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

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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*

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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.,

[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

[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).

[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

[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

[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

[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,

[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

[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

[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

[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]

[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

[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]

[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

[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,

[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

[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*.

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]

[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

[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

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

[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

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.

[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

	CT & NY	Outside the 2nd Circuit	*t-score*
Principal Outstanding ($)	30.73	31.15	*0.53*
Loan Amount ($)	20,169	20,506	*3.60*
FICO Score	690	689	*-0.14*
Ask Price ($)	13.53	13.76	*0.32*
Term (Months)	50.06	50.68	*5.16*
Loan Age (Months)	16.94	16.28	*-6.30*
Interest Rate	19%	19%	*0.84*
Fifteen	0.51	0.48	*-4.99*
Num. Obs.	*10,543*	*84,675*	

Panel B: Notes Backed by Current Loans

	CT & NY	Outside the 2nd Circuit	*t-score*
Principal Outstanding ($)	33.23	33.62	*1.54*
Loan Amount ($)	19,736	20,008	*10.00*
FICO Score	695	694	*-6.03*
Ask Price ($)	33.60	34.00	*1.56*
Term (Months)	47.93	48.43	*14.38*
Loan Age (Months)	14.24	13.75	*-16.69*
Interest Rate	17%	17%	*-7.59*
Fifteen	0.41	0.40	*-10.87*
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

	CT & NY	No Usury	*t-score*
Principal Outstanding ($)	30.73	31.09	*-0.39*
Loan Amount ($)	20,169	20,795	*-4.65*
FICO Score	690	689	*0.97*
Ask Price ($)	13.53	13.08	*0.40*
Term (Months)	50.06	50.88	*-4.41*
Loan Age (Months)	16.94	16.57	*2.39*
Interest Rate	19%	19%	*-1.24*
Fifteen	0.51	0.48	*3.15*
Num. Obs.	*10,543*	*7,246*	

Panel B: Notes Backed by Current Loans

	CT & NY	No Usury	*t-score*
Principal Outstanding ($)	33.23	34.49	*-3.57*
Loan Amount ($)	19,736	20,406	*-17.35*
FICO Score	695	693	*13.15*
Ask Price ($)	33.60	34.90	*-3.56*
Term (Months)	47.93	48.70	*-15.21*
Loan Age (Months)	14.24	13.58	*14.89*
Interest Rate	17%	17%	*-1.85*
Fifteen	0.41	0.40	*11.14*
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

	CT & NY	PSM Sample	*t-score*
Principal Outstanding ($)	30.73	31.01	*-0.29*
Loan Amount ($)	20,169	20,008	*1.29*
FICO Score	690	690	*-1.42*
Ask Price ($)	13.53	13.84	*-0.36*
Term (Months)	50.06	50.21	*0.94*
Loan Age (Months)	16.94	17.01	*-0.48*
Interest Rate	19%	18%	*3.15*
Fifteen	0.51	0.51	*0.19*
Num. Obs.	*10,543*	*10,543*	

Panel B: Notes Backed by Current Loans

	CT & NY	PSM Sample	*t-score*
Principal Outstanding ($)	33.23	33.42	*-0.58*
Loan Amount ($)	19,558	19,513	*1.26*
FICO Score	695	695	*-3.97*
Ask Price ($)	33.60	33.78	*-0.55*
Term (Months)	47.93	48.41	*10.25*
Loan Age (Months)	14.24	14.30	*-1.43*
Interest Rate	17%	17%	*6.00*
Fifteen	0.41	0.41	*-0.23*
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 $FICO\ Score = \alpha + \beta_1 Post\text{-}Madden + \beta_2 NY\_CT + \beta_3 Post*NY\_CT + Controls + \varepsilon$. 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	Sub750	Sub700	All Borrowers	Sub750	Sub700
	(1)	(2)	(3)	(1)	(2)	(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-Madden* + β_2*NY_CT* + β_3*Post*NY_CT* + *Controls* + ε. 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	No Usury State	PSM Sample	Outside 2nd Circuit	No Usury State	PSM Sample
	(1)	(2)	(3)	(1)	(2)	(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 $Delinquent = \alpha + \beta_1 Post\text{-}Madden + \beta_2 NY\_CT + \beta_3 Above16 + \beta_4 Post*NY\_CT + \beta_5 Above16*Post + \beta_6 Above16*NY\_CT + \beta_7 Above16*Post*NY\_CT + Controls + \varepsilon$. 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

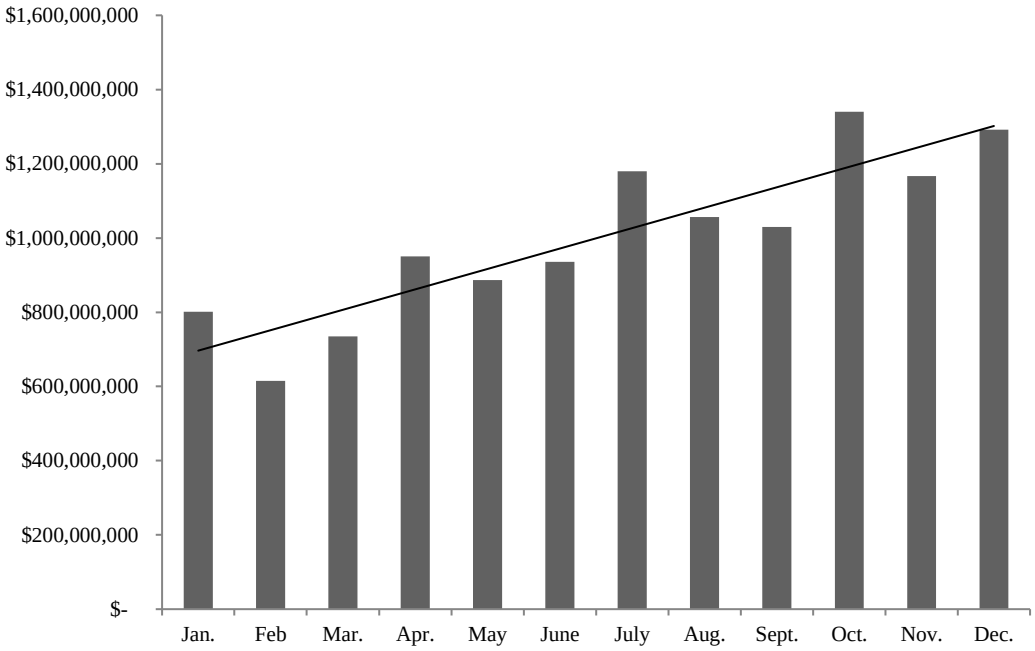


Figure 1. Summary Statistics: Value of Loans Originated by Marketplace-Lending Platforms in Our Sample.

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.

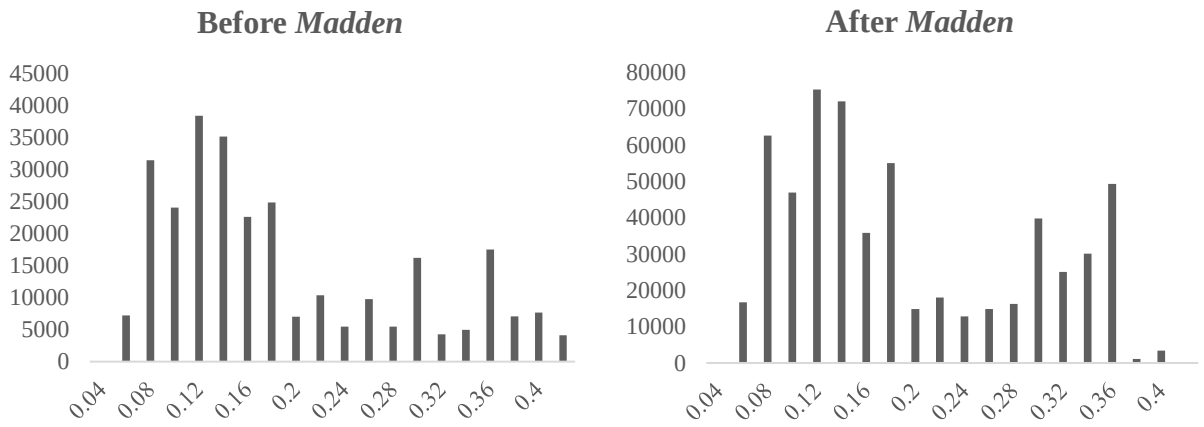


Figure 2. Summary Statistics: Distribution of Interest Rates Before and After *Madden* – Borrowers Outside the Second Circuit.

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.

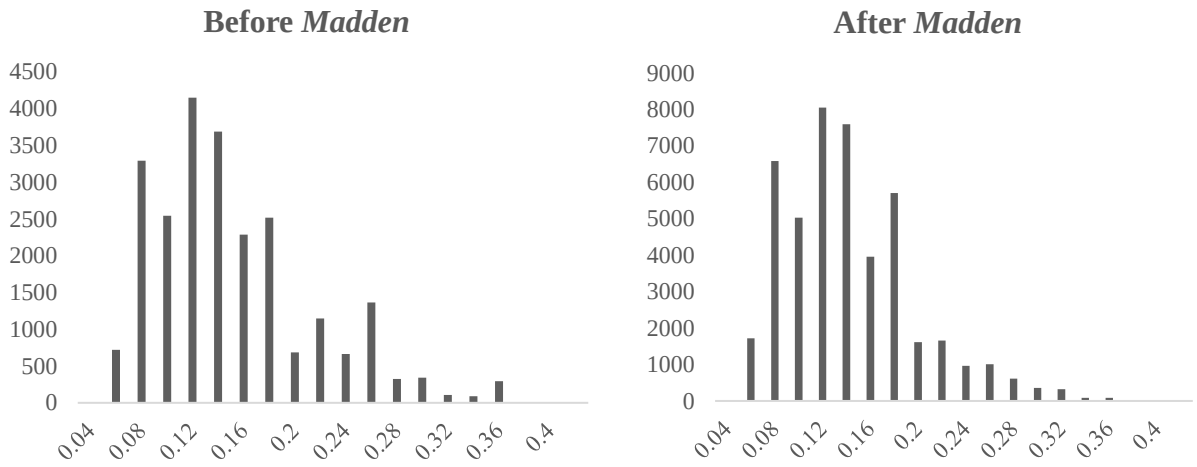


Figure 3. Summary Statistics: Distribution of Interest Rates Before and After *Madden* – Borrowers in Connecticut and New York.

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.

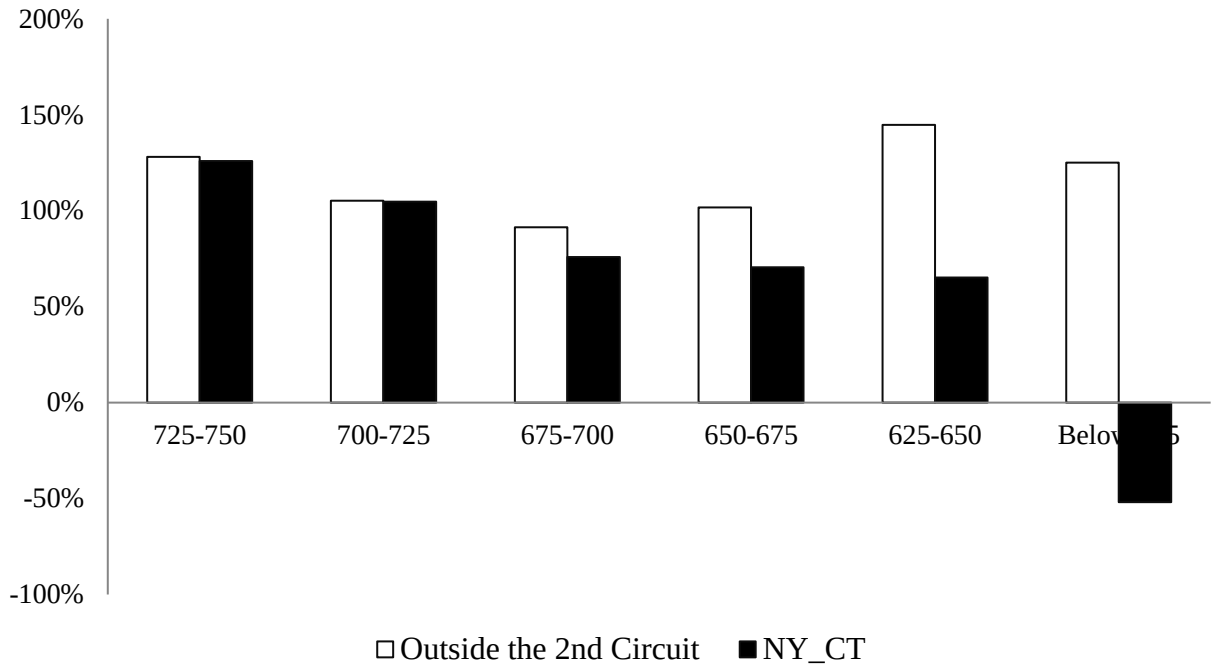


Figure 4. Summary Statistics: Growth in Loan Originations Post-*Madden.*

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.

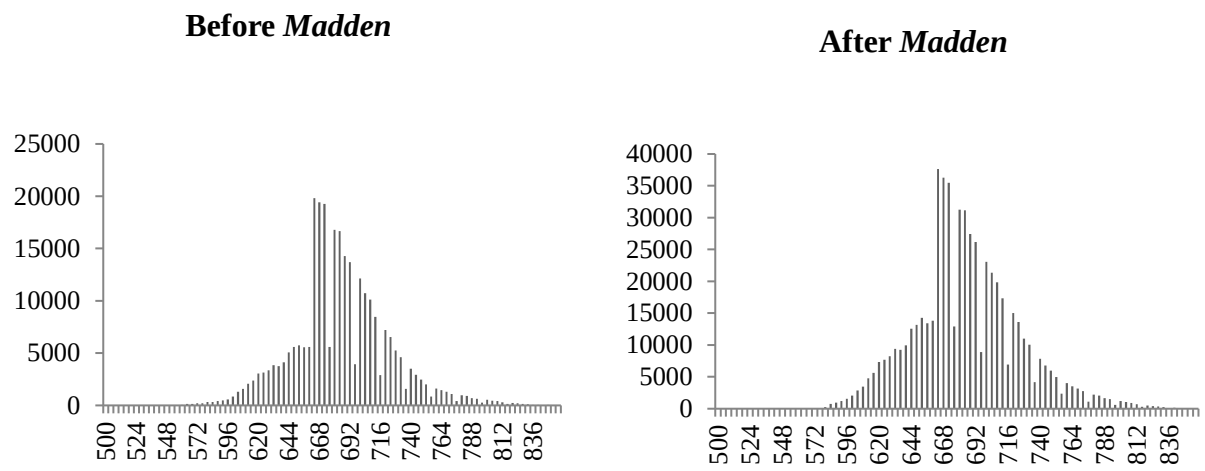


Figure 5: Summary Statistics: Distribution of FICO Scores Before and After Madden – Borrowers Outside the Second Circuit.

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.

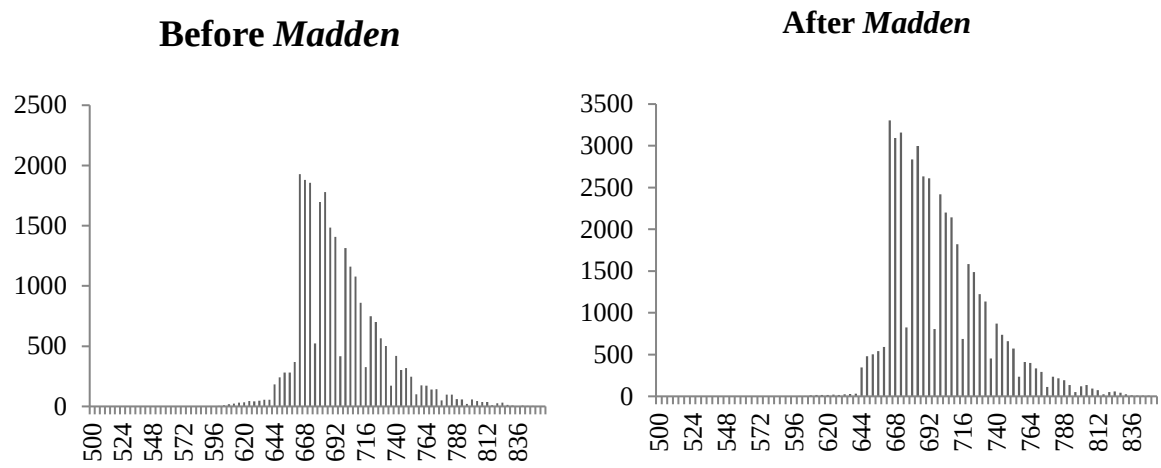


Figure 6. Summary Statistics: Distribution of FICO Scores Before and After *Madden* – Borrowers in New York and Connecticut.

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.

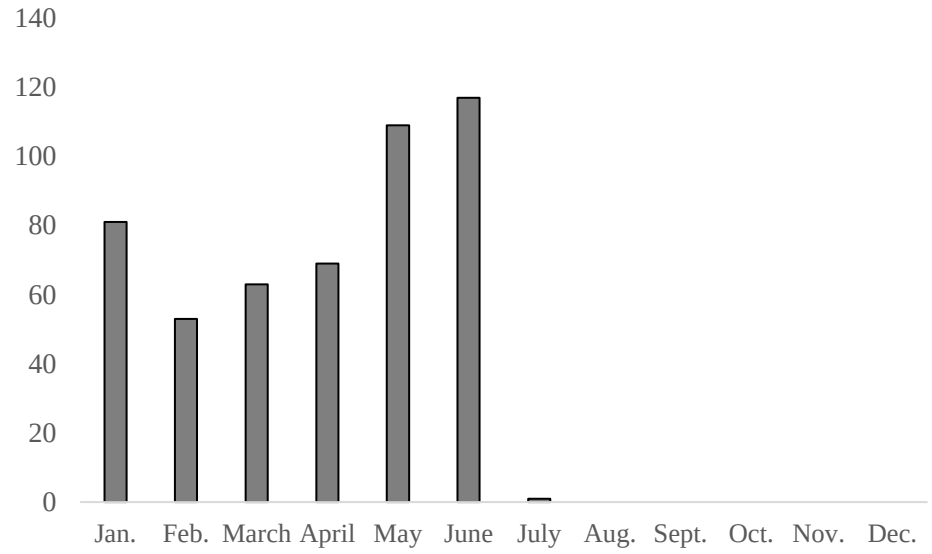
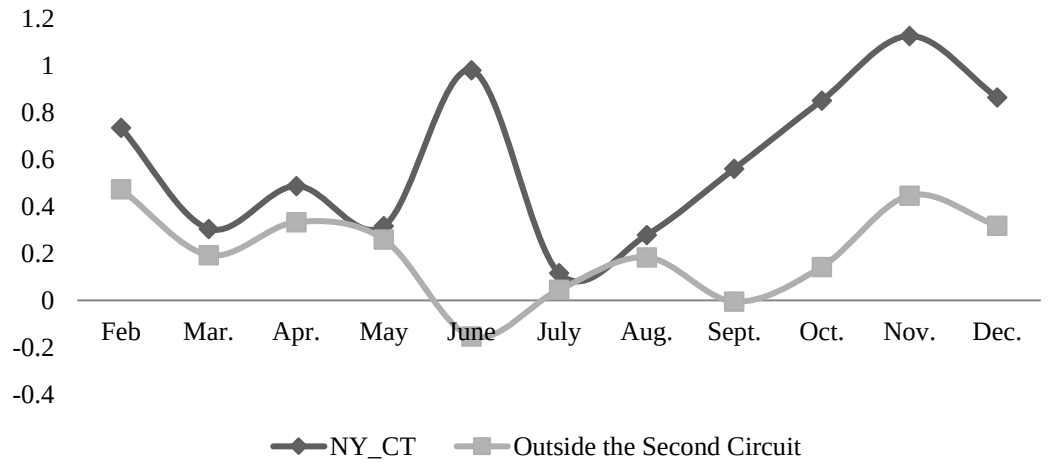


Figure 7. Summary Statistics: Loan Originations to Lower-Quality Borrowers in Connecticut and New York.

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

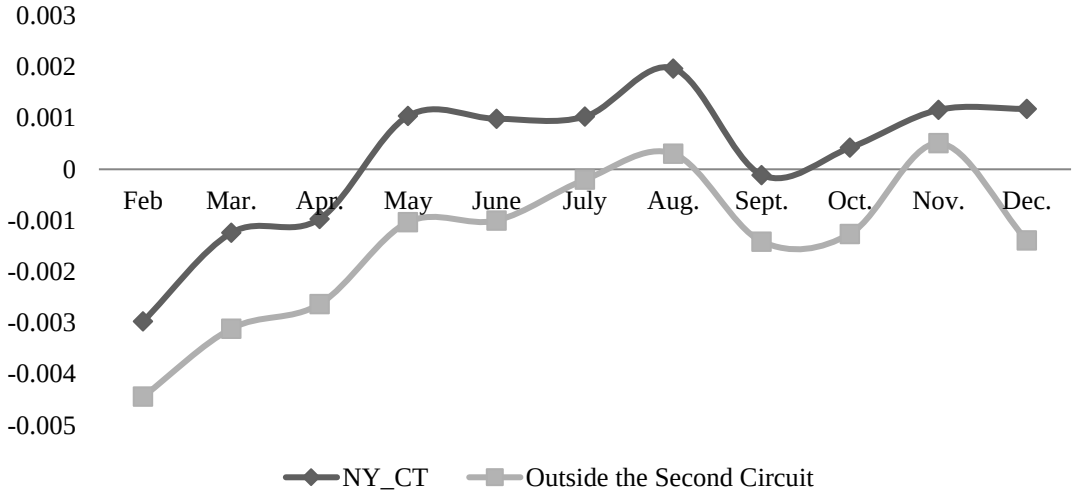


Figure 8. Parallel Trends Analysis: Discounts on Traded Notes by Month.

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.

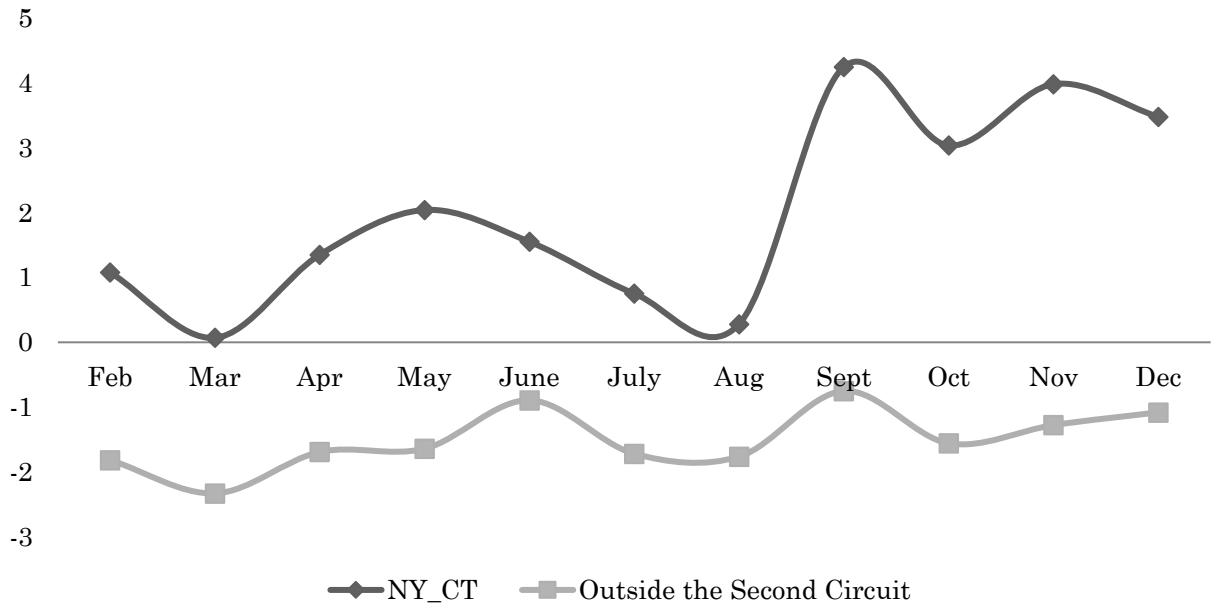
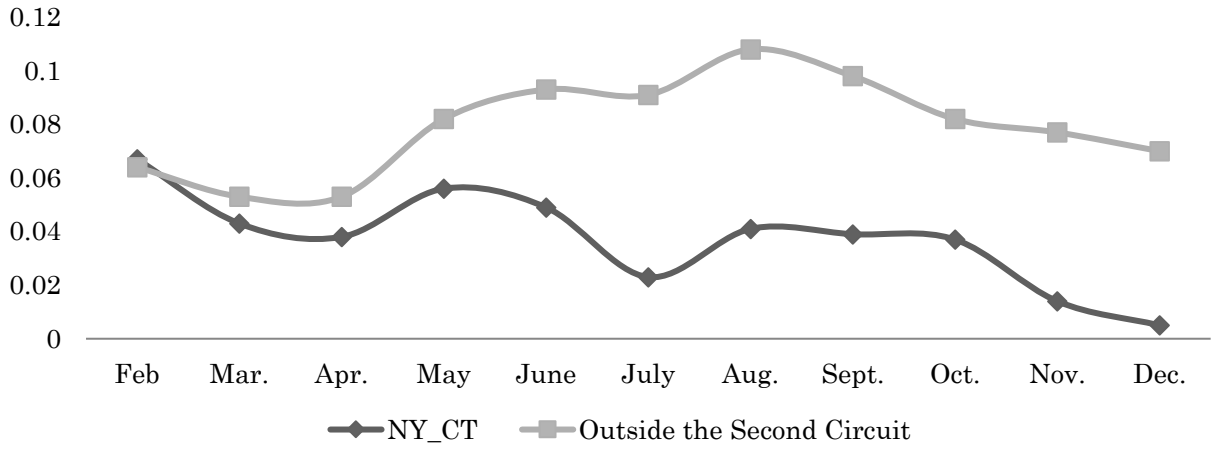


Figure 9. Parallel Trends Analysis: FICO Scores by Month.

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

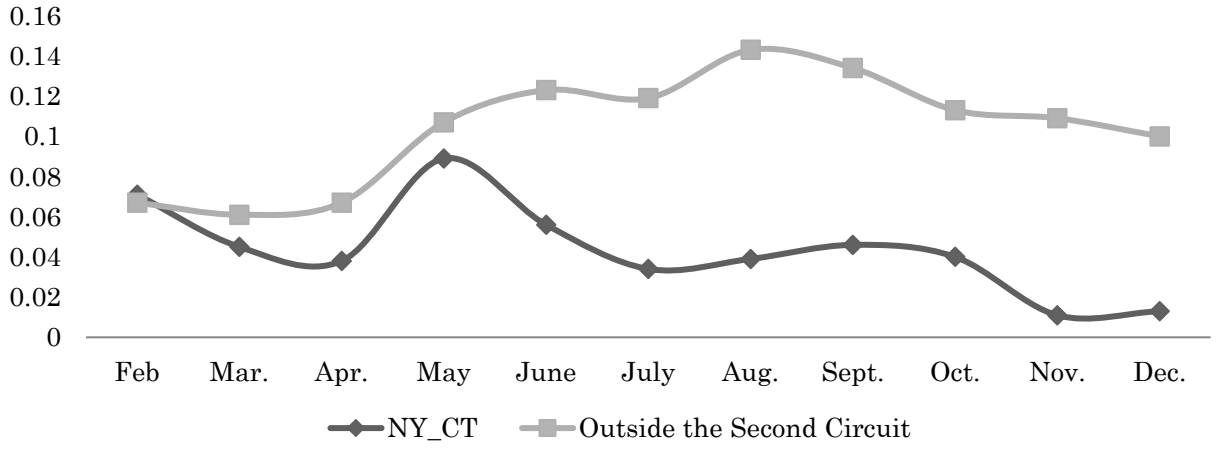


Figure 10. Parallel Trends Analysis: Natural Log of Loan Sizes by Month.

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.