8%, reflecting the high discount applied to the loan.

this hypothesis. Panel A presents results for notes backed by non-current loans, while Panel B presents results for notes backed by current loans. The variable of interest is Above16*Post-*Madden**NY_CT, which represents the interaction between Above16 (an indicator for whether the underlying loan has an interest rate above 16%, the civil usury cap in New York),[26] Post-*Madden* (an indicator for whether the trade occurred after *Madden*), and NY_CT (an indicator for whether the borrower resides in Connecticut or New York). Each panel has three columns, reflecting our three control groups. All models control for principal outstanding on the note traded, full loan amount, loan age, ask price (the price at which trades occurred), loan duration, loan interest rate, borrower FICO score, and whether the loan underlying the note was issued within the fifteen months prior to the trade date. Because the ratio of current loans to non-current loans traded varies over our sample period—and across lending platform—we also control for the daily ratio of current to non-current loans traded on the platform in question. Fixed effects are included for the grade the lending platform originally assigned the loan, and standard errors are clustered by the borrower's state of residence.

The results in Table 6 provide evidence that *Madden* reduced the price of notes backed by above-usury loans to borrowers in Connecticut and New York. Panel A analyzes notes backed by non-current loans and shows that spreads on notes backed by loans to Connecticut and New York borrowers were higher than expected following *Madden*. (One model is not statistically significant, but the other two are significant at the 5% level.) In terms of economic magnitude, the coefficient on the triple-interaction term in column (1) is 0.387, and the Stata margins command suggests that,

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[26] As noted earlier, usury rates vary significantly across the US and some states lack usury caps entirely. Thus, to make our treatment and control groups as comparable as possible, we define our Above16 dummy variable based on the civil usury rate in New York rather than assigning the variable differently in each state. The tests use the civil cap for New York rather than Connecticut, which is 12%, because the number of loans in our dataset to borrowers in New York dwarfs that to borrowers in Connecticut.

at the mean, the spread for above-usury notes in the Second Circuit post-*Madden* is approximately 0.25 higher than expected. To put this result in perspective, the mean (median) spread for notes backed by non-current loans in our sample is 2.35 (1.29), and the standard deviation is 3.54. Column (3) uses the PSM control sample presented in Table 5 and shows a similar result.

Panel B in Table 6 analyzes notes backed by current loans. Although it also shows that spreads increased post-*Madden* on notes backed by above-usury debt owed by Connecticut and New York borrowers, the magnitude of the increase is much smaller. The variable of interest is significant at 5% across the three models, but the economic magnitude of the increase is virtually zero. The smaller discount has a clear explanation, as current loans present lower risks of nonpayment than non-current loans. Accordingly, the mean (median) spread on notes backed by current loans is -0.018 (-0.0158). Nonetheless, the economic magnitude of roughly zero suggests that lenders expect borrowers who are making their payments on time to continue to do so despite the *Madden* decision. In other words, investors do not expect *Madden* to trigger widespread strategic defaults.

B.   Credit Availability for Riskier Borrowers

We next assess whether *Madden* reduced credit availability for borrowers in Connecticut and New York. We find clear evidence that it did; *Madden* reduced the flow of credit, especially to higher-risk borrowers whom lenders normally charge above-usury rates. Lenders made relatively fewer loans to higher-risk borrowers in the affected states, and the loans they did make were smaller. Because of the nature of the question, many of our results in this section are expressed visually in figures rather than regression analysis.

i.   *Madden*'s Effect on Loan Volume

We begin by examining changes in loan volume post-*Madden*. At a descriptive level, there

is clear evidence that fewer above-usury loans were issued in Connecticut and New York after the decision. In those states, the number of loans issued at rates above New York's civil usury cap of 16% increased 65% (from 7,537 to 12,425). By contrast, new loans at such rates outside the Second Circuit increased 125% (from 124,340 to 280,313). This slower growth in Connecticut and New York is highly statistically significant (t=-20.96). By contrast, no significant difference is seen for loans at rates of 16% or less. The volume of new loans at these lower rates increased 97% (from 16,683 to 32,937) in Connecticut and New York; outside the Second Circuit such loans grew 95% (from 158,288 to 308,855). These growth rates do not differ at statistically significant levels (t=1.18). These results are presented visually in Figures 2 and 3 in histograms that show the distribution of new loans at various interest rates before and after *Madden*. All histograms use a bin width of two percentage points. Although it is clear that lending at rates above 16% increased after *Madden* outside the Second Circuit, growth in Connecticut and New York seemed stunted.

ii.   *Madden***'s Effect on Marketplace Borrower Credit Quality**

There are two possible reasons why lenders made relatively fewer higher-interest loans in Connecticut and New York after *Madden.* One is that they curtailed lending to higher-risk borrowers; the other is that they charged less interest, holding borrower quality constant. To distinguish between these possibilities, Table 7 presents results of difference-in-differences regressions examining the relative change in credit quality, as measured by FICO score, for borrowers in Connecticut and New York after *Madden*. The table shows that average credit scores in Connecticut and New York rose significantly after *Madden* relative to either of the control groups.[27] (This finding is consistent with the descriptive statistics in Table 2.) Average FICO

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[27] We do not include a PSM sample in this analysis because we are attempting to capture the differences in new loan originations after *Madden*. Creating a matched sample would obfuscate these differences by forcing us to match only similar loans— thus dropping the unpaired, dissimilar loans. The matching procedure would therefore eliminate the

scores for Connecticut and New York borrowers increased roughly 2.6 to 3.0 FICO points more than expected based on the trend for borrowers outside the Second Circuit generally and in no-usury states specifically. All models in Table 7 control for the loan's interest rate, amount, and term, as well as the borrower's annual income, debt-to-income ratio, number of recent delinquencies, total credit availability, and years of employment at her current position (all variables are defined in Table 1). As before, we include fixed effects for each lending platform, and standard errors are clustered by the borrower's state of residence.

To further investigate this increase in FICO scores in Connecticut and New York, we assign borrowers to buckets based on FICO score and examine the growth in loan volume by bucket. The results, presented in Figure 4, indicate that the FICO increase was caused by a decline in lending to lower-quality borrowers. A value of 100% in the figure would reflect that twice as many loans were issued after *Madden* as before. The pre-*Madden* period runs from the beginning of 2015 to May 22, 2015, and the post-*Madden* period runs from May 23 to the end of 2015. The figure indicates that, outside the Second Circuit, loan volume to borrowers in all FICO buckets increased substantially after *Madden*. However, although growth rates for loans issued to borrowers in Connecticut and New York are roughly comparable to growth rates outside the Second Circuit for higher-quality borrowers, growth in new loans was dampened—or even declined—for lower-quality borrowers. The pattern is most obvious for the lowest-quality borrowers—those with FICO scores below 625. The growth rate for these borrowers in Connecticut and New York was negative 52%—meaning that, in absolute numbers, loan volume to these borrowers *declined* after *Madden*.

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relative differences that we intend to capture. For example, a low-FICO score borrower from outside the Second Circuit would likely not have a match in Connecticut or New York because the low-FICO score borrowers in these states disappeared.

Outside the Second Circuit, loan volume for these borrowers after *Madden* grew by 124% (that is, loan volume in absolute numbers more than doubled).

We show this pattern in more detail in Figures 5 and 6, where we plot the distribution of new loans by FICO score before and after *Madden*. All histograms in these figures use a bin width of four FICO points. Figure 5 includes all non-Second Circuit borrowers and shows a post-*Madden* increase in new loans to borrowers with FICO scores below 670. This is consistent with anecdotal evidence that marketplace lending to these borrowers grew during this period. Figure 6, which includes only borrowers in Connecticut and New York, shows a different trend. Loans to riskier borrowers appear to decline, and loans to borrowers with FICO scores below 644 virtually disappeared.

Figure 7 zooms in on the lowest-quality borrowers in our sample, showing the number of new loans issued in 2015 to borrowers in Connecticut and New York with FICO scores below 640. As the figure indicates, there was only one new loan to such borrowers in Connecticut and New York in July 2015, and none thereafter. By contrast, loan originations to such borrowers outside the Second Circuit were roughly 50% greater in the second half of 2015 than in the first half.

These findings suggest that the drop in new above-usury loans in Connecticut and New York post-*Madden* was the result of reduced lending to higher-risk borrowers rather than a drop in the quality-adjusted interest rates charged by lenders. However, to confirm this intuition, we test for evidence that pricing changed using a difference-in-differences model in which the dependent variable is the interest rate. Despite our use of various specifications—the models use a variety of control variables to capture borrower quality and test for differences in rates relative to other states and relative to loans previously issued in New York and Connecticut—we are unable to find any

evidence that quality-adjusted rates decreased in New York and Connecticut. (We omit the tables for concision.)

The finding that usury laws decrease credit availability is consistent with much prior work (e.g., Goudzwaard 1968; Shay 1970; Greer 1974; Rigbi 2013; Melzer and Schroeder 2017). However, most of these earlier studies rely on associations, whereas we show the effects of usury laws in a more tightly identified setting. As a caveat, we note that our findings do not establish that these higher-risk borrowers were unable to borrow altogether. Because we look only at loans issued through marketplace-lending platforms, we cannot rule out the possibility that these borrowers substituted into other sources of credit, including those, such as credit cards, that typically charge higher interest.

iii.   Changes in loan size

Credit availability is affected by the availability of new loans and by the terms of available loans (e.g., Ghosh, Mookherjee and Ray 1999; Stiglitz and Weiss 1981; Melzer and Schroeder 2017). Although most marketplace-lending platforms use standardized loan terms—for example, loans must be unsecured and have terms of either 36 or 60 months—loan size can range from $1000 to $35,000. It is therefore possible that *Madden* affected loan size in our sample.

Table 8 presents the results of difference-in-differences regressions testing this possibility. The dependent variable is the natural log of loan size, and the table indicates that average loan size fell roughly $400 more than expected in Connecticut and New York following *Madden*, with the greatest decreases for lower-quality borrowers. This result does not appear in the basic descriptive statistics, as it is driven by the inclusion of control variables. As before, we present results for tests using our non-Second Circuit control group (Panel A) and no-usury group (Panel B). The first column in each panel shows results for the full set of borrowers, the second for the subset of

borrowers with FICO scores below 750, and the third for the subset of borrowers with FICO scores below 700. All regressions control for the same variables as in Table 7. As before, fixed effects are included for each marketplace lending platform, and standard errors are clustered by the borrower's state of residence. The interaction term is statistically significant at 1% across all models, and the change in loan size decreases monotonically with FICO scores. This result suggests that *Madden* not only constrained credit availability by reducing loan volume, but also by reducing loan size.

In sum, we find evidence that debtholders were aware of the *Madden* decision and responded to the change in legal enforceability. First, our analysis of secondary market trading shows that investors priced the additional risk created by *Madden*—particularly when the borrower underlying the note was late on her payments. Second, we find that lenders limited credit availability in response to the decision. Loan volume decreased for those higher-risk borrowers more likely to borrow above usury rates, and even those borrowers who received loans received smaller loans than would be expected.

C.   Strategic Default

We next consider the hypothesis that *Madden* changed borrower behavior within the Second Circuit by giving borrowers an incentive to default on above-usury loans. To test for strategic default, we create a dummy variable, Delinquent, and assign it a value for each month after a loan was issued. The value is 0 until the borrower misses a payment, at which point it is 1 for that and all subsequent months.[28]

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[28] Due to data limitations, we can only determine whether a borrower missed a payment if the missing payment was not remedied by the time we received the data in January 2016. If a borrower missed a payment but remedied the delinquency before we obtained our dataset, there will be no record of that missed payment. This data limitation affects all borrowers equally, and we have no reason to believe that it biases the interaction term in our difference-in-differences regressions. However, it does bias the coefficient on the *Post* variable.

Table 9 provides the results of triple-difference regressions used to test for strategic default. The dependent variable, Delinquent, is given a monthly value of 0 until a borrower misses a payment; it then becomes 1 in that and all subsequent months. As in Table 6, the variable of interest is Above16*Post*NY_CT, which represents the triple interaction between Above16, Post-*Madden*, and NY_CT. Because we have repeat observations for the same loan, all standard errors are clustered by loan. All models include the control variables and fixed effects noted in Table 7, as well as platform fixed effects. All control variables are based on borrower and loan information at the time a borrower applied for a loan and do not update throughout the loan period.

Table 9 offers no evidence that borrowers engaged in strategic default after *Madden*; the coefficients on the variable of interest—the triple interaction term—are not significantly different from zero in any of the models. Panel A shows results from tests in which we keep delinquent borrowers in the sample in months after they miss a payment. Thus, if a borrower misses a payment in September 2015, she will also show up, with a Delinquent score of 1, in October through December. Panel B shows results in which we remove borrowers from the data after they first miss a payment. All models are Cox proportional hazard models.

In a series of unreported robustness tests, we conduct further analysis and are unable to find consistent evidence of strategic delinquencies. In particular, we look for greater rates of delinquency (1) among more sophisticated borrowers, who presumably are more likely to be aware of the decision, (2) in ZIP codes with particular demographics, (3) in geographic clusters (i.e., we test whether people are more likely to default if their neighbors do), (4) only for the subset of loans

issued before *Madden*, (5) using OLS, probit and logit, and (6) for loans above 25%, New York's criminal usury cap.[29]

In each of these robustness tests, default as a whole remains low, and we find no consistent evidence that borrowers strategically default after *Madden*. Among the models we ran for robustness, only one—an OLS model limited to borrowers with FICO scores below 700—indicated a statistically significant increase in default rates. But the result was significant at only the 10% level and was not robust to alternate specifications such as different clustering and/or control samples. We thus lack confidence that the finding is not a statistical fluke. The lack of evidence of strategic default suggests that one or more of the factors we identified earlier—lack of knowledge of the decision, uncertainty about its implications, moral compunction, or concerns with reputation risk—were important enough to prevent borrowers from defaulting despite the apparent financial incentive *Madden* gave them to do so.

D.   Loss Given Default

It may seem puzzling that investors reduced credit availability even though borrowers do not appear to strategically default. One possible explanation is that investors were hesitant to enter this market because loss given default increased even if the frequency of defaults did not. A borrower who is aware of the ruling may not strategically default, but she may take advantage of

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[29] As a matter of New York law, the civil usury cap does "not apply to defaulted obligations." *Manfra, Tordella & Brookes, Inc. v. Bunge*, 794 F.2d 61, 63 n.2 (2d Cir. 1986). There has long been legal uncertainty, however, with respect to whether New York's *criminal* usury cap applies to defaulted loans, and the Second Circuit did not address that question in *Madden*. After the Supreme Court declined to review the Second Circuit's decision, the trial court considered that question on remand, concluding that "New York's criminal usury cap applies to prevent a creditor from collecting interest about 25% on a defaulted debt." *Madden v. Midland Funding LLC*, 237 F. Supp. 3d 130, 142 (S.D.N.Y. 2017) (citing several New York cases to this effect, *e.g.*, *815 Park Ave. Owners Corp. v. Lapidus*, 227 A.D. 2d 353 (N.Y. App. Div. 1996)). Thus, a borrower who is charged interest above the civil usury cap of 16% but below the criminal usury cap of 25% may choose not to default in order to avoid losing the protections of New York's civil usury cap. By contrast, a borrower who is charged interest above the *criminal* usury cap, under the trial court's view, remains protected by New York's criminal usury law even if she chooses to default on her obligations. Thus, we test for strategic default separately with respect to loans above New York's criminal usury cap, but our results are unchanged.

the decision if she defaults for other reasons. And a debt-collector tasked with enforcing a contract is likely to be hesitant to push too hard—if he takes the borrower to court and loses, he will have set damaging precedent. Unfortunately, we do not have data on loss given default from the marketplace lenders. However, because of the importance of this possible outcome, we contacted the CFPB and requested that their economists analyze whether there was a change in loss given default post-*Madden*.

Using the CFPB's Consumer Credit Panel, one of their economists found that loss given default increased for lower quality borrowers in New York and Connecticut post-*Madden*. Their analysis includes all defaulted accounts that were active in the period from Dec. 2014 through Dec. 2016, and loss given default is defined as how much of a consumer's outstanding balance at default is eventually repaid (this variable is based on the change in balance post charge-off). The analysis excludes all cases where the debtor and debtholder settled privately because the data do not include detail on the amount of any such settlements, and it only include cases where the borrower repaid a non-zero amount of her debt.

The CFPB researcher first ran a difference-in-differences model using the full sample and found, perhaps counterintuitively, that collections upon default increased in New York and Connecticut post-*Madden*. However, further analysis shows that this result flips for lower-quality borrowers more likely to borrow above usury rates. In particular, although borrowers in New York and Connecticut pay roughly $233 more upon default than would be expected post-*Madden*, borrowers with FICO scores below 660 pay roughly $92 less than expected and borrowers with FICO scores below 600 pay roughly $172 less than expected. The CFPB models control for the borrower's credit score, credit limit, year of birth, balance at default, and census tract demographics (the demographics include controls for the tract's median income as well as the

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percentage of blacks, Hispanics, and high-school dropouts). Fixed effects are included for the

borrower's state of residence and the month of the transaction, and linear state-specific monthly

trends are also included.

Although interesting, there are two caveats to this analysis. First, the CFPB data used here

are noisier than our marketplace lending data because the CFPB data are limited to credit cards.

Therefore, some of the debt is still held by national banks (and thus unaffected by *Madden*) and

some of the debt is held by nonbanks (and thus affected by *Madden*). Second, relative to the entire

universe of transactions, the number of consumers who default and repay during the sample period

is limited. Hence, the sample size is relatively small. However, both of these caveats should bias

against finding a result.[30]

E.   Robustness

For a difference-in-differences analysis to produce a valid estimate of the treatment effect,

the treatment and control samples need not be identical, but the difference between the groups

should be consistent but for the shock examined. Hence, in this section we report the results of

parallel trends analyses. We show monthly trends for each of the significant results presented in

our main regressions: discounts on secondary-market trading, FICO scores, and loan size.

i.   Secondary-Market Trading

Figure 8 presents parallel trends analyses corresponding to our regressions analyzing

Madden's impact on the trading price of notes backed by current and non-current loans. Panel A

shows the results for non-current loans, and Panel B shows the results for current loans. The figures

in each panel plot the trend lines for two regressions, one using borrowers from Connecticut and

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[30] As outside researchers, we were unable to access the CFPB data and therefore did not derive this analysis ourselves. We are deeply grateful to Ryan Sandler for volunteering his time and expertise to help us conduct this analysis.

New York, and the second using borrowers outside the Second Circuit. The regressions are the same as those used in Panels A and B of Table 6, except that NY_CT, Post-*Madden*, and the triple interaction term are replaced with monthly indicators reflecting the month in which the trade occurred (the indicator for January is omitted due to collinearity). The figure plots the coefficients on the interactions between Above16 and each monthly indicator.

Interestingly, Panel A indicates that it took several months for the full effect of *Madden* to materialize. Although the pre-*Madden* spread on notes backed by non-current loans in Connecticut and New York was slightly higher than the spread on notes backed by non-current loans outside the Second Circuit, the deviation between these lines widened significantly starting only in September. We do not see a similar trend in Panel B for notes backed by current loans. However, the lack of a visual trend in Panel B is not surprising given Table 6's finding that the economic magnitude of the discount applied to above-usury loans made to borrowers in New York and Connecticut post-*Madden* is very close to zero.

ii.   Borrower Quality

Figure 9 presents the parallel trends analysis for the regression analyzing *Madden's* effect on FICO scores. The regression specification is the same as in Table 7, except we replace the prior variables of interest—NY_CT, Post-*Madden*, and the resulting interaction term—with monthly indicator variables reflecting the month in which the loan was issued. As before, the first line presents coefficients on monthly indicators from a regression using borrowers from Connecticut and New York, and the second presents coefficients for a regression using borrowers from outside the Second Circuit. The figure plots the coefficients on the monthly indicators. Although FICO scores for Connecticut and New York borrowers were higher than for those outside the Second Circuit throughout the year, the difference is roughly constant until September, when it widens

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significantly. This result is consistent with Figure 8, and with anecdotal evidence, both of which indicate that it took several months for *Madden* to have its full impact on markets.

iii.   Loan Size

Figure 10, which presents an analysis of *Madden*'s effect on the natural log of loan size, shows a similar trend. Panel A shows results for the full set of borrowers, while Panel B includes only the subset of borrowers with FICO scores below 700. The regression specification is the same as in Table 8, except we replace the prior variables of interest with monthly indicator variables. As before, the indicators reflect the month in which the loan was issued; the first regression uses only loans to borrowers in Connecticut and New York, and the second uses only loans to borrowers outside the Second Circuit. Interestingly, the figure suggests that relative loan size in Connecticut and New York fell as early as June, suggesting that lenders initially responded to *Madden* by making smaller loans and only later reduced loan volume.

The trends analyses highlight an important question: why were *any* loans issued at interest rates above 16% in Connecticut and New York after *Madden*? There are several possible explanations, but the trends analyses corroborate anecdotal evidence we heard from practitioners that it took several months to respond to the decision. Some market participants reported that they were not aware of the decision until weeks or even months after it was issued. Moreover, even after lenders and investors learned of the decision, it was such a surprise that they and their counsel needed time to modify their business practices.

Legal uncertainty also may help explain continued lending at above-usury rates after *Madden.* As we have noted, it remained possible through the end of our sample period that the Supreme Court would ultimately reverse the decision or that the defendant debtholder would prevail on other theories of enforceability. Lenders presumably were heterogeneous in the

probabilities they assigned to these possible outcomes; those who assigned high probabilities might have felt that the potential returns from lending above 16% continued to justify the risks.[31]

5.   Conclusion

Using proprietary data from three marketplace-lending platforms, we study the impact of an unexpected judicial decision that introduced significant uncertainty about the legal enforceability of a large volume of outstanding consumer loans. The decision applies in three states, but we focus on two of those states—Connecticut and New York—because the law of those states declares usurious loans void. Because the case has a limited geographic reach, we use a difference-in-differences design. We find clear evidence that the decision changed the behavior of lenders. Secondary-market trading data indicate that debtholders adjusted to increased legal risk by paying less for notes backed by above-usury loans to borrowers in Connecticut and New York. Lenders also restricted credit availability—measured by both loan size and volume—after the decision, with the largest impact being on higher-risk borrowers. Despite that lenders modified their behavior, our evidence suggests that they did not expect widespread consumer default—an expectation borne out by our analysis of borrower behavior directly. Taken together, our results shed light on the effect of legal enforceability on consumer lending.

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[31] A final consideration is that some of the platforms made innovative legal changes that they hoped would neutralize *Madden*. For example, in February 2016, the only public marketplace lender, Lending Club, arranged for its originating bank to hold onto a small fraction of platform-arranged loans in order to permit Lending Club to argue that the *Madden* holding does not apply because its loans are not entirely in the hands of nonbank investors (Demos and Rudegeair, 2016). Prosper Funding LLC, the second largest marketplace lender, made a similar change soon thereafter. Some investors may have been willing to continue lending at above-usury rates because they believed that such changes had a good chance of protecting them.

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Table 1
Descriptive Statistics: Loan and Borrower Characteristics

	Panel A: Outside the Second Circuit			Panel B: No Usury States			Panel C: PSM		
	NY & CT	Outside the 2nd Circuit	t-test	NY & CT	No Usury States	t-test	NY & CT	PSM	t-test
Loan Amount ($)	14,206	12,598	-49.10	14,206	12,695	-33.13	13,934	14,052	-4.98
Term (Months)	43.26	43.65	8.82	43.26	43.88	10.30	42.94	43.36	-12.83
Interest Rate	13.80%	18.58%	123.73	13.80%	18.56%	109.66	12.94%	13.00%	-3.75
Annual Income ($)	77,714	65,821	-14.32	77,714	65,694	-28.12	78,463	74,104	20.03
Debt-to-Income	19.39%	24.65%	-45.52	19.39%	25.36%	-45.40	19.70	21.33	-53.68
Delinquencies	0.31	0.25	-20.12	0.31	0.24	-14.37	0.36	0.35	2.88
Available Credit ($)	19,138	14,894	-44.13	19,138	15,345	-24.29	18,103	17,000	13.95
Employment (Years)	7.11	5.32	-69.39	7.11	5.38	-48.15	7.03	6.93	5.52
FICO Score	696.22	682.82	-87.60	696.22	682.92	-67.41	695.48	694.64	8.82
Num. Obs.	*66,437*	*841,446*		*66,437*	*63,942*		*57,654*	*57,654*	

Note. Using our primary-lending dataset, this table presents characteristics of the loans and borrowers in our treatment and control groups. Panel A compares loans to borrowers in Connecticut (CT) and New York (NY), our treatment group, with loans to all borrowers located outside the Second Circuit. Panel B compares loans in our treatment group with loans to borrowers in states lacking usury caps. Panel C compares loans in our treatment group to our propensity score matched (PSM) sample used in the delinquency analysis in Table 9. All values are presented at the mean.

Table 2
Descriptive Statistics: Loan and Borrower Characteristics Before and After *Madden*

	Panel A: Connecticut & New York			Panel B: Outside the Second Circuit			Panel C: No Usury States		
	Before *Madden*	After *Madden*	t-score	Before *Madden*	After *Madden*	t-score	Before *Madden*	After *Madden*	t-score
Loan Amount ($)	13,983	14,325	5.08	12,529	12,631	5.37	12,472	12,809	4.92
Term (Months)	43.55	43.11	-4.97	43.76	43.60	-6.40	44.03	43.81	-2.40
Interest Rate	14.38%	13.49%	-19.89	18.53%	18.60%	2.79	18.82%	18.43%	-4.81
Annual Income ($)	75,510	78,891	4.82	66,144	65,666	-0.96	65,229	65,932	1.27
Debt-to-Income	18.19%	20.03%	20.11	24.55%	24.70%	3.08	25.61%	25.23%	-3.04
Delinquencies	0.307	0.314	0.98	0.26	0.24	-10.09	0.25	0.24	-1.90
Available Credit ($)	18,338	19,566	4.92	14,738	14,969	4.27	14,725	15,663	4.49
Employment (Years)	6.50	7.44	17.70	5.25	5.36	7.03	5.12	5.52	7.52
FICO Score	693.57	697.64	15.37	682.76	682.85	1.03	681.81	683.49	5.21
Num. Obs.	*24,220*	*45,362*		*282,628*	*589,168*		*22,467*	*43,811*	

Note. Using our primary-lending dataset, this table compares loans issued before and after *Madden*. Panel A reflects loans to borrowers in Connecticut and New York. Panel B reflects loans to all borrowers located outside of the Second Circuit. Panel C reflects loans to borrowers located in states without usury limits. All values are presented at the mean.

Table 3
Descriptive Statistics: Notes Underlying Secondary-Market Trades - Outside the Second Circuit

Panel A: Notes Backed by Non-Current Loans				Panel B: Notes Backed by Current Loans			
	CT & NY	Outside the 2nd Circuit	t-score		CT & NY	Outside the 2nd Circuit	t-score
Principal Outstanding ($)	30.73	31.15	0.53	Principal Outstanding ($)	33.23	33.62	1.54
Loan Amount ($)	20,169	20,506	3.60	Loan Amount ($)	19,736	20,008	10.00
FICO Score	690	689	-0.14	FICO Score	695	694	-6.03
Ask Price ($)	13.53	13.76	0.32	Ask Price ($)	33.60	34.00	1.56
Term (Months)	50.06	50.68	5.16	Term (Months)	47.93	48.43	14.38
Loan Age (Months)	16.94	16.28	-6.30	Loan Age (Months)	14.24	13.75	-16.69
Interest Rate	19%	19%	0.84	Interest Rate	17%	17%	-7.59
Fifteen	0.51	0.48	-4.99	Fifteen	0.41	0.40	-10.87
Num. Obs.	*10,543*	*84,675*		*Num. Obs.*	*130,092*	*1,226,167*	

Note. Using our secondary-market dataset, this table presents descriptive statistics for notes traded on the exchanges run by the marketplace platforms in our sample. The table compares our treatment group (notes based on loans in Connecticut (CT) and New York (NY)) with our main control group (notes based on loans outside the Second Circuit). The notes are divided based on whether they are backed by loans to borrowers who are no longer current on their payments or by loans to borrowers who are current on their payments. All values are presented at the mean.

Table 4
Descriptive Statistics: Notes Underlying Secondary-Market Trades – No Usury States

Panel A: Notes Backed by Non-Current Loans				Panel B: Notes Backed by Current Loans			
	CT & NY	No Usury	*t-score*		CT & NY	No Usury	*t-score*
Principal Outstanding ($)	30.73	31.09	*-0.39*	Principal Outstanding ($)	33.23	34.49	*-3.57*
Loan Amount ($)	20,169	20,795	*-4.65*	Loan Amount ($)	19,736	20,406	*-17.35*
FICO Score	690	689	*0.97*	FICO Score	695	693	*13.15*
Ask Price ($)	13.53	13.08	*0.40*	Ask Price ($)	33.60	34.90	*-3.56*
Term (Months)	50.06	50.88	*-4.41*	Term (Months)	47.93	48.70	*-15.21*
Loan Age (Months)	16.94	16.57	*2.39*	Loan Age (Months)	14.24	13.58	*14.89*
Interest Rate	19%	19%	*-1.24*	Interest Rate	17%	17%	*-1.85*
Fifteen	0.51	0.48	*3.15*	Fifteen	0.41	0.40	*11.14*
Num. Obs.	*10,543*	*7,246*		*Num. Obs.*	*130,092*	*94,440*	

Note. Using our secondary-market dataset, this table presents descriptive statistics for notes traded on the exchanges run by the marketplace platforms in our sample. The table compares our treatment group (notes based on loans in Connecticut (CT) and New York (NY)) with our no usury control group (notes based on loans in states lacking usury caps). The notes are divided based on whether they are backed by loans to borrowers who are no longer current on their payments or by loans to borrowers who are current on their payments. All values are presented at the mean.

Table 5
Descriptive Statistics: Notes Underlying Secondary-Market Trades – PSM Sample

Panel A: Notes Backed by Non-Current Loans				Panel B: Notes Backed by Current Loans			
	CT & NY	PSM Sample	t-score		CT & NY	PSM Sample	t-score
Principal Outstanding ($)	30.73	31.01	-0.29	Principal Outstanding ($)	33.23	33.42	-0.58
Loan Amount ($)	20,169	20,008	1.29	Loan Amount ($)	19,558	19,513	1.26
FICO Score	690	690	-1.42	FICO Score	695	695	-3.97
Ask Price ($)	13.53	13.84	-0.36	Ask Price ($)	33.60	33.78	-0.55
Term (Months)	50.06	50.21	0.94	Term (Months)	47.93	48.41	10.25
Loan Age (Months)	16.94	17.01	-0.48	Loan Age (Months)	14.24	14.30	-1.43
Interest Rate	19%	18%	3.15	Interest Rate	17%	17%	6.00
Fifteen	0.51	0.51	0.19	Fifteen	0.41	0.41	-0.23
Num. Obs.	*10,543*	*10,543*		*Num. Obs.*	*124,000*	*124,000*	

Note. Using our secondary-market dataset, this table presents descriptive statistics for notes traded on the exchanges run by the marketplace platforms in our sample. The table compares the treatment group (notes based on loans in Connecticut (CT) and New York (NY)) with our propensity score matched (PSM) sample. The notes are divided based on whether they are backed by loans to borrowers who are no longer current on their payments or by loans to borrowers who are current on their payments. All values are presented at the mean.

Table 6
Triple Difference Results: Change in Secondary-Market Trading Prices Post-*Madden*

	Panel A: Notes based on Non-Current Loans			Panel B: Notes based on Current Loans		
	Outside the 2nd Circuit	No Usury States	PSM Sample	Outside the 2nd Circuit	No Usury States	PSM Sample
	(1)	(2)	(3)	(1)	(2)	(3)
Post-*Madden*	-0.0213	-0.206	0.0444	0.002**	0.002**	0.0025**
	(0.0726)	(0.147)	(0.127)	(0.000)	(0.0004)	(0.0002)
NY_CT	0.0841	-0.285	0.139	0.001**	0.0007	0.0005
	(0.154)	(0.191)	(0.172)	(0.0002)	(0.001)	(0.000)
Above16	-0.140	-0.536*	0.0107	-0.001**	0.001	0.000110
	(0.0863)	(0.226)	(0.143)	(0.000)	(0.001)	(0.001)
Post*NY_CT	-0.158	0.0749	-0.169	-0.0004*	-0.001	-0.0004
	(0.262)	(0.278)	(0.272)	(0.0002)	(0.0004)	(0.0003)
Above16*Post	-0.0806	0.264	-0.147	0.0029**	0.002*	0.0012*
	(0.078)	(0.194)	(0.130)	(0.000)	(0.001)	(0.0005)
Above16*NY_CT	-0.185	0.356	-0.180	-0.000	-0.001+	-0.0008+
	(0.112)	(0.212)	(0.138)	(0.000)	(0.001)	(0.0005)
Above16*Post*NY_CT	0.387*	0.0163	0.433*	0.0006*	0.0018*	0.001*
	(0.181)	(0.236)	(0.202)	(0.000)	(0.001)	(0.0004)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan Grade FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	95,218	17,633	21,086	1,356,259	221,922	248,000
R-squared	0.060	0.058	0.064	0.110	0.126	0.127

Note. Results are from estimating $Spread = \alpha + \beta_1 Post\text{-}Madden + \beta_2 NY\_CT + \beta_3 Above16 + \beta_4 Post*NY\_CT + \beta_5 Above16*Post + \beta_6 Above16*NY\_CT + \beta_7 Above16*Post*NY\_CT + Controls + \varepsilon$. The dependent variable is note spread, defined as yield to maturity based on the note's trading price minus the underlying loan's interest rate. Panel A uses only notes backed by non-current loans, and Panel B uses only notes backed by current loans. In each panel, column (1) uses all borrowers outside the Second Circuit as the control group, column (2) uses borrowers in states lacking usury caps, and column (3) uses the propensity score matched (PSM) control group. All specifications include loan grade fixed effects (Loan Grade FE) and controls. Standard errors, in parentheses, are clustered by the borrower's state of residence.

$^+ p < .10.$

$^* p < .05.$

$^{**} p < .01.$

Table 7
Difference-in-Differences Results: Change in Borrower FICO Scores Post-*Madden*

	Outside the 2nd Circuit (1)	No Usury States (2)
Post-*Madden*	-0.785**	-0.287
	(0.221)	(0.540)
NY_CT	-0.254	0.195
	(0.405)	(0.733)
Post*NY_CT	3.040**	2.627**
	(0.252)	(0.574)
Controls	Yes	Yes
Lender FE	Yes	Yes
Observations	907,883	130,379
R-squared	0.520	0.457

Note. Results are from estimating *FICO Score* = α + β₁*Post-Madden* + β₂*NY_CT* + β₃*Post*NY_CT* + *Controls* + ε. The dependent variable is the midpoint of the borrower's four-point FICO range. The columns compare borrowers in Connecticut and New York to all borrowers (1) outside the Second Circuit and (2) in no-usury states specifically. All specifications include lender fixed effects (Lender FE) and controls. Standard errors, in parentheses, are clustered by the borrower's state of residence.

** $p < .01$.

Table 8
Difference-in-Differences Results: Change in Loan Size Post-*Madden*

	Panel A: Outside the Second Circuit			Panel B: No Usury States		
	All Borrowers (1)	Sub750 (2)	Sub700 (3)	All Borrowers (1)	Sub750 (2)	Sub700 (3)
Post-*Madden*	0.040**	0.043**	0.062**	0.028**	0.029**	0.046**
	(0.010)	(0.011)	(0.014)	(0.005)	(0.004)	(0.007)
NY_CT	0.020$^+$	0.020$^+$	0.031*	0.018	0.017	0.027*
	(0.010)	(0.011)	(0.014)	(0.011)	(0.010)	(0.011)
Post* NY_CT	-0.043**	-0.046**	-0.062**	-0.032**	-0.033**	-0.048**
	(0.011)	(0.011)	(0.014)	(0.006)	(0.006)	(0.009)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	907,883	857,544	635,219	130,379	122,147	85,672
R-squared	0.346	0.353	0.357	0.335	0.340	0.347

Note. Results are from estimating *Loan Amount* $= \alpha + \beta_1 Post\text{-}Madden + \beta_2 NY\_CT + \beta_3 Post*NY\_CT + Controls + \varepsilon$. The dependent variable is the natural log of the loan amount. Panel A uses all borrowers outside the Second Circuit as the control group, and Panel B uses only borrowers from states without usury caps as the control group. In each panel, column (1) uses the full set of borrowers, column (2) uses only borrowers with FICO scores below 750 (Sub750), and column (3) uses only borrowers with FICO scores below 700 (Sub700). All specifications include lender fixed effects (Lender FE) and controls. Standard errors, in parentheses, are clustered by the borrower's state of residence.

$^+ p < .10.$

$^* p < .05.$

$^{**} p < .01.$

Table 9
Triple Difference Results: Change in Borrower Delinquencies Post-*Madden*

	Panel A: All Borrower Months			Panel B: Through Initial Default Only		
	Outside 2nd Circuit (1)	No Usury State (2)	PSM Sample (3)	Outside 2nd Circuit (1)	No Usury State (2)	PSM Sample (3)
Post-*Madden*	-0.006**	-0.008**	-0.005**	-0.001**	-0.002**	-0.001**
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
NY_CT	-0.000	-0.000	-0.000	-0.000	-0.000	0.000
	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Above16	0.007**	0.005*	0.008**	0.001**	-0.000	-0.001
	(0.001)	(0.002)	(0.002)	(0.000)	(0.001)	(0.001)
Post*NY_CT	-0.001	0.001	-0.001**	-0.000	0.000	-0.000
	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Above16*Post	-0.010**	-0.007**	-0.009**	-0.002**	-0.001	-0.000
	(0.001)	(0.002)	(0.001)	(0.000)	(0.001)	(0.001)
Above16*NY_CT	-0.001	0.002	-0.002	-0.000	0.001	0.001
	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)	(0.002)
Above16*Post* NY_CT	-0.001	-0.004	-0.002	0.000	-0.001	-0.001
	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,366,222	389,339	452,091	2,351,868	386,706	449,169

Note. Results are from estimating *Delinquent* = α + β_1*Post-Madden* + β_2*NY_CT* + β_3*Above16* + β_4*Post*NY_CT* + β_5*Above16*Post* + β_6*Above16*NY_CT* + β_7*Above16*Post*NY_CT* + *Controls* + ε. Panel A keeps borrowers in the sample after they are delinquent, while Panel B includes only through the borrower's initial delinquency. In each panel, column (1) uses all borrowers outside the Second Circuit as the control group, column (2) uses all borrowers in states without usury caps as the control group, and column (3) uses the propensity-score matched (PSM) sample as the control. The analysis is presented using the Cox proportional hazard model. All specifications include lender fixed effects (Lender FE) and controls. Standard errors, in parentheses, are clustered by loan.

+ $p < .10$.

* $p < .05$.

** $p < .01$

Figures Legend

Figure 1. Summary Statistics: Value of Loans Originated by Marketplace-Lending Platforms in Our Sample

Figure 2 Summary Statistics: Distribution of Interest Rates Before and After *Madden* – Borrowers Outside the Second Circuit

Figure 3. Summary Statistics: Distribution of Interest Rates Before and After *Madden* – Borrowers in Connecticut and New York

Figure 4. Summary Statistics: Growth in Loan Originations Post-*Madden*

Figure 5. Summary Statistics: Distribution of FICO Scores Before and After *Madden* – Borrowers Outside the Second Circuit

Figure 6. Summary Statistics: Distribution of FICO Scores Before and After *Madden* – Borrowers in New York and Connecticut

Figure 7. Summary Statistics: Loan Originations to Lower-Quality Borrowers in Connecticut and New York

Figure 8. Parallel Trends Analysis: Discounts on Traded Notes by Month

Figure 9. Parallel Trends Analysis: FICO Scores by Month

Figure 10. Parallel Trends Analysis: Loan Sizes by Month
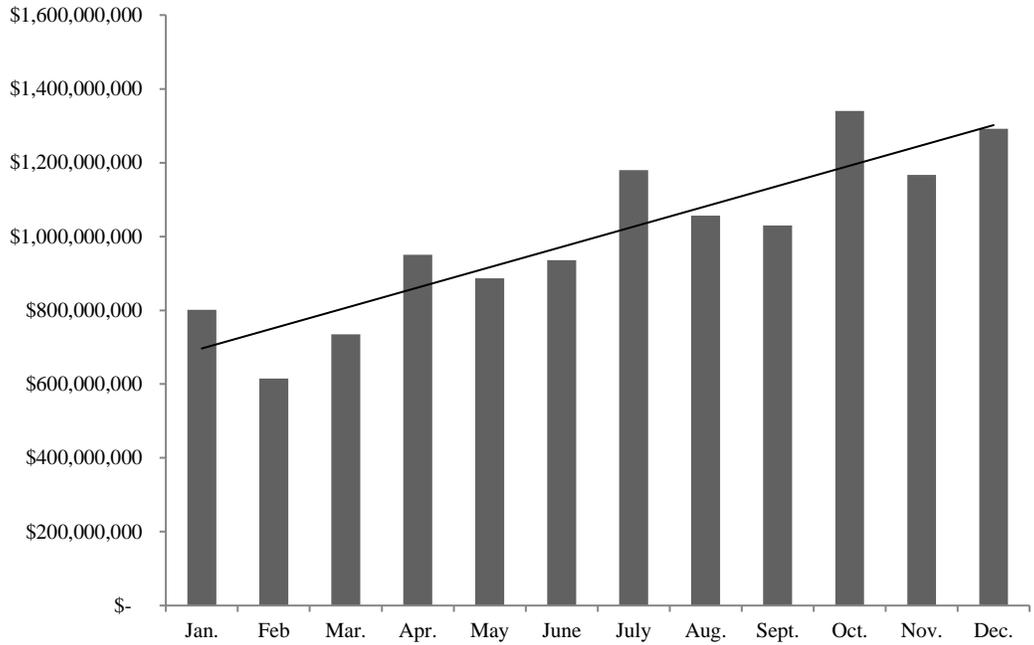
Figure 1. Summary Statistics: Value of Loans Originated by Marketplace-Lending Platforms in Our Sample.

Note. The figure shows the total value of all loans originated by the three lending platforms in our study in each month

of 2015. The trend line is plotted on the figure.
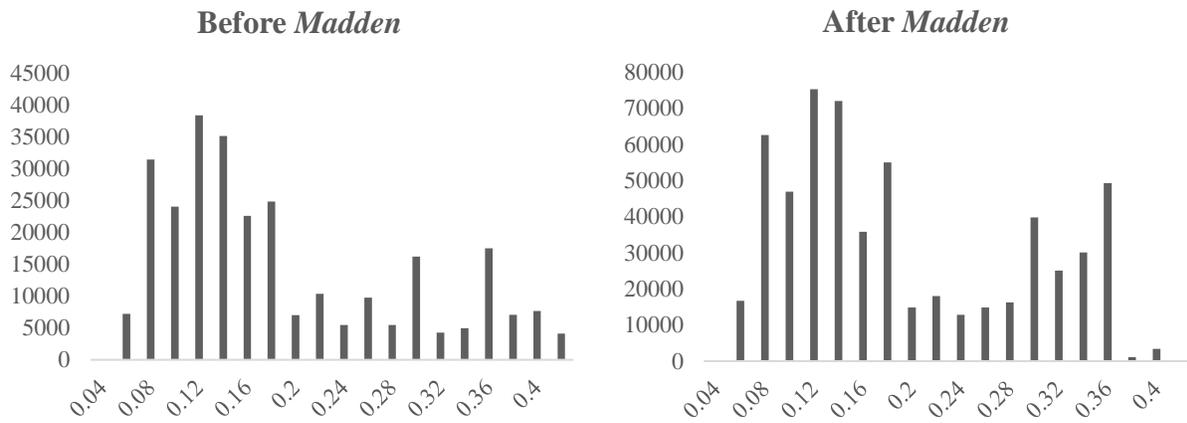
Figure 2. Summary Statistics: Distribution of Interest Rates Before and After *Madden* – Borrowers Outside the

Second Circuit.

Note. The histograms show the distribution of interest rates before and after *Madden* for borrowers outside the Second

Circuit. All histograms use a bin width of two percentage points.
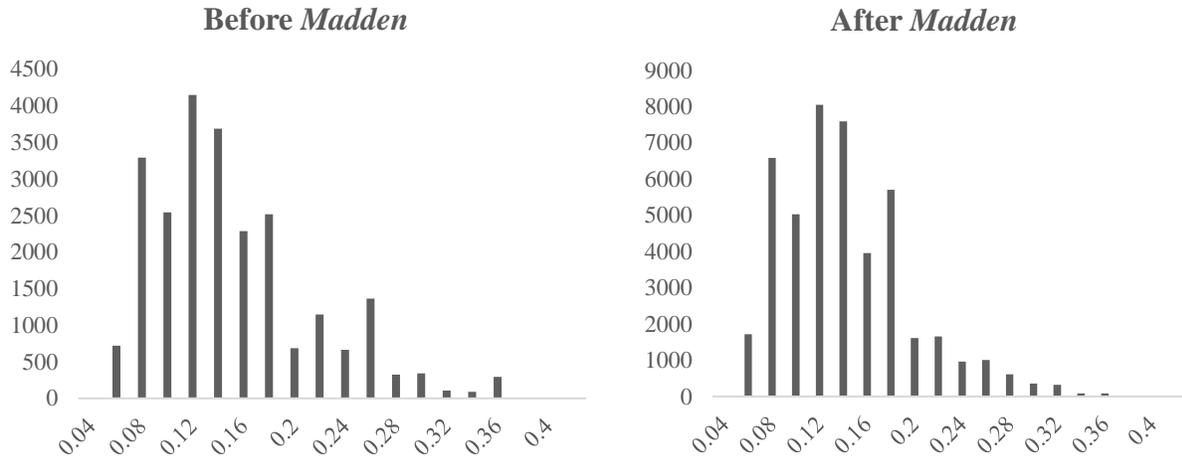
Figure 3. Summary Statistics: Distribution of Interest Rates Before and After *Madden* – Borrowers in Connecticut and New York.

Note. The histograms show the distribution of interest rates before and after *Madden* for borrowers in Connecticut and New York. All histograms use a bin width of two percentage points.

Figure 4. Summary Statistics: Growth in Loan Originations Post-*Madden.*

Note. The figure shows post-*Madden* growth in loan originations (a value of 100% would reflect that twice as many loans were issued after *Madden* than before). The pre-*Madden* period runs from the beginning of 2015 to May 22, 2015, and the post-*Madden* period runs from May 23 to the end of 2015.

Before *Madden*
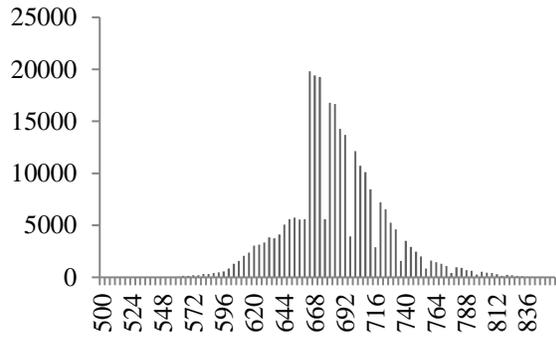
After *Madden*
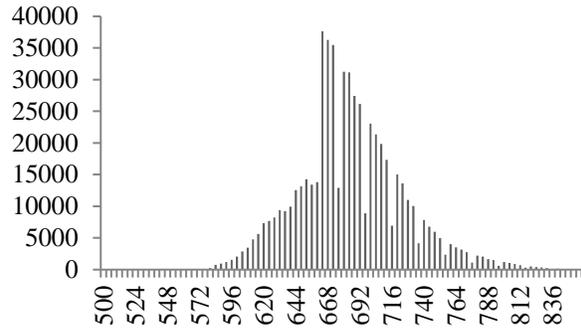
Figure 5: Summary Statistics: Distribution of FICO Scores Before and After Madden – Borrowers Outside the Second Circuit.

Note. The histograms show the distribution of FICO scores before and after *Madden* for borrowers outside the Second Circuit. All histograms use a bin width of four FICO points.

Before *Madden*   **After *Madden***
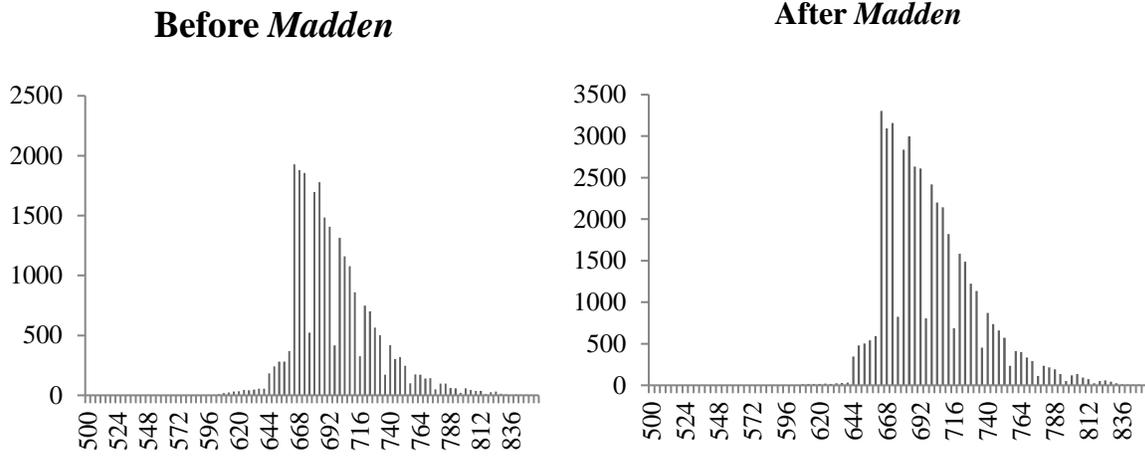
Figure 6. Summary Statistics: Distribution of FICO Scores Before and After *Madden* – Borrowers in New York and

Connecticut.

Note. The histograms show the distribution of FICO scores before and after *Madden* for borrowers in Connecticut

and New York. All histograms use a bin width of four FICO points.

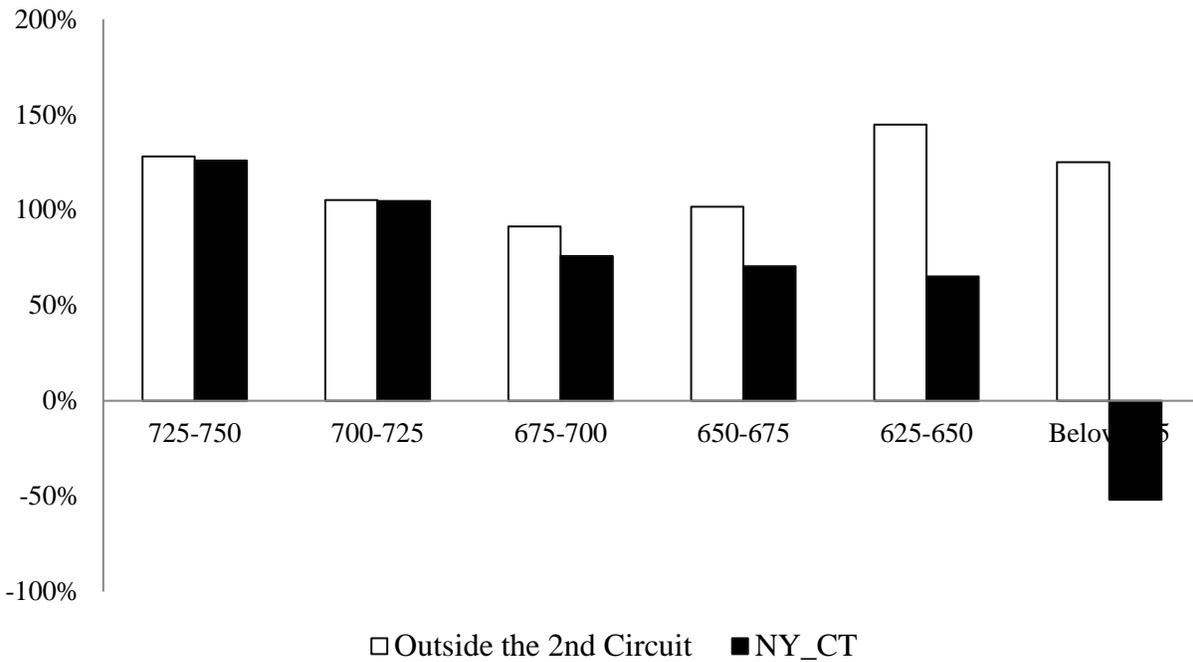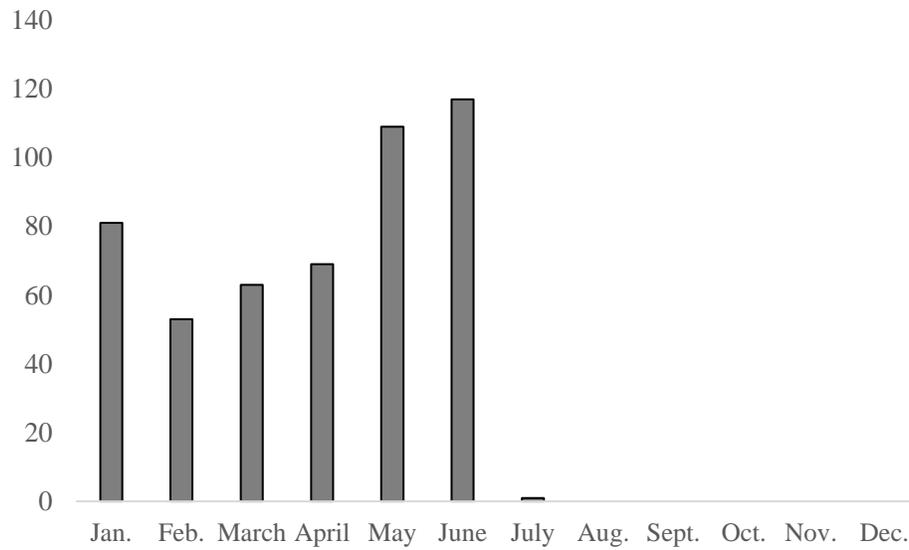**Figure 7.** Summary Statistics: Loan Originations to Lower-Quality Borrowers in Connecticut and New York.

Note. The figure shows the number of loans originated to borrowers in Connecticut and New York with FICO scores below 640 for each month of 2015.

Panel A. Notes backed by non-current loans
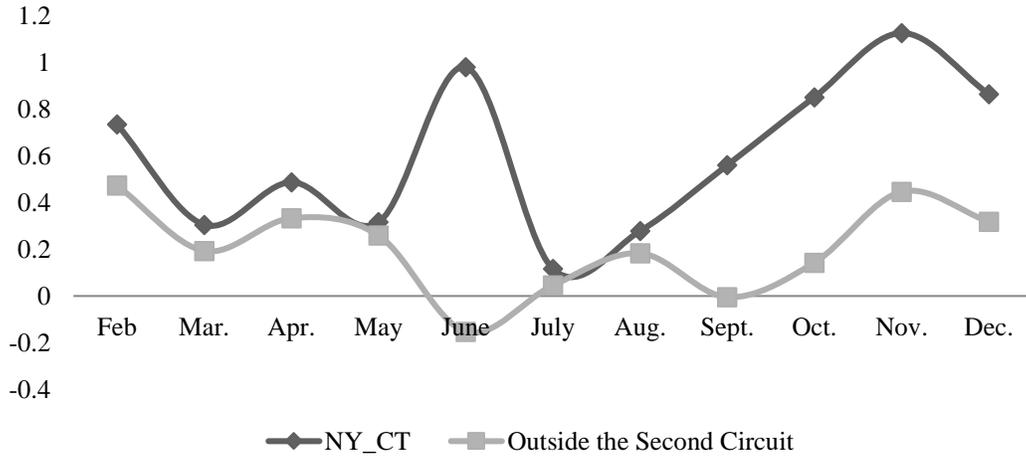
Panel B. Notes backed by current loans
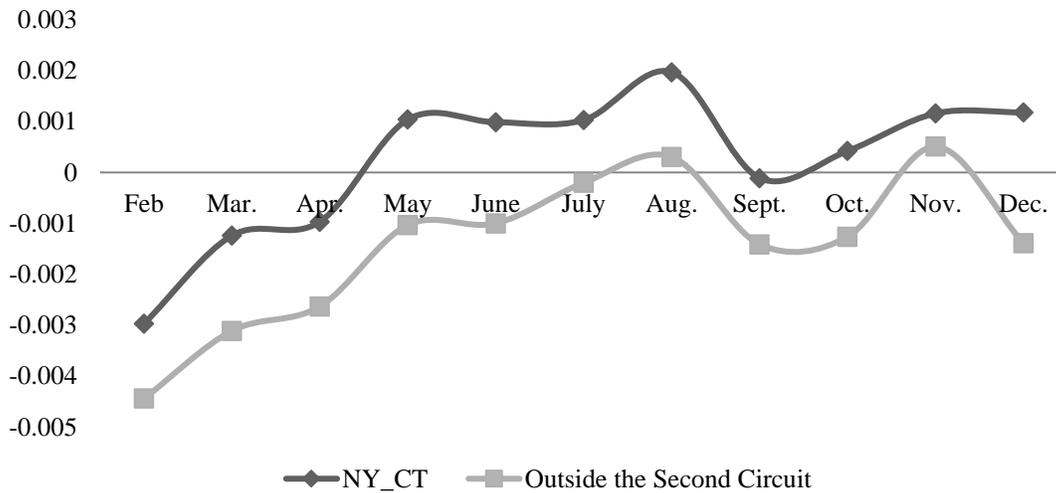
Figure 8. Parallel Trends Analysis: Discounts on Traded Notes by Month.

Note. Each figure presents the coefficients on monthly interaction terms from a pair of regressions. Panel A includes only notes traded based on non-current loans, and Panel B includes only notes traded based on current loans. In each panel, the first line represents results for notes backed by loans to borrowers in Connecticut and New York (NY_CT), and the second is for notes backed by loans to borrowers outside the Second Circuit. The sample and regression specification are the same as in Table 6, except that we replace the prior variables of interest with dummy variables for each month from February through December and interact those dummies with Above16, an indicator for whether the loan's interest rate is above 16%. The monthly indicators reflect the month in which the trade occurred.
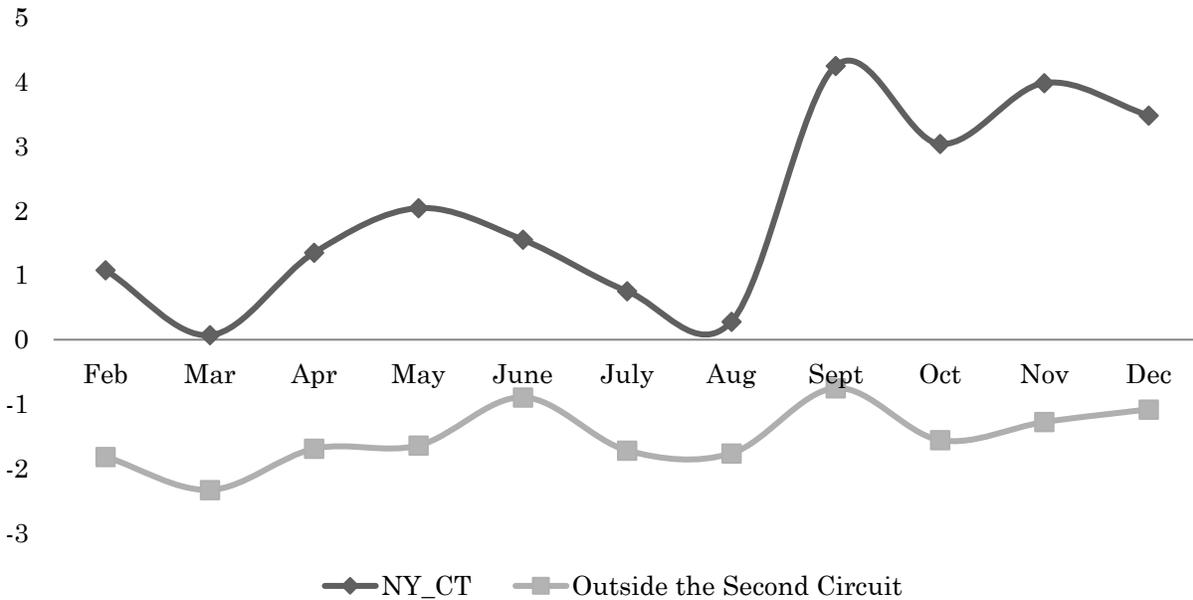
Figure 9. Parallel Trends Analysis: FICO Scores by Month.

Note. The figure presents the coefficients on monthly indicators from two regressions. The first regression includes only borrowers located in New York and Connecticut (NY_CT), and the second includes only borrowers located outside of the Second Circuit. The sample and regression specification are the same as in Table 8, except that we replace the prior variables of interest (NY_CT, Post-*Madden*, and the resulting interaction term) with dummy variables for each month from February through December. The monthly indicators reflect the month when the loan was issued.

Panel A. All Borrowers
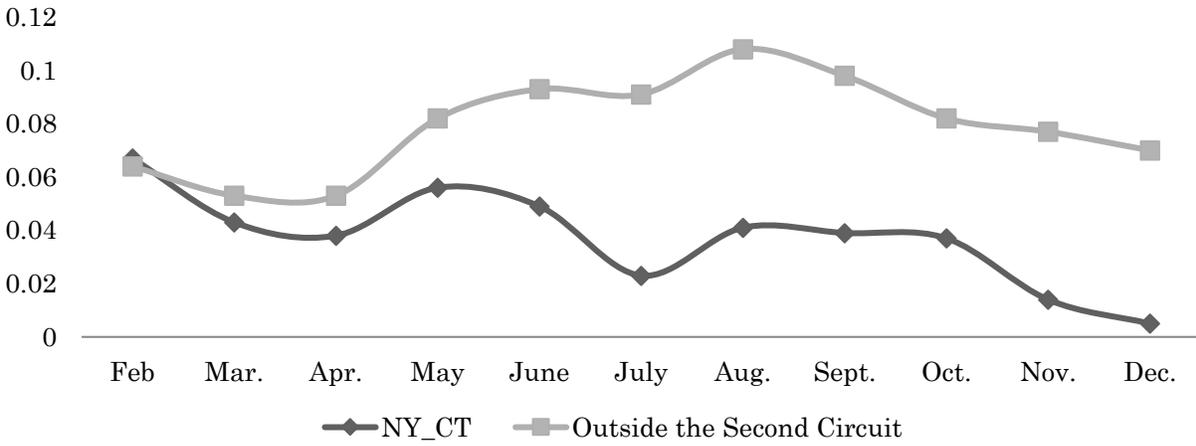
Panel B. Borrowers with FICO Scores below 700
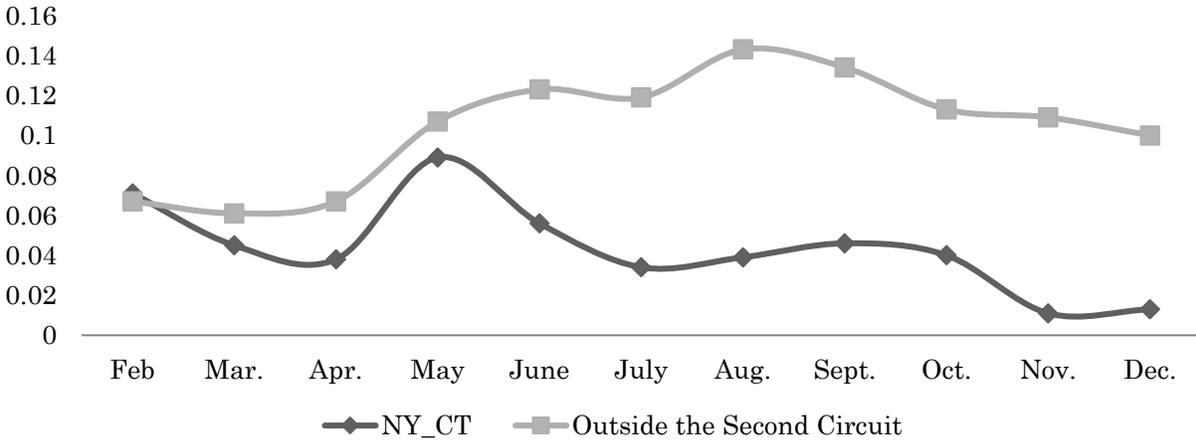
Figure 10. Parallel Trends Analysis: Natural Log of Loan Sizes by Month.

Note. The figures present the coefficients on monthly indicators from two regressions. Panel A includes the full sample of borrowers, and Panel B includes only the sample of borrowers with FICO scores below 700. In each panel, one line shows the result from a regression for borrowers in Connecticut and New York (NY_CT), while the second shows the result for borrowers outside of the Second Circuit. The sample and regression specification are the same as in Table 9, except that we replace the prior variables of interest (NY_CT, Post-*Madden*, and the resulting interaction term) with dummy variables for each month from February through December. The monthly indicators reflect the month when the loan was issued.

APPENDIX B

What Happens when Loans Become Legally Void?
Evidence from a Natural Experiment

Colleen Honigsberg
Robert J. Jackson, Jr.
Richard Squire

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ABSTRACT

We use a natural experiment—an unexpected judicial decision—to study how the legal enforceability of consumer loans affects borrower behavior. In May 2015, a federal court ruled that the usury laws of three states—New York, Connecticut, and Vermont—were enforceable with respect to a subset of consumer loans that market participants had previously assumed were exempt from those laws. The decision was important because, in New York and Connecticut, borrowers on usurious loans have no obligation to repay any interest or principal. Using proprietary data from three marketplace lenders, we use a difference-in-differences design to study the decision's effects. We find no evidence that consumers engaged in strategic default. However, upon examination of secondary market trading, we find that delinquent loans issued above usury caps trade at a discount. We also show that the decision reduced credit availability for riskier borrowers, who are more likely to borrow at rates above usury limits.

Keywords: usury law, strategic default, consumer lending, marketplace lending, *Madden v. Midland*

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1.   Introduction

State usury statutes—laws that regulate the interest rate that a lender may charge a borrower—are ubiquitous in the United States. Yet they are largely irrelevant in modern American finance, because federal law has long preempted state usury statutes for purposes of most borrowing. In May 2015, however, an unexpected judicial decision, *Madden v. Midland Funding LLC*, activated long-dormant usury limits for a subset of loans governed by the laws of three states: New York, Vermont, and Connecticut. Specifically, the court held that state usury laws were not preempted by federal law for loans held by nonbank investors.

Because usurious loans in New York and Connecticut are void—that is, the borrower has no legal obligation to repay any outstanding principal or interest—the decision provided borrowers with incentives to default, allowing for study of the importance of legal enforceability in consumer lending, an important question given the theoretical intuition that, "if the consequences of default are less severe, borrowers will be more likely to default" (Zywicki, 2015). Further, because many consumer loans are securitized and traded, the setting allows for study of lenders' expectations regarding default. Finally, because the decision provides lenders with significant incentives to stop lending at rates above usury limits—potentially cutting off credit for higher-risk borrowers—the decision allows for study of the effects of usury laws on the availability of consumer credit.

We use proprietary data from three of the largest marketplace lenders to run difference-in-differences tests comparing loans issued in New York and Connecticut to loans issued in states unaffected by *Madden*. Marketplace lending, which matches borrowers to lenders quickly and efficiently, is a growing source of consumer credit. Although *Madden* activated usury limits for a wide range of loans and thus may well have effects far beyond the marketplace-lending context, we focus on this relatively narrow setting because we obtain high-quality data from marketplace

lending platforms that allow us to trace the loan process through many different points in time. A limited number of papers have used publicly available data from a single marketplace lender (e.g., Rigby, 2013), but we are not aware of any other papers that use the private dataset we examine here—which contains additional loans, as well as additional detail on loans and borrowers, not included in public databases.

Our study points to three principal findings. First, our evidence suggests that legal enforceability does not drive consumer default, as we find no evidence that consumers strategically defaulted on loans above usury limits after *Madden* rendered those loans potentially void.[1] Strategic default is a growing topic in the finance and economics literature, particularly since the financial crisis, during which homeowners faced incentives to walk away from underwater mortgages (e.g., Foote, Gerardi & Willen, 2008; Guiso, Sapienza & Zingales, 2013; Mayer et al., 2014). To the best of our knowledge, however, empirical evidence on strategic default is limited to mortgages. Although the incentives to default on unsecured consumer loans following *Madden* seem more straightforward than those homeowners faced during the crisis, there are several possible reasons why we find no evidence of this behavior. Borrowers may have been unaware of the decision, even though—as we describe below—there is evidence that investors were aware of the ruling. Or perhaps borrowers were concerned with reputational risk, even though it is unclear whether a borrower who chooses to default on a legally void loan can be penalized.[2] It is also

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[1] Although usurious loans in New York and Connecticut are void, as noted below arguments unaddressed by the *Madden* decision and left for resolution by the lower courts could eventually allow nonbank investors to enforce loans issued above usury caps in those states. As such, for ease of exposition we say that *Madden* merely rendered these loans *potentially* void.

[2] Unlike the homeowners who walked away from underwater mortgages, it is far from clear whether consumers who strategically defaulted on consumer loans after *Madden* would face reputational harm. For example, credit-reporting agencies have not resolved whether, in the wake of *Madden*, a consumer's credit score can be reduced merely because the borrower defaults on a loan she has no legal obligation to pay. Indeed, some consumer advocates object to the use of the word "default" in this context, arguing that borrowers cannot default on a loan they have no legal obligation to pay.

possible that borrowers chose not to default due to non-pecuniary factors such as morality (Guiso, et al., 2013), or that borrowers are waiting on the courts to resolve the remaining legal questions raised by the *Madden* decision.[3]

Second, we find evidence that, although investors are aware of the decision and price in the increased legal risk, they do not expect widespread strategic default. By analyzing secondary market trading of marketplace loans, we show that, following *Madden,* investors apply larger discounts to loans above usury caps in New York and Connecticut when borrowers are late on their payments. By contrast, we find only limited evidence that investors apply larger discounts to loans above usury caps in New York and Connecticut that are current—that is, where borrowers are paying on time. Taken together, these findings indicate that investors are aware of the *Madden* decision and its potential to harm their ability to collect on the loans, but that they do not expect widespread strategic default.

Finally, we show that the imposition of usury caps decreases credit availability. In particular, the decision led to a decrease in marketplace loans issued above usury caps in New York and Connecticut. Further tests show that this decrease is driven by much lower loan volumes to higher-risk borrowers who would have paid rates above usury limits. This finding is consistent with basic economic intuition and prior literature showing an association between credit availability and usury law (e.g., Benmelech and Moskowitz, 2010). Although it may seem puzzling that fewer loans would be issued to this higher-risk segment if there was no evidence (or

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[3] Although the *Madden* court made clear that *federal* law does not shield loans held by nonbank investors from usury caps, as explained in more detail below, the court did not address two additional arguments that might lead courts to conclude that usury caps do not apply to such loans. First, the court left unresolved whether choice-of-law provisions in these types of consumer loan agreements should be given effect. Because many loan agreements select law from states without usury limits, enforcing such clauses could protect lenders from the effects of the decision. Second, even if federal law does not shield nonbank investors from usury caps, the court left unresolved whether state law could protect these investors. Indeed, in urging the Supreme Court not to review *Madden*, the Solicitor General expressly noted that the resolution of these issues in creditors' favor by the lower courts might make the Supreme Court's intervention unnecessary (Solicitor General, 2016).

expectation) of strategic default, the finding is intuitive if we consider that the expected loss on a default is likely to be far greater if the lender has limited legal ability to enforce the loan.

Our study contributes broadly to the literature on consumer finance and strategic default. Although the dollar value of household finance dominates the size of the corporate sector (Trufano, 2009), consumer finance is difficult to study because individuals guard their financial information jealously (Campbell, 2006). Using our proprietary data, we are able to overcome this obstacle. Moreover, we contribute to the growing literature on strategic default by analyzing not only *whether* consumers default—but also *whether investors expect* them to do so. To our knowledge, the prior literature on strategic default has analyzed whether borrowers strategically default on mortgages. Here we are able to analyze not only the presence of default, but also the expectation of default. And we are able to do so in a new setting: consumer lending.

We also contribute to the literature on law and debt contracting more generally. Significant prior literature has studied how legal institutions are related to corporate debt contracts and loan syndication (e.g., Qian and Strahan, 2007; Lerner and Schoar, 2005). Although these papers focus on a broad range of differences in law ranging from corporate law (Wald and Long, 2007) to bankruptcy law (Davydenko and Franks, 2008), they focus almost exclusively on statutory law.[4] By contrast, our paper examines the effects of a decision by a significant federal court. Judicial decisions are critical for debt contracting in the United States, but they are difficult to study empirically because economically meaningful changes in the relevant law governing debt contracts are relatively rare. *Madden* provides a unique opportunity to understand how judicial opinions are incorporated into the contracting process. For example, as we discuss below, we found that the

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[4] One rare exception is Honigsberg, Katz & Sadka (2014), which examines both statutory and common law.

marketplace lenders we study took roughly two months to adjust their lending practices fully to the decision.

Finally, our findings contribute to the literature on the influence of legal institutions on behavior. Legal theorists have long debated whether enforcement mechanisms are necessary to ensure contractual performance, or whether reputational sanctions, the parties' taste for fairness, or other factors can be effective substitutes for legally enforceable agreements (e.g., Schwartz & Scott, 2003; Rabin, 1993). Economists have also considered whether promise-keeping has been adopted as a default rule among consumers and firms, and the implications of that default for resource allocation (Chen, 2000). Little work, however, has benefited from an empirical setting like ours, where a sudden change in law plausibly frees consumers from the legal obligation to pay unsecured debts.

The remainder of the Article proceeds as follows. Part 2 reviews the legal and institutional environment of state usury laws and their application to marketplace lending platforms. Part 3 describes our data, and Part 4 describes our results and methodology. Part 5 concludes.

2. Legal and Institutional Background

A. State Usury Statutes and Federal Preemption

Dating back to the Old Testament, usury laws cap the interest rate that lenders may charge on loans. The policy merits of such caps have been debated for generations (e.g., Leviticus; Shanks, 1967; Homer & Sylla, 2005). Opponents argue that usury limits exclude higher-risk borrowers from credit markets—or, worse, require them to resort to more expensive, and even black-market, sources of credit (Bentham, 1787; Ryan, 1924). On the other hand, supporters of usury caps argue

that they reduce the market power of lenders and prevent naïve borrowers from agreeing to loan terms on which they may eventually default (NCLC, 2016).

Whatever the merits of this debate, most American states have adopted usury statutes that expressly cap interest rates. Although both the rate caps and the penalties for violating usury statutes vary significantly across states, the penalties for lenders making usurious loans are often significant. In nearly all states with usury caps, the lender is required to return to the borrower any interest paid above the usury cap, and in many of these states the lender may be required to pay treble that amount.[5] And in some states, including New York and Connecticut, a loan above the usury limit is null and void: that is, the borrower is entitled to keep the principal as a gift and need not pay any fees associated with the loan.[6] Although usury laws are frequently associated with payday lending, usury limits are often low enough to capture a significant portion of consumer lending—indeed, some states set limits as low as 5 percent for consumer loans.[7]

Despite the ubiquity of these laws, they are largely irrelevant to modern American lending markets. The reason is that the National Bank Act ("NBA") preempts state usury limits, rendering these caps inoperable for most loans. For loans made by national banks, the NBA establishes a usury limit equal to the limit of the state in which the bank is "located."[8] This is one reason many

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[5] See, e.g., CAL. CIV. CODE § 1916-3 (providing for treble damages of usurious interest in California).

[6] See N.Y. GEN. OBL. L. § 5-501(1). As Stein (2001) explains, in New York, "[i]f a loan is usurious, it becomes wholly void": the "lender forfeits all principal and interest (the loan becomes a gift)"; see also *Seidel v. 18 East 17th Street Owners*, 598 N.E. 2d 7, 9 (N.Y. 1992) ("The consequences to the lender of a usurious loan [in New York] can be harsh: the borrower is relieved of all further payment—not only interest but also outstanding principal . . . New York usury laws historically have been severe in comparison to the majority of States."); *Ferrigno v. Cromwell Development Assoc.*, 44 Conn. App. 439, 439 (App. Ct. Conn. 1997) ("Loans with interest rates in excess of [the usury cap in Connecticut] are prohibited [by statute] and as a penalty no action may be brought to collect principal or interest on any such prohibited loan.").

[7] See Ga. Code Ann. § 7-4-18 (West 2016). See also, e.g., Ala. Code § 8-8-1, Minn. Stat. Ann. § 334.01 (West), 41 Pa. Stat. Ann. § 201 (West) (establishing a usury limit of 6% for loans below $50,000).

[8] The National Bank Act of 1864 expressly allows national banks to "charge on any loan . . . interest at the rate allowed by the laws of the State, Territory, or District where the bank is located, or at a rate of 1 per centum in excess of the discount on ninety-day commercial paper in effect at the Federal reserve bank in the Federal reserve district where the bank is located, whichever may be the greater." 12 U.S.C. § 85 (2016).

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banks, and particularly those that engage in significant consumer lending, are chartered in states such as South Dakota, which has no usury limit. Pursuant to NBA preemption, banks chartered in these states may charge rates that would otherwise be usurious in the borrower's home state (Smith, 2009).

As securitization—that is, the issuance of loan-backed securities—of consumer loans has become more common, a question arises when a loan that is *issued* in compliance with the applicable usury cap is later sold to a lender in another state, potentially implicating another state's usury laws. The traditional rule under usury law is that loans are "valid when made," meaning that a change in the identity of a loan's owner does not alter the loan's enforceability. The valid-when-made rule—sometimes called the "cardinal law of usury"—is well-established, and, until recently, federal courts applied this rule when determining the NBA's preemptive scope. For example, in 2000, the Eighth Circuit decided *Krispin* v. *May Department Stores Co.,* a case in which a national bank extended credit on credit cards but later sold the receivables to a department store. Delinquent borrowers sued the store, arguing that the late fees they had been charged were, under the laws of the borrowers' home state, usurious. The Eighth Circuit held these claims preempted by the NBA because the fees were not usurious under the laws of the state in which the originating bank was located.[9]

B.   Marketplace Lending and State Usury Law

Consumers have increasingly sought new sources of credit in the years since the financial crisis, and one source of such credit is marketplace lending: platforms that match willing lenders

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[9] *Krispin v. May Department Stores Co*., 218 F.3d 939 (2000). Five years later, the Eighth Circuit again applied the valid-when-made rule to dismiss state-law usury claims based on loans issued by a national bank. *Phipps* v. *FDIC*, 417 F.3d 1006 (8th Cir. 2005). The Supreme Court first recognized the valid-when-made rule (though outside the context of the NBA) in 1833. Nichols v. Fearson, 32 U.S. (7 Pet.) 103, 109.

with borrowers to facilitate loans. Marketplace lending platforms issued some $5.5 billion in loans in 2014 (SBA, 2015), but the market is growing quickly; the three marketplace platforms we study here alone issued more than $12 billion in loans in 2015. The entire market is expected to grow to more than $150 billion in annual loan originations over the next decade (PWC, 2015).

The general idea of marketplace lending is to match prospective borrowers to willing lenders through a simple online platform that enables rapid funding decisions (Treasury, 2016). Although there are differences in procedure across platforms, the general framework for marketplace lending is as follows. First, a borrower submits an application with standard information, including her credit information, employment history, and the purpose of the loan. The platform uses a proprietary algorithm to assign a risk grade to the proposed loan and then posts the loan request on the platform's website, where investors can, in turn, search for specific loans that meet their desired risk characteristics. If enough investors are willing to fund the loan, the loan is then originated by a federally insured national bank pursuant to an agreement between that bank and the marketplace platform. The bank used by a number of marketplace platforms, WebBank, is located in Utah—a state with no usury limit (Treasury, 2016). The originating bank then sells pieces of the loan to the investors that have agreed to fund the commitment. The platform generally receives an origination fee upon the initiation of the loan and a servicing fee over its lifetime.

Several commentators have celebrated the emergence of marketplace lending as a means of additional competition for providing consumer credit (e.g., Economist, 2014). The platforms typically charge lower rates, on average, than those charged by traditional banks for credit cards or installment loans—and their existence creates competition that may result in lower rates[10]

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[10] We note that the generalizations in the text may not describe small-business lending as well as consumer lending. Some recent work suggests that small businesses can, and often do, borrow at lower rates from banks than they can through marketplace platforms (Federal Reserve Board, 2014; SBA, 2015).

(Economist, 2014; Vermont Dept. of Fin. Reg., 2015). Because a majority of consumers who borrow through marketplace platforms use the loan to consolidate or repay higher-interest credit card or installment debt (PWC, 2015), the argument goes, the availability of marketplace lending effectively saves consumers the difference between prevailing credit-card rates and marketplace lending rates. Especially for higher-risk, lower-quality borrowers, this difference can be significant.

These marketplace lending platforms rely on the common law of NBA preemption to avoid the application of state usury laws.[11] For example, since marketplace loans are initiated by a national bank but then immediately change hands, platforms rely on the valid-when-made doctrine to shield marketplace loans from usury caps. And marketplace loans, like other forms of consumer credit, are often securitized—that is, transferred to an entity that issues notes to investors—so that investors can diversify their exposure to these loans. Indeed, according to one estimate, some $5 billion in notes based upon marketplace consumer loans was issued in 2015 alone (PeerIQ, 2015). Investors in these notes, too, rely upon NBA preemption to ensure that the loans underlying the notes are not subject to state usury laws.[12]

C.   The Second Circuit's *Madden* Decision

Until last year, commentators and counsel relied upon prior legal precedent to conclude that marketplace loans, and notes based upon such loans, were not subject to state usury laws by operation of NBA preemption. In May 2015, however, the Second Circuit stunned markets in

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[11] As noted above, there is significant heterogeneity in the business models of marketplace lending platforms, and some make use of state-chartered banks, rather than national banks, in the issuance of their loans. *Madden* did not explicitly consider the federal-law provision addressing usury preemption for state-chartered banks, Section 27 of the Federal Deposit Insurance Act ("FDIA"), 12 U.S.C. § 1831d. Nevertheless, these provisions are sufficiently similar that market participants could well expect that loans initiated by platforms using state-chartered banks would, in light of *Madden*, not benefit from FDIA preemption of state usury caps.

[12] To provide context on the extent to which the marketplace lending business model relies upon the courts' historical approach to the law of NBA preemption, we note that the *Madden* decision is disclosed as a risk factor in prospectuses for notes backed by marketplace loans (e.g., Prosper Funding LLC, 2016).

Madden v. *Midland Funding LLC,* concluding that National Bank Act preemption does *not* apply to loans initiated by a national bank but later sold to a nonbank third party.

The plaintiff in *Madden* is a New Yorker, Saliha Madden, who defaulted on her credit-card debt. Her card was issued by Bank of America, and her account operated by FIA Card Services, a national bank based in Delaware—a state that permits banks to charge rates that would be usurious in New York. After Madden defaulted, FIA sold her debt to Midland Funding, a debt collector. Midland sent Madden a collection notice, seeking repayment of a balance calculated using an interest rate of 27%, the rate specified in her credit-card agreement. Madden then brought a putative class action against Midland on behalf of herself and other residents of New York, claiming that the debts of the class are void by operation of New York's usury law, which sets a civil cap of 16% and a criminal cap of 25%. The district court held Madden's claim preempted by the National Bank Act.13

On May 22, 2015, the Second Circuit reversed, holding that the NBA's preemptive scope no longer applied to Madden's debt once it was sold to an entity that was not a national bank.14 The NBA only preempts state laws whose application might "significantly interfere" with the exercise of the national banking power, and the court found that this requirement was not met in Madden's case. Hence, the NBA did not preempt New York's usury laws, and these laws applied to Madden's credit card balances. Because, under New York's usury laws, neither principal nor interest may be collected on a usurious loan, the Second Circuit's decision effectively canceled the plaintiff's outstanding credit-card balance—and those of others in her class.

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13 See Stipulation for Entry of Judgment for Defendants for Purpose of Appeal, Madden v. Midland Funding LLC, No. 11-CV-8149 (May 30, 2014) ("preemption of New York's usury laws applies to non-bank assignees of national banks, regardless of whether the national bank retains any interest in or control over the assigned accounts."). We note that Madden's claims actually focused on New York's *criminal* usury statute, which makes it a Class E felony to charge interest of more than 25%. N.Y. PENAL LAW § 190.40.
14 *Madden v. Midland Funding, LLC*, 786 F.23d 246, 250 (2d Cir. 2015).

Madden was a surprise to market participants. Although the ruling activated usury laws only for loans held by nonbank investors such as hedge funds, today nonbank investors hold significant amounts of debt (Buhayar, 2016). Hence, the ruling had implications for a wide range of loans. In the flurry of law-firm memoranda that followed, counsel warned investors that the Second Circuit's decision "could significantly disrupt the secondary market for bank loans originated by national banks" (Ropes & Gray, 2015). Another large New York law firm remarked:

> Perhaps most troubling about the opinion . . . is a cursory statement, which was made without explanation or supporting data, indicating that application of state usury laws to third-party assignees of bank-originated loans would not prevent or "significantly interfere" with the exercise of national bank powers . . . . Inexplicably, the court failed to realize the significance that its ruling would have on the ability of banks to sell their loans in the secondary market. Given that non-bank purchasers will be unable to enforce the terms of a loan according to the original agreement between the bank and borrower, [the decision] will undoubtedly chill the market for . . . . securitizations and bank loan programs with third parties [such as marketplace lending] (Paul Hastings, 2015).

In response to the Second Circuit's decision, Midland petitioned the Second Circuit to rehear the case; when the petition was denied, Midland promptly filed a petition for *certiorari* in the Supreme Court of the United States. Midland's petition argued, among other things, that the Second Circuit's decision "threatens to inflict catastrophic consequences on secondary markets that are essential to the operation of the national banking system and the availability of consumer credit."[15] Upon receipt of Midland's petition, the Supreme Court requested the Solicitor General's view of the case. Although the Solicitor General explained to the Court that the Second Circuit had "erred" and that the *Madden* "decision is incorrect," the brief concluded that the Supreme Court's review was not warranted—in part because the lower courts have yet to address other arguments that could affect the outcome of the case (Solicitor General, 2016).

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[15] Pet. for Cert. in Midland Funding LLC et. al v. Saliha Madden, No. 15-610 (Nov. 10, 2015).

Unfortunately for Midland, in June 2016, the Supreme Court followed the Solicitor General's advice and declined to hear the case. The case has now been remanded to the trial court, as the parties attempt to resolve two independent legal bases on which the lenders in *Madden* itself, and cases like it, may be able to avoid invalidation of their loans. First, the parties will address whether choice-of-law provisions in the agreement at issue in *Madden*, which point to Delaware, should be given effect. Although these provisions are almost always enforced in commercial agreements between sophisticated parties, their enforcement is less consistent in the consumer context (Honigsberg et al., 2014). If the court concludes that the loans should be governed under Delaware law—under which the loan in question is not usurious—Madden's claims will likely be dismissed. Second, even if the court concludes that the loan is governed by the law of Madden's home state of New York, the parties will debate whether the common law of New York might separately embrace the valid-when-made doctrine. Again, if New York law itself incorporates the valid-when-made rule, Madden-like claims that loans can be rendered usurious by virtue of the identity of the lender will likely be dismissed.

3.   Data

To study how the *Madden* decision affected consumer lending, we executed agreements with three of the largest marketplace lending platforms in the United States, pursuant to which the platforms agreed to share loan-level data with us for purposes of this study.[16] These firms agreed to share two types of data with us: information on primary lending activity—that is, loans arranged

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[16] Our nondisclosure agreements with these three companies prohibit us from identifying the firms by name, but we note that all three are among the largest—if not the largest—marketplace platforms in the United States (Federal Reserve Board, 2014).

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through their platforms—and information on secondary trading of notes based on those loans. We use the aggregated data from all three platforms for our analysis.

The first dataset consists of merged loan-level data on loans arranged through the three platforms. In total, these platforms issued almost 950,000 loans worth nearly $12 billion during calendar year 2015, the period we study.[17] The loans ranged from $1,000 to $35,000 in value, with a mean (median) value of about $12,500 ($10,500). Figure 1 below presents the total value of loans originated by the three platforms we study for each month in 2015 and shows the overall growth in this market.

[Insert Figure 1 Here.]

The interest rates on the loans in our sample ranged from 5% to 66%, with a mean (median) value of 18% (15%). In addition to loan characteristics, such as the loan's interest rate, amount, and term, our dataset also includes the following characteristics for each borrower in our sample: annual income, debt-to-income ratio, number of recent delinquencies, total credit availability, months of employment in the borrower's current position, and, finally, an estimate of each borrower's FICO score. Because the platforms were unable to provide us with actual FICO scores due to privacy concerns, we instead obtain four-point ranges: for example, we know that a particular borrower's FICO score ranges from 660 to 664. In the analyses using FICO scores, we use the midpoint of these ranges.

For two reasons, the *Madden* decision offers a unique empirical setting in which to examine the effects of changes in common law on consumer lending. First, the decision was by all accounts a surprise, offering a plausibly exogenous shock to market expectations about the state of the law.

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[17] One of the three marketplace platforms included in our study includes both a "market-based" program, in which investors can select the loan they wish to fund, and a smaller "take it or leave it" program, in which investors must accept a full package of loans on an all-or-nothing basis. Because only one of the marketplace platforms we worked with offers this program, we omit the loans from this program in our analysis.

Second, the decision today applies only to a subset of the market—Vermont, Connecticut, and New York, the states subject to the Second Circuit's jurisdiction. This offers us a plausible set of treatment and control states that permit us to examine the effect of the decision.

First, we consider the proper treatment group. *Madden* applies in Vermont, Connecticut, and New York, but these three states differ in their treatment of usurious loans. In particular, as noted above, such loans are void in Connecticut and New York. By contrast, in Vermont the loan remains valid, but the borrower need not pay interest above the permissible rate, and in a lawsuit against the lender may recover any such interest already paid, interest thereon, and reasonable attorney's fees.18 Because the law awards very different damages—and therefore creates different incentives to strategically default—we are hesitant to group these three states for empirical purposes. Hence, our analysis below includes only New York and Connecticut in our "treatment" group, and Vermont is dropped from the tests. As a practical matter, however, we note that the inclusion of Vermont makes very little difference in our results, as we have relatively few observations in that state.

Second, we consider the proper control group. Here we note that, until the Supreme Court denied *certiorari* in June 2016, *Madden* had four potential dispositions: (1) The Supreme Court grants *certiorari* and affirms; (2) The Supreme Court denies *certiorari*, and courts outside the Second Circuit find *Madden* persuasive and adopt its holding in their own jurisdictions; (3) The Supreme Court denies *certiorari*, and courts outside the Second Circuit do not find *Madden* persuasive and do not adopt its holding; or (4) The Supreme Court grants *certiorari* and reverses. *Madden*'s predicted effects on loans to borrowers in any particular jurisdiction will depend on (1)

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18 Vt. Stat. Ann. tit. IX, § 50(a)(2016).

the probabilities that market participants assigned to the Court's four potential dispositions and (2) that state's usury law.

We note, however, that loan activity for borrowers in states *without* usury limits should be unaffected regardless of *Madden*'s ultimate disposition. Therefore, loans made to these borrowers likely reflect the cleanest control group for our empirical analysis. As such, although our first control group includes all non-Second Circuit borrowers, our second control includes only borrowers from states that lack usury laws.[19] Finally, when appropriate, we include a third control group created using propensity score matching (PSM)—a statistical technique that allows us to match the loans made to borrowers in New York or Connecticut with a comparable set of loans made to borrowers outside the Second Circuit. The PSM sample is created using nearest-neighbor matching without replacement, meaning that we match each treatment loan-borrower pair with the most similarly situated control loan-borrower and do not reuse observations. However, as we describe below, the type of borrowers obtaining loans after *Madden* significantly changed in New York and Connecticut, making it difficult to match observations in these states with observations in other states. Because of this, the matched sample is not well-balanced across the control variables. As such, although we include the PSM sample for completeness, we note the limitations of the analysis and include a robustness section with additional tests.

Table 1 below provides summary statistics for these groups. Panel A presents characteristics for the full sample, Panel B presents characteristics for the "no state usury limit" sample, and Panel C presents characteristics for the PSM sample. We create the PSM sample by predicting a borrower's propensity to default based on the variables in Table 1.

[Insert Table 1 Here.]

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[19] The states that do not impose usury limits by statute are Mississippi, New Hampshire, New Mexico, South Dakota, Virginia, and Utah.

As shown, the borrowers in our sample tend to be in the same credit range as the average American borrower. The mean (median) FICO score in our sample is 684 (681.5). By comparison, the mean FICO score in the United States is 695 (FICO, 2015). (As a general rule, a score within the range of 670 to 739 is considered "good" (Experian, 2015).) Our borrowers—like the majority of marketplace borrowers, as described above—cite debt consolidation and repayment of credit card balances as the most common reason for borrowing through a marketplace platform.[20]

Two of the marketplace platforms in our sample not only initiate loans directly but also allow investors to trade those loans—or an increment thereof—through a secondary-trading platform.[21] These platforms allow investors to place trades for increments as small as $25 for notes backed by marketplace loans. Our trading dataset includes more than 1.3 million trades in sizes ranging from $25 to $12,000 provided to us by these two marketplace platforms. Approximately 93% of the trades in this dataset are for notes backed by current loans; the other 7% are for notes backed by non-current loans. Table 2 below provides summary statistics on treatment and control groups for these data.

[Insert Table 2 Here.]

Panel A of Table 2 presents characteristics for the full sample, and Panel B presents characteristics for the PSM sample. Because the change in law may have disparate effects on notes backed by both non-current and current loans, we present the characteristics separately for each sample. We create the PSM sample by estimating the probability that the note traded will be based on a loan made to a borrower in New York or Connecticut, where the prediction model includes

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[20] Of course, other borrowers requested loans for a wide range of reasons—including for special events, like weddings and home-improvement projects—but it appears that most borrowers in our sample obtained marketplace loans because doing so allowed them to repay already-existing debt.

[21] Although some marketplace lenders sell notes based on bundled loans, we analyze trading of notes based on individual loans. The investors in these notes, which are primarily institutions such as hedge funds, are therefore able to identify the borrower's state of residence.

the variables included in Table 2. As noted, we match the observations using nearest-neighbor matching without replacement.

4.   Methodology & Results

 This section presents our methodology and results. As described below, we find no evidence that borrowers engage in strategic default, nor that investors anticipate widespread strategic default. We do, however, find evidence that investors are aware of the decision, and that they discount a subset of loans because of the increased legal risk associated with the possibility that usury laws might invalidate those loans. Finally, we show that *Madden* caused a reduction in loan volume for the higher-risk borrowers most likely to have loans above usury caps.

A.   Strategic Default

As noted above, under the usury laws of New York and Connecticut, a lender has no legal right to collect interest or principal on a usurious loan. By suddenly activating those laws, thus, *Madden* gave borrowers an incentive to default on loans with rates above the usury limit. To test for strategic default, we create a variable called "Delinquent" that is equal to 1 if a borrower misses her payment for that month. If the borrower pays on time, Delinquent is set to 0.[22] We create the variable Delinquent for each loan starting one month after the loan is issued. For example, if a loan was originated in February and the borrower paid on time each month, the Delinquent variable

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[22] Due to data limitations, we can only determine whether a borrower missed a payment if the missing payment has not been remedied by the time we received the data in January 2016. If a borrower missed a payment but remedied the delinquency before we obtained our dataset, there will be no record of that missed payment. This data limitation affects all borrowers equally, and we have no reason to believe that it biases the interaction term in our difference-in-differences regressions. However, it does bias the coefficient on the *Post* variable. Because we obtained the data in January 2016, the borrowers were more likely to have remedied payments missed at the beginning of 2015 than at the end of 2015, causing the data to mechanically suggest that there were significantly more defaults following *Madden*. We thus caution that the significance on the *Post* variable should not be interpreted as an increase in defaults, as it is a mechanical effect of the data.

would be set to 0 for every month from March through December. We then conduct difference-in-differences regressions using only the sample of loans with interest rates above 16%—the usury cap in New York[23]—to determine whether borrowers in New York or Connecticut were relatively more likely to be delinquent based on trends among borrowers in the control groups.

Table 3 provides the results of these regressions. Panel A in Table 3 does not remove delinquent borrowers from the sample—for example, if a borrower first misses a payment in September, he will also show up as "Delinquent" in October, November, and December. Panel B, however, removes borrowers after the first missed payment. For example, a borrower who first misses a payment in September will not show up in the data in October, November, or December.[24] The first three columns in each panel include the full set of loans, and the final three columns are limited to loans issued before *Madden*.

The variable of interest is Post*NY_CT, which represents the interaction between Post-*Madden*, an indicator for the months after *Madden* was decided, and NY_CT, an indicator for whether the borrower resides in New York or Connecticut. Because we have repeat observations for the same loan, all standard errors are clustered by loan. All models control for the loan's interest rate, amount, and term, as well as the borrower's annual income, debt-to-income ratio, number of recent delinquencies, total credit availability, and years of employment at her current position. All control variables are based on the borrower and loan information at the time the borrower applied for the loan and do not update throughout the loan period. To address the significant heterogeneity in lending procedures among marketplace lenders, we add fixed effects for each lending platform.

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[23] Although the usury cap in Connecticut is 12%, we use the usury cap for New York because the number of loans issued to borrowers in New York dwarfs that issued to borrowers in Connecticut.

[24] The PSM samples are created using only the set of eligible observations. Note that the initial samples in Models (3) and (6) omitted loans with rates below 16%, and the sample in Model (6) further removed borrowers after the first missed payment.

[Insert Table 3 Here.]

Table 3 offers no evidence that borrowers have engaged in strategic delinquencies since *Madden*. Although generally positive, the coefficients on the variable of interest—the interaction term—are not significantly different from zero in any of the models. Moreover, in a series of unreported robustness tests, we conduct further analysis and are unable to find consistent evidence of strategic delinquencies. In particular, we look for greater rates of default (1) among more sophisticated borrowers who are more likely to be aware of the decision, (2) in ZIP codes with particular demographics, and (3) in clusters (i.e., whether people are more likely to default if others geographically close to them have defaulted). Despite the use of these different tests and subsamples, default as a whole remains low and we are unable to produce evidence that borrowers are strategically defaulting after *Madden*.[25]

On the one hand, we are surprised to find no evidence of strategic default. Prior work has found that consumers responded strategically to mortgage-modification opportunities offered following the recent financial crisis (Mayer et al., 2014), and the incentives to strategically default in this context appear to be more straightforward than the incentives to default on mortgages.[26] On the other hand, there are a number of reasons why consumers may not default. First, they may be unaware of the decision—even if, as we describe below, there is evidence that investors were aware of the decision.[27] Second, borrowers might not engage in strategic default because of non-

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[25] Among all of the models that we ran for robustness, only one—which included only borrowers with FICO scores below 700—provided evidence that borrowers were engaging in greater levels of default at statistically significant levels. However, this result was only significant at 10% and not robust to alternate specifications (e.g., different clustering or control samples), so we are not confident that the finding was more than a statistical fluke.

[26] The decision to default on an unsecured consumer loan involves far fewer complications than a decision to default on a mortgage. For example, the borrower need not be concerned about the many hardships of moving her home when contemplating default.

[27] We note that, in April 2016, a proposed class-action lawsuit seeking damages for usurious lending was filed on behalf of consumers who borrowed through the Lending Club platform, an event that may lead to more widespread consumer knowledge of *Madden* and its implications. See *Bethune v. Lending Club Corp. et al.*, No. 1:16-cv-02578-

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pecuniary factors. Guiso et al. (2013), for example, find that 82.3% of survey respondents indicated that it is morally wrong to walk away from a house when one can afford to pay the monthly mortgage. Third, borrowers may be concerned that their reputation (i.e., credit score) would suffer, despite the fact that it is unclear whether borrowers may be penalized by credit agencies for defaulting on a loan that is legally void. Finally, borrowers may be concerned that there will be future legal ramifications if they deliberately default on these loans. Not only are borrowers and lenders waiting for the courts to resolve the remaining questions raised by the case, but borrowers may be concerned that aggressive debt collectors will bring actions against them even if the loans are legally void, causing them to incur the costs of defending such actions.

B.   Secondary Market Trading

Next, we examine whether *Madden* affected secondary-market trading of notes backed by marketplace loans. If market participants expect *Madden* to have a persistent legal impact, we should see a decrease in the price of notes backed by above-cap loans to borrowers in states affected by *Madden* (or, conversely, an increase in the discount investors apply to such notes). Such a decrease would reflect an increase in the nonpayment risk associated with the loans that back such notes.

Using the trading data we obtained from these platforms, we begin by calculating the discount that investors apply to each note: that is, the difference between the price paid for the note and the value of the underlying loans if paid in full.[28] Following investors in this field, when a loan

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NRB (S.D.N.Y. April 6, 2016). Because our data extend only through the end of 2015, however, it is possible that consumers remained unaware of the decision during the period we study here.

[28] We calculate the spread as yield to maturity minus the loan's interest rate. The yield to maturity is calculated based on the investor's purchase price; that is, yield to maturity reflects the yield that will be earned if the note is paid in full. For example, if the amount an investor pays will yield a return of 10.30% (if paid in full) and the interest rate is 12%,

trades at a discount, we refer to that difference as the "spread." Such a discount reflects the market's perception that the projected payout is insufficient to compensate the debtholder fully for the time value of money plus the perceived nonpayment risk. Because of the risk that underlying loans may be uncollectible in New York and Connecticut after *Madden*, we expect that the spread for loans above usury caps will increase after the decision—reflecting purchasers' insistence that they be compensated for the legal risk created by the decision.

To test whether the spread significantly increased for notes backed by above-usury loans in New York and Connecticut, Table 4 below presents the results of a series of difference-in-differences regressions. As noted previously, notes traded on secondary markets can be backed either by "non-current" loans, where the borrower is late on her payments but has not yet defaulted, or by "current" loans, where the borrower is current on her payments. Because we expect that the effect of *Madden* will be most prominent for notes backed by non-current loans, where the risk of nonpayment is especially high, we analyze current and non-current loans separately. Panel A includes only notes backed by non-current loans, and Panel B includes only current loans.

Further, because we have no theoretical reason to expect that loans below usury caps traded at a greater discount after *Madden*, we separately analyze loans above and below usury caps. The table thus divides our sample into the set of loans with interest rates over 16% (the usury limit in New York), and the set of loans with interest rates under 16%. All models control for the principal outstanding, loan amount, loan age, ask price, loan duration, loan interest rate, the borrower's FICO score, and whether the loan underlying the note was issued within the previous fifteen

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the spread would be -1.70%. The spread on current loans is usually negative, reflecting that the investor expects to receive greater dollar value over the life of the loan than she is willing to pay for that loan today. By contrast, the spread on non-current loans is usually positive; the investors demand very high yield to maturity rates because they know the loans are likely to default. For example, an investor may require a non-current loan bearing an interest rate of 12% to have a yield of 20% (if paid in full). The spread in such an instance would be 8%, reflecting the high discount applied to the loan.

months. Because the ratio of current to non-current loans traded varies over our sample period—and across lending platform—we also control for the daily ratio of current to non-current loans traded on the platform in question. Fixed effects are included for the grade the lending platform originally assigned the loan, and standard errors are clustered by the borrower's state of residence.

[Insert Table 4 Here.]

Panel A of Table 4 provides evidence that *Madden* caused a subset of notes to trade at a discount. Model (1) in Panel A analyzes notes traded based on non-current loans above 16% and indicates that the spread for loans issued to borrowers in New York and Connecticut is approximately 0.23 higher than expected. To put this result in perspective, the mean (median) spread for non-current loans in our sample is 2.35 (1.29), and the standard deviation is 3.54. Notably, Model (2) indicates that, after *Madden*, loans with interest rates *below* New York's usury cap do not trade at a larger discount than expected. This finding gives us confidence that our results for above-cap loans are driven by *Madden*: since loans with interest rates below the cap would not have been directly affected by *Madden*, there is no theoretical reason to expect an increase in spreads for these loans. Models (3) and (4) use the PSM sample presented in Table 2 and show a similar trend.[29]

Finally, we note with interest that Panel B of Table 4 provides only limited evidence that *Madden* produced an increase in spreads on notes backed by *current* loans—that is, loans where the borrower is paying on time. Because Panel A of Table 4 shows that investors are aware of the *Madden* decision and its implications for their ability to collect these loans, we would predict a decrease in prices of current loans to borrowers in affected states, reflecting investors' expectation that some such loans may eventually enter default—and then, by reason of *Madden*, be

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[29] The interaction term in the models using states without usury caps as the control sample are positive but not statistically significant—perhaps due to the lower number of observations. We omit these models for concision.

uncollectible. Of course, we would expect this price decrease to be smaller than that observed for non-current loans, which generally present higher risks of nonpayment—but we would still expect to detect *some* decrease in the prices of notes backed by usurious loans in New York and Connecticut.

However, we only find evidence that these notes trade at a discount using the PSM sample—and, even then, the result is only significant at 10%. To put the coefficient of 0.00074 on the interaction term in perspective, the mean (median) spread on current loans is -0.018 (-0.0158). We interpret the limited significance here as evidence that investors are aware that the decision may hamper their ability to collect if a borrower defaults, but that they expect a marketplace borrower who is currently making her payments on time to continue making those payments. That is, because default rates tend to be very low and investors do not expect widespread strategical default, they apply only a minor discount to current loans.

C.   Credit Availability for Riskier Borrowers

Finally, a straightforward prediction is that *Madden* will reduce the volume of new loans issued with interest rates above usury limits. We find clear evidence of this result. For loans below New York's usury cap of 16%, growth in loan volume in New York and Connecticut after *Madden* was statistically comparable to growth outside the Second Circuit. In New York and Connecticut, the number of loans issued with rates below 16% increased by 97% (from 16,683 to 32,937). Outside the Second Circuit, we find a similar increase of 95% (from 158,288 to 308,855). These growth rates do not differ at statistically significant levels (t=1.18).

By contrast, growth rates for loans above New York's usury cap of 16% are highly disparate. Outside the Second Circuit, the number of loans issued with rates above 16% increased

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by 125% (from 124,340 to 280,313). In New York and Connecticut, however, growth was just 65% (from 7,537 to 12,425). This relatively lower growth rate in New York and Connecticut is highly statistically significant (t=-20.96).

A visual portrayal of this evidence is presented in Figure 2 below, which provides histograms showing the distribution of interest rates in New York and Connecticut—and from all states outside the Second Circuit—before and after *Madden*. Although far fewer loans were issued with relatively high interest rates in New York and Connecticut after *Madden*, we see the opposite trend in other states, where there was significant growth in loans with higher rates.

[Insert Figure 2 Here.]

This leads us to question why fewer loans with high rates were issued in New York and Connecticut. Two explanations seem plausible. It could be that there were changes in the composition of borrowers (i.e., less credit was extended to riskier borrowers more likely to borrow above usury caps). Or it may be that lenders lowered the interest rate they demanded for loans in New York and Connecticut (i.e., lenders were willing to issue the same loan for a lower price).[30]

i.   *Madden*'s Effect on Marketplace Borrower Credit Quality

To distinguish between these possibilities, we start by examining average marketplace borrower quality at a summary level. Table 5 below provides descriptive statistics for the borrowers in our sample in New York and Connecticut, non-Second Circuit jurisdictions, and in states with no usury cap both before and after *Madden*.

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[30] In theory, lenders might respond to a perceived inadequacy of the legally enforceable interest rate by demanding collateral and shortening terms on loans to high-risk borrowers. (Empirically, prior work has demonstrated that lenders in the auto-lending context have made such adjustments (Melzer and Schroeder, 2015)). On the marketplace lending platforms we study, however, lenders have no mechanism for securing loans, and loan terms are largely standardized as a matter of the platforms' policies. We therefore do not expect that adjustments along these dimensions will mitigate the impact of *Madden* on the volume of loans to higher-risk borrowers, at least during the period analyzed here.

[Insert Table 5 Here.]

Table 5 suggests that borrower quality increased in New York and Connecticut in the wake of *Madden*. We note, for example, that the average marketplace borrower's annual income rose significantly in these states, but that there was no corresponding statistically significant increase in other jurisdictions. And we see a much larger increase in the average borrower's FICO score in New York and Connecticut than in either control group.

We test this trend more formally in Table 6 below, which presents a difference-in-differences regression analysis examining the relative change in credit quality, as measured by FICO score, for borrowers in New York and Connecticut after *Madden*.

[Insert Table 6 Here.]

Table 6 shows that, after *Madden,* average credit scores for borrowers in New York and Connecticut rose significantly relative to borrowers in either control group.[31] In particular, average FICO scores for borrowers in these states increased by roughly 2.6 to 3.0 FICO points more than would have been expected based on the trend for borrowers in other circuits over this same period. All models control for the variables in Table 1: the loan's interest rate, amount, and term, as well as the borrower's annual income, debt-to-income ratio, number of recent delinquencies, total credit availability, and years of employment at her current position. As before, we include fixed effects for each lending platform, and standard errors are clustered by the borrower's state of residence.

To further investigate this increase in average FICO scores, we assign borrowers to buckets based on FICO score and examine the growth in loan volume by bucket. The results, presented in

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[31] We do not include a PSM sample in this analysis because we are attempting to capture the differences in new loan originations after *Madden*. Creating a matched sample would obfuscate these differences by forcing us to match only similar loans—and dropping the unpaired, dissimilar loans. The matching procedure would thus eliminate the relative differences that we intend to capture. For example, a low-FICO score borrower from outside the Second Circuit would likely not have a match in New York or Connecticut because the low-FICO score borrowers in these states disappeared.

Figure 3, provide summary evidence that the increase in FICO scores was caused by a decline in loan volume to lower quality borrowers.

[Insert Figure 3 Here.]

Figure 3 shows that, outside the Second Circuit, loan volume for borrowers in all FICO buckets increased substantially after *Madden*—and that a significant portion of the growth was driven by lower-quality borrowers. In New York and Connecticut, however, growth after *Madden* appears comparable to other circuits only for borrowers with FICO scores over 700. For borrowers with FICO scores under 700, Figure 3 shows, growth in New York and Connecticut appeared to lag behind growth in other circuits. The pattern is most obvious for the very lowest-quality borrowers—those with FICO scores below 625. Outside the Second Circuit, loan volume for these borrowers after *Madden* grew by 124% (that is, loan volume in absolute numbers more than doubled). By contrast, the same statistic for borrowers in New York and Connecticut was negative 52%—meaning that, in absolute numbers, loan volume to these borrowers *declined* after *Madden*.

We see a similar trend when we plot the distribution of FICO scores before and after *Madden* in Figure 4.[32] The first set of histograms include all non-Second Circuit borrowers and show a relative increase in borrowers with FICO scores below 670 after the Madden decision. This is consistent with anecdotal evidence that marketplace lending was growing among these borrowers. The next set of histograms include only borrowers in New York and Connecticut and show a different trend. If anything, loans to riskier borrowers appeared to decline; after *Madden*, loans to borrowers with FICO scores below 644 virtually disappeared.

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[32] All histograms use a bin width of four FICO points.

Our findings that the imposition of usury caps reduced credit availability for higher-risk borrowers are consistent with prior work on usury laws.[33] Benmelech and Moskowitz (2010), for example, study the usury laws in place in the United States during the 19th century and find a negative relationship between credit availability and usury thresholds. Similarly, Goudzwaard (1968), Shay (1970), Greer (1974), Rigby (2013), and Melzer and Schroeder (2015) each found evidence suggesting that state usury statutes constrain credit and affect lending. However, most of these studies rely on associations, whereas we provide evidence on the effects of usury laws in a more tightly-identified setting.

ii.   Lower interest rates for comparable loans

In addition to a decrease in loan volume, it is possible that interest rates decreased because lenders reduced pricing in these states. If the loans are not already priced as cheaply as possible—that is, the lender earns rents—it may be more profitable for a lender to lower its rates than to fail to issue a loan. We test for evidence that pricing changed using a difference-in-differences model in which the dependent variable is the interest rate. Despite our use of various specifications—we test for differences in rates relative to other states and relative to loans previously issued in New York and Connecticut—we are unable to find any evidence that prices decreased in New York and Connecticut. Although we omit the tables for concision, we note that our finding that pricing did not change is consistent with conversations with marketplace lenders.

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[33] We note that our finding that marketplace-loan volume for these borrowers decreased does not necessarily imply that these consumers were unable to borrow altogether. It is possible, for example, that these borrowers chose to substitute into slightly higher-cost sources of credit, such as credit cards.

iii.   The market's incorporation of *Madden*

One final question is why there are any loans issued above 16% in New York and Connecticut after *Madden*. There are several possible explanations. First, the marketplace lenders told us that it took several months to respond to the decision. Some market participants indicated that they weren't aware of the decision until weeks or even months after it was issued. Moreover, even after the potential effects of *Madden* became clear, the decision was such a surprise that investors and their counsel needed time to make corresponding changes in their business practices. Consistent with this anecdotal evidence, we find that most of the loans above usury caps in New York and Connecticut were issued shortly after *Madden*—indeed, we observe *zero* loans to borrowers with credit scores below 625 in New York or Connecticut after July 2015.

Second, some of the platforms made innovative legal changes that they hoped could negate *Madden*. For example, in February 2016, the only public marketplace lender, Lending Club, arranged for the originating bank to hold a very small portion of all Lending Club loans in the hopes that this practice would circumvent *Madden* by allowing Lending Club to argue that its loans are not entirely in the hands of nonbank investors (Demos & Rudegeair, 2016). Some lenders likely felt comfortable issuing loans above usury caps because they believed these types of changes would protect them.

D.   Robustness

For a difference-in-differences analysis to produce a valid estimate of the treatment effect, the treatment and control samples need not be identical, but the difference between the two groups should be consistent prior to the shock examined. Our primary analyses attempt to address potential concerns regarding the differences between the treatment and control groups through

multiple control groups, but in this section we also examine the monthly trends in our regression variables.

To compare the difference between the treatment and control samples over time, Figure 5 plots the coefficients on monthly indicators from three regressions in which the dependent variable is the borrower's FICO score. The indicators reflect the month in which the loan was issued. The first regression uses only borrowers from New York and Connecticut; the second regression includes only borrowers from outside the Second Circuit; and the third regression includes only borrowers from states with no usury limits. The regression specification is the same as that presented in Table 6, except that we replace the prior variables of interest—NY_CT, Post Madden, and the resulting interaction term—with monthly indicator variables.

[Insert Figure 5 Here.]

Although FICO scores for borrowers in New York and Connecticut are consistently higher than FICO scores in other jurisdictions throughout the year, that difference is roughly constant until September, at which point average FICO scores in New York and Connecticut significantly increase relative to both control groups. This is consistent with anecdotal evidence that it took several months for the full impact of *Madden* to reach markets. In this regard, market participants appear to take longer to adapt to changes in common law than to changes in statutory law. Many studies find an immediate effect of a change in statutory law, perhaps because the parties are aware of the statute in advance and anticipate its enactment.

Figure 6 presents a similar analysis for the trading spread on notes backed by non-current loans in the treatment and control samples. As in Figure 5, we plot the coefficients on monthly indicators from February through December for three separate regressions. The indicators reflect the month in which the trade on the relevant note occurred. The regression specification is the

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same as that presented in Table 4, except that we replace the prior variables of interest—NY_CT, Post Madden, and the resulting interaction term—with the monthly indicator variables.

[Insert Figure 6 Here.]

Like Figure 5, Figure 6 indicates that it took several months for the full effect of *Madden* to materialize. Although the spread on notes backed by loans in New York and Connecticut is generally slightly higher than the spread on notes backed by loans outside the Second Circuit before *Madden*, the figure shows that spread increased significantly in August and, with the exception of October, remained significantly higher thereafter.

5.   Conclusion

Using proprietary data from three marketplace lenders, we study a surprising judicial decision that activated state usury laws for a subset of loans issued to borrowers in Connecticut, New York, and Vermont. Because usurious loans are void in New York and Connecticut, our setting allows us to test how consumers and investors respond when loans are plausibly void as a matter of law. We find no evidence that borrowers strategically defaulted on these loans, suggesting that legal enforceability does not drive consumer default. Further, although we find that investors priced the increased legal risk created by the decision, our evidence suggests that investors did not expect widespread default. Nonetheless, the evidence shows that investors declined to issue loans to higher-risk borrowers in affected states—indicating that the imposition of usury caps reduced credit availability. Taken together, these findings shed light on the influence of common law on consumer lending, and how market participants can be expected to respond to changes in that law.

Table 1. Descriptive Statistics: Loan and Borrower Characteristics. This table presents mean characteristics for the loans and borrowers in our sample. Panel A compares loan characteristics for loans issued to borrowers in New York and Connecticut with characteristics for all loans issued to borrowers outside the Second Circuit. Panel B compares loan characteristics for loans issued to borrowers in New York and Connecticut with characteristics for all loans issued to borrowers who reside in states that lack usury caps. Panel C compares loan characteristics for loans issued to borrowers in New York and Connecticut with characteristics for loans in the propensity score matched sample used in Table 3. Loan Amount reflects the dollar value of the loan. Term represents the loan's duration and is expressed in months. Interest Rate reflects the annual percentage rate charged to the borrower. Annual Income represents the borrower's annual income. Debt-to-Income reflects the borrower's total monthly debt payments, excluding the requested loan and any mortgage payments, divided by the borrower's monthly income and is expressed in percentage terms. Delinquencies reflects the number of recent delinquencies in the borrower's credit file. Available Credit reflects the borrower's total revolving credit balance. Employment represents the number of years the borrower has been employed at her current position. FICO Score reflects the midpoint of the borrower's four-point FICO range. All values are presented at the mean.

	Panel A: Full Sample			Panel B: No Usury States			Panel C: PSM		
	NY & CT	Other Circuits	*t-test*	NY & CT	No Usury States	*t-test*	NY & CT	PSM	*t-test*
Loan Amount	14,206	12,598	*-49.10*	14,206	12,695	*-33.13*	14,531	15,209	*12.52*
Term (Months)	43.26	43.65	*8.82*	43.26	43.88	*10.30*	48.82	49.49	*9.12*
Interest Rate	13.80%	18.58%	*123.73*	13.80%	18.56%	*109.66*	19.82%	19.73%	*-4.74*
Annual Income	77,714	65,821	*-14.32*	77,714	65,694	*-28.12*	69,866	69,691	*-0.21*
Debt-to-Income	19.39%	24.65%	*-45.52*	19.39%	25.36%	*-45.40*	22.50%	24.12%	*23.50*
Delinquencies	0.31	0.25	*-20.12*	0.31	0.24	*-14.37*	0.41	0.43	*2.82*
Available Credit	19,138	14,894	*-44.13*	19,138	15,345	*-24.29*	16,898	15,959	*-5.79*
Employment (Years)	7.11	5.32	*-69.39*	7.11	5.38	*-48.15*	7.26	7.06	*-5.25*
FICO Score	696.22	682.82	*-87.60*	696.22	682.92	*-67.41*	680.33	680.71	*2.66*
Num. Obs.	*66,437*	*841,446*		*66,437*	*63,942*		52,562	52,562	

Table 2. Descriptive Statistics: Characteristics of Notes Underlying Trades. This table presents descriptive statistics for notes traded on the secondary-market exchanges run by the marketplace platforms in our sample. Panel A presents characteristics for the full sample of notes traded, and Panel B presents characteristics for notes traded that are included in the propensity score matched sample. Each panel separately presents characteristics for notes backed by loans to borrowers who are no longer current on their payments and for notes backed by loans that have been issued to borrowers who are current on their payments. Principal Outstanding reflects the outstanding principal on the loan at the time the note was bought. Loan Amount is the total value of the loan underlying each note. FICO Score reflects the midpoint of the borrower's four-point FICO range. Ask Price reflects the amount the purchaser paid for the note. Loan Duration reflects the number of months the underlying loan was outstanding and is expressed in months. Loan Age reflects the number of months between the time that the underlying loan was issued and the time that the trade of the note was executed. Interest Rate reflects the interest rate on the loan underlying the note. Fifteen is a dummy variable reflecting whether the loan underlying the note was issued within the previous fifteen months. All values are presented at the mean.

Panel A: Full Sample

	Notes Backed by Non-Current Loans				Notes Backed by Current Loans		
	NY & CT	Other Circuits	t-score		NY & CT	Other Circuits	t-score
	(1)	(2)			(3)	(4)	
Principal Outstanding	30.73	31.15	0.53	Principal Outstanding	33.23	33.62	1.54
Loan Amount	20,169	20,506	3.60	Loan Amount	19,736	20,008	10.00
FICO Score	690	689	-0.14	FICO Score	695	694	-6.03
Ask Price	13.53	13.76	0.32	Ask Price	33.60	34.00	1.56
Loan Duration	50.06	50.68	5.16	Loan Duration	47.93	48.43	14.38
Loan Age	16.94	16.28	-6.30	Loan Age	14.24	13.75	-16.69
Interest Rate	19%	19%	0.84	Interest Rate	17%	17%	-7.59
Fifteen	0.51	0.48	-4.99	Fifteen	0.41	0.40	-10.87
Num. Obs.	*10,543*	*84,675*		*Num. Obs.*	*130,092*	*1,226,167*	

Panel B: PSM Sample

	Notes Backed by Non-Current Loans				Notes Backed by Current Loans		
	NY & CT	PSM	t-score		NY & CT	PSM	t-score
	(1)	(2)			(3)	(4)	
Principal Outstanding	30.73	31.01	0.29	Principal Outstanding	33.19	33.58	1.11
Loan Amount	20,169	20,008	-1.29	Loan Amount	19,723	19,658	-1.78
FICO Score	690	690	1.42	FICO Score	694	695	4.03
Ask Price	13.53	13.84	0.36	Ask Price	33.59	33.98	1.08
Loan Duration	50.06	50.21	0.94	Loan Duration	48.59	48.11	9.95
Loan Age	16.94	17.01	0.48	Loan Age	14.49	14.60	2.52
Interest Rate	19%	18%	-3.15	Interest Rate	17%	17%	-7.59
Fifteen	0.51	0.51	-0.19	Fifteen	0.42	0.42	0.35
Num. Obs.	*10,543*	*10,543*		*Num. Obs.*	*124,000*	*124,000*	

Table 3. Difference-in-Differences Results: Change in Borrower Delinquencies Post-*Madden*. The table below presents the change in borrower delinquencies for loans issued to borrowers in New York and Connecticut relative to delinquencies for loans issued to borrowers in other jurisdictions after *Madden*. The dependent variable, Delinquent, is set to 1 if the borrower missed her payment for that month and is otherwise set to 0. Panels A includes all monthly defaults, and Panel B includes only through the borrower's initial delinquency. The first three columns in each panel include the full set of loans, whereas the final three columns are limited to loans issued before *Madden*. In each panel, Model (1) compares borrowers in New York and Connecticut relative to borrowers in all other jurisdictions; Model (2) compares borrowers located in New York and Connecticut relative to borrowers located in states with no usury cap; and Model (3) uses only the PSM matched sample. All regressions are limited to loans for which the borrower's annual percentage rate is greater than 16%, New York's usury cap. The analysis is presented using logit, but in unreported analysis we find consistent results using OLS and probit models. All regressions control for the loan's interest rate, amount, and term, as well as the borrower's income, debt-to-income ratio, number of recent delinquencies, total credit availability, and months of employment at her current position. Fixed effects are included for each marketplace lending platform. Standard errors are clustered by loan, and statistical significance of 10%, 5%, and 1% is indicated by *, **, and ***, respectively.

$$Delinquent = a + \beta_1 Post\text{-}Madden + \beta_2 NY\_CT + \beta_3 Post^*NY\_CT + Controls + \varepsilon$$

Panel A: All Monthly Defaults

	All Loans in Sample			Loans Issued before *Madden*		
	Full Sample (1)	No Usury State (2)	PSM Sample (3)	Full Sample (1)	No Usury State (2)	PSM Sample (3)
Post *Madden*	1.67***	1.27***	1.65***	1.92***	1.51***	2.03***
	(0.10)	(0.27)	(0.37)	(0.10)	(0.27)	(0.37)
NY_CT	0.12	-0.36	0.20	0.13	-0.36	-0.27
	(0.31)	(0.41)	(0.48)	(0.31)	(0.41)	(0.53)
Post * NY_CT	0.02	0.45	0.042	0.07	0.50	0.38
	(0.29)	(0.39)	(0.46)	(0.29)	(0.38)	(0.52)
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	551,584	92,331	105,047	354,203	38,365	58,591

Panel B: Through Initial Default

	All Loans in Sample			Loans Issued before *Madden*		
	Full Sample (1)	No Usury State (2)	PSM Sample (3)	Full Sample (1)	No Usury State (2)	PSM Sample (3)
Post *Madden*	0.87***	0.54**	1.63***	0.993***	0.645**	1.773***
	(0.10)	(0.28)	(0.37)	(0.0954)	(0.280)	(0.336)
NY_CT	0.20	-0.22	0.04	0.199	-0.216	0.0830
	(0.28)	(0.38)	(0.48)	(0.283)	(0.378)	(0.472)
Post * NY_CT	-0.08	0.26	0.17	-0.0170	0.347	-0.618
	(0.29)	(0.39)	(0.46)	(0.291)	(0.393)	(0.480)
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	544,559	92,331	104,491	347,993	37,226	57,589

Table 4. Difference-in-Differences Results: Change in Secondary Market Trading Post-*Madden*. The table below presents the trading discount applied to loans issued to borrowers in New York and Connecticut relative to loans issued to borrowers in other jurisdictions. The dependent variable reflects the "spread," defined as the total return the purchaser will receive on the note if the loan is paid in full minus the return the seller received for the loan. Higher spreads indicate greater discounts. Panel A uses only non-current loans (i.e., loans for which the borrower is late on her payments), and Panel B uses only current loans. In each panel, Models (1) and (2) compare borrowers in New York and Connecticut relative to borrowers outside the Second Circuit, and Models (3) and (4) use only the PSM matched sample. The odd-numbered columns include only notes based on loans with interest rates above 16% (the usury cap in New York), and the even-numbered columns include only notes based on loans with interest rates below 16%. All regressions control for the principal outstanding, accrued interest, loan age, loan term, the borrower's FICO score, whether the loan was issued in the past fifteen months, and the daily ratio of current loans relative to non-current loans traded on the platform. Fixed effects for the grade the lending platform originally assigned the loan are also included. Standard errors are clustered by the borrower's state, and statistical significance of 10%, 5%, and 1% is indicated by *, **, and ***, respectively.

$$Spread = a + \beta_1 Post\text{-}Madden + \beta_2 NY\_CT + \beta_3 Post^*NY\_CT + Controls + \varepsilon$$

	Panel A: Non-Current Loans				Panel B: Current Loans			
	Full Sample		PSM Sample		Full Sample		PSM Sample	
	Above	Below	Above	Below	Above	Below	Above	Below
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Post *Madden*	-0.099	0.063	-0.042	0.026	0.0003*	0.0004**	-0.0003	0.00
	-0.071	-0.132	-0.096	-0.148	-0.0002	-0.0002	0.00	0.00
NY_CT	-0.111***	0.001	-0.123	0.048	0.004***	0.003***	0.0038***	0.002***
	-0.04	-0.0769	-0.083	-0.159	-0.0002	-0.0002	-0.0004	-0.0003
Post*NY_CT	0.230**	-0.113	0.263**	-0.061	0.0002	-0.0003	0.00074*	-0.0003
	-0.097	-0.229	-0.116	-0.235	-0.0003	-0.0002	-0.0004	-0.0004
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	66,912	28,306	14,666	6,420	707,690	648,569	129,963	118,037
R-squared	0.063	0.115	0.064	0.136	0.145	0.086	0.142	0.16

Table 5. Descriptive Statistics: Borrower and Loan Characteristics Before and After *Madden*. This table below presents descriptive statistics for loans that were originated before and after *Madden*. Panel A reflects only loans to borrowers located in New York and Connecticut; Panel B reflects only loans to borrowers located outside of the Second Circuit; and Panel C reflects only loans to borrowers located in states without usury limits. All variables are as defined in Table 1, and all values are presented at the mean.

	Panel A: New York & Connecticut			Panel B: Other Circuits			Panel C: No Usury States		
	Before *Madden*	After *Madden*	t-score	Before *Madden*	After *Madden*	t-score	Before *Madden*	After *Madden*	t-score
Loan Amount	13,983	14,325	5.08	12,529	12,631	5.37	12,472	12,809	4.92
Term (Months)	43.55	43.11	-4.97	43.76	43.60	-6.40	44.03	43.81	-2.40
Interest Rate	14.38%	13.49%	-19.89	18.53%	18.60%	2.79	18.82%	18.43%	-4.81
Annual Income	75,510	78,891	4.82	66,144	65,666	-0.96	65,229	65,932	1.27
Debt-to-Income	18.19%	20.03%	20.11	24.55%	24.70%	3.08	25.61%	25.23%	-3.04
Delinquencies	0.307	0.314	0.98	0.26	0.24	-10.09	0.25	0.24	-1.90
Available Credit	18,338	19,566	4.92	14,738	14,969	4.27	14,725	15,663	4.49
Employment (Years)	6.50	7.44	17.70	5.25	5.36	7.03	5.12	5.52	7.52
FICO Score	693.57	697.64	15.37	682.76	682.85	1.03	681.81	683.49	5.21
Num. Obs.	*24,220*	*45,362*		*282,628*	*589,168*		*22,467*	*43,811*	

Table 6. Difference-in-Differences Results: Change in Borrower FICO Scores Post-*Madden*. The table below presents the change in FICO scores for borrowers in New York and Connecticut relative to the change in FICO scores for borrowers in the control samples after *Madden*. Model (1) uses all borrowers outside the Second Circuit as the control sample; and Model (2) uses only borrowers from states without usury caps as the control sample. All regressions control for the loan's interest rate, amount, and term, as well as the borrower's income, debt-to-income ratio, number of recent delinquencies, total credit availability, and months of employment at her current position. Fixed effects are included for each marketplace lending platform. Standard errors are clustered by the borrower's state of residence, and statistical significance of 10%, 5%, and 1% is indicated by *, **, and ***, respectively.

$$FICO\ Score = \alpha + \beta_1 Post\text{-}Madden + \beta_2 NY\_CT + \beta_3 Post^*NY\_CT + Controls + \varepsilon$$

	Full Sample (1)	No Usury States (2)
Post *Madden*	-0.785***	-0.287
	(0.221)	(0.540)
NY_CT	-0.254	0.195
	(0.405)	(0.733)
Post*NY_CT	3.040***	2.627***
	(0.252)	(0.574)
Controls	Yes	Yes
Lender FE	Yes	Yes
Observations	907,883	130,379
R-squared	0.520	0.457

Figure 1. Summary Statistics: Value of Loans Originated by Three Marketplace Platforms in 2015. The figure below presents the value of all loans originated by the three lending platforms in our study in each month of 2015. The trend line, which is plotted on the figure, reflects the growth in the industry.
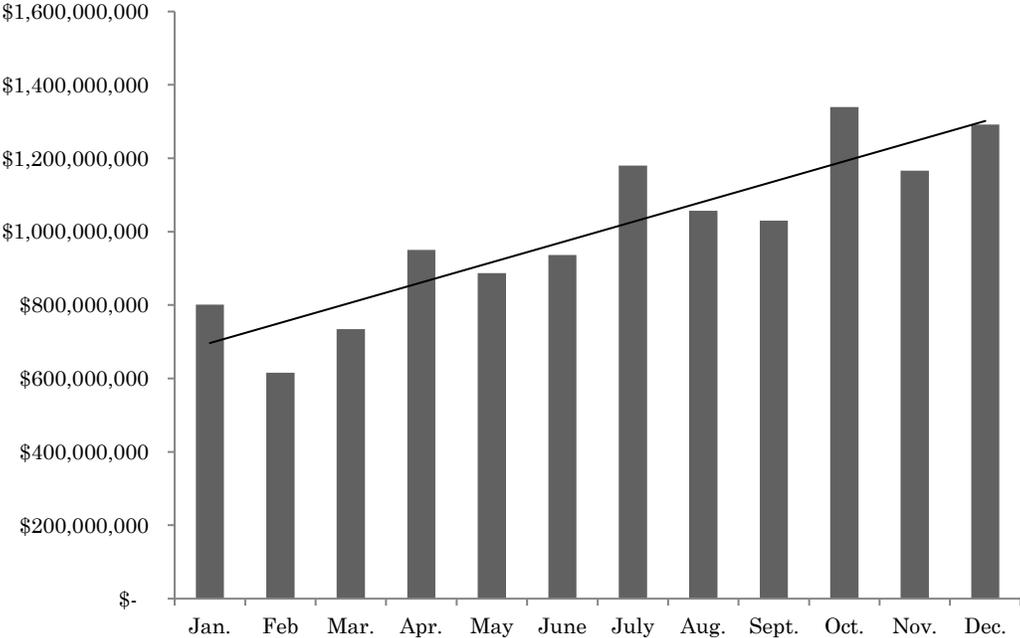
Figure 2. Summary Statistics: Distribution of Interest Rates Before and After *Madden*. The histograms below present the distribution of interest rates for all borrowers who were issued loans in the marketplace data we study. The first set of histograms include all borrowers from outside the Second Circuit and show a relative increase in interest rates after the *Madden* decision. The next set of histograms include all borrowers in New York and Connecticut and show a different trend—if anything, the percentage of loans issued at the highest interest rates appears to decrease. All histograms use a bin size of four percentage points.

Borrowers Outside the Second Circuit
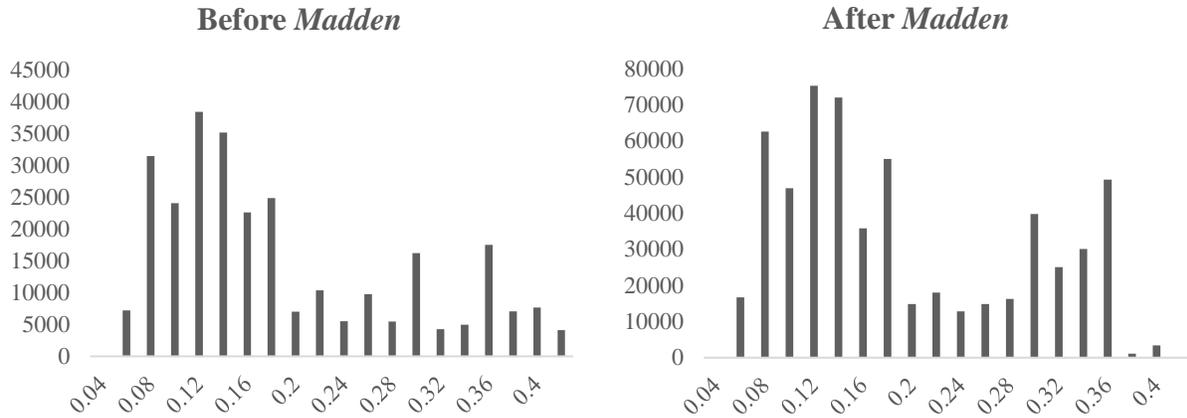
Borrowers in New York and Connecticut
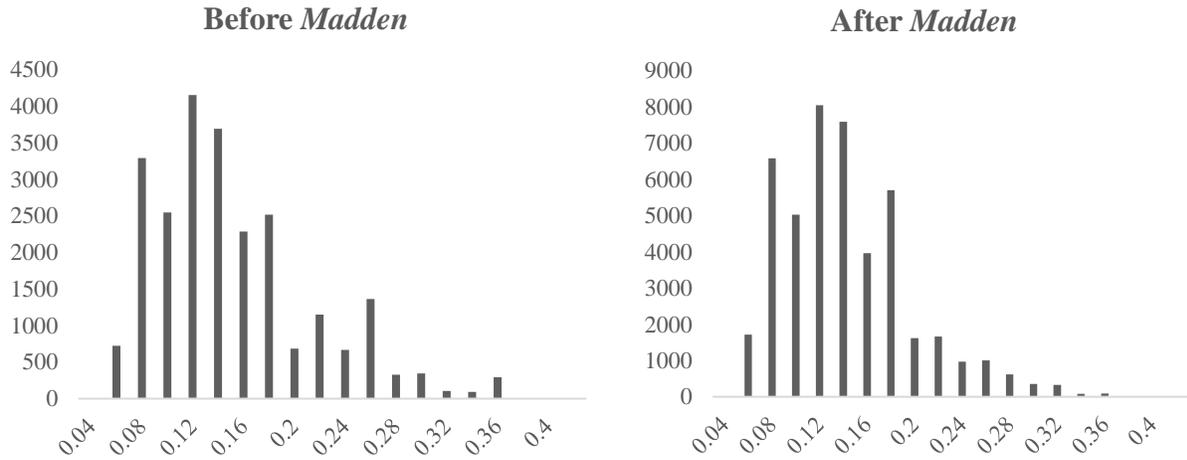
Figure 3. Summary Statistics: Growth in Loan Volume Post-*Madden*. The figure below shows the growth in loan volume for loans issued to borrowers in New York and Connecticut relative to loans issued to borrowers outside the Second Circuit. The borrowers are broken down into buckets by FICO score, and the sample includes all loans issued during calendar year 2015 (i.e., the "before" period includes all loans issued in 2015 before *Madden*, and the "after" period includes all loans issued in 2015 after *Madden*). The figure shows that growth rates for loans issued to borrowers in New York and Connecticut are roughly comparable to loan volume nationwide for high-quality borrowers, but that growth in loans was dampened—or even declined—for lower-quality borrowers.
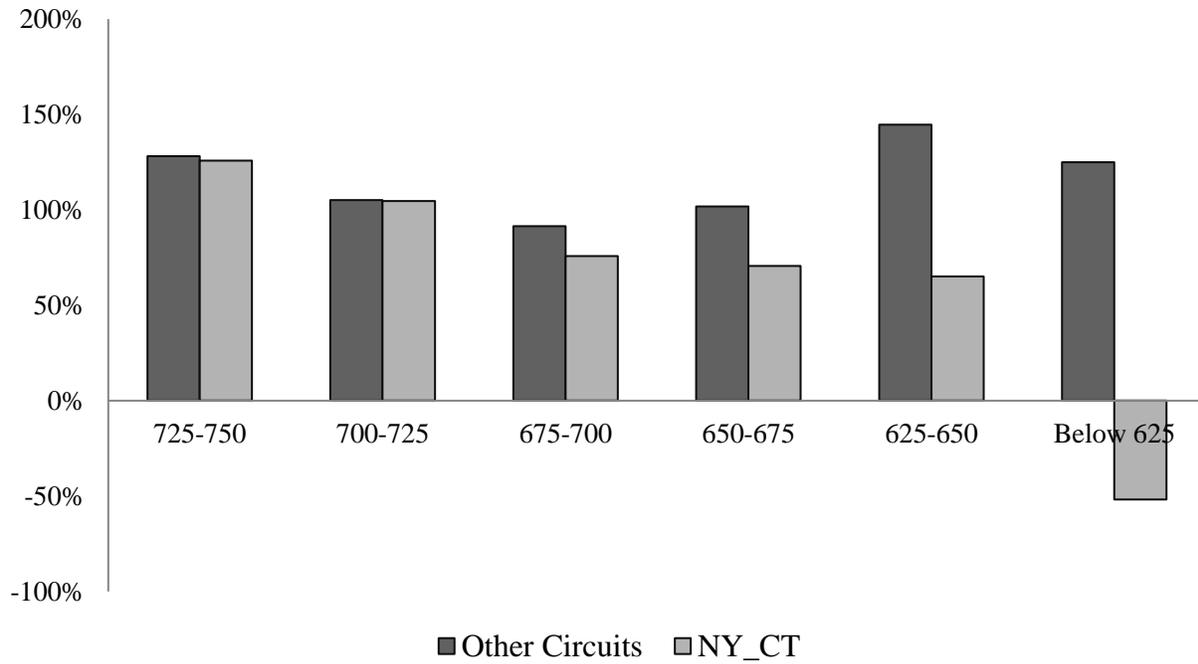
Figure 4. Summary Statistics: Distribution of FICO Scores Before and After *Madden*. The histograms below present the distribution of FICO scores for all borrowers who were issued loans in the marketplace data we study. The first set of histograms include all borrowers from outside the Second Circuit and show a relative increase in borrowers with FICO scores below 670 after the *Madden* decision. The next set of histograms include all borrowers in New York and Connecticut and show a different trend—if anything, loans to riskier borrowers appeared to decline, as loans to borrowers with FICO scores below 644 were virtually nonexistent. All histograms use a bin size of four FICO points.

Borrowers Outside the Second Circuit
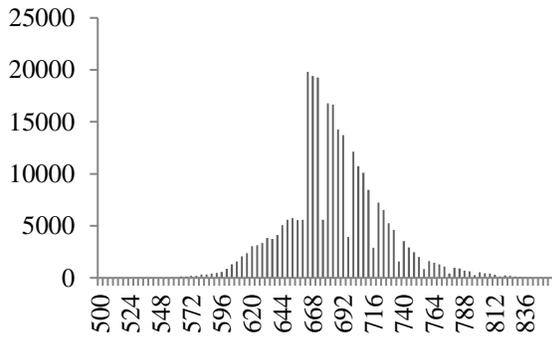
Before *Madden*

After *Madden*
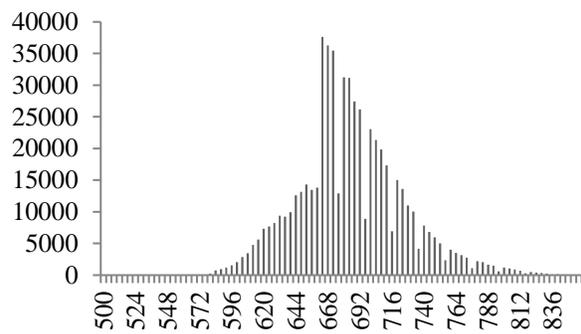
Borrowers in New York and Connecticut

Before *Madden*
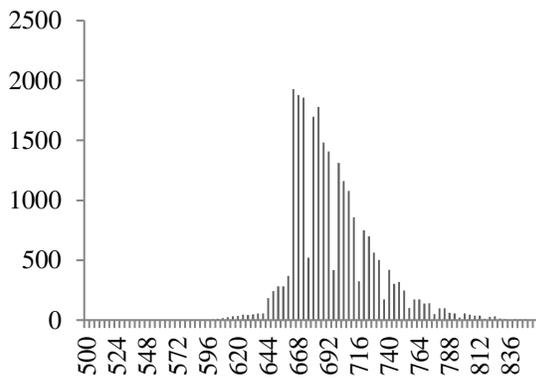
After *Madden*
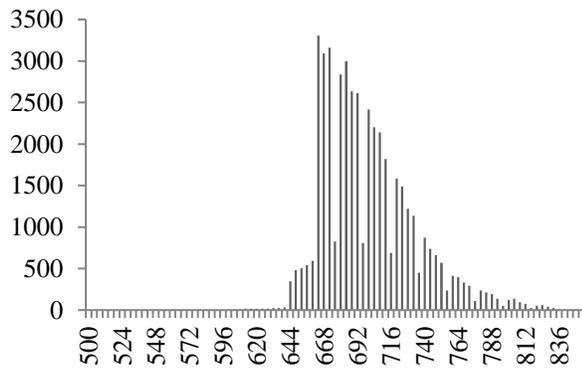
Figure 5. Coefficients on Monthly Indicator Variables: FICO Score by Month. The figure below presents the coefficients on monthly indicators from three separate regressions. The first regression includes only borrowers located in New York and Connecticut, the second regression includes only borrowers located outside of the Second Circuit, and the third regression includes only borrowers located in states that lack usury limits. The regression specification is the same as that presented in Table 6, except that we replace the prior variables of interest (NY_CT, Post *Madden*, and the resulting interaction term) with dummy variables for each month from February through December. The monthly indicators reflect the month when the loan was issued.
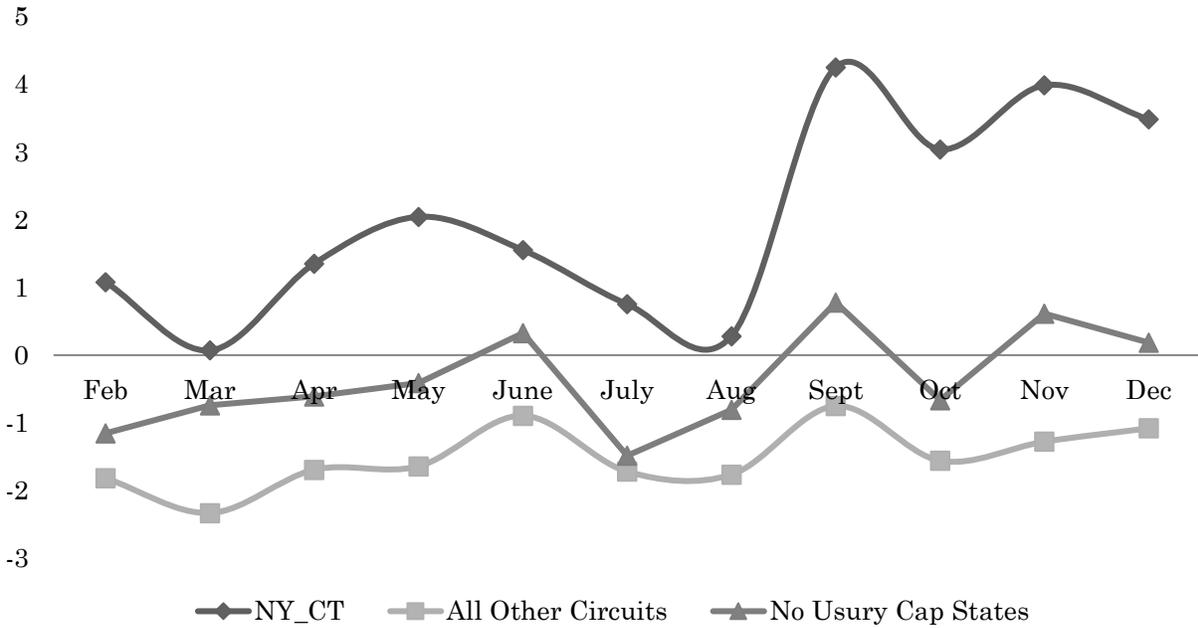
Figure 6. Coefficients on Monthly Indicators: Discount on Non-Current Loans. The figure below presents the coefficients on monthly indicators from three separate regressions. The first regression includes only notes traded based on loans to borrowers located in New York and Connecticut, the second regression includes only notes traded based on loans to borrowers located outside of the Second Circuit, and the third regression includes only the notes included in the PSM control group. The regression specification is the same as that presented in Table 4, except that we replace the prior variables of interest (NY_CT, Post *Madden*, and the resulting interaction term) with dummy variables for each month from February through December. The monthly indicators reflect the month in which the trade occurred. Only notes based on non-current loans (i.e., loans for which the borrower is not current on her payments) are included.
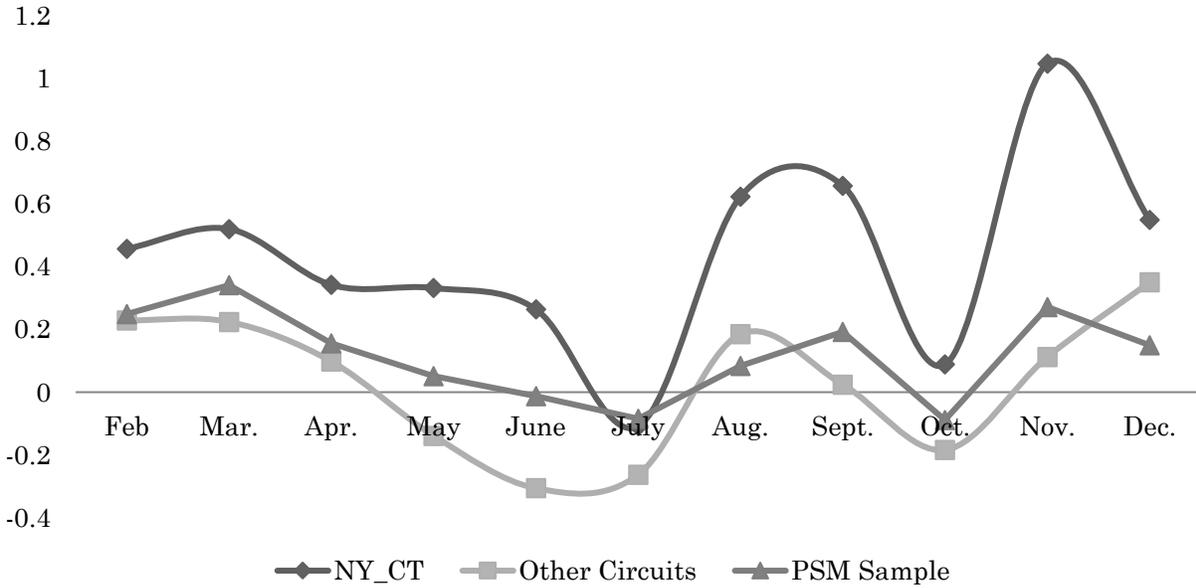
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APPENDIX C

The Real Effects of Financial Technology: Marketplace Lending and Personal Bankruptcy[*]

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ABSTRACT

We examine how financial technology affects household hardship in terms of personal bankruptcy. We exploit an exogenous source of variation in marketplace lending, a court verdict that renders any above-usury loans issued by banks to Connecticut and New York residents null and void if the loans are sold outright to non-banks. We document a persistent rise in personal bankruptcies following the verdict and a severe decline in marketplace lending, particularly among low-income households. Marketplace loan defaults and consumer credit by banks and finance companies remain unaffected, suggesting that increases in personal bankruptcy arise principally from reversing access to new lending technology.

I. 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. The 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. There is a concern that increasing the availability of credit will push individuals to over-indebtedness, default and bankruptcy. In this paper, we investigate the effect of new financial technology on personal bankruptcy focusing on a relatively new type of credit, marketplace loans.

A marketplace loan is a form of fixed-rate unsecured consumer debt issued by an online lending platform connecting borrowers with investors. Investors supply funds directly to borrowers via the platform. Alternatively marketplace lenders may partner with a bank to originate loans.[1] As of 2017, $21 billion in marketplace loans are outstanding in the U.S.[2] Marketplace loans are predominantly used for debt consolidation, small businesses, mortgage and education financing, as well as medical expenses, and are an important source of funds to previously credit rationed borrowers (De Roure, Pelizzon, and Tasca, 2016; Jagtiani and Lemieux, 2017; Schweitzer and Barkley, 2017). Marketplace credit is granted more quickly than traditional forms of finance (Fuster, Plosser, Schnabl and Vickery, 2018) and on average marketplace borrowers enjoy a lower cost of debt refinancing, particularly credit card debt (Balyuk, 2017).

Fintech lending offers potential benefits and risks for households in terms of affecting personal bankruptcy. Increasing credit card borrowing, as well as unforeseen income shocks and medical bills, are among the main determinants of personal bankruptcy (Domowitz and Sartain, 1999; White, 2007; Gross and Notowidigdo, 2011). To the extent that individuals prefer to avoid bankruptcy, rather than default strategically to discharge debt, marketplace lending has the potential to lower debt refinancing costs and provide households with liquidity in the face of income or expenses shocks, thus reducing the incidence of bankruptcy. However, the rapid expansion of marketplace credit, on the other hand, may increase the number of bankruptcy cases by increasing consumer debt (Gross and Souleles, 2002; Fay, Hurst, and White, 2002; Dick and Lehnert, 2010; Livshits, Magee and Tertilt, 2007, 2010, 2016). Besides marketplace lending possibly throwing borrowers into a debt-trap of over-borrowing, the concern is that marketplace loans worsen the risk-composition of borrowers by providing credit to less credit-worthy households (Jagtiani and Lemieux, 2017).

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1. Upon receiving a loan application from the platform, the fronting bank originates the loan and sells it to the platform. Marketplace platforms finance the loan purchase by selling notes to investors who pledged to fund the loan.
2. Cambridge Centre for Alternative Finance (CCFAF, 2017) report available at https://www.jbs.cam.ac.uk/faculty-research/centres/alternative-finance/publications/hitting-stride/, Federal Reserve G19 (2017) and TransUnion Industry Insights Report, Q4 2017, https://newsroom.transunion.com/consumer-credit-market-concludes-2017-on-a-high-note/.

To empirically test the *ex-ante* ambiguous relationship between marketplace lending and personal bankruptcy, we exploit the decision by the U.S. Second Circuit Court of Appeals in the case of *Madden vs Midland Funding LLC* (*Madden*). 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 the loans are held by non-bank financial institutions. While the case was unrelated to marketplace lending, it cast doubt on the enforceability of marketplace loans as the majority of these loans are originated by a fronting bank and immediately sold to marketplace platforms, which are non-bank financial institutions under current OCC rules.[3] The verdict primarily affected marketplace lending, as opposed to other non-bank and bank lending, as the court noted the limited scope and reach of its verdict. *Madden* only applies if a bank issues and immediately assigns a loan – an outright debt sale – to a non-bank and, ex-post loan assignment, the loan's interest rate is raised beyond the borrower's state usury limit and if the bank retains no ongoing economic interest in the loan. This is reflected in rating agency and industry reports, and legal briefs which singularly concentrated on *Madden*'s effect on marketplace lending.[4]

We identify the effect of marketplace lending on bankruptcy filing using difference-in-difference estimations. We compare changes in bankruptcy filings and marketplace lending in the treatment (Connecticut and New York) and control group (all other states), before and after the treatment event.[5]

We find that *Madden* triggers Lending Club and Prosper, the two largest U.S. marketplace lenders, to reduce lending in the states affected by the verdict. Consistent with classical price theory, the interest rate controls imposed by *Madden* result in credit rationing. Our treatment event thereby provides a quasi-natural experiment allowing us to derive novel insights into how price controls affect credit supply in financial markets augmented by new lending technology. The number of marketplace loans declines by 13.4%, a reduction from 900 to 780 marketplace loans for an average state. Marketplace lending volume per month declines by 10%, a reduction from $13 million to $11.7 million for the average state. Credit rationing intensifies in line with borrower credit risk. Loans with the best credit risk are left unaffected, while lending to the most risky borrowers falls by most.

Using monthly data from the U.S. Courts Administrative Office, we show that there are 8% more personal bankruptcy filings in Connecticut and New York relative to other states following *Madden*. In absolute terms, bankruptcy filings increase on average from 1,573 to 1,698 cases. While the magnitude of this result is smaller than estimates by related studies (Dick and Lehnert, 2010; Morgan, Strain and Seblani, 2012) the effect is economically significant.

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3. The Office of the Comptroller of the Currency (OCC) is considering special purpose bank charters for fintech lenders: https://www.occ.gov/topics/responsible-innovation/comments/special-purpose-national-bank-charters-for-fintech.pdf.

4. Fitch, "Challenges Linger as U.S. Marketplace Lending ABS Rises," Reuters, (Sep. 10, 2015); Moody's, "Denial of Madden appeal credit negative for marketplace loans and related ABS," Moody's Investor Service, (June 30, 2016); Jones Day, "Secondary Loan Markets Post-Madden: Solving Secondary Market Sales and Liquidity Issues," (Nov.1, 2016).

5. 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. The treatment group thus includes Connecticut and New York to preserve treatment group homogeneity.

We attribute the increase in the incidence of personal bankruptcy following *Madden* to the reduction in marketplace lending. This hypothesis is supported by a number of further results.

First, we find that the rise in personal bankruptcy is proportional to the reduction in marketplace lending across income groups. While high-income households neither experience marketplace credit rationing nor a hike in bankruptcy cases, low-income households experience the most severe rationing of marketplace credit (64%) and the largest rise in personal bankruptcy (8.5%) following the verdict.

Second, we observe an economically and statistically significant decline in marketplace loans for medical cost and debt refinancing, including for refinancing credit card debt. Medical expenses are known to be an important determinant of personal bankruptcy, particularly for low-income households (Gross and Notowidigdo, 2011) and, at the margin, the cost of high credit card debt is the single largest factor contributing to bankruptcy (Domowitz and Sartain, 1999). Our findings suggest that the reduction in marketplace credit for medical expenses and debt refinancing are key channels via which the rationing of marketplace credit increases personal bankruptcy filings.

Third, we strongly reject plausible alternative explanations for the increase in bankruptcy following *Madden* other than the reduction in marketplace lending. We document that the volume of lending by banks and other non-bank lenders is left unaffected by *Madden*. This formally confirms the point raised above that the consequences of *Madden* are limited to the enforceability of marketplace loans and suggest that the increase in bankruptcy rates following *Madden* arises predominantly from changes in marketplace lending. Further, we show that the estimated effect of *Madden* on precipitating bankruptcy is robust to controlling for a wide variety of consumer credit, including (i) credit card lending from banks, bankcard companies, national credit card companies, credit unions as well as savings and loan associations, (ii) student loans from banks, credit unions and other financial institutions and federal and state governments, as well as (iii) auto loans from banks, credit unions, savings and loan associations, as well as automobile dealers and automobile financing companies.

We also rule out that the increase in bankruptcy is due to borrowers switching to forms of high-interest credit, such as payday loans, which, next to credit card debt, are strongly associated with household hardship. We exploit the fact that payday lending is illegal in New York but permitted in Connecticut. If the rise in bankruptcies were due to payday lending, the increase in bankruptcy would be higher in Connecticut where payday lending is legal. However, we observe a larger increase in bankruptcy filings in New York.

Finally, we rule out that the rise in bankruptcy following the verdict could be the result of an increase in defaults by marketplace borrowers in the affected states. This may occur if marketplace borrowers are over-indebted and default after being unable to obtain additional marketplace loans in the affected states. If this were the case, *Madden* constraining the high-risk above-usury segment of the marketplace lending industry would have positive welfare effects. Yet, we find that *Madden* leaves the number of non-performing marketplace loans unaffected, suggesting that existing marketplace borrowers are not contributing to the rise in bankruptcy following the treatment event.

In sum, our findings suggest that restrictions on marketplace lending have adverse welfare effects in terms of raising the incidence of personal bankruptcy. Moreover, we document that bankruptcy filings remain persistently higher in the affected states instead of being merely a temporary adjustment of households in response to the abrupt compression of marketplace lending.

Our estimation model controls for a variety of factors affecting marketplace lending and personal bankruptcy filings, including marketplace loan demand and macroeconomic conditions, as well as any unobserved differences over time and across states. The results hold across an array of econometric specifications, variable and treatment group definitions, as well as being robust to alternative clustering and bootstrapping of standard errors, and matched sampling. Importantly, we control for access to other forms of non-bank lending besides marketplace lending, such as payday loans, and the availability of other consumer credit, including credit card loans.

Bankruptcy is important and affects households' welfare. Following bankruptcy, an individual's credit record is tarnished for up to ten years, leading to difficulties with borrowing, renting housing and finding employment (Han and Li, 2011). Even when a filing is unsuccessful, bankruptcy depresses annual earnings and increases rates of foreclosure and individual mortality (Dobbie and Song, 2015). Aside from households, there are large macroeconomics costs. 750,000 people in the U.S. filed for bankruptcy in 2016. This wiped out $118 billion in debt and makes bankruptcy more costly in per capita terms than health insurance (Mahoney, 2015; Fisher, 2017).[6] Credit losses impose costs on taxpayers, given that bankruptcy-related losses are tax-deductible[7], and on future borrowers by raising risk-adjusted interest rates (Gropp, Scholz, and White, 1997; Berkowitz and White, 2004).

We contribute to a pressing policy debate about the effects of fintech lending. While *Madden* only directly applies to New York, Connecticut, and Vermont, other districts, such as Colorado, may follow the reasoning of the verdict.[8] Legislative efforts seek to overrule the *Madden* verdict. The H.R.3299 bill pending currently in the U.S. Senate argues that the ruling led to a "lack of access to safe and affordable financial services" for the poorest households.[9] Our study provides detailed material evidence to inform this claim.[10] Our findings moreover suggest that, in the absence of a clear regulatory framework for fintech lending, the verdict also had the unintended consequence of raising personal bankruptcies. Understanding the real effects of financial technology helps to inform the intense regulatory deliberations on the wider fintech industry currently taking place at the OCC, FDIC, Federal Reserve, Treasury, and the Basel Committee on Banking Supervision.

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6. US Courts, 2016 Report of Statistics Required by the *Bankruptcy Abuse Prevention and Consumer Protection Act of 2005*, http://www.uscourts.gov/statistics-reports/bapcpa-report-2016.

7. Congressional Budget Office, Personal Bankruptcy: A Literature Review, https://www.cbo.gov/sites/default/files/cbofiles/ftpdocs/24xx/doc2421/bankruptcy.pdf.

8. See the action brought by Colorado's Attorney General in defence of the state's Uniform Consumer Credit Code against other major non-bank online lenders in *Mead v. Marlette Funding LLC* and *Mead v. Avant of Colorado LLC*.

9. H.R.3299 Act, https://www.congress.gov/bill/115th-congress/house-bill/3299.

10. It is beyond the scope of this paper to comment on the efficacy of the bill as a regulatory response to *Madden*.

Our research builds on and substantially extends Rigbi (2013) and Honigsberg, Jackson and Squire (2018) who offer a preliminary analysis of marketplace lending restrictions complementary to this paper. We depart from these papers by analyzing how marketplace credit availability affects household welfare. Honigsberg et al. (2018) show using regression models how the verdict affects secondary-market trading prices, borrower quality, loan default probability and marketplace *loan size*. Yet Honigsberg et al. (2018) do not provide an econometric analysis of how the verdict affects the *number* and *total volume* of marketplace loans which we supply.[11]

Our paper offers an econometrically robust analysis of how *Madden* affects the number and volume of marketplace credit, in addition to being the first study of marketplace lending's impact on personal bankruptcy. Showing that marketplace lending seems to have an impact on household welfare that is qualitatively different from other forms of unsecured consumer lending, this paper adds to the burgeoning literature on the effects of traditional and alternative finance on economic hardship. We find that marketplace lending seems to be inversely related to personal bankruptcy, in contrast to other forms of consumer credit, including bank credit (Dick and Lehnert, 2010), credit card debt (Domowitz and Sartain, 1999; Gross and Souleles, 2002; Fay, Hurst, and White, 2002; and Livshits, Macgee and Tertilt, 2016) and payday loans.[12] We also contribute to the promising but still nascent literature on the effects of technological progress in financial markets (Livshits, Macgee and Tertilt, 2010; Athreya et al., 2012; Einav, Jenkins, and Levin, 2012; Narajabad, 2012; and Drozd and Serrano-Paul, 2017). Similar to our paper, the prior literature focuses on credit markets, given the industry's intense use of information technology.

In further contrast to prior work, our study focuses on marketplace lending, which differs from other forms of alternative finance and traditional consumer credit. Relative to credit card debt, marketplace platforms allow for more in-depth screening (Fuster, Plosser, Schnabl and Vickery, 2018) and, relative to payday loans, marketplace loans tend to carry lower interest rates. Our finding that marketplace lending helps lower personal bankruptcies among low-income households provides empirical evidence for theoretical models (Vallee and Zeng, 2018) showing that the technology behind marketplace lending may improve the efficiency of financial intermediation. While our paper suggests that the lending technology associated with marketplace credit may have some positive welfare effects compared with other forms of costly credit, how marketplace lending affects household hardship along other dimensions, aside from bankruptcy, is left for future research.

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11. Honigsberg et al. (2018) present histograms graphically depicting the number of loans provided to borrowers in the affected states before and after *Madden* but aside from histograms do not provide an econometric analysis of this issue.

12. The adverse effects of costly credit range from more checking account overdrafts (Zinman, 2010), involuntary bank account closure (Campbell, Tufano and Martinez-Jerez, 2012), poor job performance (Carell and Zinman, 2014), late bills for mortgages, rent and utilities (Melzer, 2011), missed child support payments and food stamp use (Melzer, forthcoming). Carter and Skimmyhorn (2017) dispute Carell and Zinman (2014). Few studies find positive effects which are often limited to developing countries or natural disasters. See Karlan and Zinman (2010), Morse (2011) and Dobridge (2018).

The following section discusses the institutional setting. Section III develops testable hypotheses. Section IV covers the data and empirical strategy. Section V presents results and Section VI offers concluding remarks.

II. BACKGROUND: PERSONAL BANKRUPTCY, USURY LAWS, MARKETPLACE LENDING AND THE *MADDEN* COURT CASE

This section discusses the institutional background covering the bankruptcy code (Section A), relevant usury laws (Section B), and the marketplace lending industry in the U.S. (Section C) as well as details of the *Madden* court case (Section D).

II.A. Personal Bankruptcy in the U.S.

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

There are different chapters (7, 11, 12 or 13) that can be filed for in the U.S.[13] Chapter 7 wipes out the dischargeable debt after any non-exempt assets have been sold. However, many creditors filing under this chapter do not have any or little 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 Ch.13, Chapter 11 allows individuals to restructure their debt, but debtors are not required to turn over their disposable income as is required under Ch. 13. Bankruptcy cases under Ch. 11 are substantially more complex and expensive than Ch. 7 and Ch. 13 cases and are usually filed by corporates rather than individuals or personal businesses. Chapter 12 allows certain agricultural businesses, such as farmers and commercial fishermen, to file for personal business bankruptcy.

Bankruptcy filings in the U.S. in recent years have been in decline. 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 under Ch. 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 and now there are about 25,000 business filings per year, down from about 45,000 filings per year prior to 2014.[14] The nationwide trend is also reflected in the decline in personal bankruptcies in the states affected by *Madden*. In this paper we examine if placing restrictions on marketplace lending is contributing to or hindering the downward trend in the number of bankruptcy filings in the affected states relative to the states left unaffected by *Madden*.

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13. US Courts Basics: www.uscourts.gov/services-forms/bankruptcy/bankruptcy-basics/process-bankruptcy-basics.
14. American Bankruptcy Institute (2018): https://www.abi.org/newsroom/bankruptcy-statistics.

II.B. Usury Laws in the U.S.

The Code of Laws of the United States states that for national banks the interest rate on a loan 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 bank is located."[15]

Usury limits and penalties vary by state, borrower type, and loan term.[16] Some states like Utah have no usury limit, while others have high interest caps and harsh penalties. 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. The owner of a usurious loan in New York forfeits any interest as well as the complete principal of the loan.[17]

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 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 today on many types of loans, including marketplace loans (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 can charge interest up to the rate in which the bank is headquartered, irrespective of borrower's state of residence. 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 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 percent, irrespective of a borrower's home state usury limit (Rigbi, 2013).[18] Lending Club and Prosper achieved this by partnering with WebBank, an FDIC-insured bank chartered in a state with no usury ceiling. When the

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15. US Code (2018) http://uscode.house.gov/browse/prelim@title12/chapter2/subchapter4&edition=prelim.
16. The discussion is based on Marvin (2016).
17. N.Y. Penal Law 190.40. New York State Senate, https://www.nysenate.gov/legislation/laws/PEN/190.40.
18. Lending Club went national in December 2007. Prosper started offering 36% loans to borrowers in all states, except Texas, from April 2008.

partnering bank receives a loan application for instance from Lending Club, the bank originates the loan and sells it to the lending platform which then sells notes to investors pledging to fund the loan. This model allows marketplace lending platforms to 'export' the no-usury limit of Utah, WebBank's home state, 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*, a court case not directly related to the marketplace industry at all, 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.

II.C. Marketplace Lending in the U.S.

The growth of the marketplace lending industry has been rapid.[19] Within five years, Lending Club, the largest platform in the U.S., rose from holding a 1% share of all outstanding personal loan balances in 2012 to issuing one of every ten personal loans in 2017.[20] The industry has evolved from peer-to-peer lending into what is now described as 'marketplace lending'. Self-directed retail investors have come to play a small role in the provision of funds for these platforms relative to institutional investors such as banks, asset managers, insurance companies, hedge funds and other large non-bank investors.[21] While there is a large number of marketplace platforms, the two largest platforms, Lending Club and Prosper hold a significant market share, with Lending Club accounting for 45% of all marketplace lending in the U.S in 2017. Although it is based entirely online, the industry is still heavily geographically concentrated and most of the alternative financing comes from investors in and goes to borrowers residing in California, New York and Texas (CCFAF, 2017).

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 and the feasible maximum interest rate, besides providing other application information to the platform. Investors choose which proposals to fund and whether to fund a portion or the full amount requested. Once sufficiently funded, the loan is originated. Interest rates ranges between 5.8%–36% and loans are amortized via monthly payments over 3–5 years. Lending Club's personal loans range up to $40,000 and Proper's range between $2,000–$35,000. Marketplace borrowers have on average $62,000 in annual income.[22] The speed, automation and sophistication of fintech credit scoring models as well as the use of alternative information normally ignored by traditional lenders, makes marketplace lending an innovative financial technology.

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19. See aforementioned CCFAF (2017), Federal Reserve G19 (2017) and TransUnion (2017).
20. Lending Club, Investor Day Presentation (2017), http://ir.lendingclub.com/Cache/1001230258.pdf.
21. Lending Club, ibid.
22. Lending Club, ibid.

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. Innovative in this origination process is the creation not of a single but of two promissory notes: first, the liability between the borrowers and the marketplace platform and, second, the liability between the marketplace platform and the investors funding 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).

II.D. Treatment Event: Madden vs. Midland Funding LLC

The marketplace lending model came under scrutiny when *Madden* suddenly raised the question whether the marketplace platform, instead of the fronting bank, is the 'true lender'. The treatment event poses the issue whether marketplace lenders, by partnering with a bank in a state with no usury laws, may rely on the federal preemption of state usury laws, which under the *National Bank Act* and the subsequent *Federal Deposit Insurance Act* has been reserved for national and state-charted banks, including their agents and subsidiaries.[23] The marketplace lenders became vulnerable to regulatory action as well as private civil litigation, as evinced by *Madden*.

The following describes the sequence of events relating to the court case *Madden vs. Midland Funding LLC*, the treatment event.[24]

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. Alongside the transfer came an amendment in the loan terms, as allowed for in the terms and conditions of the credit card agreement, determining Delaware as the jurisdiction to be applied in case of a lawsuit. In 2008 Ms. Madden became delinquent on the 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.[25]

Neither Midland nor the affiliated Midland Funding Credit Management Inc. are chartered national banks, unlike Bank of America and FIA. In November 2010, Midland attempted to collect payments from Ms. Madden at 27 percent interest as permitted by Delaware usury law. In response

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23. Under the FDIC Act, state-chartered banks enjoy the same federal pre-emption as national banks under the NBA.
24. The exposition is based on Mason (2016), Marvin (2017), and Honigsberg, Jackson and Squire (2018).
25. Midland (2018) https://www.midlandcreditonline.com/who-is-mcm/midland-credit-management-real-company/.

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 percent. Midland objected maintaining that 27 percent 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 that 27 percent 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."[26]*

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 in loans being facilitated by a bank but immediately sold outright to marketplace platforms, which are currently designated as non-bank financial institutions by the OCC.

We focus on the rationing of marketplace lending, as opposed to other forms of non-bank lending as well as bank lending, as the transmission channel via which *Madden* affects personal bankruptcies. The reason is that the effect of the *Madden v Midland Funding LLC* case is limited to a specific set of loans. In reaching its verdict, the Second Circuit court noted the scope and reach of its decision by distinguishing its case from three separate previous legal precedents (Jones Day, 2016). First, any revolving loans, such as credit cards, in which the bank retains an interest is left unaffected by *Madden* (see *Krispin v May*). Second, *Madden* does not apply to any closed-end loans, such as mortgages, if the bank charges the interest rate (see *Phipps v FDIC*). Third, *Madden* does not affect

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

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 immediately assigns a loan – an outright debt sale – 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 other words, in the view of 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.[27]

Both Lending Club and Prosper have attempted to cushion the impact of the verdict by restructuring their business model. The restructuring involves letting the fronting bank originating loans retain an interest in the loan after it was sold to the marketplace platform. Had the national bank that originated the loan retained some interest in Ms. Madden's loan after assigning it to the debt collector, Midland could be considered as a 'subsidiary' or 'agent' of the national bank and, thereby, circumvent the borrower's state usury laws. Despite restructuring their origination model by having the originating bank retain an interest in the issued marketplace loans, the regulatory uncertainty remains. Lending Club and Prosper continue to point out in their investment prospectus, as filed with the Securities and Exchange Commission (SEC), that *Madden* poses risks to the loan origination model of marketplace lenders.[28]

Since May 2015, policy uncertainty continues regarding the enforceability of above-usury marketplace loans in New York, Connecticut and Vermont. A request by Midland to reopen and rehear the case was rejected by the Second Circuit court and the U.S. Supreme Court also declined to consider an appeal of the case. In February 2018, the U.S. Congress passed the 'Protecting Consumers' Access to Credit Act' which would overturn the *Madden* ruling. But the law has to yet be passed by the Senate and signed by the President before becoming effective law.

In sum, the *Madden* case cast a significant shadow on fintech lending by suddenly rendering marketplace loans subject to a borrower's state usury ceilings.

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27. Fitch, "Challenges Linger as U.S. Marketplace Lending ABS Rises," Reuters, (September 10, 2015); Moody's, "Denial of Madden appeal credit negative for marketplace loans and related ABS," Moody's Investor Service, (30 June 2016); Jones Day, "Secondary Loan Markets Post-Madden: Solving Secondary Market Sales and Liquidity Issues," (November 1, 2016); and Chapman and Cutler LLP, "The Regulation of Marketplace Lending: A Summary of the Principal Issues" (April 2018).

28. See Appendix B for the Lending Club Prospectus (2017) and Prosper Prospectus (2018). For instance, Lending Club notes: "If a borrower were to successfully bring claims against us for state usury law violations, and the rate on that borrower's personal loan was greater than that allowed under applicable state law, we could be subject to fines and penalties, including the voiding of loans and repayment of principal and interest to borrowers and investors."

III. HYPOTHESES DEVELOPMENT

III.A. The Effect of Madden on Marketplace Lending

Economic theory on the effects of usury laws and interest rate controls informs our prior expectations about how *Madden* affects marketplace loan availability. [29] As early as Locke (1691) it was recognized that usury limits can trigger credit rationing. *Madden* provides a quasi-natural experiment which 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 the 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 (hidden information). The seminal models by Jaffee and Russell (1976), Stiglitz and Weiss (1981), and Bester (1985), suggest that, 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, above the risk-adjusted profit maximizing level, a rise in interest rates can lead to a fall in credit supply. The more elastic the loan supply, the more any reductions in the price of credit will be offset by credit rationing.

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's terms more specifically to borrower characteristics (Hynes and Posner, 2002; 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 *Madden's* effect on marketplace lending:

Hypothesis I: *Following Madden, the volume and number of marketplace loans decrease.*

Hypothesis II: *The marketplace credit rationing effects of Madden are more severe for borrowers with a poor credit rating.*

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29. 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. E.g. Greer (1975), Wolkin and Navratil (1981), Villegas (1982), Peterson (1983), and more recently Temin and Voth (2007) and Benmelech and Moskowitz (2010).

III.B. The Effect of Marketplace Lending Restrictions on Bankruptcy Filing

Households may file for bankruptcy due to an unwillingness to pay debts. An individual may decide to file for bankruptcy if this yields net balance sheet benefits in terms of the filer's net asset position. Bankruptcy filings may increase if the financial costs of filing fall.[30] Bankruptcy filings may also rise if the benefits of filing, most importantly the amount of debt discharged, increase.

Households may also file for bankruptcy due to an inability to pay debts. To the extent that individuals prefer to avoid bankruptcy, marketplace loans could ease financial distress by allowing household to refinance existing debt carrying a higher interest rate. Marketplace loans may help to smooth adverse and possibly unforeseen shocks to income or expenses pushing households towards bankruptcy. Adverse health shocks are as a prominent factor for precipitating bankruptcy, particularly among low-income households (Domowitz and Sartain, 1999; Gross and Notowidigdo, 2011).

One plausible hypothesis is that a reduction in marketplace lending will lead to a higher number of personal bankruptcies in the affected states due to the benefits that marketplace loans provide to borrowers. Marketplace platforms provide quickly accessible consumer loans (Fuster, Plosser, Schnabl and Vickery, 2018) which are cheaper than credit cards (Balyuk, 2017) and serve previously underserved borrowers (De Roure, Pelizzon, and Tasca, 2016; Jagtiani and Lemieux, 2017; Schweitzer and Barkley, 2017; Tang, 2018). Marketplace loans are predominantly used for debt refinancing, especially credit card bills, or paying medical bills, thereby allowing borrowers to cover expenses that would otherwise contribute to household hardship and bankruptcy. In light of these considerations, marketplace lending restrictions may increase personal bankruptcy filings.

Hypothesis III (A):   *Restricting marketplace lending increases personal bankruptcy filings.*

Marketplace loans may, however, at the same time impose an additional debt burden on households which is associated with higher bankruptcy (Domowitz and Sartain, 1999; Gross and Souleles, 2002; Fay, Hurst, and White, 2002; Dick and Lehnert, 2010; Livshits, Macgee and Tertilt, 2007, 2010, 2016). In addition, Ausubel (1991) documents that individuals underestimate their ability to repay loans. Therefore, bankruptcy filing may decrease following *Madden* as the ruling reduces access to marketplace loans for less credit-worthy households.

Hypothesis III (B):   *Restricting marketplace lending decreases personal bankruptcy filings.*

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30. Financial costs include the amount of non-exempt assets that are sold to pay creditors (Gropp, Scholz and White, 1997; Fay, Hurst, and White, 2002; White, 2007) and filing costs (Gross, Notowidigdo, and Wang, 2014).

IV. DATA AND IDENTIFICATION STRATEGY

IV.A. Data

The marketplace lending data were obtained from the two leading marketplace lending platforms, Lending Club and Prosper. These datasets include detailed information on loan requests placed on each platform. We identify the borrower's state of residence and the loan listing start date, loan origination date, loan purpose, as well as the amount of money requested, the amount of funds granted, and the internal risk rating of the applicant. The loan-level data also allows us to calculate the monthly number of non-performing loans per state.

On average, 900 marketplace loans are outstanding in each state every month. The average marketplace borrower in our sample applies for a loan of $14,367. The average marketplace loan default rate is 7.8% at an interest rate of 9.32%. Differentiating borrowers by credit risk, these figures range from an average loan size of $10,385 at 10% interest with default rates of 10% for the riskiest borrowers to an average loan size of $14,077 at 6% interest with default rates of 2.7% for the least risky marketplace borrower group. Many loans are requested for debt refinancing (69.84%), small personal business loans (9.56%) and medical expenses (7.64%).[31]

Bankruptcy filing data were obtained from the Administrative Office of the U.S. Courts. This dataset provides information on the number of bankruptcy cases filed per month in every state since 2013 and allows us to distinguish between various chapters under which petitions were filed as well as between personal business and consumer bankruptcies. We obtain information on the number of filings differentiated by the annual income of each filer and the total amount of assets held by individuals filing in each state per month.

On average, 4.56 individuals file for personal bankruptcy for every 10,000 people of working age in each state every month. In absolute terms, 1,573 people file for personal bankruptcy in each state every month, of which 1,017 cases and 542 cases are Chapter 7 and Chapter 13 filings respectively. Of the total number of bankruptcy filings, the share of consumer bankruptcy and personal business bankruptcies is, respectively, 96.18% and 3.82%. Filers have an average income of $37,000, with income for Ch. 7 filers ($36,000) being lower than Ch.13 filers ($40,000). Households filing for consumer bankruptcy have a higher income ($37,500) relative to those filing for personal business bankruptcy ($26,200).

The New York Federal Reserve Center for Microeconomic Data provides us with information on the annual volume of consumer lending in each 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

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31. 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). See Table A.1 in Appendix A for statistics based on funds channeled through Lending Club and Prosper.

as federal and state governments) and auto loans (from banks, credit unions, savings and loan associations, as well as automobile dealers and automobile financing companies). We supplement our bankruptcy filings and marketplace lending data with monthly U.S. Bureau of Labor Statistics unemployment rates and labor force data.

The sample period covers the 60-month period from January 2013 to December 2017 for all U.S. states. We remove states from the sample whose residents were or still are unable to raise funds through Prosper and Lending Club. Based on our loan-level dataset, these states are Iowa, Maine, Mississippi, Nebraska, North Dakota, and West Virginia. [32] Our final sample includes 2,700 observations for 45 states. Table I presents summary statistics for the variables used in our regressions. Appendix A, Table A.1 presents important further summary statistics.

[TABLE I - SUMMARY STATISTICS]

IV.B. Main Outcome Variables

The volume of marketplace lending and bankruptcy filings per month in each state are the main outcome variables of interest.

To examine how *Madden* affects the intensive and extensive margin of marketplace credit supply, we analyze the verdict's effect on the dollar volume and number of marketplace loans. Second, we estimate how the treatment event affects marketplace borrowers across different risk profiles. Third, to measure how the treatment event affects marketplace credit supply across loans for different purposes, we calculate the dollar amount of marketplace loans requested for debt refinancing, medical bills and small business expenses, all of which ought to help households avoid filing for bankruptcy. We estimate the effect of *Madden* on the total volume of these loan categories and the volume of loans borrowed for all other purposes.

To test the effect of *Madden* on bankruptcy filing rates, we, fourthly, calculate the total number of bankruptcies filed per month scaled by the size of the workforce in each state, measured in 10,000s residents of working age. Fifth, we differentiate the total number of filings into personal business and consumer bankruptcy filings in each state per month and by the chapter of the bankruptcy filing. Finally, we calculate the number of all different chapter filings scaled by the workforce for total bankruptcy cases as well as for personal business and consumer filings separately. [33] All our dependent variables (denoting marketplace lending and bankruptcy filings) enter the regressions as a log of one plus the value of the variable. [34]

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32. For the current data on borrower eligibility by state for Prosper see https://www.prosper.com/plp/legal/compliance/ and for Lending Club see https://help.lendingclub.com/hc/en-us/articles/213706208-Qualifying-for-a-personal-loan.

33. 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.

34. We scale bankruptcy rates by the workforce to account for the size of the state population and to make our results comparable with existing studies. Since monthly population data are not available we use the number of the workforce as

IV.C. Identification Strategy

We test the hypotheses linking marketplace lending restrictions to personal bankruptcy using difference-in-differences estimations exploiting the *Madden* court verdict as an exogenous source of variation in marketplace lending. We compare the evolution of the volume and the number of marketplace loans and bankruptcy filings between the treatment (Connecticut and New York) and control group (all other states) before and after the verdict. We estimate specifications of the following form:

(1) $\qquad Ln(Y)_{sm} = \beta_1 Madden_m * State_s + \beta_2 State_s + \beta_3 * Madden_m + \varepsilon_{sm}.$

Y denotes our outcome variables for state *s* in month *m*. *Madden* 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. *State* is a dummy variable equal to 1 for Connecticut and New York, and zero for all other U.S. states.[35]

Madden has implications for Connecticut, Vermont and New York. However, 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. Vermont borrowers are obliged to pay back the principal amount and interest up to usury limit. The treatment of marketplace loans extended to borrowers residing in Vermont significantly differs from the two other states in the Second Circuit such that we only include Connecticut and New York. This preserves homogeneity within the treatment group.[36]

The economic interpretation of the regression coefficients is as follows. β_1 measures the effect of *Madden* on our dependent variables. It captures the change in the volume or number of marketplace loans and bankruptcy filings in New York and Connecticut relative to the change in those variables in all other states. $State_s$ controls for permanent differences between states in the treatment and control groups. Therefore, β_2 captures time-invariant differences in the volume of marketplace loans and number of bankruptcy filings. $Madden_m$ controls for trends common to all states in the sample. In this case, β_3 absorbs any time trend in the volume of marketplace loans and bankruptcy filings.

provided by the Bureau of Labor Statistics. For robustness, Appendix A, Table A.4 presents alternative measures of bankruptcy rates. In Panel A bankruptcy rates are scaled by workforce but not expressed in logarithm. In Panel B bankruptcies are not scaled by workforce and expressed as log *(1+x)*. In Panel C bankruptcies are not scaled by workforce and expressed as log *(x)*. All these regressions yield results similar to our baseline results.

35. Additionally, we estimate our results using a matched sample. Our matching procedure follows Lemmon and Roberts (2010) nearest neighbor matching method. We match states based on the marketplace lending volume prior to treatment event. We use a *probit* model to estimate the effect of the average pre-treatment marketplace lending volume in each state on the probability of a state being in the treatment group. We then compute propensity scores using the estimates obtained from the *probit* regressions. States' nearest neighbors are states with the most similar propensity score. For each treated state we choose four nearest neighbor states from the control group. The results, presented in Table A.2, are in line with our main results. We also match treatment group states with two control group states. The results remain unchanged and are available upon request.

36. Table A.3 in Appendix A presents the results of tests that include Vermont in the treatment group.

We augment the baseline specification (Eq. 1) with a set of control variables, state and month fixed effects, which absorb *State* and *Madden*, to render our estimations robust against unobserved differences across states and time as well as to account for any changes in the macroeconomic environment and marketplace loan demand. The resulting auxiliary specification takes the form:

$$(2) \qquad Ln(Y)_{sm} = \alpha_s + \beta Madden_m * State_s + \delta Controls_{sm} + \gamma_m + \varepsilon_{sm}.$$

The control variables included are unemployment rates for each state and month (*Unemployment*), the total value of assets of individuals filing for bankruptcy (*Total assets*) and the volume of funds requested by borrowers through both marketplace platforms (*Requested funds*) as well as state and month fixed effects (α_s and γ_m). We cluster heteroscedasticity-adjusted standard errors at the state-level to account for serial correlation (Bertrand, Duflo, and Mullainathan, 2004).[37]

IV.D. Difference-in-Difference Assumptions

The quality of statistical inference from difference-in-difference estimations relies on the strength of the underlying identifying assumptions.

The first assumption requires the treatment event to be exogenous. Section II.D established that the *Madden* ruling provides an exogenous event to study the effect of marketplace lending restrictions on bankruptcy rates. The case involved credit card debt sold by FIA, a national bank in Delaware to Midland, a purchaser of unresolved consumer debt, and the case was in no way related to the marketplace industry. There is also no evidence that the court took into consideration conditions related to bankruptcy rates prevailing in the states of the Second Circuit when making the decision.

The second assumption of a difference-in-difference estimation requires the treatment and control groups to be observationally similar. States outside the jurisdiction of the Second Circuit need to constitute a valid counterfactual for the treated states. To establish this, we compare the trends in the evolution of the key outcome variables. Figure 1 shows that, prior to the court ruling, both marketplace lending and bankruptcy rates in the control and treatment group states evolve in a parallel manner for the 12-month period preceding the treatment event. In Appendix A, Table A.10 we also find that the relevant differences in marketplace lending volume and bankruptcy rates between the affected and unaffected states in the pre-treatment period are marginal. For this purpose, recall that difference-in-difference estimations do not require identical levels of the variables between the treatment and control group as any level differences are removed by the inclusion of fixed effects (Lemmon and Roberts, 2010; Roberts and Whited, 2013). These tests suggest that the control group is observationally similar to the treatment group in terms of our main outcome measures.

[FIGURE I - PARALLEL TRENDS]

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37. Alternatively, Table A.5 in Appendix A shows tests with bootstrapped standard errors from which we obtain similar inferences as the baseline regressions. Table A.6 presents results with standard errors clustered at the state-quarter level.

In the following, we discuss the effect of *Madden* on marketplace lending (Section A) and personal bankruptcy filing (Section B) and analyze these effects across different income groups (Section C). We evaluate plausible alternative explanations for the observed rise in bankruptcy filings following the verdict (Section D). Finally, we analyze the persistence of the effects from marketplace lending restrictions on precipitating personal bankruptcy (Section E).

V.A. Does the Madden Verdict Affect Marketplace Lending?

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

Table II, Panel A shows the marketplace credit rationing following *Madden* on the intensive margin, i.e. the volume of marketplace lending. Marketplace lending volume in Connecticut and New York declines between 10% (*t*-statistic -7.64) and 14.6% (*t*-statistic -4.63) following *Madden*.[38]

There is significant heterogeneity in the magnitude of this effect across different risk classes of borrowers. Using borrowing ratings by Prosper and Lending Club, we construct seven borrower credit risk rating categories.[39] The lowest (Rating 1) denotes the riskiest borrowers, while the highest (Rating 7) denotes the least risky borrowers. We find statistically significant reductions in the lending provided to borrowers with the four lowest ratings for which lending volume falls between 28% (borrower Rating 4) and 82% (borrower Rating 1).[40] In contrast, lending volume increase between 3.8% and 2.1% for more credit-worthy borrowers (ratings 6 and 7), respectively. However, only the effect on borrowers with Rating 6 is statistically significant.

Our finding that the magnitude of marketplace credit rationing is larger in market segments with higher credit risk is intuitive. The riskiest loan applicants are most likely to borrow at above usury rates and are most likely to be affected by *Madden* given that the verdict rendered state usury ceilings binding for marketplace loans in the treated states. Appendix A, Table A.1 reports the maximum values of interest rates per credit rating. Along the lower spectrum of the credit risk scale (1—5) they are respectively: 31%, 30.75%, 25.9%, 19.9% and 16.3%.[41] All these exceed the statutory civil usury

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38. To calculate the % change in the dependent variable we use the following formula: $\Delta y = 100 * \left(exp^{\beta} - 1 \right)$. For instance, a coefficient of -0.172 on the interaction term between *Madden* and *State* (Panel A of Table 2) suggests that, following the court ruling, marketplace lending dropped in Connecticut and New York by $100 * \left(exp^{-0.158} - 1 \right) = 14.6\%$.

39. Lending Club ratings vary from A(1) to G(7) while Prosper from HR(1), E(2), D(3), C(4), B(5), A(6) to AA(7).

40. We non-statistically significant 1% reduction in marketplace lending volume to borrowers with a rating of 5.

41. Since we are interested in examining the impact of marketplace lending restrictions on bankruptcy rates we use borrower ratings instead of looking at the effect on loans with above-usury interest rates. Interest rates reflect not only the riskiness of the borrower but also loan conditions, including maturity and loan volume.

limit in Connecticut (12%) and New York (16%) meaning that borrowers with the lowest credit ratings are most likely to feel the credit rationing effect.

[TABLE II - THE EFFECT OF *MADDEN* ON MARKETPLACE LENDING]

Table II, Panel B reports the marketplace credit rationing effect of *Madden* on the extensive margin in terms of reductions in the number of marketplace loans. The court ruling has a statistically significant negative effect on the number of marketplace loans, which fall by 16% (13%) in specification 1 (2). Analyzing the evolution of the number of loans by borrower riskiness we observe significant reductions in marketplace loans only for the riskier borrowers.

Table II, Panel C shows the marketplace credit rationing effect differentiated by loan purpose. We are particularly concerned with loans which may help individuals avoid filing for bankruptcy. Out-of-pocket medical bills cause one quarter of personal bankruptcies, particularly among low-income households (Gross and Notowidigdo, 2011). High credit card debt is the single largest factor contributing to bankruptcy at the margin (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. In addition, loans for small personal businesses might be relevant for bankruptcy as (iii) personal business loans are often requested for financing equipment purchases or covering unexpected business expenses required for continuing operating a personal business. Significant reductions in this type of marketplace lending may help to explain the observed changes in personal business bankruptcy filings.[42]

Results in Table II, Panel C show that the total volume of these three types of loans together (*Relevant loans*) falls by 10% in Connecticut and New York relative to all other U.S. states following *Madden*. We observe a large drop in the volume marketplace loans for debt refinancing (15%), small businesses loans (33%) and, in particular, loans for medical procedures (68%). The volume of loans acquired for all other purposes declines by 15%.[43]

In sum, there is a significant reduction in the volume and number of marketplace loans. We find that rationing of marketplace credit is particularly severe for borrowers at the lower end of the credit rating spectrum, which confirms results by Honigsberg, Jackson and Squire (2018). The least risky borrowers are left unaffected by the court verdict. We furthermore find that the types of marketplace loans relevant for staving off bankruptcy, such as credit card financing and small business loans, experience a drop and loans for medical expenses record the largest decline.

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42. As for the controls, lending volume is negatively correlated with the total amount of assets of bankruptcy filers and the unemployment rate, although the coefficients on the former are not significant. The volume of marketplace funds requested rises with the volume of granted funds.

43. 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.

V.B. 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 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 state and scale it by the size of the state workforce.

Table III, Panel A presents *Madden*'s effect on the total number of bankruptcies, including personal business and consumer (non-business) bankruptcies. Following the verdict, the total number of bankruptcy filings, irrespective of the chapter under which bankruptcy is filed, is 8% higher in Connecticut and New York (*t*-statistic 2.60) relative to the states in the control group. The estimated coefficient on the interaction term between *Madden* and *State* is positive and statistically significant in regressions where the dependent variable denotes Chapter 7 and Chapter 13 bankruptcy filings. Chapter 7 filings increase by 6% (*t*-statistic 3.87) and Chapter 13 cases jump by 11% (*t*-statistic 2.58). Chapter 11 and Chapter 12 bankruptcy filings are unaffected.[44]

Table III, Panels B and C present, respectively, the number of personal business and consumer bankruptcy filings separately. Personal business bankruptcy petitions surge by 2.3% (*t*-statistic 1.48) and consumer bankruptcy cases increase by 7.6% (*t*-statistic 2.84). Table III, Panel B shows that, among personal business bankruptcy cases, only Chapter 7 filings record a statistically significant increase of 1.8%. Table III, Panel C indicates that the rise in consumer bankruptcy filings following the treatment event is driven by a statistically significant 5.6% increase in Chapter 7 filings and an 11% rise in Chapter 13 filings.[45]

Overall, the results in Table III suggest that restricting marketplace lending increases personal bankruptcy filings, which is evidence in support of Hypothesis III(A).

[TABLE III - THE EFFECT OF *MADDEN* ON PERSONAL BANKRUPTCY]

V.C Difference in Marketplace Credit Rationing and Rise in Bankruptcy across Income Groups

We use data on the annual income of bankruptcy filers and marketplace borrowers and re-estimate the auxiliary specification (Eq. 2) for different income ranges. We split borrowers and bankruptcy filers into five income groups: with an annual income <\$25,000 (range 1), \$25,000-\$49,999 (range 2), \$50,000-\$74,999 (range 3), \$75,000-\$100,000 (range 4), and finally with an annual income >\$100,000 (range 5).[46] Table IV shows the effect of *Madden* on the volume and number of marketplace loans (Panel A) and bankruptcy filings (panel B) across different income groups.

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44. Recall that Chapter 11 bankruptcy cases are usually filed by corporate businesses rather than individuals or personal businesses. Bankruptcy under Chapter 12 is available to farmers and commercial fishermen.
45. Table III, Panel C excludes estimations for Ch. 12 bankruptcy filings since these are business bankruptcies.
46. Specification (1) yields materially equivalent results. We report tests only for specification (2) to preserve space.

Table IV, Panel A shows that borrowers on lower incomes experience significantly more credit rationing. The lending volume to Connecticut and New York borrowers with an annual income of less than $25,000 (range 1) declines by 64% following the court ruling (coefficient -1.022), relative borrowers in all other states. The fall in marketplace credit supply is smaller for groups with higher annual income. Relatively high income borrowers (range 4) observe only a small fall in marketplace lending volume of 6.2%. No credit rationing effect of *Madden* can be observed for borrowers with the highest annual income (range 5).

Table IV, Panel B shows a complementary pattern for bankruptcy filings. Connecticut and New York residents on low incomes file significantly more for bankruptcy following *Madden* relative to residents with higher annual income. We observe no effect of *Madden* increasing personal bankruptcy among individuals with the highest income. The biggest hike in bankruptcy filings occurs for the population on the lowest income. The size of the increase in bankruptcy filings falls proportionally to an increase in annual income. Increases in the incidence of bankruptcy among individuals in the lowest three income brackets are 8.5%, 7.3% and 4.7% respectively.

In sum, individuals are more likely to experience personal bankruptcy the larger the contraction in marketplace lending to that income group. Households which experience no reduction in marketplace lending do not exhibit increases in bankruptcy filings. These results further corroborate Hypothesis III(A) that marketplace lending restrictions lead to an increase in personal bankruptcy filings across different income groups, with lower income groups experiencing more marketplace credit rationing and a larger increase in personal bankruptcy.

[TABLE IV - THE EFFECT OF *MADDEN* ACROSS DIFFERENT INCOME GROUPS]

Overall, our results suggest that marketplace lending may help households, particularly those on low incomes, avoid bankruptcy and suggest that the screening and lending technology behind marketplace credit may have some positive welfare effects compared with other forms of costly credit, such as payday loans and credit card debt, associated with worsening personal bankruptcy.

Our results are in contrast to prior work on credit card and payday lending which tends to increase personal bankruptcy (Domowitz and Sartain, 1999; Gross and Souleles, 2002; Fay, Hurst, and White, 2002; Dick and Lehnert, 2010; Skiba and Tobacman, 2015; Livshits, Macgee and Tertilt, 2007, 2010, 2016). Marketplace lending reducing the incidence of personal bankruptcy among low-income households may be explained by the fact that, relative to traditional lenders, marketplace platforms use information previously ignored by traditional lenders (Jagtiani and Lemieux, 2017) allowing for more in-depth screening of borrowers (Fuster, Plosser, Schnabl and Vickery, 2018) and, relative to payday loans, marketplace loans tend to carry lower interest rates. This suggests that the financial technology behind marketplace lending may improve the efficiency of financial intermediation (Vallee and Zang, 2018).

V.D. Rejecting Alternative Explanations for the Increase in Bankruptcy Filings

In this section we test and reject plausible alternative explanations tracing the increase in personal bankruptcy following *Madden* to factors other than marketplace credit rationing. It is possible that *Madden* might have an effect on lending by other non-bank financial institutions as well as bank loans that are intended to be sold outright to non-banks. *Madden* may reduce the liquidity and secondary market value of such loans leading to a reduction in their origination volume. It is alternatively also possible that *Madden* coincides with increases in other types of consumer credit which may explain the rise in bankruptcy rates.

First, to test whether *Madden* affects other types of consumer credit we turn to data provided by the New York Federal Reserve's Consumer Credit Panel.[47] These data provide us with the year-end volume of credit card loans, auto loans and student loans originated in each U.S. state. Figure II provides a graph illustrating the effect of *Madden* on marketplace loans and other consumer loans. As these data on non-marketplace loans are available at an annual frequency, we annualize marketplace loan volume to provide a better comparison. Figure II shows that, apart from marketplace lending, other types of consumer loans are not significantly affected by *Madden*.

To provide a formal test, we modify specification (2) and let the dependent variable be, respectively, the total annual volume of marketplace loans, credit card loans, auto loans and student loans. We replace month fixed effects with year fixed effects. The results are presented in Table V, Panel A. Apart from marketplace loans, *Madden* does not affect any other type of consumer credit. Next, we test whether controlling for these consumer loans affects the size of the estimated effect of *Madden* raising personal bankruptcies as presented in Table III. In Table V, Panel B we examine the effect of *Madden* on bankruptcy rates. We annualize bankruptcy rates by calculating the total of all, business bankruptcy and consumer bankruptcy rates. Here we also find that controlling for credit card debt, auto loans and student loans does not alter the results previously presented in Table III.[48]

[TABLE V - *MADDEN* AND NON-MARKETPLACE CONSUMER CREDIT]

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47. The Federal Reserve Bank of New York's Center for Microeconomic Data provides household debt statistics by state in its *Quarterly Report on Household Debt and Credit*. See https://www.newyorkfed.org/microeconomics/databank.html.

48. In an additional robustness test, instead of year-end annual NY Fed data on consumer credit, we use quarterly data from SNL Financial covering consumer lending by traditional financial institutions operating in each state, including commercial and savings banks, credit unions as well as savings and loan associations. We document that *Madden* does not affect lending provided by traditional financial institutions in New York and Connecticut and find that controlling for this lending also does not our baseline results. This additional check further refutes the idea that the observed rise in the number of individuals filing for bankruptcy following *Madden* is due to credit rationing by traditional lenders. These results are presented in Table A.8 and A.9 in Appendix A. In the main tests, however, we use NY Fed data for two reasons. First, the SNL Financial data do not allow us to observe bank lending at the state level while the NY Fed data do allow for this identification. Second, the NY Fed data comprehensively cover consumer lending by both banks and non-banks, including financing companies, and are based on a nationally representative random sample from Equifax credit-report data.

Second, the increase in bankruptcy may be due to credit-rationed high-risk marketplace borrowers switching from marketplace platforms to high-interest credit such as payday loans, which are a well-known predictor of household hardship. To test this hypothesis, we exploit the fact that payday lending is illegal in New York state, while residents of Connecticut are able to obtain payday loans legally. We separately include New York (NY) and Connecticut (CT) in the treatment group. We first compare CT to all other states, excluding NY from the analysis, and, secondly, exclude CT from our sample in order to compare NY to all other states. Table VI presents the results. This test refutes the idea that an increase in payday lending may be responsible for the increase in bankruptcy rates. Importantly, we find that the effect of *Madden* on bankruptcy filings is statistically significant comparing CT (Panel A) and NY (Panel B) to other states. In fact, the effect of *Madden* on bankruptcy rates is stronger in NY than in CT. If consumers switching to other non-bank lending such as payday lending were responsible for the rise in bankruptcy following *Madden*, one would observe a stronger effect of the verdict on bankruptcy filings in CT where payday lending is legally available. However, we document that the treatment event raises bankruptcy rates more in NY compared to CT. This is attributable to the fact that the volume of marketplace lending as a share of the national total is much higher in NY than in CT.[49] This robustness test also shows that the rise in personal bankruptcy is proportional to the reduction in marketplace lending across states, further lending credence to interpreting changes in bankruptcy rates following *Madden* as arising primarily from changes in marketplace lending.

[TABLE VI - THE EFFECT OF *MADDEN* ON PERSONAL BANKRUPTCY BY AFFECTED STATE]

Finally, the increase in bankruptcy may be due to borrowers defaulting on their marketplace loans. The premise behind this alternative explanation, which we reject, is that 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. We replace the dependent variable with the number of charged-off loans in order to test this. Table VII, Panel B shows that the coefficients on the interaction term between *Madden* and *State* are not statistically significant which evinces that existing marketplace borrowers are not contributing to the rise in personal bankruptcy induced by *Madden*.[50]

[TABLE VII - THE EFFECT OF *MADDEN* ON MARKETPLACE LOAN DEFAULTS]

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49. Appendix A, Table A.1 shows that New York and Connecticut's share of total marketplace lending volume is 7.5% and 1.4% respectively.
50. This result is intuitive given that *Madden* leads to a contraction in lending to the riskiest borrowers. In Table A.7 in Appendix A we find that the average quality of borrowers (as measured using Prosper and Lending Club internal risk classifications) increases.

V.E. The Persistence of Madden's Effects

Our final test examines the persistency of the results presented in Tables II and III. We test whether the observed impact of *Madden* is merely a surprise effect and temporary adjustment by households in response to the unforeseen marketplace credit rationing in the first year following the court ruling, or if the effect on raising the incidence of personal bankruptcy is persistent.

To test the persistence of *Madden*'s effects we construct two new variables. The variable *SR-Madden* is equal to 1 for the twelve months following court ruling (June 2015 to May 2016), and zero otherwise, and captures the short-run effects of *Madden*. The variable *LR-Madden* is equal to 1 for the months from June 2016 to December 2017, and zero otherwise, and measures the long-run effect of restrictions on marketplace lending. We interact both terms with *State* and use it instead of the *Madden*State* interaction in specifications (1) and (2).

Table VIII documents that *Madden* leads to a persistent increase in the number of bankruptcies. In fact, the rationing of marketplace credit and the rise in personal bankruptcy intensifies over time. The marketplace lending volume drops by 7.3% in the short-run and by 12.1% in the long-run. The resulting effects on personal bankruptcy are proportional to the persistence and intensification of marketplace credit rationing over time. Following marketplace credit rationing, the number of bankruptcy cases increases by 6.8% in the first twelve months and by 9% in the months one year after the court verdict.[51] These estimates reveal that the increase in bankruptcy is not merely the result of a transitory adjustment of households in response to the abrupt pullback of marketplace credit following *Madden*. The results indicate that restricting marketplace lending increases personal bankruptcy filings persistently.

[TABLE VIII – *MADDEN*'S PERSISTENT EFFECT ON CREDIT RATIONING AND BANKRUPTCY]

VI. CONCLUDING REMARKS

We assess the real effects of financial technology in terms of its impact on household hardship. We document that the suddenly binding constraint of statutory interest rate limits placed on marketplace loans by a court verdict leads to a significant pullback of marketplace lending and is associated with a rise in personal bankruptcy. Our results suggest that withdrawing access to new lending technology has adverse welfare effects in terms of raising the incidence of personal bankruptcy, particularly among households on low incomes.

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51. The increase in Chapter 13 bankruptcies is less pronounced in the first year, while Chapter 7 cases increase homogenously in the short- and long-run, apart from Chapter 7 business bankruptcies.

While our paper suggests that marketplace lending may have some positive welfare effects compared with other forms of costly credit, such as payday loans and credit card debt, which are associated with worsening the incidence of personal bankruptcy, the next important step is to assess how marketplace lending affects other outcomes measuring household welfare aside from bankruptcy.

Our 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, our findings suggest that improving fintech lending regulations may improve access to marketplace funding and help alleviate financial hardship in terms of personal bankruptcy among low-income households.[52] Policymakers in the U.S. are debating whether to overturn the verdict of the Second Circuit Court of Appeals. The H.R.3299 bill currently pending in the U.S. Senate argues that *Madden* led to a "lack of access to safe and affordable financial services" for the poorest households. Our paper provides material evidence to inform this claim. 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. 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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52. There exist a number of concerns regarding marketplace lending relating to consumer protection and market conduct as well as implications for macroeconomic 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).

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TABLE I

SUMMARY STATISTICS

Variable	N	Mean	St Dev	Min	Median	Max
Dependent variables						
LN(1+Volume of marketplace lending)	2,700	15.66	1.28	8.29	15.77	18.89
LN(1+Volume of marketplace lending) Borrower Rating 1	2,700	9.78	3.80	0.00	10.95	14.79
LN(1+Volume of marketplace lending) Borrower Rating 2	2,700	12.09	1.94	0.00	12.38	15.55
LN(1+Volume of marketplace lending) Borrower Rating 3	2,700	13.18	1.73	0.00	13.38	16.51
LN(1+Volume of marketplace lending) Borrower Rating 4	2,700	13.95	1.46	0.00	14.11	17.18
LN(1+Volume of marketplace lending) Borrower Rating 5	2,700	14.40	1.32	0.00	14.51	17.54
LN(1+Volume of marketplace lending) Borrower Rating 6	2,700	14.26	1.39	0.00	14.36	17.53
LN(1+Volume of marketplace lending) Borrower Rating 7	2,700	13.56	1.65	0.00	13.69	17.33
LN(1+Number of marketplace loans)	2,700	6.11	1.24	0.69	6.23	9.25
LN(1+Number of marketplace loans) Borrower Rating 1	2,700	1.87	1.17	0.00	1.79	5.56
LN(1+Number of marketplace loans) Borrower Rating 2	2,700	2.95	1.18	0.00	3.04	6.26
LN(1+Number of marketplace loans) Borrower Rating 3	2,700	3.70	1.24	0.00	3.78	6.80
LN(1+Number of marketplace loans) Borrower Rating 4	2,700	4.42	1.25	0.00	4.53	7.53
LN(1+Number of marketplace loans) Borrower Rating 5	2,700	4.83	1.25	0.00	4.94	7.94
LN(1+Number of marketplace loans) Borrower Rating 6	2,700	4.78	1.23	0.00	4.88	7.97
LN(1+Number of marketplace loans) Borrower Rating 7	2,700	4.11	1.29	0.00	4.17	7.66
LN(1+Relevant loans)	2,700	15.52	1.26	8.29	15.63	18.73
LN(1+Debt refinancing loans)	2,700	15.27	1.27	8.29	15.37	18.45
LN(1+Medical expenses loans)	2,700	10.06	3.63	0.00	11.08	14.55
LN(1+Small business loans)	2,700	10.28	3.51	0.00	11.24	14.80
LN(1+Other loans)	2,700	13.57	1.53	0.00	13.69	17.09
LN(1+Number of bankruptcies/workforce)	2,700	1.63	0.42	0.38	1.66	2.64
LN(1+Number of chapter 7 bankruptcies/workforce)	2,700	1.32	0.37	0.30	1.35	2.31
LN(1+Number of chapter 11 bankruptcies/workforce)	2,700	0.04	0.11	0.00	0.03	2.00
LN(1+Number of chapter 12 bankruptcies/workforce)	2,700	0.00	0.01	0.00	0.00	0.08
LN(1+Number of chapter 13 bankruptcies/workforce)	2,700	0.80	0.46	0.05	0.76	2.19
LN(1+Number of business bankruptcies/workforce)	2,700	0.13	0.12	0.00	0.11	2.06
LN(1+Number of chapter 7 business bankruptcies/workforce)	2,700	0.08	0.04	0.00	0.08	0.64
LN(1+Number of chapter 11 business bankruptcies/workforce)	2,700	0.04	0.11	0.00	0.02	2.00
LN(1+Number of chapter 12 business bankruptcies/workforce)	2,700	0.00	0.01	0.00	0.00	0.08
LN(1+Number of chapter 13 business bankruptcies/workforce)	2,700	0.01	0.01	0.00	0.01	0.16
LN(1+Number of consumer bankruptcies/workforce)	2,700	1.60	0.43	0.37	1.62	2.63
LN(1+Number of chapter 7 consumer bankruptcies/workforce)	2,700	1.30	0.37	0.30	1.33	2.30
LN(1+Number of chapter 11 consumer bankruptcies/workforce)	2,700	0.01	0.01	0.00	0.00	0.12
LN(1+Number of chapter 13 consumer bankruptcies/workforce)	2,700	0.80	0.46	0.03	0.75	2.19
LN(1+Number of marketplace loan defaults)	2,700	3.45	1.38	0.00	3.58	7.06
LN(1+Number of marketplace loan defaults) Borrower Rating 1	2,700	0.55	0.67	0.00	0.00	3.33
LN(1+Number of marketplace loan defaults) Borrower Rating 2	2,700	1.16	0.94	0.00	1.10	4.39
LN(1+Number of marketplace loan defaults) Borrower Rating 3	2,700	1.74	1.16	0.00	1.79	5.12
LN(1+Number of marketplace loan defaults) Borrower Rating 4	2,700	2.14	1.23	0.00	2.20	5.45
LN(1+Number of marketplace loan defaults) Borrower Rating 5	2,700	2.40	1.26	0.00	2.48	5.93
LN(1+Number of marketplace loan defaults) Borrower Rating 6	2,700	1.96	1.18	0.00	1.95	5.54
LN(1+Number of marketplace loan defaults) Borrower Rating 7	2,700	0.99	0.92	0.00	0.69	4.25
LN(1+Non-marketplace consumer loans)	900.00	19.74	2.56	12.27	19.47	24.13
Main explanatory variables						
Court ruling*State	2,700	0.02	0.15	0	0	1
State	2,700	0.04	0.21	0	0	1
Court ruling	2,700	0.52	0.50	0	1	1
Control variables						
Unemployment (% of workforce)	2,700	5.38	1.46	2.10	5.20	10.40
LN(1+Total assets)	2,700	11.20	2.67	0.00	11.66	20.18
LN(1+Requested funds)	2,700	17.57	1.41	8.29	17.70	20.91

Notes. This table presents summary statistics for all dependent and explanatory variables. All variables are measured at a monthly frequency apart from *Income*. Income is measured at quarterly frequency.

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TABLE II
THE EFFECT OF *MADDEN* ON MARKETPLACE LENDING

Panel A: Intensive margin

Dependent variable: LN(1+Volume of marketplace loans)

Borrower rating:	ALL	ALL	1	2	3	4	5	6	7
Madden*State	-0.158***	-0.102***	-1.715***	-0.654***	-0.471***	-0.328***	-0.021	0.038**	0.021
	(-4.63)	(-7.64)	(-7.69)	(-10.67)	(-13.07)	(-12.51)	(-0.59)	(2.42)	(0.72)
State	1.096*								
	(1.81)								
Madden	0.890***								
	(30.55)								
Unemployment		-0.018***	0.400*	0.261***	0.111*	0.020	-0.008	0.007	0.090
		(-3.09)	(1.91)	(3.44)	(1.78)	(0.61)	(-0.89)	(0.43)	(1.21)
Total assets		-0.003	0.018	0.012	-0.048	-0.084	0.003	-0.030	-0.024
		(-1.01)	(0.27)	(0.35)	(-0.89)	(-1.10)	(0.32)	(-0.97)	(-0.79)
Requested funds		0.531***	0.963***	0.528***	0.803***	0.669***	0.714***	1.191***	1.285***
		(13.07)	(8.65)	(11.10)	(9.65)	(8.59)	(18.19)	(15.12)	(7.36)
State FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.147	0.993	0.570	0.679	0.764	0.897	0.967	0.920	0.835
SE Cluster	State	State	State	State	State	State	State	State	State

Panel B: Extensive margin

Dependent variable: LN(1+Number of marketplace loans)

Borrower rating:	ALL	ALL	1	2	3	4	5	6	7
Madden*State	-0.174***	-0.134***	-0.799***	-0.793***	-0.519***	-0.359***	-0.039	0.002	-0.005
	(-5.55)	(-7.62)	(-8.46)	(-28.41)	(-29.88)	(-21.89)	(-0.79)	(0.12)	(-0.36)
State	1.073*								
	(1.75)								
Madden	0.871***								
	(36.84)								
Controls	NO	YES	YES	YES	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.147	0.994	0.858	0.930	0.961	0.978	0.986	0.985	0.976
SE Cluster	State	State	State	State	State	State	State	State	State

Panel C: By purpose of the loan

Dependent variables:	LN(1+Relevant loans)	LN(1+Relevant loans)	LN(1+ debt refinancing loans)	LN(1+ medical expenses loans)	LN(1+small business loans)	LN(1+other loans)
Madden*State	-0.160***	-0.101***	-0.162***	-1.130***	-0.395***	-0.164***
	(-4.65)	(-8.67)	(-6.92)	(-4.96)	(-2.78)	(-7.19)
State	1.074*					
	(1.78)					
Madden	0.846***					
	(27.72)					
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.136	0.992	0.990	0.613	0.512	0.908
SE Cluster	State	State	State	State	State	State

Notes. This table reports the coefficients and t-statistics (in parentheses). Standard errors are clustered at the state level. The results in Panels A and B document the effect of *Madden* on the amount and number of marketplace loans obtained by borrowers through Lending Club and Prosper, respectively. The results presented in Panel C document the effect of *Madden* on the amount of loans by loan purpose. The main explanatory variable is an interaction term between the variable *Madden* (equal to 1 for months after the announcement of the verdict in *Madden vs Midland LLC* in May 2015, and zero otherwise) and *State* (equal to 1 for 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 Lending Club and Prosper by residents in each state per month (*Requested funds*). State and month fixed effects are included ("YES") or not included ("NO").
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

TABLE III
THE EFFECT OF *MADDEN* ON PERSONAL BANKRUPTCY

PANEL A: Total bankruptcies

Dependent variable: LN(1+Total number of bankruptcies/workforce)

	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 12	Chapter 13
Madden*State	0.067**	0.079**	0.059***	0.005	-0.000	0.103**
	(2.35)	(2.60)	(3.87)	(0.45)	(-1.56)	(2.58)
State	-0.346***					
	(-5.56)					
Madden	-0.169***					
	(-12.08)					
Unemployment		0.038***	0.047***	0.003*	0.001**	0.008
		(3.73)	(4.69)	(1.96)	(2.15)	(0.99)
Total assets		-0.008**	-0.012***	0.009**	0.000	-0.002
		(-2.47)	(-4.39)	(2.63)	(1.13)	(-0.78)
Requested funds		-0.008	-0.005	-0.004	-0.000	-0.001
		(-0.85)	(-0.60)	(-0.86)	(-0.52)	(-0.24)
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.063	0.959	0.950	0.714	0.196	0.977
SE Cluster	State	State	State	State	State	State

PANEL B: Business bankruptcies

Dependent variable: LN(1+Number of business bankruptcies/workforce)

	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 12	Chapter 13
Madden*State	0.021	0.023	0.018**	0.005	-0.000	0.001
	(1.34)	(1.48)	(2.49)	(0.52)	(-1.56)	(1.41)
State	-0.023					
	(-1.35)					
Madden	-0.031***					
	(-9.90)					
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.016	0.743	0.478	0.714	0.196	0.236
SE Cluster	State	State	State	State	State	State

PANEL C: Consumer bankruptcies

Dependent variable: LN(1+Number of consumer bankruptcies/workforce)

	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 13
Madden*State	0.064**	0.076***	0.056***	0.000	0.103**
	(2.58)	(2.84)	(3.77)	(0.22)	(2.55)
State	-0.349***				
	(-5.49)				
Madden	-0.167***				
	(-11.92)				
Controls	NO	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700
R-squared	0.061	0.963	0.950	0.684	0.977
SE Cluster	State	State	State	State	State

Notes. This table reports the coefficients and t-statistics (in parentheses). Standard errors are clustered at the state level. The results in Panels A, B and C document the effect of *Madden* on the number of total, business and consumer bankruptcy filings, respectively. The main explanatory variable is an interaction term between the variable *Madden* (equal to 1 for months after the announcement of the verdict in *Madden vs Midland LLC* in May 2015, and zero otherwise) and *State* (equal to 1 for 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 Lending Club and Prosper by residents in each state per month (*Requested funds*). State and month fixed effects are included ("YES") or not included ("NO").
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

TABLE IV
THE EFFECT OF *MADDEN* ACROSS DIFFERENT 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:	LN(1+ Volume of loans)	LN(1+ Number of loans)	LN(1+ Volume of loans)	LN(1+ Number of loans)	LN(1+ Volume of loans)	LN(1+ Number of loans)	LN(1+ Volume of loans)	LN(1+ Number of loans)	LN(1+ Volume of loans)	LN(1+ Number of loans)
Madden*State	-1.022***	-0.519***	-0.558***	-0.475***	-0.316***	-0.269***	0.026	-0.064***	-0.006	-0.029
	(-4.05)	(-4.96)	(-5.08)	(-6.11)	(-5.60)	(-5.26)	(1.31)	(-5.23)	(-0.30)	(-1.63)
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
State FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.572	0.850	0.884	0.970	0.932	0.980	0.897	0.985	0.931	0.986
SE Cluster	State	State	State	State	State	State	State	State	State	State

Panel B: Bankruptcy rates

Dependent variable: LN(1+Number of bankruptcies/workforce)

Income range:	<$25,000			$25,000-$49,999			$50,000-$74,999			$75,000-$99,999			>$100,000		
Bankruptcy type:	Total	Business	Consumer	Total	Business	Consumer	Total	Business	Consumer	Total	Business	Consumer	Total	Business	Consumer
Madden*State	0.085***	0.009*	0.081***	0.073***	0.002**	0.071***	0.047***	0.000	0.046***	0.002	0.001***	0.001	0.000	0.000	0.000
	(7.96)	(1.95)	(7.65)	(5.11)	(2.47)	(4.59)	(5.66)	(0.44)	(5.65)	(0.15)	(3.53)	(0.05)	(0.56)	(0.69)	(0.50)
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
State 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
Observations	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.938	0.523	0.938	0.937	0.302	0.937	0.915	0.224	0.915	0.848	0.113	0.848	0.117	0.043	0.119
SE Cluster	State	State	State	State	State	State	State	State	State	State	State	State	State	State	State

Notes. This table reports the coefficients and t-statistics (in parentheses). Standard errors are clustered at the state level.. The results in Panel A explain the effect of *Madden* on the amount and number of marketplace loans obtained by borrowers through Lending Club and Prosper. Panel B documents the effect of the *Madden* 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 *Madden* (equal to 1 for months after the announcement of the verdict in *Madden vs Midland LLC* in May 2015, and zero otherwise) and *State* (equal to 1 for 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 Lending Club and Prosper by residents in each state per month (*Requested funds*). State and month fixed effects are included ("YES") or not included ("NO").
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level

TABLE V

MADDEN AND NON-MARKETPLACE CONSUMER CREDIT

Panel A: Effect of Madden on non-marketplace consumer credit

Dependent variable:	LN(1+Volume of marketplace loans)	LN(1+Credit card loans)	LN(1+ Auto loans)	LN(1+Student loans)
Madden*State	-0.098***	-0.004	-0.019*	-0.010
	(-6.50)	(-0.47)	(-1.87)	(-0.38)
Unemployment	-0.017**	0.001	-0.021***	-0.019**
	(-2.46)	(0.36)	(-4.05)	(-2.65)
Total assets	0.011	-0.010	-0.010	-0.004
	(0.42)	(-1.60)	(-1.23)	(-0.46)
Requested funds	0.406***	-0.005	0.029***	-0.008
	(28.14)	(-1.18)	(10.21)	(-0.88)
State FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Observations	225	225	225	225
R-squared	0.999	0.994	0.992	0.990
SE Cluster	State	State	State	State

Panel B: Effect of Madden on personal bankruptcy controlling for non-marketplace consumer credit

Dependent variable:	LN(1+Total bankruptcies/ workforce)		LN(1+Total business bankruptcies/ workforce)		LN(1+Total consumer bankruptcies/workforce)	
Madden*State	0.084**	0.067***	0.022	0.022*	0.066**	0.064***
	(2.45)	(3.90)	(1.25)	(1.92)	(2.58)	(4.24)
Unemployment	0.017		0.006		0.017	
	(1.36)		(1.31)		(1.39)	
Total assets		-0.022		0.024*		-0.028
		(-0.88)		(1.95)		(-1.13)
Requested funds		0.018		0.001		0.017
		(1.40)		(0.21)		(1.36)
Credit card loans (ln)		1.249***		0.191		1.224***
		(2.83)		(1.08)		(2.81)
Auto loans (ln)		-1.181***		-0.074		-1.205***
		(-3.53)		(-0.65)		(-3.60)
Student loans (ln)		0.059		-0.084		0.064
		(0.28)		(-1.08)		(0.29)
State FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Observations	225	225	225	225	225	225
R-squared	0.986	0.989	0.965	0.969	0.984	0.990
SE Cluster	State	State	State	State	State	State

Notes. This table reports the coefficients and t-statistics (in parentheses). Standard errors are clustered at the state level. The results in Panel A document the effect of *Madden* on the annual volume of marketplace loans, credit card loans, auto loans and student loans. The results in Panel B document the effect of *Madden* on the number of total, business and consumer bankruptcy filings, while controlling for the volume of credit card loans, auto loans and student loans. Bankruptcies are measured as totals in each year. The main explanatory variable is an interaction term between the variable *Madden* (equal to 1 for months after the announcement of the verdict in *Madden vs Midland LLC* in May 2015, and zero otherwise) and *State* (equal to 1 for the affected states Connecticut and New York, and zero otherwise). Control variables include: yearly average 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 annual dollar amount of funds requested through Lending Club and Prosper by residents in each state per month (*Requested funds*). State and month fixed effects are included ("YES") or not included ("NO").
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

TABLE VI

THE EFFECT OF *MADDEN* ON PERSONAL BANKRUPTCY BY AFFECTED STATE

PANEL A: Treatment group includes only Connecticut

Dependent variable: LN(1+Total number of bankruptcies/workforce)

	All chapters	*All chapters*	*Chapter 7*	*Chapter 11*	*Chapter 12*	*Chapter 13*
Madden*State	0.032**	0.043***	0.052***	-0.010***	-0.000	0.051***
	(2.30)	(2.86)	(3.60)	(-6.65)	(-1.03)	(4.38)
Controls	NO	YES	YES	YES	YES	YES
State FE/Month FE	NO	YES	YES	YES	YES	YES
Observations	2,640	2,640	2,640	2,640	2,640	2,640
R-squared	0.052	0.959	0.950	0.716	0.196	0.977
SE Cluster	State	State	State	State	State	State

Dependent variable: LN(1+Number of business bankruptcies/workforce)

	All chapters	*All chapters*	*Chapter 7*	*Chapter 11*	*Chapter 12*	*Chapter 13*
Madden*State	-0.001	-0.002	0.009***	-0.007***	-0.000	0.002**
	(-0.17)	(-0.69)	(3.86)	(-4.83)	(-1.03)	(2.26)
Controls	NO	YES	YES	YES	YES	YES
State FE/Month FE	NO	YES	YES	YES	YES	YES
Observations	2,640	2,640	2,640	2,640	2,640	2,640
R-squared	0.017	0.744	0.478	0.715	0.196	0.236
SE Cluster	State	State	State	State	State	State

Dependent variable: LN(1+Number of consumer bankruptcies/workforce)

	All chapters	*All chapters*	*Chapter 7*	*Chapter 11*	*Chapter 13*
Madden*State	0.035**	0.046***	0.052***	-0.003***	0.050***
	(2.53)	(3.06)	(3.56)	(-8.84)	(4.35)
Controls	NO	YES	YES	YES	YES
State FE/Month FE	NO	YES	YES	YES	YES
Observations	2,640	2,640	2,640	2,640	2,640
R-squared	0.050	0.963	0.950	0.686	0.977
SE Cluster	State	State	State	State	State

PANEL B: Treatment group includes only New York

Dependent variable: LN(1+Total number of bankruptcies/workforce)

	All chapters	*All chapters*	*Chapter 7*	*Chapter 11*	*Chapter 12*	*Chapter 13*
Madden*State	0.101***	0.115***	0.066***	0.020***	-0.001**	0.156***
	(7.22)	(7.69)	(4.58)	(10.44)	(-2.51)	(13.48)
Controls	NO	YES	YES	YES	YES	YES
State FE/Month FE	NO	YES	YES	YES	YES	YES
Observations	2,640	2,640	2,640	2,640	2,640	2,640
R-squared	0.051	0.959	0.950	0.715	0.195	0.977
SE Cluster	State	State	State	State	State	State

Dependent variable: LN(1+Number of business bankruptcies/workforce)

	All chapters	*All chapters*	*Chapter 7*	*Chapter 11*	*Chapter 12*	*Chapter 13*
Madden*State	0.042***	0.043***	0.027***	0.017***	-0.001**	0.001
	(13.64)	(12.21)	(12.24)	(8.71)	(-2.51)	(0.90)
Controls	NO	YES	YES	YES	YES	YES
State FE/Month FE	NO	YES	YES	YES	YES	YES
Observations	2,640	2,640	2,640	2,640	2,640	2,640
R-squared	0.016	0.743	0.479	0.714	0.195	0.232
SE Cluster	State	State	State	State	State	State

Dependent variable: LN(1+Number of consumer bankruptcies/workforce)

	All chapters	*All chapters*	*Chapter 7*	*Chapter 11*	*Chapter 13*
Madden*State	0.093***	0.107***	0.060***	0.004***	0.156***
	(6.60)	(7.16)	(4.11)	(12.62)	(13.58)
Controls	NO	YES	YES	YES	YES
State FE/Month FE	NO	YES	YES	YES	YES
Observations	2,640	2,640	2,640	2,640	2,640
R-squared	0.050	0.963	0.951	0.688	0.977
SE Cluster	State	State	State	State	State

Notes. This table reports the coefficients and t-statistics (in parentheses). Standard errors are clustered at the state level. The results in Panel A and B document the effect of *Madden* on the number of total, business and consumer bankruptcy filings, respectively. 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 *Madden* (equal to 1 for months after the announcement of the verdict in *Madden vs Midland LLC* in May 2015, and zero otherwise) and *State* (equal to 1 for 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 Lending Club and Prosper by residents in each state per month (*Requested funds*). State and month fixed effects are included ("YES") or not included ("NO").
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level

TABLE VII

THE EFFECT OF *MADDEN* ON MARKETPLACE LOAN DEFAULTS

Dependent variable: LN(1+Number of marketplace loan defaults)

Borrower rating:	ALL	ALL	1	2	3	4	5	6	7
Madden*State	0.034	0.037	-0.011	-0.002	-0.083*	0.014	0.067*	-0.045	-0.086
	(0.76)	(1.38)	(-0.18)	(-0.06)	(-1.86)	(0.47)	(1.99)	(-1.45)	(-0.85)
State	1.099								
	(1.56)								
Madden	-0.049								
	(-1.43)								
Unemployment		0.017	0.060***	-0.001	0.005	0.041*	0.027	0.041*	-0.042*
		(1.22)	(3.07)	(-0.04)	(0.26)	(1.79)	(1.50)	(1.73)	(-1.87)
Total assets		-0.004	0.046	0.075	0.040	-0.001	-0.022	0.007	0.022
		(-0.13)	(0.71)	(1.44)	(0.94)	(-0.02)	(-0.45)	(0.07)	(0.40)
Requested funds		0.566***	0.011	0.112***	0.224***	0.273***	0.323***	0.283***	0.089***
		(11.78)	(0.56)	(6.30)	(5.33)	(6.62)	(6.90)	(8.74)	(3.33)
Controls	NO	YES	YES	YES	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.035	0.917	0.610	0.781	0.862	0.892	0.901	0.878	0.749
SE Cluster	State	State	State	State	State	State	State	State	State

Notes. This table reports the coefficients and t-statistics (in parentheses). Standard errors are clustered at the state level. The presented results document the effect of *Madden* on the number of marketplace loan defaults. The main explanatory variable is an interaction term between the variable *Madden* (equal to 1 for months after the announcement of the verdict in *Madden vs Midland LLC* in May 2015, and zero otherwise) and *State* (equal to 1 for 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 Lending Club and Prosper by residents in each state per month (*Requested funds*). State and month fixed effects are included ("YES") or not included ("NO").
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

TABLE VIII

MADDEN'S PERSISTENT EFFECT ON CREDIT RATIONING AND BANKRUPTCY

PANEL A: Marketplace lending

Dependent variable: LN(1+Volume of marketplace loans)

Borrower rating:	*ALL*	*1*	*2*	*3*	*4*	*5*	*6*	*7*
SR-Madden*State	-0.073***	-1.204***	-0.297***	-0.209***	-0.142***	-0.063**	-0.027	-0.059
	(-4.50)	(-5.01)	(-3.20)	(-4.97)	(-3.27)	(-2.65)	(-1.08)	(-1.32)
LR-Madden*State	-0.121***	-2.037***	-0.880***	-0.637***	-0.445***	0.005	0.078***	0.071*
	(-8.99)	(-8.58)	(-13.07)	(-12.97)	(-10.75)	(0.11)	(4.16)	(1.89)
Controls/State FE/ Month FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.993	0.570	0.680	0.764	0.897	0.967	0.920	0.835
SE Cluster	State	State	State	State	State	State	State	State

Dependent variable: LN(1+Number of marketplace loans)

Borrower rating:	*ALL*	*1*	*2*	*3*	*4*	*5*	*6*	*7*
SR-Madden*State	-0.082***	-0.135	-0.201***	-0.162***	-0.154***	-0.068*	-0.033	-0.034**
	(-3.60)	(-1.04)	(-3.79)	(-9.62)	(-6.50)	(-1.88)	(-1.13)	(-2.11)
LR-Madden*State	-0.167***	-1.219***	-1.167***	-0.744***	-0.489***	-0.020	0.024**	0.013
	(-10.68)	(-15.27)	(-33.89)	(-34.15)	(-22.71)	(-0.35)	(2.07)	(0.84)
Controls/State FE/ Month FE	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.994	0.863	0.933	0.962	0.979	0.986	0.985	0.976
SE Cluster	State	State	State	State	State	State	State	State

PANEL B: Bankruptcy rates

Dependent variable: LN(1+Total number of bankruptcies/workforce)

	All chapters	*Chapter 7*	*Chapter 11*	*Chapter 12*	*Chapter 13*
SR-Madden*State	0.066***	0.060***	0.002	0.001*	0.071***
	(4.78)	(5.11)	(0.36)	(1.92)	(4.91)
LR-Madden*State	0.087**	0.058***	0.007	-0.001*	0.124**
	(2.05)	(3.14)	(0.45)	(-1.91)	(2.17)
Controls/State FE/Month FE	YES	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700
R-squared	0.959	0.950	0.714	0.196	0.977
SE Cluster	State	State	State	State	State

Dependent variable: LN(1+Number of business bankruptcies/workforce)

	All chapters	*Chapter 7*	*Chapter 11*	*Chapter 12*	*Chapter 13*
SR-Madden*State	0.016*	0.013**	0.002	0.001*	0.002*
	(1.70)	(2.36)	(0.33)	(1.92)	(1.70)
LR-Madden*State	0.027	0.021**	0.006	-0.001*	0.001
	(1.37)	(2.52)	(0.54)	(-1.91)	(0.86)
Controls/State FE/Month FE	YES	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700
R-squared	0.743	0.478	0.714	0.196	0.236
SE Cluster	State	State	State	State	State

Dependent variable: LN(1+Number of consumer bankruptcies/workforce)

	All chapters	*Chapter 7*	*Chapter 11*	*Chapter 13*
SR-Madden*State	0.065***	0.058***	0.001	0.070***
	(5.03)	(4.90)	(0.49)	(4.70)
LR-Madden*State	0.084**	0.054***	0.000	0.124**
	(2.21)	(3.07)	(0.13)	(2.17)
Controls/State FE/Month FE	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700
R-squared	0.963	0.950	0.684	0.977
SE Cluster	State	State	State	State

Notes. This table replicates the results presented in Table II (Panel A and B) and Table III. We replace the interaction term *Madden*State* as the main explanatory variable with SR-Madden*State and SR-Madden*State capturing the short-run and long-run effects of *Madden*.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

FIGURE I
PARALLEL TRENDS

LN(1+Volume of marketplace loans)

LN(1+Number of marketplace loans)

LN(1+Total number of bankruptcies)
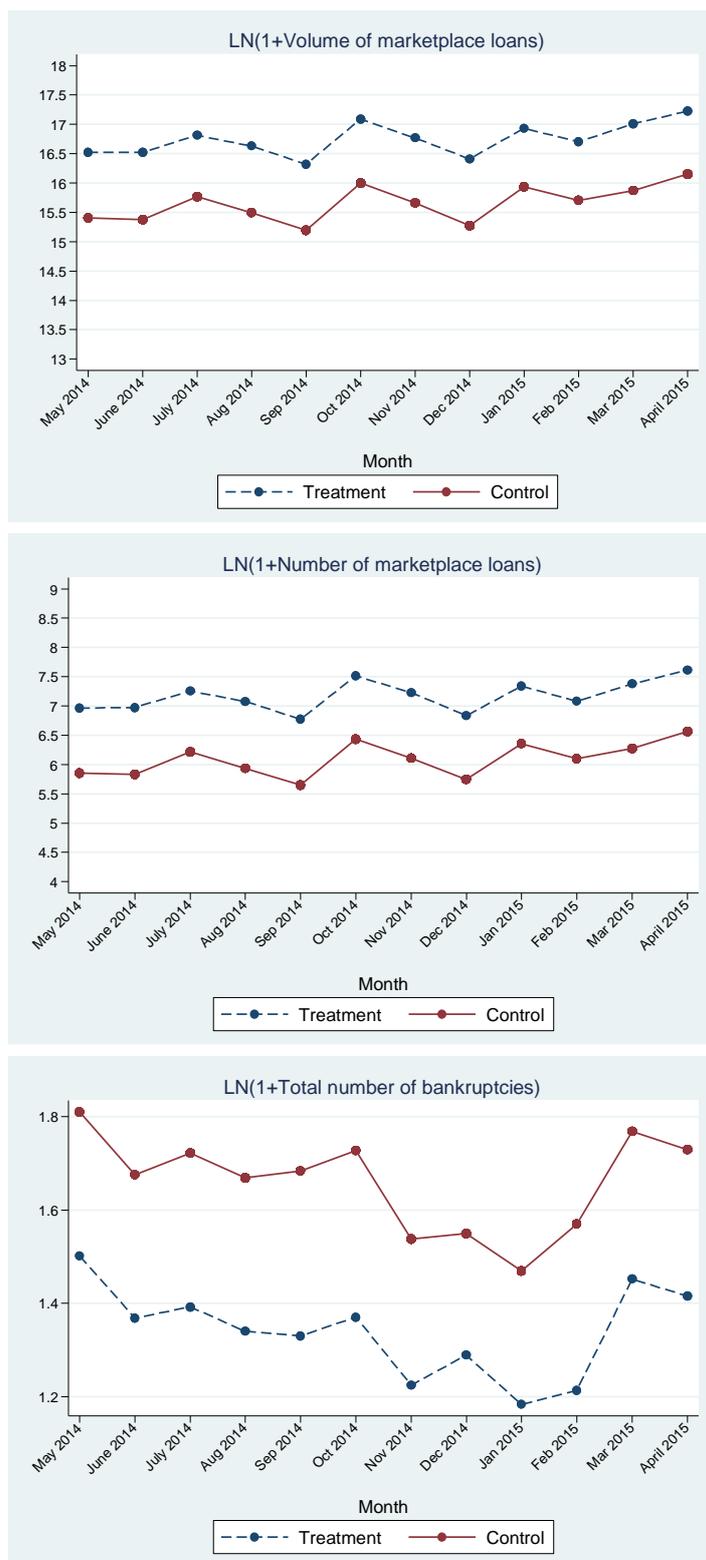
Notes. This figure presents the trends in the evolution of marketplace lending and total bankruptcy filings in the treatment and control group states in the 12 months preceding the treatment event. The figure shows that, prior to the court ruling, both marketplace lending and bankruptcy rates in the control and treatment group states evolve in a parallel manner.

FIGURE II
EFFECT OF MADDEN ON CONSUMER LOANS
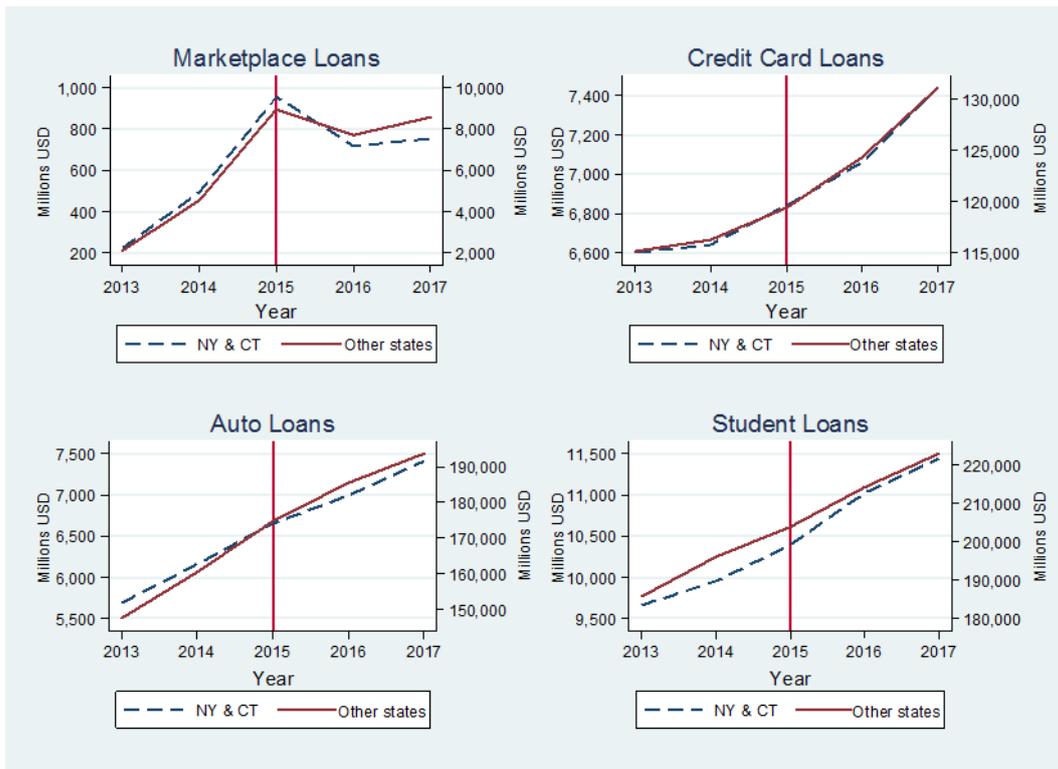
Notes. This figure presents the trends in the evolution of marketplace lending, credit card loans, auto loans and student loans prior to and following *Madden* verdict. It shows that apart from marketplace lending, other types of consumer loans are not significantly affected by *Madden*. A formal test for this, where we let the dependent variable be, respectively, the total annual volume of marketplace loans, credit card loans, auto loans and student loans, is presented in Table V, Panel A.

Appendix A – Additional Tests

TABLE A.1

ADDITIONAL SUMMARY STATISTICS

Panel A: Court district level data

Variable	N	Mean	St Dev	Min	Median	Max
Dependent variables						
Volume of marketplace lending	2,700	13,000,000.00	18,100,000.00	4,000.00	7,078,644.00	159,000,000.00
Volume of marketplace lending Borrower Rating 1	2,700	125,766.30	209,791.70	0.00	57,150.00	2,643,925.00
Volume of marketplace lending Borrower Rating 2	2,700	436,396.00	621,838.20	0.00	236,912.50	5,651,712.00
Volume of marketplace lending Borrower Rating 3	2,700	1,202,876.00	1,681,499.00	0.00	649,150.00	14,900,000.00
Volume of marketplace lending Borrower Rating 4	2,700	2,455,936.00	3,404,691.00	0.00	1,342,738.00	28,900,000.00
Volume of marketplace lending Borrower Rating 5	2,700	3,701,912.00	5,158,227.00	0.00	2,006,050.00	41,400,000.00
Volume of marketplace lending Borrower Rating 6	2,700	3,233,284.00	4,587,314.00	0.00	1,728,838.00	41,100,000.00
Volume of marketplace lending Borrower Rating 7	2,700	1,804,184.00	2,736,030.00	0.00	880,825.00	33,500,000.00
Number of marketplace loans	2,700	900.81	1,237.28	1.00	507.00	10,432.00
Number of marketplace loans Borrower Rating 1	2,700	12.24	21.35	0.00	5.00	259.00
Number of marketplace loans Borrower Rating 2	2,700	35.79	51.39	0.00	20.00	521.00
Number of marketplace loans Borrower Rating 3	2,700	78.80	108.53	0.00	43.00	899.00
Number of marketplace loans Borrower Rating 4	2,700	163.76	222.67	0.00	92.00	1,870.00
Number of marketplace loans Borrower Rating 5	2,700	249.68	343.62	0.00	139.00	2,802.00
Number of marketplace loans Borrower Rating 6	2,700	233.20	322.48	0.00	130.50	2,896.00
Number of marketplace loans Borrower Rating 7	2,700	127.34	187.12	0.00	64.00	2,112.00
Relevant loans	2,700	11,100,000.00	15,300,000.00	4,000.00	6,118,925.00	136,000,000.00
Debt refinancing loans	2,700	8,648,005.00	12,000,000.00	4,000.00	4,732,488.00	103,000,000.00
Medical expenses loans	2,700	148,947.00	246,008.70	0.00	64,950.00	2,086,036.00
Small business loans	2,700	156,252.40	249,318.60	0.00	76,050.00	2,672,050.00
Other loans	2,700	1,888,847.00	2,926,664.00	0.00	885,744.00	26,500,000.00
Number of bankruptcies	2,700	1,573.30	1,637.89	17.00	1,145.50	13,839.00
Number of chapter 7 bankruptcies	2,700	1,017.42	1,142.27	13.00	736.00	11,039.00
Number of chapter 11 bankruptcies	2,700	13.49	22.39	0.00	6.00	306.00
Number of chapter 12 bankruptcies	2,700	0.68	1.15	0.00	0.00	9.00
Number of chapter 13 bankruptcies	2,700	541.51	611.85	2.00	356.00	3,167.00
Number of business bankruptcies	2,700	46.55	58.97	0.00	29.00	441.00
Number of chapter 7 business bankruptcies	2,700	30.28	39.81	0.00	19.00	329.00
Number of chapter 11 business bankruptcies	2,700	11.41	20.16	0.00	5.00	306.00
Number of chapter 12 business bankruptcies	2,700	0.68	1.15	0.00	0.00	9.00
Number of chapter 13 business bankruptcies	2,700	3.98	5.39	0.00	2.00	45.00
Number of consumer bankruptcies	2,700	1,526.75	1,588.53	16.00	1,112.00	13,401.00
Number of chapter 7 consumer bankruptcies	2,700	987.14	1,107.13	13.00	714.00	10,716.00
Number of chapter 11 consumer bankruptcies	2,700	2.08	4.37	0.00	0.00	43.00
Number of chapter 13 consumer bankruptcies	2,700	537.53	608.35	1.00	352.50	3,153.00
Number of bankruptcies/workforce	2,700	4.56	2.34	0.47	4.24	12.99
Number of chapter 7 bankruptcies/workforce	2,700	3.00	1.44	0.36	2.87	9.04
Number of chapter 11 bankruptcies/workforce	2,700	0.05	0.22	0.00	0.03	6.40
Number of chapter 12 bankruptcies/workforce	2,700	0.00	0.01	0.00	0.00	0.09
Number of chapter 13 bankruptcies/workforce	2,700	1.51	1.42	0.06	1.13	7.96
Number of business bankruptcies/workforce	2,700	0.15	0.25	0.00	0.12	6.86
Number of chapter 7 business bankruptcies/workforce	2,700	0.09	0.05	0.00	0.08	0.89
Number of chapter 11 business bankruptcies/workforce	2,700	0.05	0.22	0.00	0.02	6.40
Number of chapter 12 business bankruptcies/workforce	2,700	0.00	0.01	0.00	0.00	0.09
Number of chapter 13 business bankruptcies/workforce	2,700	0.01	0.01	0.00	0.01	0.18
Number of consumer bankruptcies/workforce	2,700	4.41	2.31	0.44	4.06	12.89
Number of chapter 7 consumer bankruptcies/workforce	2,700	2.91	1.42	0.36	2.78	8.94
Number of chapter 11 consumer bankruptcies/workforce	2,700	0.01	0.01	0.00	0.00	0.13
Number of chapter 13 consumer bankruptcies/workforce	2,700	1.50	1.42	0.03	1.12	7.94
LN(1+Number of bankruptcies)	2,700	6.73	1.32	2.89	7.04	9.54
LN(1+Number of chapter 7 bankruptcies)	2,700	6.31	1.25	2.64	6.60	9.31
LN(1+Number of chapter 11 bankruptcies)	2,700	1.92	1.22	0.00	1.95	5.73
LN(1+Number of chapter 12 bankruptcies)	2,700	0.36	0.52	0.00	0.00	2.30
LN(1+Number of chapter 13 bankruptcies)	2,700	5.39	1.64	1.10	5.88	8.06
LN(1+Number of business bankruptcies)	2,700	3.30	1.10	0.00	3.40	6.09
LN(1+Number of chapter 7 business bankruptcies)	2,700	2.90	1.08	0.00	3.00	5.80
LN(1+Number of chapter 11 business bankruptcies)	2,700	1.79	1.18	0.00	1.79	5.73
LN(1+Number of chapter 12 business bankruptcies)	2,700	0.00	0.01	0.00	0.00	0.08
LN(1+Number of chapter 13 business bankruptcies)	2,700	1.20	0.88	0.00	1.10	3.83
LN(1+Number of consumer bankruptcies)	2,700	6.69	1.34	2.83	7.01	9.50
LN(1+Number of chapter 7 consumer bankruptcies)	2,700	6.28	1.26	2.64	6.57	9.28
LN(1+Number of chapter 11 consumer bankruptcies)	2,700	0.67	0.83	0.00	0.00	3.78
LN(1+Number of chapter 13 consumer bankruptcies)	2,700	5.38	1.65	0.69	5.87	8.06
Average interest rate on marketplace loan	2,700	9.32	2.20	0.13	9.24	14.93
Average interest rate on marketplace loan Borrower Rating 1	2,700	9.81	8.83	0.00	8.19	30.99
Average interest rate on marketplace loan Borrower Rating 2	2,700	10.61	6.25	0.00	10.17	30.75
Average interest rate on marketplace loan Borrower Rating 3	2,700	11.71	4.53	0.00	11.56	25.87
Average interest rate on marketplace loan Borrower Rating 4	2,700	10.32	3.44	0.00	10.17	19.92
Average interest rate on marketplace loan Borrower Rating 5	2,700	10.56	2.47	0.00	10.76	16.29
Average interest rate on marketplace loan Borrower Rating 6	2,700	8.33	1.93	0.00	8.60	13.11
Average interest rate on marketplace loan Borrower Rating 7	2,700	5.75	1.25	0.00	5.78	8.90
Average rating of marketplace borrowers	2,700	5.00	0.20	2.00	5.00	6.08
Number of marketplace loan defaults	2,700	70.85	110.55	0.00	35.00	1,164.00
Number of marketplace loan defaults Borrower Rating 1	2,700	1.27	2.19	0.00	0.00	27.00

Number of marketplace loan defaults Borrower Rating 2	2,700	4.20	6.71	0.00	2.00	80.00
Number of marketplace loan defaults Borrower Rating 3	2,700	10.21	16.42	0.00	5.00	167.00
Number of marketplace loan defaults Borrower Rating 4	2,700	16.35	25.24	0.00	8.00	232.00
Number of marketplace loan defaults Borrower Rating 5	2,700	22.21	34.99	0.00	11.00	375.00
Number of marketplace loan defaults Borrower Rating 6	2,700	13.18	21.60	0.00	6.00	254.00
Number of marketplace loan defaults Borrower Rating 7	2,700	3.43	6.17	0.00	1.00	69.00
Non- marketplace consumer loans	2,700	3,430,000,000	6,380,000,000	212,705.40	285,000,000	30,300,000,000
Control variables						
Unemployment (% of workforce)	2,700	5.38	1.46	2.10	5.20	10.40
Total assets	2,700	570,920.60	12,100,000	0.00	115,699.20	582,000,000
Requested funds	2,700	96,200,000	142,000,000	4,000	48,500,000	1,210,000,000

Panel B: Other summary statistics

Variable	Mean	Min	Max
Total business bankruptcy fillings/Total bankruptcy fillings	3.82%	0.00%	66.13%
Total consumer bankruptcy fillings/Total bankruptcy fillings	96.18%	33.87%	100.00%
Total Chapter 7 bankruptcy fillings/Total bankruptcy fillings	68.52%	21.03%	96.94%
Total Chapter 11 bankruptcy fillings/Total bankruptcy fillings	1.21%	0.00%	61.69%
Total Chapter 12 bankruptcy fillings/Total bankruptcy fillings	0.08%	0.00%	6.90%
Total Chapter 13 bankruptcy fillings/Total bankruptcy fillings	30.15%	3.06%	78.77%
Chapter 7 business bankruptcy fillings/Total bankruptcy fillings	67.68%	0.00%	100.00%
Chapter 11 business bankruptcy fillings/Total bankruptcy fillings	20.39%	0.00%	100.00%
Chapter 12 business bankruptcy fillings/Total bankruptcy fillings	2.33%	0.00%	100.00%
Chapter 13 business bankruptcy fillings/Total bankruptcy fillings	9.29%	0.00%	100.00%
Chapter 7 consumer bankruptcy fillings/Total bankruptcy fillings	68.94%	19.34%	97.56%
Chapter 11 consumer bankruptcy fillings/Total bankruptcy fillings	0.13%	0.00%	4.17%
Chapter 13 consumer bankruptcy fillings/Total bankruptcy fillings	30.93%	2.44%	80.66%
Marketplace loan value: Borrower rating 1/Total marketplace loans	0.94%	0.00%	16.26%
Marketplace loan value: Borrower rating 2/Total marketplace loans	3.56%	0.00%	100.00%
Marketplace loan value: Borrower rating 3/Total marketplace loans	9.32%	0.00%	36.00%
Marketplace loan value: Borrower rating 4/Total marketplace loans	18.85%	0.00%	51.12%
Marketplace loan value: Borrower rating 5/Total marketplace loans	28.65%	0.00%	66.67%
Marketplace loan value: Borrower rating 6/Total marketplace loans	25.31%	0.00%	66.24%
Marketplace loan value: Borrower rating 7/Total marketplace loans	13.37%	0.00%	34.67%
Number of marketplace loans: Borrower rating 1/Total number of marketplace loans	1.28%	0.00%	22.22%
Number of marketplace loans: Borrower rating 2/Total number of marketplace loans	4.21%	0.00%	100.00%
Number of marketplace loans: Borrower rating 3/Total number of marketplace loans	8.83%	0.00%	33.33%
Number of marketplace loans: Borrower rating 4/Total number of marketplace loans	18.27%	0.00%	47.06%
Number of marketplace loans: Borrower rating 5/Total number of marketplace loans	27.59%	0.00%	50.00%
Number of marketplace loans: Borrower rating 6/Total number of marketplace loans	26.28%	0.00%	53.85%
Number of marketplace loans: Borrower rating 7/Total number of marketplace loans	13.54%	0.00%	33.68%
Relevant marketplace loan value/Total marketplace loan value	87.04%	45.54%	100.00%
Debt consolidation marketplace loan value/Total marketplace loan value	69.84%	39.54%	100.00%
Small business marketplace loan value/Total marketplace loan value	9.56%	0.03%	15.56%
Medical expenses marketplace loan value/Total marketplace loan value	7.64%	0.02%	38.33%
Other marketplace loan value/Total marketplace loan value	12.96%	0.75%	100.00%

Panel C: Marketplace loans and bankruptcy filings by treatment state

Affected state: Connecticut

Variable	U.S. Total	Connecticut Total	Connecticut Total as % of U.S. Total
Volume of marketplace loans ($)	35,000,000,000	502,000,000	1.430%
Number of marketplace loans	2,432,191	33,844	1.392%
Total bankruptcy filings	4,247,918	31,860	0.750%
Business bankruptcy filings	125,688	1,257	0.999%
Consumer bankruptcy filings	4,122,230	30,603	0.742%

Affected state: New York

Variable	U.S. Total	New York Total	New York Total as % of U.S. Total
Volume of marketplace loans ($)	35,000,000,000	2,640,000,000	7.552%
Number of marketplace loans	2,432,191	183,524	7.546%
Total bankruptcy filings	4,247,918	163,109	3.840%
Business bankruptcy filings	125,688	8,539	6.794%
Consumer bankruptcy filings	4,122,230	154,570	3.750%

Affected state: Vermont

Variable	U.S. Total	Vermont Total	Vermont Total as % of U.S. Total
Volume of marketplace loans ($)	35,000,000,000	59,500,000	0.170%
Number of marketplace loans	2,432,191	4,446	0.183%
Total bankruptcy filings	4,247,918	3,426	0.081%
Business bankruptcy filings	125,688	208	0.165%
Consumer bankruptcy filings	4,122,230	3,218	0.078%

Notes. This table presents additional summary statistics.

TABLE A.2
RESULTS BASED ON MATCHED SAMPLE

PANEL A: Marketplace lending

Dependent variable: LN(1+Volume of marketplace loans)

Borrower rating:	ALL	ALL	1	2	3	4	5	6	7
Madden*State	-0.185***	-0.107***	-0.734***	-0.560***	-0.422***	-0.354***	-0.028	0.004	0.041
	(-5.70)	(-7.58)	(-5.30)	(-18.23)	(-11.23)	(-15.22)	(-0.70)	(0.24)	(0.95)
Observations	600	600	600	600	600	600	600	600	600
R-squared	0.176	0.994	0.662	0.939	0.975	0.985	0.990	0.990	0.870
Controls	NO	YES	YES	YES	YES	YES	YES	YES	YES
State FE & Month FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
SE Cluster	State	State	State	State	State	State	State	State	State

Dependent variable: LN(1+Number of marketplace loans)

Borrower rating:	ALL	ALL	1	2	3	4	5	6	7
Madden*State	-0.206***	-0.145***	-0.925***	-0.834***	-0.550***	-0.387***	-0.049	-0.009	-0.003
	(-6.13)	(-8.05)	(-8.20)	(-30.26)	(-17.63)	(-18.02)	(-0.94)	(-0.61)	(-0.11)
Observations	600	600	600	600	600	600	600	600	600
R-squared	0.178	0.995	0.903	0.940	0.974	0.985	0.991	0.991	0.178
Controls	NO	YES	YES	YES	YES	YES	YES	YES	YES
State FE & Month FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
SE Cluster	State	State	State	State	State	State	State	State	State

Dependent variables:	LN(1+Relevant loans)	LN(1+Relevant loans)	LN(1+ debt refinancing loans)	LN(1+ medical expenses loans)	LN(1+small business loans)	LN(1+other loans)
Madden*State	-0.186***	-0.107***	-0.168***	-0.632**	-0.426**	-0.151***
	(-6.01)	(-7.68)	(-6.61)	(-2.28)	(-2.82)	(-7.99)
Controls	NO	YES	YES	YES	YES	YES
State FE & Month FE	NO	YES	YES	YES	YES	YES
Observations	600	600	600	600	600	600
R-squared	0.164	0.994	0.994	0.690	0.663	0.990
SE Cluster	State	State	State	State	State	State

PANEL B: Bankruptcy rates

Dependent variable: LN(1+Total number of bankruptcies/workforce)

	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 12	Chapter 13
Madden*State	0.112**	0.115**	0.091**	0.003	-0.000	0.150**
	(2.50)	(2.44)	(2.61)	(0.25)	(-1.33)	(3.09)
Controls	NO	YES	YES	YES	YES	YES
State FE & Month FE	NO	YES	YES	YES	YES	YES
Observations	600	600	600	600	600	600
R-squared	0.261	0.967	0.954	0.373	0.240	0.981
SE Cluster	State	State	State	State	State	State

Dependent variable: LN(1+Number of business bankruptcies/workforce)

	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 12	Chapter 13
Madden*State	0.019	0.018	0.017*	0.002	-0.000	-0.000
	(1.07)	(0.99)	(2.06)	(0.19)	(-1.33)	(-0.31)
Controls	NO	YES	YES	YES	YES	YES
State FE & Month FE	NO	YES	YES	YES	YES	YES
Observations	600	600	600	600	600	600
R-squared	0.102	0.538	0.721	0.338	0.240	0.371
SE Cluster	State	State	State	State	State	State

Dependent variable: LN(1+Number of consumer bankruptcies/workforce)

	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 13
Madden*State	0.112**	0.115**	0.090**	0.001	0.151**
	(2.64)	(2.57)	(2.57)	(0.55)	(3.09)
Controls	NO	YES	YES	YES	YES
State FE & Month FE	NO	YES	YES	YES	YES
Observations	600	600	600	600	600
R-squared	0.259	0.968	0.955	0.698	0.981
SE Cluster	State	State	State	State	State

Notes. This table presents estimates using a matched sample. The matching procedure follows the nearest neighbor matching method by Lemmon and Roberts (2010). We match states based on the volume of marketplace lending prior to the treatment event. For each treated state we choose four nearest neighbor states from the control group.

TABLE A.3
INCLUDING VERMONT IN THE TREATMENT GROUP

PANEL A: Marketplace lending

Dependent variable: LN(1+Volume of marketplace loans)

Borrower rating:	*ALL*	*ALL*	*1*	*2*	*3*	*4*	*5*	*6*	*7*
Madden*State	-0.115**	-0.095***	-1.809***	-0.780***	-0.036	-0.296***	-0.022	0.106*	0.398
	(-2.38)	(-7.48)	(-8.64)	(-6.93)	(-0.10)	(-7.78)	(-0.88)	(1.85)	(1.32)
Observations	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.120	0.993	0.571	0.681	0.763	0.897	0.967	0.920	0.836
Controls	NO	YES	YES	YES	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
SE Cluster	State	State	State	State	State	State	State	State	State

Dependent variable: LN(1+Number of marketplace loans)

Borrower rating:	*ALL*	*ALL*	*1*	*2*	*3*	*4*	*5*	*6*	*7*
Madden*State	-0.145***	-0.129***	-0.855***	-0.845***	-0.538***	-0.400***	-0.059	0.004	0.021
	(-3.81)	(-9.07)	(-11.00)	(-21.95)	(-28.43)	(-13.16)	(-1.61)	(0.29)	(0.84)
Observations	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.120	0.994	0.862	0.933	0.962	0.979	0.986	0.985	0.976
Controls	NO	YES	YES	YES	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
SE Cluster	State	State	State	State	State	State	State	State	State

Dependent variables:	LN(1+Relevant loans)	LN(1+Relevant loans)	LN(1+ debt refinancing loans)	LN(1+ medical expenses loans)	LN(1+small business loans)	LN(1+other loans)
Madden*State	-0.101*	-0.119***	-0.198***	-0.338***	-0.203	-0.161***
	(-1.74)	(-3.86)	(-6.66)	(-2.56)	(-0.64)	(-7.45)
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.110	0.945	0.941	0.562	0.461	0.882
SE Cluster	State	State	State	State	State	State

PANEL B: Bankruptcy rates

Dependent variable: LN(1+Total number of bankruptcies/workforce)

	All chapters	*All chapters*	*Chapter 7*	*Chapter 11*	*Chapter 12*	*Chapter 13*
Madden*State	0.043	0.042	0.049***	0.004	0.001	0.041
	(1.48)	(1.12)	(3.08)	(0.59)	(0.90)	(0.73)
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.111	0.959	0.950	0.714	0.196	0.976
SE Cluster	State	State	State	State	State	State

Dependent variable: LN(1+Number of business bankruptcies/workforce)

	All chapters	*All chapters*	*Chapter 7*	*Chapter 11*	*Chapter 12*	*Chapter 13*
Madden*State	0.023**	0.023**	0.019***	0.004	0.001	-0.000
	(2.15)	(2.19)	(3.74)	(0.67)	(0.90)	(-0.14)
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.018	0.743	0.479	0.714	0.196	0.236
SE Cluster	State	State	State	State	State	State

Dependent variable: LN(1+Number of consumer bankruptcies/workforce)

	All chapters	*All chapters*	*Chapter 7*	*Chapter 11*	*Chapter 13*
Madden*State	0.040	0.038	0.045***	0.000	0.042
	(1.40)	(1.03)	(2.79)	(0.29)	(0.75)
Controls	NO	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700
R-squared	0.109	0.962	0.950	0.684	0.976
SE Cluster	State	State	State	State	State

Notes. This table reproduces the results presented in Tables II and III with Vermont included in the treatment group.

PANEL A: Measuring bankruptcy as bankruptcy/workforce

Dependent variable: Total number of bankruptcies/workforce

	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 12	Chapter 13
Madden*State	0.569***	0.683***	0.460***	0.006	-0.000	0.217***
	(4.27)	(4.87)	(5.47)	(0.54)	(-1.58)	(2.97)
Observations	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.067	0.945	0.912	0.497	0.194	0.975
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
SE Cluster	State	State	State	State	State	State

Dependent variable: Number of business bankruptcies/workforce

VARIABLES	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 12	Chapter 13
Madden*State	0.026	0.027	0.020**	0.006	-0.000	0.001
	(1.45)	(1.61)	(2.59)	(0.62)	(-1.58)	(1.42)
Observations	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.005	0.556	0.456	0.497	0.194	0.236
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
SE Cluster	State	State	State	State	State	State

Dependent variable: Number of consumer bankruptcies/workforce

VARIABLES	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 13
Madden*State	0.543***	0.656***	0.440***	0.001	0.216***
	(4.52)	(5.17)	(5.48)	(0.22)	(2.94)
Observations	2,700	2,700	2,700	2,700	2,700
R-squared	0.065	0.950	0.912	0.683	0.975
Controls	NO	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES
SE Cluster	State	State	State	State	State

PANEL B: Measuring bankruptcy as the log of one plus bankruptcy

Dependent variable: LN(1+Total number of bankruptcies)

	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 12	Chapter 13
Madden*State	0.063**	0.074**	0.050***	-0.004	-0.042	0.223**
	(2.09)	(2.10)	(2.85)	(-0.02)	(-1.22)	(2.36)
Observations	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.009	0.993	0.992	0.841	0.384	0.988
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
SE Cluster	State	State	State	State	State	State

Dependent variable: LN(1+Number of business bankruptcies)

VARIABLES	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 12	Chapter 13
Madden*State	0.118	0.132	0.173*	0.017	-0.042	-0.009
	(0.86)	(0.98)	(1.84)	(0.07)	(-1.22)	(-0.12)
Observations	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.029	0.926	0.917	0.815	0.384	0.750
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
SE Cluster	State	State	State	State	State	State

Dependent variable: LN(1+Number of consumer bankruptcies)

VARIABLES	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 13
Madden*State	0.059*	0.071**	0.045**	0.175	0.225**
	(1.97)	(2.17)	(2.56)	(0.61)	(2.34)
Observations	2,700	2,700	2,700	2,700	2,700
R-squared	0.008	0.994	0.992	0.781	0.988
Controls	NO	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES
SE Cluster	State	State	State	State	State

PANEL C: Measuring bankruptcy as the log of bankruptcy

Dependent variable: LN(Total number of bankruptcies)

	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 12	Chapter 13
Madden*State	0.075*	0.087**	0.063***	0.038	0.032	0.236**
	(1.98)	(2.15)	(3.27)	(0.13)	(0.54)	(2.43)
Observations	2,700	2,700	2,700	2,360	1,016	2,700
R-squared	0.058	0.958	0.953	0.681	0.757	0.954
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
SE Cluster	State	State	State	State	State	State

Dependent variable: LN(Number of business bankruptcies)

VARIABLES	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 12	Chapter 13
Madden*State	0.137	0.150	0.192*	0.051	0.032	-0.132
	(0.94)	(1.04)	(1.85)	(0.20)	(0.54)	(-1.00)
Observations	2,689	2,689	2,669	2,318	1,016	2,129
R-squared	0.055	0.642	0.485	0.651	0.757	0.452
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
SE Cluster	State	State	State	State	State	State

Dependent variable: LN(Number of consumer bankruptcies)

VARIABLES	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 13
Madden*State	0.071**	0.084**	0.058***	0.351	0.238**
	(2.17)	(2.35)	(3.08)	(1.13)	(2.40)
Observations	2,700	2,700	2,700	1,347	2,700
R-squared	0.056	0.960	0.954	0.728	0.953
Controls	NO	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES
SE Cluster	State	State	State	State	State

Notes. This table reproduces the results presented in Table III with the dependent variable being the number of bankruptcies scaled by the size of the workforce (measured in 10,000 workers) in Panel A; with the dependent variable expressed as the logarithm of one plus the number of bankruptcies (not scaled by workforce) in Panel B; and with the dependent variable expressed as the logarithm of the number of bankruptcies (not scaled by workforce) in Panel C.

TABLE A.5

BOOTSTRAPPED STANDARD ERRORS

PANEL A: Marketplace lending

Dependent variable: LN(1+Volume of marketplace loans)

Borrower rating:	*ALL*	*ALL*	*1*	*2*	*3*	*4*	*5*	*6*	*7*
Madden*State	-0.158**	-0.102***	-1.715***	-0.654***	-0.471***	-0.328***	-0.021	0.038	0.021
	(-2.09)	(-6.49)	(-6.15)	(-7.06)	(-6.96)	(-6.16)	(-0.79)	(1.12)	(0.51)
Observations	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.147	0.993	0.570	0.679	0.764	0.897	0.967	0.920	0.835
Controls	NO	YES	YES	YES	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES	YES	YES	YES

Dependent variable: LN(1+Number of marketplace loans)

Borrower rating:	*ALL*	*ALL*	*1*	*2*	*3*	*4*	*5*	*6*	*7*
Madden*State	-0.174**	-0.134***	-0.799***	-0.793***	-0.519***	-0.359***	-0.039**	0.002	-0.005
	(-2.08)	(-7.07)	(-7.62)	(-8.49)	(-8.82)	(-8.83)	(-2.07)	(0.10)	(-0.21)
Observations	2,705	2,705	2,705	2,705	2,705	2,705	2,705	2,705	2,705
R-squared	0.116	0.993	0.856	0.930	0.959	0.977	0.985	0.983	0.974
Controls	NO	YES	YES	YES	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES	YES	YES	YES

Dependent variables:	LN(1+Relevant loans)	LN(1+Relevant loans)	LN(1+ debt refinancing loans)	LN(1+ medical expenses loans)	LN(1+small business loans)	LN(1+other loans)
Madden*State	-0.160***	-0.101***	-0.162***	-1.129***	-0.399*	-0.163***
	(-2.81)	(-5.95)	(-7.76)	(-4.89)	(-1.86)	(-5.01)
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.136	0.992	0.990	0.613	0.512	0.908
SE Cluster	State	State	State	State	State	State

PANEL B: Bankruptcy filings

Dependent variable: LN(1+Total number of bankruptcies)

	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 12	Chapter 13
Madden*State	0.067***	0.079***	0.059***	0.005	-0.000	0.103***
	(2.89)	(6.33)	(4.98)	(0.88)	(-0.98)	(9.44)
Observations	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.063	0.959	0.950	0.714	0.196	0.977
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
SE Cluster	State	State	State	State	State	State

Dependent variable: LN(1+Number of business bankruptcies)

VARIABLES	All chapters	All chapters	Chapter 7	Chapter 11	Chapter 12	Chapter 13
Madden*State	0.021**	0.023***	0.018***	0.005	-0.000	0.001
	(2.52)	(3.53)	(4.49)	(0.83)	(-0.90)	(1.27)
Observations	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.016	0.743	0.478	0.714	0.196	0.236
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
SE Cluster	State	State	State	State	State	State

Dependent variable: LN(1+Number of consumer bankruptcies)

VARIABLES	All chapters	All chapters	Chapter 7	Chapter 11		Chapter 13
Madden*State	0.064**	0.076***	0.056***	0.000		0.103***
	(2.53)	(6.26)	(4.80)	(0.54)		(8.10)
Observations	2,700	2,700	2,700	2,700		2,700
R-squared	0.061	0.963	0.950	0.684		0.977
Controls	NO	YES	YES	YES		YES
State FE	NO	YES	YES	YES		YES
Month FE	NO	YES	YES	YES		YES
SE Cluster	State	State	State	State		State

Notes. This table reproduces the results presented in Tables II and III with bootstrapped standard errors.

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TABLE A.6
STANDARD ERRORS CLUSTERED AT THE STATE-MONTH LEVEL

PANEL A: Marketplace lending

Dependent variable: LN(1+Volume of marketplace loans)

Borrower rating:	*ALL*	*ALL*	*1*	*2*	*3*	*4*	*5*	*6*	*7*
Madden*State	-0.158*	-0.102***	-1.715***	-0.654***	-0.471***	-0.328***	-0.021	0.038	0.021
	(-1.85)	(-6.42)	(-6.27)	(-7.01)	(-6.92)	(-6.76)	(-0.85)	(1.30)	(0.53)
Observations	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.147	0.993	0.570	0.679	0.764	0.897	0.967	0.920	0.835
Controls	NO	YES	YES	YES	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
SE Cluster:	State-Month	State-Month	State-Month	State-Month	State-Month	State-Month	State-Month	State-Month	State-Month

Dependent variable: LN(1+Number of marketplace loans)

Borrower rating:	*ALL*	*ALL*	*1*	*2*	*3*	*4*	*5*	*6*	*7*
Madden*State	-0.172**	-0.134***	-0.799***	-0.793***	-0.519***	-0.359***	-0.039*	0.002	-0.005
	(-2.07)	(-7.85)	(-7.88)	(-8.69)	(-9.37)	(-9.26)	(-1.87)	(0.10)	(-0.20)
Observations	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.147	0.994	0.858	0.930	0.961	0.978	0.986	0.985	0.976
Controls	NO	YES	YES	YES	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES	YES	YES	YES
SE Cluster:	State-Month	State-Month	State-Month	State-Month	State-Month	State-Month	State-Month	State-Month	State-Month

Dependent variables:	LN(1+Relevant loans)	LN(1+Relevant loans)	LN(1+ debt refinancing loans)	LN(1+ medical expenses loans)	LN(1+small business loans)	LN(1+ other loans)
Madden*State	-0.160***	-0.101***	-0.162***	-1.129***	-0.399*	-0.163***
	(-2.81)	(-6.15)	(-8.13)	(-4.51)	(-1.76)	(-5.20)
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
Observations	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.136	0.992	0.990	0.613	0.512	0.908
SE Cluster:	State-Month	State-Month	State-Month	State-Month	State-Month	State-Month

PANEL B: Bankruptcy filings

Dependent variable: LN(1+Total number of bankruptcies)

	All chapters	*All chapters*	*Chapter 7*	*Chapter 11*	*Chapter 12*	*Chapter 13*
Madden*State	0.067***	0.079***	0.059***	0.005	-0.000	0.103***
	(2.74)	(6.94)	(5.33)	(0.85)	(-1.08)	(8.48)
Observations	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.063	0.959	0.950	0.714	0.196	0.977
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
SE Cluster:	State-Month	State-Month	State-Month	State-Month	State-Month	State-Month

Dependent variable: LN(1+Number of business bankruptcies)

VARIABLES	*All chapters*	*All chapters*	*Chapter 7*	*Chapter 11*	*Chapter 12*	*Chapter 13*
Madden*State	0.021**	0.023***	0.018***	0.005	-0.000	0.001
	(2.53)	(3.39)	(5.17)	(0.81)	(-1.08)	(1.26)
Observations	2,700	2,700	2,700	2,700	2,700	2,700
R-squared	0.016	0.743	0.478	0.714	0.196	0.236
Controls	NO	YES	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES	YES
SE Cluster:	State-Month	State-Month	State-Month	State-Month	State-Month	State-Month

Dependent variable: LN(1+Number of consumer bankruptcies)

VARIABLES	*All chapters*	*All chapters*	*Chapter 7*	*Chapter 11*	*Chapter 13*
Madden*State	0.064***	0.076***	0.056***	0.000	0.103***
	(2.63)	(7.02)	(5.02)	(0.57)	(8.48)
Observations	2,700	2,700	2,700	2,700	2,700
R-squared	0.061	0.963	0.950	0.684	0.977
Controls	NO	YES	YES	YES	YES
State FE	NO	YES	YES	YES	YES
Month FE	NO	YES	YES	YES	YES
SE Cluster:	State-Month	State-Month	State-Month	State-Month	State-Month

Notes. This table reproduces the results presented in Tables II and III with standard errors clustered at the state and month level.

TABLE A.7

THE EFFECT OF *MADDEN* ON MARKETPLACE BORROWER QUALITY

Dependent variable: LN(Average rating of marketplace borrowers)		
Madden*State	0.038***	0.043***
	(13.96)	(20.31)
State	0.004	
	(0.82)	
Madden	0.002	
	(0.79)	
Unemployment rate		-0.003
		(-0.92)
Total assets		0.000
		(0.03)
Requested funds		-0.003
		(-0.92)
Observations	2,700	2,700
R-squared	0.035	0.600
State FE	NO	YES
Month FE	NO	YES
SE Cluster	State	State

Notes. This table presents the effect of *Madden* on the rating of marketplace borrowers. Main explanatory variable is an interaction term between variable *Court ruling* (equal 1 for months after the announcement of the *Madden vs Midland LLC* verdict in May 2015, zero otherwise) and *State* (equal 1 for affected states Connecticut and New York, zero otherwise). Control variables include: state unemployment rates measured at monthly frequency (*Unemployment*), the logarithm of average total assets of residents filing for bankruptcy in each state and month (*Total assets*), and the logarithm of dollar amount of funds requested through Lending Club and Prosper by residents in each state and month (*Requested funds*). State and month fixed effects are included ("YES") or not included ("NO").
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

TABLE A.8
CONTROLLING FOR NON-MARKETPLACE CONSUMER CREDIT

Dependent variable:	LN(1+Non-Marketplace Consumer loans)	LN(1+Non-Marketplace Consumer loans)	LN(1+Total bankruptcies/ workforce)	LN(1+Total business bankruptcies/ workforce)	LN(1+Total consumer bankruptcies/ workforce)
	(1)	(2)	(3)	(4)	(5)
Madden*State	-0.084	-0.074	0.077**	0.024	0.074***
	(-1.21)	(-1.27)	(2.59)	(1.55)	(2.82)
State	3.358***				
	(8.20)				
Madden	0.143**				
	(2.15)				
Unemployment		-0.036	0.038***	0.008***	0.038***
		(-0.94)	(3.68)	(2.85)	(3.62)
Total assets		0.020	-0.019	-0.005	-0.016
		(0.45)	(-1.68)	(-0.68)	(-1.46)
Requested funds		0.001	-0.007	-0.003	-0.007
		(0.09)	(-0.82)	(-0.73)	(-0.83)
Non-marketplace consumer loans			-0.019	0.016	-0.024
			(-0.62)	(1.42)	(-0.75)
State FE	NO	YES	YES	YES	YES
Month FE	NO	NO	YES	YES	YES
Quarter FE	NO	YES	NO	NO	NO
Observations	900	900	2,700	2,700	2,700
R-squared	0.072	0.995	0.959	0.741	0.963
SE Cluster	State	State	State	State	State

Notes. This table presents in Columns 1 and 2 the results for the effect of *Madden* on the volume of consumer loans originated by traditional financial institutions in each state and quarter. Columns 3-5 report the results for the effect of *Madden* on bankruptcy filings with the volume of lending provided by traditional financial institution included as a control variable. The main explanatory variable is an interaction term between the variable *Madden* (equal to 1 for months after the announcement of the verdict in *Madden vs Midland LLC* in May 2015, and zero otherwise) and *State* (equal to 1 for the affected states Connecticut and New York, and zero otherwise). Control variables in Column 1 and 2 include quarterly averages of: 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 dollar amount of funds requested through Lending Club and Prosper by residents in each state and month (*Requested funds*). Control variables in Columns 3-5 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 dollar amount of funds requested through Lending Club and Prosper by residents in each state and month (*Requested funds*). Standard errors are clustered at the state level and t-statistics are presented in parentheses. State and quarter/month fixed effects are included ("YES") or not included ("NO").
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

TABLE A.9
CONTROLLING FOR NON-MARKETPLACE CONSUMER CREDIT USING QUARTERLY DATA

Dependent variable:	LN(1+Total bankruptcies/ workforce)	LN(1+Total business bankruptcies/ workforce)	LN(1+Total non-business bankruptcies/ workforce)
Madden*State	0.095**	0.026	0.094***
	(2.60)	(0.85)	(2.73)
Unemployment	0.072***	0.014*	0.073***
	(3.61)	(1.89)	(3.64)
Total assets	-0.027	0.001	-0.026
	(-0.93)	(0.05)	(-0.91)
Requested funds	-0.008	-0.010*	-0.009
	(-0.60)	(-1.69)	(-0.60)
Non-marketplace consumer loans	0.020	0.045	0.014
	(0.43)	(1.66)	(0.30)
State FE	YES	YES	YES
Quarter FE	YES	YES	YES
Observations	900	900	900
R-squared	0.873	0.901	0.873
SE Cluster	State	State	State

Notes. This table presents the effect of *Madden* on the number of bankruptcy filings. Main explanatory variable is an interaction term between variable *Madden* (equal 1 for months after the announcement of the *Madden vs Midland LLC* verdict in May 2015, zero otherwise) and *State* (equal 1 for affected states Connecticut and New York, zero otherwise). Control variables include quarterly averages of: monthly state unemployment rates (*Unemployment*), the logarithm of average total assets of residents filing for bankruptcy in each state and month (*Total assets*), the logarithm of dollar amount of funds requested through Lending Club and Prosper by residents in each state and month (*Requested funds*) and lending provided by traditional financial institutions (Non-Marketplace Consumer loans). Standard errors are clustered at the state level and t-statistics are presented in parentheses. State and quarter fixed effects are included ("YES") or not included ("NO").
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

TABLE A.10
TREATMENT AND CONTROL GROUP DIFFERENCES

Variable	Period	Control Mean	Treatment Mean	Difference	T-statistic
LN(1+Volume of marketplace loans)	t-1	16.15	17.22	-1.07	-1.37
LN(1+Volume of marketplace loans)	t-2	15.87	17.01	-1.14	-1.46
LN(1+Volume of marketplace loans)	t-3	15.70	16.70	-1.00	-1.25
LN(1+Volume of marketplace loans)	t-4	15.94	16.93	-0.99	-1.23
LN(1+Volume of marketplace loans)	t-5	15.27	16.41	-1.14	-1.42
LN(1+Volume of marketplace loans)	t-6	15.66	16.77	-1.11	-1.39
LN(1+Volume of marketplace loans)	t-7	16.00	17.09	-1.09	-1.36
LN(1+Volume of marketplace loans)	t-8	15.19	16.32	-1.13	-1.44
LN(1+Volume of marketplace loans)	t-9	15.49	16.63	-1.14	-1.45
LN(1+Volume of marketplace loans)	t-10	15.77	16.81	-1.04	-1.30
LN(1+Volume of marketplace loans)	t-11	15.38	16.53	-1.15	-1.43
LN(1+Volume of marketplace loans)	t-12	15.41	16.52	-1.11	-1.43
LN(1+Number of marketplace loans)	t-1	6.56	7.61	-1.05	-1.36
LN(1+Number of marketplace loans)	t-2	6.28	7.38	-1.10	-1.43
LN(1+Number of marketplace loans)	t-3	6.10	7.08	-0.98	-1.25
LN(1+Number of marketplace loans)	t-4	6.36	7.34	-0.98	-1.24
LN(1+Number of marketplace loans)	t-5	5.74	6.83	-1.09	-1.40
LN(1+Number of marketplace loans)	t-6	6.11	7.23	-1.12	-1.44
LN(1+Number of marketplace loans)	t-7	6.44	7.52	-1.08	-1.34
LN(1+Number of marketplace loans)	t-8	5.65	6.77	-1.12	-1.44
LN(1+Number of marketplace loans)	t-9	5.94	7.08	-1.14	-1.46
LN(1+Number of marketplace loans)	t-10	6.22	7.25	-1.03	-1.31
LN(1+Number of marketplace loans)	t-11	5.83	6.97	-1.14	-1.45
LN(1+Number of marketplace loans)	t-12	5.86	6.96	-1.10	-1.44
LN(1+Total bankruptcies/workforce)	t-1	1.73	1.42	0.31	1.07
LN(1+Total bankruptcies/workforce)	t-2	1.77	1.45	0.32	1.06
LN(1+Total bankruptcies/workforce)	t-3	1.57	1.21	0.36	1.23
LN(1+Total bankruptcies/workforce)	t-4	1.47	1.18	0.29	0.95
LN(1+Total bankruptcies/workforce)	t-5	1.55	1.29	0.26	0.93
LN(1+Total bankruptcies/workforce)	t-6	1.53	1.22	0.31	1.11
LN(1+Total bankruptcies/workforce)	t-7	1.73	1.37	0.36	1.19
LN(1+Total bankruptcies/workforce)	t-8	1.68	1.33	0.35	1.24
LN(1+Total bankruptcies/workforce)	t-9	1.67	1.34	0.33	1.07
LN(1+Total bankruptcies/workforce)	t-10	1.72	1.39	0.33	1.12
LN(1+Total bankruptcies/workforce)	t-11	1.68	1.37	0.31	1.03
LN(1+Total bankruptcies/workforce)	t-12	1.81	1.50	0.31	1.08

Notes. This table presents the mean values for our main dependent variables, differences in the means as well as t-statistics for the treatment and control group in the 12 months preceding the treatment event.

Appendix B – Treatment Event: Madden and Marketplace Lending

(1.) Prosper acknowledging risk emanating from the *Madden* court verdict in SEC filing:

"In addition, it is possible that state usury laws may impose liability that could affect an assignee's (i.e., PFL's and/or an investor who purchases Borrower Loans from PFL) ability to continue to charge to borrowers the interest rates that they agreed to pay at origination of their Borrower Loans. In particular, one recent judicial decision by the Court of Appeals for the Second Circuit, Madden v. Midland Funding, LLC (786 F.3d 246 (2d Cir. 2015)), concluded that the debt buyer of a charged off credit card account could not rely on the National Bank Act's preemption of state interest rate limits for interest at rates imposed by the debt buyer after charge-off. The decision has created some uncertainty as to whether non-bank entities purchasing loans originated by a bank may rely on federal preemption of state usury laws, and the decision may create an increased risk of litigation by plaintiffs challenging our ability to collect interest in accordance with the terms of Borrower Loans. Although the Madden decision specifically addressed preemption under the National Bank Act, such decision could support future challenges to federal preemption for other institutions, including an FDIC-insured, state chartered industrial bank like WebBank.

On November 10, 2015, the defendant in the Madden case filed a petition for a writ of certiorari with the United States Supreme Court for further review of the Second Circuit's decision. On June 27, 2016, the United States Supreme Court denied the petition and refused to review the case, leaving the decision of the Second Circuit intact and binding on federal courts in Connecticut, New York and Vermont. Although there can be no assurances as to the outcome of any potential litigation, or the possible impact of the litigation on our marketplace, we believe the Madden case addressed facts that are not presented by our marketplace lending platform and thus would not apply to Borrower Loans. Nevertheless, we and our counsel are monitoring the matter closely and, as developments warrant, we, of course, will consider any necessary changes to our marketplace required to avoid the impact of this case on our business model. Because of investor demand, the maximum annual percentage rate offered through our marketplace may be lower in some states than others."

Source: Prosper Marketplace, Prospectus, as filed with the SEC: https://prosper.com/Downloads/Legal/Prosper_Prospectus_2018-03-12.pdf.

(2.) Lending Club acknowledging risk emanating from the *Madden* court verdict in SEC filing:

"If the loans originated through our marketplace were found to violate a state's usury laws, and/or we were found to be the true lender (as opposed to our issuing bank(s)), your investment may lose substantial value and you may lose all of the interest due on your Note.

The interest rates that are charged to borrowers and that form the basis of payments to investors through our marketplace are enabled by legal principles including (i) the application of federal law to enable an issuing bank that originates the loan to export the interest rates of the jurisdiction where it is located, (ii) the application of common law "choice of law" principles based upon factors such as the loan document's terms and where the loan transaction is completed to provide uniform rates to borrowers, or (iii) the application of principles that allow the transferee of a loan to continue to collect interest as provided in the loan document. WebBank, the primary issuing bank of the loans originated through our marketplace, is chartered in, and operates out of, Utah, which allows parties to generally agree by contract to any interest rate. Certain states, including Utah, have no statutory interest rate limitations on personal loans, while other jurisdictions have a maximum rate. In some jurisdictions, the maximum rate is less than the current maximum rate offered by WebBank through our platform. If the laws of such jurisdictions were found to govern the loans originated through our marketplace (in conflict with the principles described above), those loans could be in violation of such laws.

In May 2015, the U.S. Court of Appeals for the Second Circuit issued its decision in Madden v. Midland Funding, LLC that interpreted the scope of federal preemption under the National Bank Act and held that a nonbank assignee of a loan originated by a national bank was not entitled to the benefits of federal preemption of claims of usury. The Second Circuit denied the defendant's (Midland Funding) motion to reconsider the decision and remanded the case to address choice of law matters. The Second Circuit's decision is binding on federal courts located in Connecticut, New York, and Vermont, but the decision could also be adopted by other courts. The defendant petitioned the U.S. Supreme Court to review the decision and in March 2016, the Court invited the Solicitor General to file a brief expressing the views of the U.S. on the petition. The Solicitor General filed an amicus brief that stated the Second Circuit decision was incorrect, but that the case was not yet ready to be heard by the Supreme Court. In June 2016, the Supreme Court declined to hear the case. The Federal District Court is now hearing the case in regard to Midland's alternative claim under a choice of law analysis, and application of state law. The outcome could create potential liability under state statutes such as usury and consumer protection statutes. [...]

If a borrower were to successfully bring claims against us for state usury law violations, and the rate on that borrower's personal loan was greater than that allowed under applicable state law, we could be subject to fines and penalties, including the voiding of loans and repayment of principal and interest to borrowers and investors. We might decide to limit the maximum interest rate on certain loans originated through our marketplace, and we might decide to originate loans under state-specific licenses, where such a ruling is applicable. These actions could adversely impact our returns on the corresponding member loans and Notes. Further, if we were unable to partner with another issuing bank, we would have to substantially modify our business operations from the manner currently contemplated and would be required to maintain state-specific licenses and only provide a limited range of interest rates for personal loans, all of which would substantially reduce our operating efficiency and attractiveness to investors and possibly result in a decline in our operating results.

There has been (and may continue to be) other litigation challenging lending arrangements where a bank or other third party has made a loan and then sells and assigns it to an entity that is engaged in assisting with the origination and servicing of a loan."

Source: Lending Club, Prospectus for Public Offering, as filed with the SEC: http://ir.lendingclub.com/Cache/c2000698265.html.