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

Colleen Honigsberg   *Stanford University*

Robert J. Jackson, Jr.   *US Securities and Exchange Commission*

Richard Squire   *Fordham University*

Abstract

We use a natural experiment—an unexpected judicial decision—to study how the 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. 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.

1. Introduction

Most US states have usury statutes that cap the interest rates that lenders may charge. Yet these statutes have only a marginal impact on consumer lending be-

cause 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* (786 F.3d 246 [2d Cir. 2015]), 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, which means 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, which allows 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, which makes them vulnerable to *Madden*'s holding that loans transferred to nonbanks are no longer exempt from state usury laws. 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 have high-quality data from marketplace-lending platforms that allow us to trace the loan process through different points in time.

During the year 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, we examine 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. We find that lenders 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. Our data set contains information on both current loans, where the borrower is current on her payments, and noncurrent loans, where the borrower is late on her payments but has not yet defaulted. Although we find statistically significant discounts for both types of loans, the discount is highly economically meaningful for notes backed by noncurrent loans but close to 0 for current loans. These findings indicate that debt holders 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 after *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 Fair Isaac Corporation (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 and with prior literature showing a negative association between credit availability and usury law (for example, Benmelech and Moskowitz 2010).

With respect to borrowers' behavior, we find no evidence that the decision caused borrowers to default strategically on above-usury loans. Strategic default has been a growing topic in the finance and economics literature since the financial crisis, during which many homeowners faced incentives to walk away from underwater mortgages (see, for example, 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 (for example, Schwartz and Scott 2003; Rabin 1993). Recent work has tested these

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[1] As noted earlier, both lenders and consumers could view the case as creating legal ambiguity regarding the enforceability of the loans rather than truly voiding the loans. It is also possible that borrowers chose not to default because of nonpecuniary factors such as morality (Guiso, Sapienza, and Zingales 2013) or concerns about reputational risk. However, it is far from clear whether borrowers who strategically defaulted on consumer loans after *Madden* 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,* she 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.

questions empirically by studying strategic default in the context of mortgages (for example, Foote, Gerardi, and Willen 2008; Guiso, Sapienza, and Zingales 2013; Mayer et al. 2014). We extend those studies by examining strategic default in a new setting, consumer lending—a market that, despite its very significant size, has been difficult to study because of 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 may be incentivized to overcomply, modifying their behavior so that it is no longer socially optimal (Calfee and Craswell 1984; Craswell and Calfee 1986). For example, as applied to our setting, lenders who supplied socially optimal levels of credit prior to *Madden* could be incentivized to overcomply 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 literature has studied how legal institutions are related to corporate debt contracts and loan syndication (for example, Qian and Strahan 2007; Lerner and Schoar 2005). Although those 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 2 months to adjust their lending practices to the decision. From a methodological perspective, this finding suggests that researchers should be cautious when conducting event studies to evaluate the effects of unexpected court decisions and should set the event window carefully.

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

2. Legal and Institutional Background

2.1. 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 (for example, Lev. 25–37 [NIV]; 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 lenders' 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 US 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 that amount (see, for example, Cal. Civ. Code, sec. 1916-3, which provides for treble damages of usurious interest). Perhaps most severe are the laws 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 (see N.Y. Gen. Oblig. Law, sec. 5-501[1]).[2] 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 (Ga. Code Ann., sec. 7-4-18 [West 2016]; see also Ala. Code, sec. 8-8-1; Minn. Stat. Ann., sec. 334.01 [West]; 41 Pa. Stat. Ann. 201 [West], which establish a usury limit of 6 percent for loans below $50,000).

Despite their pervasiveness, usury laws have relatively little effect on modern American lending markets. The reason is that federal law preempts state usury limits, which renders 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.[3] Loans made by state-chartered banks can preempt usury limits through a similar provision in the Federal De-

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[2] As Stein (2001, p. 26) explains, in New York, "If 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]), which notes, "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." See also *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."

[3] The National Bank Act (NBA) 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. sec. 85).

posit Insurance Act (sec. 27; 12 U.S.C. 1831d).[4] That 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).

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 (that is, converted to marketable securities and resold to other investors). Such practices present the legal question of whether a loan issued by a national bank continues to be exempt from the usury laws of the borrower's state after it 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.[5] For example, in *Krispin v. May Department Stores Co.* (218 F.3d 939 [2000]), 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.[6]

2.2. *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

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[4] *Madden* did not explicitly address the federal law provision regarding usury preemption for state-chartered banks. Nevertheless, the preemption of the Federal Deposit Insurance Act (FDIA) is sufficiently similar to the NBA's preemption provision that market participants have assumed that loans initiated through state-chartered banks would be similarly affected.

[5] 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* (No. 12-1274, 2014 WL 2404300 [W. Va. May 30, 2014]) 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 US District Court for the Central District of California held in *Consumer Financial Protection Bureau v. CashCall, Inc.* (No. 2:15-cv-07522) 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, *Consumer Financial Protection Bureau v. CashCall, Inc.*, No. 17-8006 (9th Cir. January 13, 2017).

[6] 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).

credit-card debt. Her card was 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 percent 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 percent and a criminal cap of 25 percent. 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.[7]

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 (*Madden v. Midland Funding LLC,* 786 F.3d 246, 250). The NBA preempts only 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.

2.3. 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. On receipt of Midland's petition, the Supreme Court requested the solicitor general's 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, Brief for the United States as Amicus Curiae, *Midland Funding LLC v. Saliha Madden,* No. 15-610 [May 24, 2016]).

The ensuing legal developments in the Second Circuit have not been favorable for Midland or other nonbank debt holders. First, in April 2016, a proposed class-action lawsuit seeking damages for usurious lending was filed on behalf of consumers who borrowed through the LendingClub platform (*Bethune v. LendingClub Corp.,* No. 1:16-cv-02578-NRB [S.D.N.Y. April 6, 2016]), an event

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[7] "[P]reemption 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" (Stipulation for Entry of Judgment for Defendants for Purpose of Appeal, *Madden v. Midland Funding LLC,* No. 11-CV-8149 [May 30, 2014]). We note that Madden's claims focused on New York's criminal usury statute, which makes it a class E felony to charge interest of more than 25 percent (N.Y. Penal Law, sec. 190.40).

that may lead to more widespread consumer knowledge of *Madden*.[8] 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 that, according to regulators, attempted to charge usurious interest in violation of state law.[9] Finally, in February 2017, the lower courts rejected Midland's argument that the agreement should be governed by Delaware law[10] and agreed to certify a class of plaintiffs (a crucial step in class-action 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 by which *Madden* may ultimately be overturned. First, the Protecting Consumers' Access to Credit Act of 2017 passed by the House of Representatives overturns the decision. However, it is unclear whether the act will pass the Senate. 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.[11] And in Pennsylvania, the attorney

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[8] In a win for LendingClub, the court in *Bethune* granted LendingClub'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, in which the plaintiff alleges 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's preemption question. See *Blyden v. Navient Corp.,* No. 5:14-CV-2456, 2015 WL 4508069 (C.D. Ca. July 23, 2015), which dismisses the plaintiff's complaint but gives her leave to amend; see also *MacDonald v. CashCall Corp.,* No. 16-2781, 2017 WL 1536427 (D.N.J. April 28, 2017), in which the court declined to dismiss a similar suit raising claims under New Jersey's usury laws.

[9] 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" (Consent Order, *In re National Credit Adjusters, LLC* [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 (*In re National Credit Adjusters,* para. 15, 16, 32).

[10] Because choice-of-law provisions in the agreement at issue in *Madden* state that the agreement is 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.

[11] 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, which would permit 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 the American Bankers Association that cites a draft of this paper.

general sued a group of online nonbank lenders for lending at interest rates in excess of the state's usury cap.[12] 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, 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.

2.4. 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 (US 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 (Segal 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, her 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 request on the platform's website, where investors can search for loans that meet their desired risk characteristics. On finding a match, investors can fund the loan in full or in part. When one investor or more has 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 (US Department of the Treasury 2016). The originating bank promptly transfers its interest in the loan to the investors that have agreed to fund it. The platform generally receives an origination fee on 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 (see, for example, *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 and Agency of Com-

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[12] 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. January 14, 2016]). The case has yet to reach a final judgment.

merce and Community Development 2015; PricewaterhouseCoopers 2015).[13] 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 on marketplace consumer loans was issued in 2015 alone (Iyvengar and Reed 2015). Investors in these notes, too, rely on 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 (for example, Prosper Funding LLC and Prosper Marketplace Inc. 2016).

2.5. 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 the period we study, and many questions remain unanswered even today. As noted above, it was still possible at the end of 2015 that the Supreme Court would ultimately reverse the decision or that the debt holder 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 debt holder'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 and Gray LLP 2015).[14] Similarly, Midland's petition for certiorari in the Supreme Court argued that the Second Circuit's decision "threatens to inflict catastrophic consequences

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[13] 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 (Mach, Carter, and Slattery 2014; Segal 2015).

[14] 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 Global Banking and Payment Systems Practice 2015).

on secondary markets that are essential to the operation of the national banking system and the availability of consumer credit" (Petition for Certiori, *Midland Funding LLC v. Saliha Madden,* No. 15-610 [November 10, 2015]).

Second, consistent with prior literature on the effects of usury laws, another prediction is that *Madden* would, in the affected states, reduce credit availability for higher-risk borrowers likely to borrow above usury rates (see, for example, Goudzwaard 1968; Shay 1970; Greer 1974; Rigbi 2013; Melzer and Schroeder 2017). If lenders cannot legally charge rates sufficient to compensate for the default risk indicated by prospective borrowers' risk profiles, they will naturally lend less. The decline in credit availability could manifest as reductions in the volume and/or size of loans.

In terms of the impact on borrowers, 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 they will not engage in such action. First, they may be unaware of the ruling. 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. We searched for but have not found evidence that the case has been publicized through these channels. 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 nonpecuniary reasons such as moral compunction. In Guiso, Sapienza, and Zingales (2013), 82.3 percent of survey respondents indicated that it is morally wrong to default on a house when one can afford to pay the monthly mortgage. Third, borrowers may be concerned that their reputation (that is, credit score) will 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 important, 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.[15] 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.

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[15] We questioned several bank managers on this point. If buying the loans would make them enforceable, we asked, why would a national bank not 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.

3. Methodology and Descriptive Statistics

3.1. 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 has 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 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 (Vt. Stat. Ann. tit. 9, sec. 50[a] [2016]). Because the laws of the three states award very different damages, we are hesitant to group them for empirical purposes. Hence, we use only Connecticut and New York in our treatment group, and the 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 those loans are not directly affected by the *Madden* decision. However, this control group also has a heterogeneity problem that 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 possibility that the Supreme Court would affirm *Madden*—which would make it applicable nationwide—could affect lenders' 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.[16]

When appropriate, we also include a third control group created using a propensity-score-matched (PSM) sample, 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, which means that we match each treatment loan-borrower pair with the most similarly situated control loan-borrower pair, 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, which makes 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 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.

3.2. 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 target these platforms. We executed agreements with three of the largest marketplace-lending platforms in the United States, pursuant to which they 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 they are among the largest—if not the largest—marketplace-lending platforms in the United States (Mach, Carter, and Slattery 2014). The firms provided two types of data: information about loans arranged through their platforms (the primary-lending data set) and information about secondary-market trading of notes backed by loans arranged on the platforms (the secondary-market data set). We use the aggregated data from all three platforms for our analysis. Although other studies examine aspects of marketplace lending using data from one lender (for example, Rigbi 2013), we are unaware of any other papers that use the private data we examine here.

Our primary-lending data set contains data on almost 950,000 loans, with a total principal amount of nearly \$12 billion.[17] All loans were issued in 2015 and

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[16] 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 (for example, 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 to states that lack usury limits entirely.

[17] One of the three marketplace platforms 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 used offers this take-it-or-leave-it program, we omit the loans from this program from our analysis.
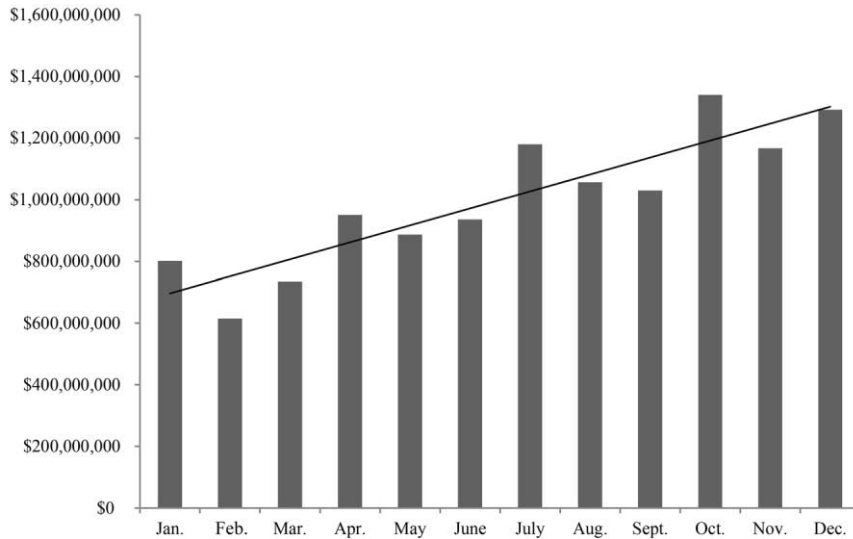
Figure 1. Value of loans originated by marketplace-lending platforms in 2015

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 percent, with a mean (median) value of 18 percent (15 percent). Figure 1 presents the total value of loans in our data set for 2015. The trend line shows the overall growth of the market.

In addition to loan characteristics such as interest rate, principal amount, and term, our primary-lending data set includes the following characteristics for each borrower: annual income, debt-to-income ratio, number of recent delinquencies, total credit availability, months of employment in her current position, and an estimated FICO score. For privacy reasons, the platforms gave us a five-point FICO range for each borrower (for example, 660–64). In the analyses using FICO scores, we use the midpoint of these ranges.

Overall, the borrowers in the primary-lending data set 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 (FICO 2015). (As a general rule, a score between 670 and 739 is considered "good.")[18] Our borrowers—like the majority of marketplace-platform 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.

Tables 1 and 2 provide summary statistics for our primary-lending data set.

[18] Experian, Infographic: What Are the Different Credit Scoring Ranges? (https://www.experian.com/blogs/ask-experian/infographic-what-are-the-different-scoring-ranges/).

Table 1

Descriptive Statistics: Characteristics of Loans and Borrowers

	New York and Connecticut	Outside the Second Circuit	t-Score	New York and Connecticut	No-Usury States	t-Score	New York and Connecticut	PSM Sample	t-Score
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 ratio	19.39	24.65	−45.52	19.39	25.36	−45.40	19.70	21.33	−53.68
Delinquencies	.31	.25	−20.12	.31	.24	−14.37	.36	.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
N	66,437	841,446		66,437	63,942		57,654	57,654	

Note. Values are means. Data are from the primary-lending data set and the propensity-score-matched (PSM) sample. FICO = Fair Isaac Corporation.

Table 2

Descriptive Statistics: Characteristics of Loans and Borrowers before and after *Madden*

	New York and Connecticut			Outside the Second Circuit			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	−.96	65,229	65,932	1.27
Debt-to-income ratio	18.19	20.03	20.11	24.55	24.70	3.08	25.61	25.23	−3.04
Delinquencies	.307	.314	.98	.26	.24	−10.09	.25	.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
N	24,220	45,362		282,628	589,168		22,467	43,811	

Note. Values are means. Data are from our primary-lending data set. FICO = Fair Isaac Corporation.

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

	New York and Connecticut	Outside the Second Circuit	t-Score
Backed by noncurrent loans:			
Principal outstanding ($)	30.73	31.15	.53
Loan amount ($)	20,169	20,506	3.60
FICO score	690	689	−.14
Ask price ($)	13.53	13.76	.32
Term (months)	50.06	50.68	5.16
Loan age (months)	16.94	16.28	−6.30
Interest rate	19	19	.84
Fifteen	.51	.48	−4.99
N	10,543	84,675	
Backed by current loans:			
Principal outstanding ($)	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	.41	.40	−10.87
N	130,092	1,226,167	

Note. Values are means. Data are from our secondary-market data set. FICO = Fair Isaac Corporation.

Table 1 compares the characteristics of loans and borrowers for the treatment and control groups, while Table 2 presents the characteristics of loans issued by group before and after *Madden*. The debt-to-income ratio reflects the borrower's total monthly debt payments, excluding the requested loan and any mortgage payments, divided by monthly income. Values for delinquencies indicate the number of recent delinquencies in the borrower's credit file. Available credit represents the borrower's total revolving credit balance. Employment values are for the borrower's current position. The FICO score reflects the midpoint of the borrower's five-point FICO range.

The data in Table 2 suggest that the quality of borrowers increased after *Madden* in Connecticut and New York but not outside the Second Circuit. For example, borrowers' average annual income rose substantially 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 Table 2.

Tables 3–5 present descriptive statistics for our secondary-market data set. 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 data set contains data provided by these platforms and includes more than 1.3 million trades ranging from $25 to $12,000. Each note traded is backed by a single loan

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

	New York and Connecticut	No-Usury States	t-Score
Backed by noncurrent loans:			
Principal outstanding ($)	30.73	31.09	−.39
Loan amount ($)	20,169	20,795	−4.65
FICO score	690	689	.97
Ask price ($)	13.53	13.08	.40
Term (months)	50.06	50.88	−4.41
Loan age (months)	16.94	16.57	2.39
Interest rate	19	19	−1.24
Fifteen	.51	.48	3.15
N	10,543	7,246	
Backed by current loans:			
Principal outstanding ($)	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	.41	.40	11.14
N	130,092	94,440	

Note. Values are means. Data are from our secondary-market data set. FICO = Fair Isaac Corporation.

(only loans originated through that platform may be traded).[19] Approximately 93 percent of the trades in this data set are for notes backed by current loans; the other 7 percent are for notes backed by noncurrent loans.

Because the change in law may have disparate effects on notes backed by noncurrent and current loans, we analyze each population separately in Tables 3–5. 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 in Tables 3–5. As noted, we match the observations using nearest-neighbor matching without replacement. The outstanding principal reflects the value at the time of the trade. The ask price is the amount the purchaser paid for the note. The loan's age is the number of months between its issue and the trading dates. Fifteen is a dummy variable indicating whether the loan underlying the note was issued within 15 months of the trading date.

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

Table 5
Notes Underlying Secondary-Market Trades:
Propensity-Score-Matched Sample

	New York and Connecticut	PSM Sample	t-Score
Backed by noncurrent loans (N = 10,543):			
Principal outstanding ($)	30.73	31.01	−.29
Loan amount ($)	20,169	20,008	1.29
FICO score	690	690	−1.42
Ask price ($)	13.53	13.84	−.36
Term (months)	50.06	50.21	.94
Loan age (months)	16.94	17.01	−.48
Interest rate	19	18	3.15
Fifteen	.51	.51	.19
Backed by current loans (N = 124,000):			
Principal outstanding ($)	33.23	33.42	−.58
Loan amount ($)	19,558	19,513	1.26
FICO score	695	695	−3.97
Ask price ($)	33.60	33.78	−.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	.41	.41	−.23

Note. Values are means. Data are from our secondary-market data set and our propensity-score-matched (PSM) sample.

4. Empirical Results

We separately analyze *Madden*'s impact on lenders and on borrowers. We find evidence that debt holders are aware of the court's 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.

4.1. 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 noncurrent loans or by current loans. We expect that the effect of *Madden* will be most prominent for notes backed by noncurrent loans, where the risk of nonpayment is especially high. Using our trading data, we calculate the spread: the discount that investors apply to each note based on the difference between

the price paid for the note and the value of the underlying loans if paid in full.[20] 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 risk of nonpayment.

Because of the risk that the underlying loans may be uncollectible in Connecticut and New York after *Madden,* we expect the spread on notes backed by above-usury loans to have increased after the decision. Table 6 presents the results of a series of triple-difference regressions testing this hypothesis for notes backed by noncurrent loans and for notes backed by current loans. The variable of interest is Above16 $\times$ Post-*Madden* $\times$ NY_CT, which represents the interaction of an indicator for whether the underlying loan has an interest rate above 16 percent, the civil usury cap in New York;[21] an indicator for whether the trade occurred after *Madden*; and an indicator for whether the borrower resides in Connecticut or New York. All models control for the outstanding principal on the note traded, the full amount of the loan, the age of the loan, the ask price, the duration of the loan, the loan's interest rate, the borrower's FICO score, and whether the loan underlying the note was issued within the 15 months prior to the trade date. Because the ratio of current loans to noncurrent loans traded varies over our sample period—and across lending platform—we also control for the daily ratio of current to noncurrent loans traded on the platform in question.

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. Spreads on notes backed by noncurrent 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 percent level.) In terms of economic magnitude, the coefficient on the triple-interaction term for loans outside the Second Circuit is .387, and the Stata margins command suggests that, at the mean, the spread for above-usury notes in the Second Circuit after *Madden* is approximately .25 higher than expected. To put this result in perspective,

[20] We calculate the spread as yield to maturity minus the loan's interest rate. The yield to maturity is calculated on the basis of 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 percent if the note was repaid in full, and the interest rate on the underling loan was 12 percent, then the spread would be −1.70 percent. The spread on current loans is usually negative, which reflects 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 noncurrent loans is usually positive; the investors demand very high yield-to-maturity rates because they know that the loans are likely to default (that is, investors pay less for the principal, thus increasing the yield if the note is paid in full). For example, an investor might require a note backed by a noncurrent loan bearing an interest rate of 12 percent to have a yield of 20 percent (if paid in full). The spread in such an instance would be 8 percent, which reflects the high discount applied to the loan.

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

Table 6

Secondary-Market Trading Prices after *Madden*

	Notes Based on Noncurrent Loans			Notes Based on Current Loans		
	Outside the Second Circuit	No-Usury States	PSM Sample	Outside the Second Circuit	No-Usury States	PSM Sample
Post-*Madden*	−.0213	−.206	.0444	.002**	.002**	.0025**
	(.0726)	(.147)	(.127)	(.000)	(.0004)	(.0002)
NY_CT	.0841	−.285	.139	.001**	.0007	.0005
	(.154)	(.191)	(.172)	(.0002)	(.001)	(.000)
Above16	−.140	−.536*	.0107	−.001**	.001	.000110
	(.0863)	(.226)	(.143)	(.000)	(.001)	(.001)
Post-*Madden* × NY_CT	−.158	.0749	−.169	−.0004*	−.001	−.0004
	(.262)	(.278)	(.272)	(.0002)	(.0004)	(.0003)
Above16 × Post-*Madden*	−.0806	.264	−.147	.0029**	.002*	.0012*
	(.078)	(.194)	(.130)	(.000)	(.001)	(.0005)
Above16 × NY_CT	−.185	.356	−.180	−.000	−.001+	−.0008+
	(.112)	(.212)	(.138)	(.000)	(.001)	(.0005)
Above16 × Post-*Madden* × NY_CT	.387*	.0163	.433*	.0006*	.0018*	.001*
	(.181)	(.236)	(.202)	(.000)	(.001)	(.0004)
N	95,218	17,633	21,086	1,356,259	221,922	248,000
R^2	.060	.058	.064	.110	.126	.127

Note. Results are from estimating Spread $= \alpha + \beta_1$Post-*Madden* $+ \beta_2$NY_CT $+ \beta_3$Above16 $+ \beta_3$Above16 $\times$ Post-*Madden* $+ \beta_4$Post-*Madden* $\times$ NY_CT $+ \beta_5$Above16 $\times$ Post-*Madden* $+ \beta_6$Above16 $\times$ NY_CT $+ \beta_7$Above16 $\times$ Post-*Madden* $\times$ NY_CT $+$ Controls $+ \varepsilon$. Data are from the secondary-market data set and the propensity-score-matched (PSM) sample. All specifications include loan-grade fixed effects and controls. Standard errors, in parentheses, are clustered by the borrower's state of residence.

$^+ p < .10.$
$^* p < .05.$
$^{**} p < .01.$

the mean (median) spread for notes backed by noncurrent loans in our sample is 2.35 (1.29), and the standard deviation is 3.54. The PSM control sample is the same as that presented in Table 5 and shows a similar result.

Although spreads increased after *Madden* on notes backed by above-usury current debt owed by Connecticut and New York borrowers, the magnitude of the increase is much smaller. The variable of interest is significant at the 5 percent level across the three models, but the economic magnitude of the increase is virtually 0. The smaller discount has a clear explanation, as current loans present lower risks of nonpayment than noncurrent loans. Accordingly, the mean (median) spread on notes backed by current loans is −.018 (−.0158). Nonetheless, the economic magnitude of roughly 0 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.

4.2. 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 rather than by regression analysis.

4.2.1. *Madden*'s Effect on Loan Volume

We begin by examining changes in loan volume after *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 percent increased 65 percent (from 7,537 to 12,425). By contrast, the number of new loans at such rates outside the Second Circuit increased 125 percent (from 124,340 to 280,313). The 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 percent or less. The volume of new loans at these lower rates increased 97 percent (from 16,683 to 32,937) in Connecticut and New York; outside the Second Circuit the volume of such loans grew 95 percent (from 158,288 to 308,855). These growth rates do not differ at statistically significant levels ($t = 1.18$). These results are presented in Figures 2 and 3 in histograms that show the distribution of new loans at various interest rates before and after *Madden*. (The histograms use a bin width of 2 percentage points.) Although it is clear that lending at rates above 16 percent increased after *Madden* outside the Second Circuit, growth in Connecticut and New York seems stunted.
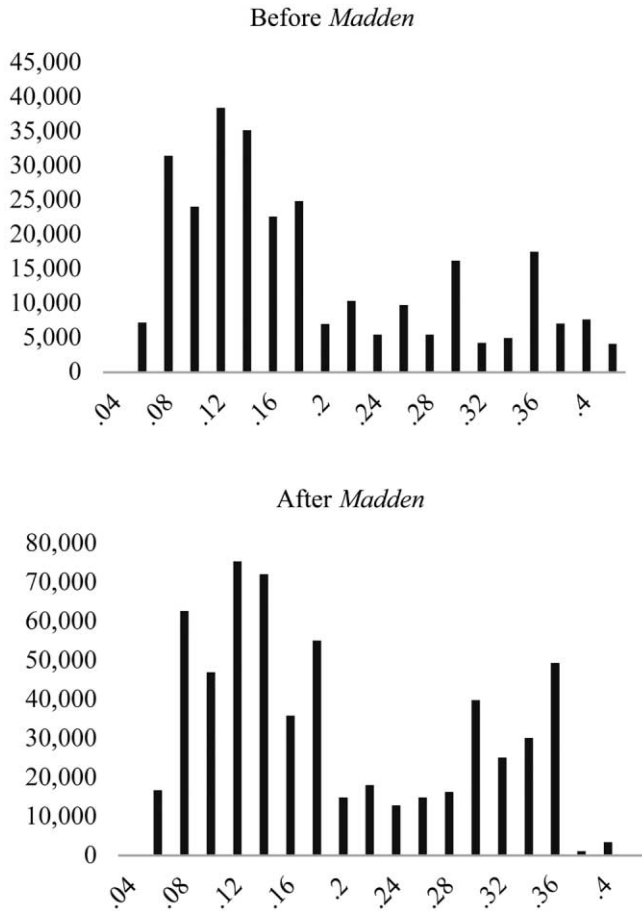
Before *Madden*

After *Madden*

Figure 2. Interest rates outside the Second Circuit

4.2.2. *Madden*'s Effect on Marketplace-Borrowers' 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 borrowers' quality constant. To distinguish between these possibilities, Table 7 presents the 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*. Table 7 shows that average credit scores in Connecticut and New York rose significantly after *Madden* relative to
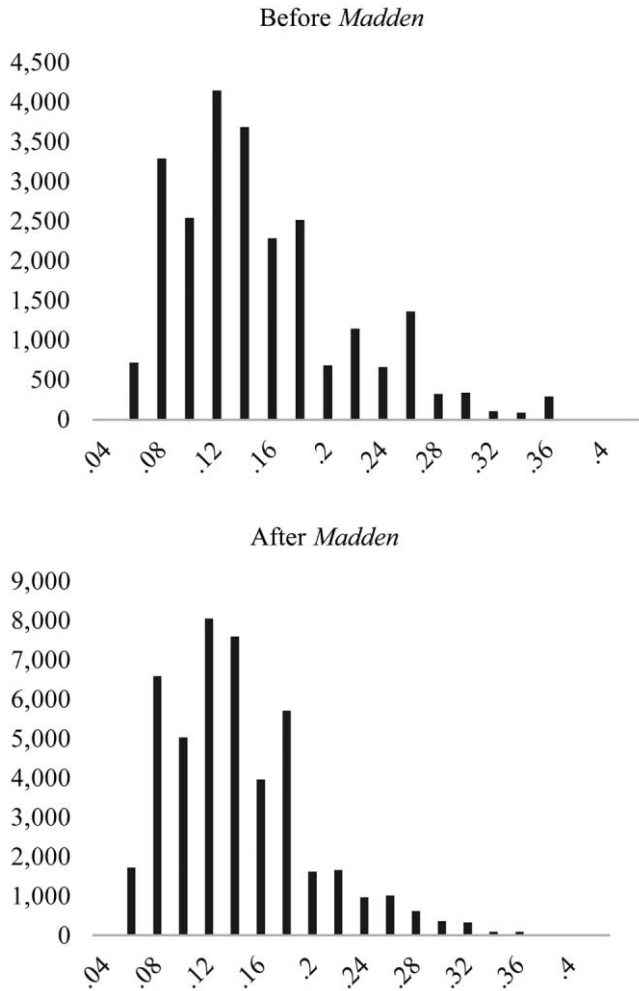
Before *Madden*

After *Madden*

Figure 3. Interest rates in New York and Connecticut

either of the control groups.[22] (This finding is consistent with the descriptive statistics in Table 2.) Average FICO scores for Connecticut and New York borrowers increased roughly 2.6–3.0 FICO points more than expected on the basis of the trend for borrowers outside the Second Circuit generally and in no-usury states

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[22] We do not include a propensity-score-matched 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 relative differences that we intend to capture. For example, a borrower with a low Fair Isaac Credit Organization (FICO) score from outside the Second Circuit would likely not have a match in Connecticut or New York because loans were not issued to low-FICO-score borrowers in these states.

	Outside the Second Circuit	No-Usury States
Post-*Madden*	−.785**	−.287
	(.221)	(.540)
NY_CT	−.254	.195
	(.405)	(.733)
Post-*Madden* × NY_CT	3.040**	2.627**
	(.252)	(.574)
N	907,883	130,379
R^2	.520	.457

Note. Results are from estimating FICO Score $= \alpha + \beta_1$Post-*Madden* $+ \beta_2$NY_CT $+ \beta_3$Post-*Madden* × NY_CT $+$ Controls $+ \varepsilon$. The dependent variable is the midpoint of the borrower's five-point range in score. All specifications include lender fixed effects and controls. Standard errors, in parentheses, are clustered by the borrower's state of residence.
     ** $p < .01$.

specifically. All models in Table 7 control for the loan's interest rate, amount, and term and the borrower's annual income, debt-to-income ratio, number of recent delinquencies, total credit availability, and years of employment at her current position.

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 percent in Figure 4 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. Figure 4 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 those outside the Second Circuit for higher-quality borrowers, the growth rates of 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 number of loans issued to these borrowers in New York and Connecticut after *Madden* was only 52 percent of that issued to these borrowers in the period before *Madden*—which means that, in absolute numbers, loan volume to those borrowers declined after *Madden*. Outside the Second Circuit, loan volume for those borrowers grew by 124 percent after *Madden* (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*. These histograms
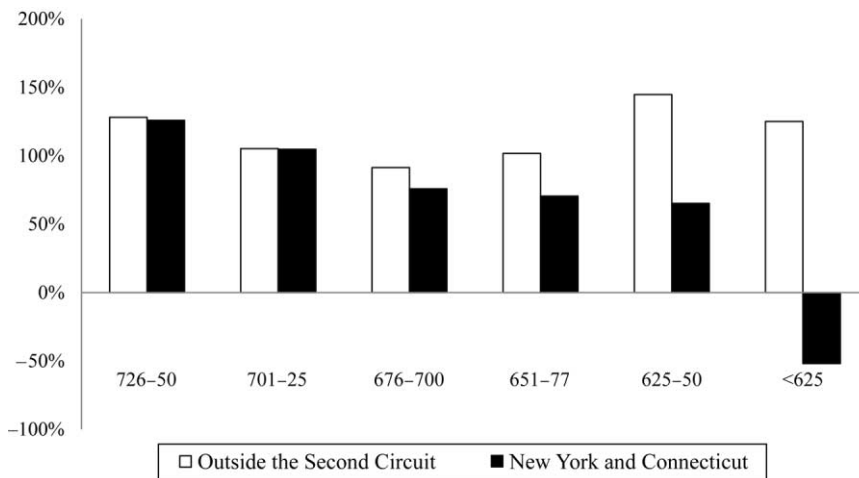
Figure 4. Loan originations after *Madden* by Fair Isaac Corporation score

use a bin width of five FICO points. Figure 5, which includes all borrowers outside the Second Circuit, 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 those borrowers grew during this period. Figure 6 shows a different trend. Loans to riskier borrowers in Connecticut and New York appear to decline, and loans to borrowers with FICO scores below 644 virtually disappear.

Figure 7 focuses 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 Figure 7 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 percent 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 after *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 use a difference-in-differences model in which the dependent variable is the interest rate to test for evidence that pricing changed. Despite our use of various specifications—the models use a variety of control variables to capture borrowers' 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 (for example, Goudzwaard 1968; Shay 1970; Greer 1974; Rigbi 2013; Melzer and Schroeder 2017). However, most of those studies rely on associations,
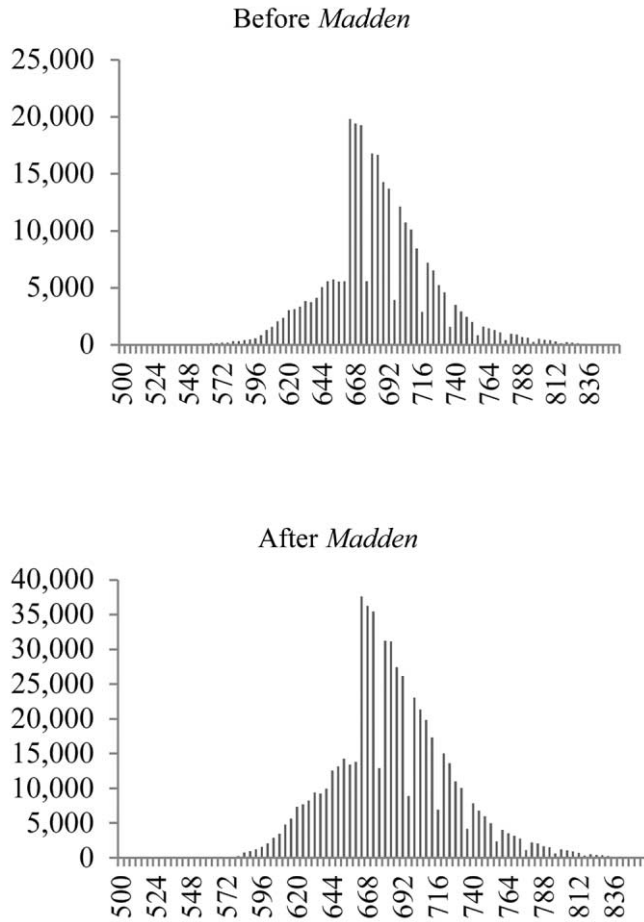
Before *Madden*

After *Madden*

Figure 5. Fair Isaac Corporation scores outside the Second Circuit

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 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.

4.2.3. Changes in Loan Size

Credit availability is affected by the availability of new loans and the terms of available loans (for example, Ghosh, Mookherjee, and Ray 2001; Stiglitz and Weiss 1981; Melzer and Schroeder 2017). Although most marketplace-lending
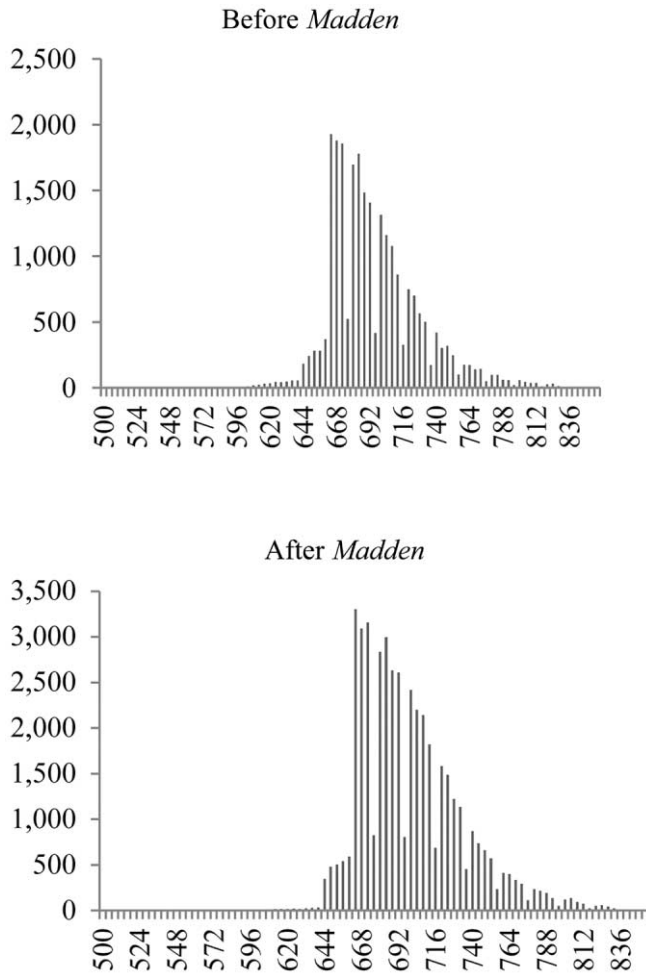
Figure 6. Fair Isaac Corporation scores in New York and Connecticut

platforms use standardized loan terms—for example, loans must be unsecured and have terms of either 36 or 60 months—a loan can range from $1,000 to $35,000. It is therefore possible that *Madden* affected the size of the loans in our sample.

Table 8 presents the results of difference-in-differences regressions testing this possibility, which indicate 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. The interaction term is statistically significant at the 1 percent level across all models,
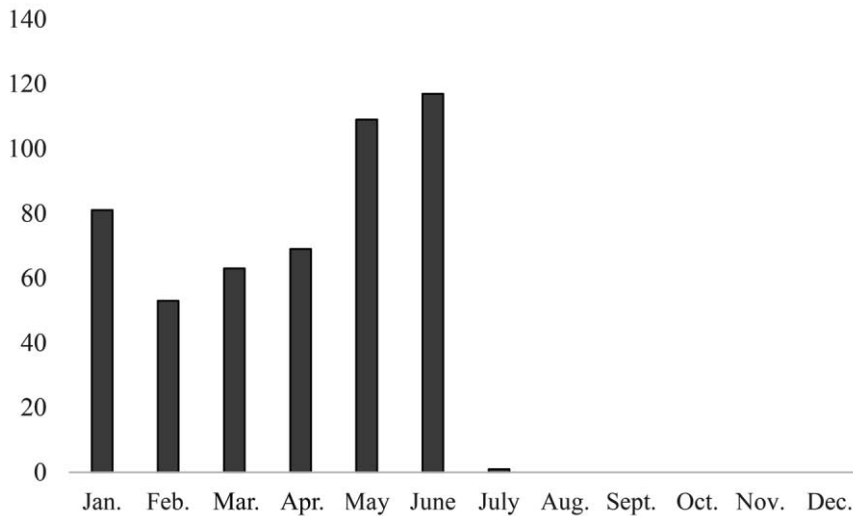
Figure 7. Loans to lower-quality borrowers in New York and Connecticut

and the change in loan size decreases monotonically with FICO scores. This result suggests that *Madden* constrained credit availability not only by reducing the volume of loans but also by reducing their size.

In sum, we find evidence that debt holders 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. The volume of loans decreased for higher-risk borrowers more likely to borrow above usury rates, and borrowers who received loans received smaller loans than would be expected.

4.3. Strategic Default

We next consider the hypothesis that *Madden* changed borrowers' behavior in the Second Circuit by giving them 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 zero until the borrower misses a payment, at which point it is one for that and all subsequent months.[23]

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[23] Because of data limitations, we can determine whether a borrower missed a payment only 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 data set, there is no record of the missed payment. This 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 Post-*Madden*.

Table 8
Loan Size after *Madden*

	Outside the Second Circuit			No-Usury States		
	All Borrowers	FICO Score under 750	FICO Score under 700	All Borrowers	FICO Score under 750	FICO Score under 700
Post-*Madden*	.040**	.043**	.062**	.028**	.029**	.046**
	(.010)	(.011)	(.014)	(.005)	(.004)	(.007)
NY_CT	.020+	.020+	.031*	.018	.017	.027*
	(.010)	(.011)	(.014)	(.011)	(.010)	(.011)
Post-*Madden* × NY_CT	−.043**	−.046**	−.062**	−.032**	−.033**	−.048**
	(.011)	(.011)	(.014)	(.006)	(.006)	(.009)
N	907,883	857,544	635,219	130,379	122,147	85,672
R^2	.346	.353	.357	.335	.340	.347

Note. Results are from estimating Loan Amount $= \alpha + \beta_1 \text{Post-}Madden + \beta_2 \text{NY\_CT} + \beta_3 \text{Post-}Madden \times \text{NY\_CT} + \text{Controls} + \varepsilon$. The dependent variable is the natural log of the loan amount. All specifications include lender fixed effects and controls. Standard errors, in parentheses, are clustered by the borrower's state of residence. FICO = Fair Isaac Corporation.

$^+ p < .10.$
$^* p < .05.$
$^{**} p < .01.$

Table 9 provides the results of triple-difference regressions used to test for strategic default. As in Table 6, the variable of interest is Above16 × Post-*Madden* × NY_CT. Because we have repeat observations for the same loan, the standard errors are clustered by loan. All models include platform fixed effects. The control variables are based on borrowers' and loan information at the time the 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 0 in any of the models. The results for all borrower months are from tests in which we keep delinquent borrowers in the sample 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 one, in the sample in October through December. Table 9 also presents results in which we remove borrowers from the sample after they miss a payment.

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 (that is, 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 ordinary least squares (OLS), probit, and logit models, and (6) for loans above 25 percent, New York's criminal usury cap.[24]

In each of these robustness tests, default overall 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—indicates a statistically significant increase in default rates. But the result was significant at only the 10 percent 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 reputational risk—were important

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[24] As a matter of New York law, the civil usury cap does "not apply to defaulted obligations" (*Manfra, Tordella, and 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 above 25% on a defaulted debt" (*Madden v. Midland Funding LLC*, 237 F. Supp. 3d 130, 142 [S.D.N.Y. 2017]) and citing several New York cases to this effect, for example, *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 percent but below the criminal usury cap of 25 percent 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.

Table 9
Borrowers' Delinquencies after *Madden*

	All Borrower Months			Until Initial Delinquency		
	Outside the Second Circuit	No-Usury States	PSM Sample	Outside the Second Circuit	No-Usury States	PSM Sample
Post-*Madden*	−.006**	−.008**	−.005**	−.001**	−.002**	−.001**
	(.000)	(.001)	(.000)	(.000)	(.000)	(.000)
NY_CT	−.000	−.000	−.000	−.000	−.000	.000
	(.000)	(.001)	(.001)	(.000)	(.000)	(.000)
Above16	.007**	.005*	.008**	.001**	−.000	−.001
	(.001)	(.002)	(.002)	(.000)	(.001)	(.001)
Post-*Madden* × NY_CT	−.001	.001	−.001**	−.000	.000	−.000
	(.000)	(.001)	(.001)	(.000)	(.000)	(.000)
Above16 × Post	−.010**	−.007**	−.009**	−.002**	−.001	−.000
	(.001)	(.002)	(.001)	(.000)	(.001)	(.001)
Above16 × NY_CT	−.001	.002	−.002	−.000	.001	.001
	(.001)	(.003)	(.002)	(.001)	(.001)	(.002)
Above16 × Post-*Madden* × NY_CT	−.001	−.004	−.002	.000	−.001	−.001
	(.002)	(.003)	(.002)	(.001)	(.002)	(.002)
N	2,366,222	389,339	452,091	2,351,868	386,706	449,169

Note. Results are from a Cox proportional hazard model estimating Delinquent $= \alpha + \beta_1$Post-*Madden* $+ \beta_2$NY_CT $+ \beta_3$Above16 $+ \beta_4$Post-*Madden* $\times$ NY_CT $+ \beta_5$Above16 $\times$ Post-*Madden* $+ \beta_6$Above16 $\times$ NY_CT $+ \beta_7$Above16 $\times$ Post-*Madden* $\times$ NY_CT $+$ Controls $+ \varepsilon$. All specifications include lender fixed effects and controls. Standard errors, in parentheses, are clustered by loan. PSM = propensity score matched.

* $p < .05$.
** $p < .01$.

enough to prevent borrowers from defaulting despite the apparent financial incentive *Madden* gave them to do so.

4.4. Loss Given Default

It may seem puzzling that investors reduced the availability of credit 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 the decision if she defaults for other reasons. And a debt collector tasked with enforcing a contract is likely to be hesitant to be too aggressive—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 Consumer Financial Protection Bureau (CFPB) and requested that its economists analyze if there was a change in loss given default after *Madden.*

Using the CFPB's Consumer Credit Panel, one of its economists found that loss given default increased for lower-quality borrowers in New York and Connecticut after *Madden.* The analysis includes all defaulted accounts that were active in the period from December 2014 through December 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 after charge-off). The analysis excludes cases in which the debtor and debt holder settled privately because the data do not include detail on the amount of any such settlements, and it includes only cases in which the borrower repaid a nonzero amount of her debt.

The CFPB researcher first ran a difference-in-differences model using the full sample and found, perhaps counterintuitively, that collections on default increased in New York and Connecticut after *Madden.* However, further analysis shows that this result is reversed for lower-quality borrowers more likely to borrow above usury rates. In particular, although borrowers in New York and Connecticut pay roughly $233 more on default than would be expected after *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 (including controls for the tract's median income and 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 they are limited to credit cards. Therefore, some of the debt is still held by national banks (and thus is unaffected by *Madden*), and some of the debt is held by nonbanks

(and thus is 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.[25]

4.5. 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, here 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.

4.5.1. 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 noncurrent loans. The graphs plot the trend lines for two regressions, one using borrowers from Connecticut and New York and one using borrowers outside the Second Circuit. The regressions are the same as those used in Table 6, except that NY_CT, Post-*Madden,* and the triple-interaction term are replaced with monthly indicators reflecting the month of the trade (the indicator for January is omitted because of collinearity). Figure 8 plots the coefficients on the interactions between Above16 and each monthly indicator.

Interestingly, the sample of loans backed by noncurrent loans indicates that it took several months for the full effect of *Madden* to materialize. Although the pre-*Madden* spread in Connecticut and New York was slightly higher than outside the Second Circuit, the deviation between these lines widens significantly starting only in September. We do not see a similar trend for the notes backed by current loans. However, the lack of a visual trend for current loans is not surprising given the finding in Table 6 that the economic magnitude of the discount applied to above-usury loans made to borrowers in New York and Connecticut after *Madden* is very close to 0.

4.5.2. 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. Figure 9 plots the coefficients on the

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[25] As outside researchers, we were unable to access the Consumer Financial Protection Bureau 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.
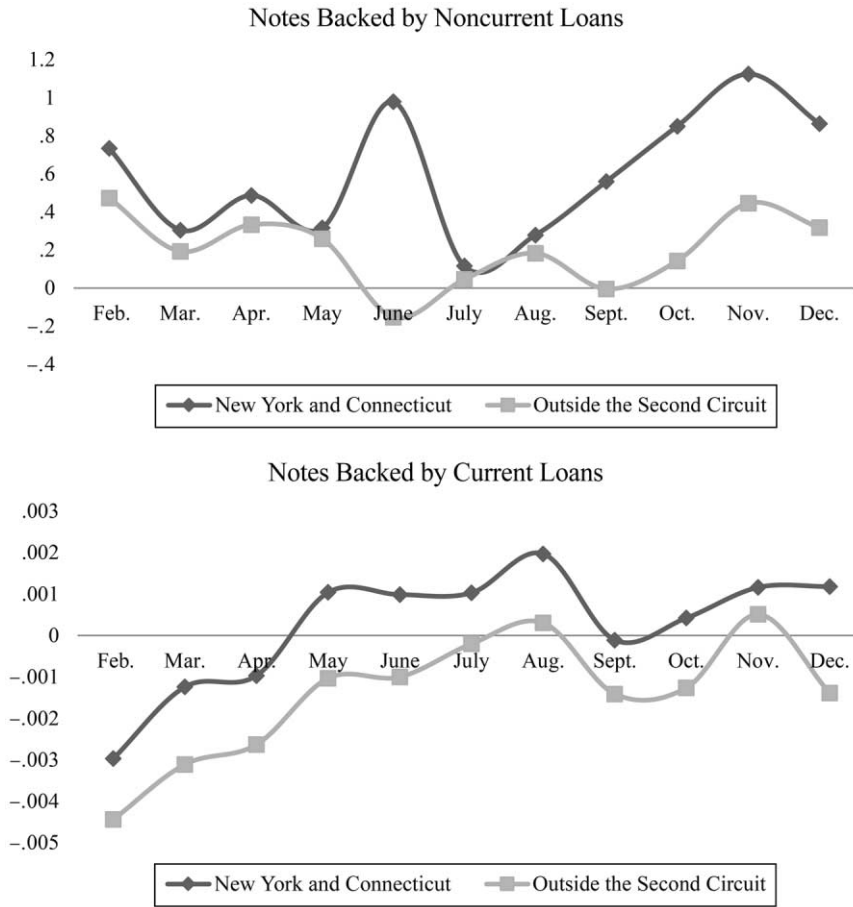
Figure 8. Parallel trends analysis: discounts on traded notes

monthly indicators for regressions using the two samples. Although FICO scores for Connecticut and New York borrowers were higher than 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.

4.5.3. Loan Size

Figure 10, which presents an analysis of *Madden*'s effect on the natural log of loan size, shows a similar trend. The regression specification is the same as in Table 8, except we replace the variables of interest with monthly indicator vari-
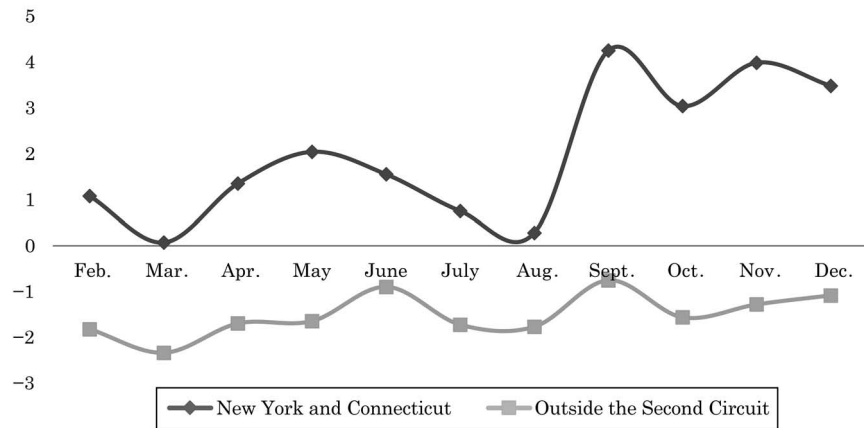
Figure 9. Parallel trends analysis: Fair Isaac Corporation scores

ables that reflect the month in which the loan was issued. Interestingly, Figure 10 indicates that relative loan size in Connecticut and New York fell as early as June, which suggests that lenders initially responded to *Madden* by making smaller loans and only later reduced their volume.

The trends analyses highlight an important question: why were any loans issued at interest rates above 16 percent in Connecticut and New York after *Madden*? There are several possible explanations, but the trends analyses corroborate anecdotal evidence 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 noted, it remained possible through the end of our sample period that the Supreme Court would ultimately reverse the decision or that the defendant debt holder 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 percent continued to justify the risks.[26]

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[26] 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, LendingClub, arranged for its originating bank to hold onto a small fraction of platform-arranged loans to permit LendingClub to argue that the *Madden* holding does not apply because its loans are not entirely in the hands of nonbank investors (Rudegeair and Demos 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.
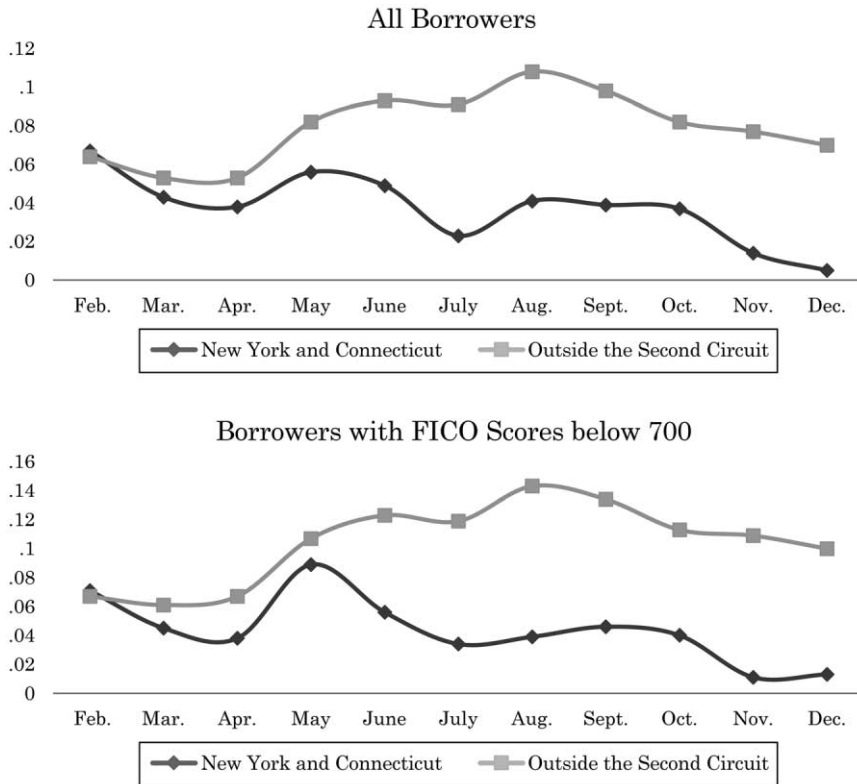
All Borrowers

Borrowers with FICO Scores below 700

Figure 10. Parallel trends analysis: loan sizes

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—Connecticut and New York—because their laws declare 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 debt holders 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 loan size and volume—after the decision, with the largest impact being on higher-risk borrowers. Even though lenders modified their behavior, our evidence suggests that they did not expect widespread consumer default—an expectation borne out by our analysis of borrowers' behavior. Taken together, our results shed light on the effect of legal enforceability on lending to consumers.

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