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

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ABSTRACT

We exploit a natural experiment—an unexpected judicial decision—to study how the enforceability of consumer loans affects lending behavior. In May 2015, a federal court decision essentially switched on the usury laws of three states: New York, Connecticut, and Vermont. The decision was significant because usurious loans are void—the borrower has no legal obligation to repay the principal—in New York and Connecticut. Using proprietary data from three marketplace lenders, we use a difference-in-differences design to study the decision's effects. We find no evidence that consumers engaged in strategic default. But by analyzing trading on secondary markets, we find that investors price the increased legal risk for loans above usury caps in New York and Connecticut when the borrower is late on her payments. We also show that the decision significantly reduced credit availability for riskier borrowers who are more likely to borrow at rates above usury limits.

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

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1. Introduction

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

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

We use proprietary data from three of the largest marketplace lenders to run difference-in-differences tests comparing loans issued in New York and Connecticut to loans issued in states unaffected by *Madden*. Marketplace lending, which matches borrowers to lenders quickly and

efficiently, is a growing source of consumer credit. Although *Madden* activated usury limits for a wide range of loans and thus may well have effects far beyond the marketplace-lending context, we focus on this relatively narrow setting because we obtain high-quality data from marketplace lending platforms that allow us to trace the loan process through many different points in time. A limited number of papers have used publicly available data from a single marketplace lender (e.g., Rigby, 2013), but we are not aware of any other papers that use the private dataset we examine here—which contains additional loans, as well as additional detail on loans and borrowers, not included in public databases.

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

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

even though it is unclear whether a borrower who chooses to default on a legally void loan can be penalized.[2] It is also possible that borrowers chose not to default due to non-pecuniary factors such as morality (Guiso, et al., 2013), or that borrowers are waiting on the courts to resolve the remaining legal questions raised by the *Madden* decision.[3]

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

Finally, we show that the imposition of usury caps decreases credit availability. In particular, the decision led to a decrease in marketplace loans issued above usury caps in New York and Connecticut. Further tests show that this decrease is driven by much lower loan

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

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

volumes to higher-risk borrowers who would have paid rates above usury limits. This finding is consistent with basic economic intuition and prior literature showing an association between credit availability and usury law (e.g., Benmelech and Moskowitz, 2010). Although it may seem puzzling that fewer loans would be issued to this higher-risk segment if there was no evidence (or expectation) of strategic default, the finding is intuitive if we consider that the expected loss on a default is likely to be far greater if the lender has limited legal ability to enforce the loan.

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

We also contribute to the literature on law and debt contracting more generally. Significant prior literature has studied how legal institutions are related to corporate debt contracts and loan syndication (e.g., Qian and Strahan, 2007; Lerner and Schoar, 2005). Although these papers focus on a broad range of differences in law ranging from corporate law (Wald and Long, 2007) to bankruptcy law (Davydenko and Franks, 2008), they focus almost exclusively on statutory law.[4] By contrast, our paper examines the effects of a decision by a

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

significant federal court. Judicial decisions are critical for debt contracting in the United States, but they are difficult to study empirically because economically meaningful changes in the relevant law governing debt contracts are relatively rare. *Madden* provides a unique opportunity to understand how judicial opinions are incorporated into the contracting process. For example, as we discuss below, we found that the marketplace lenders we study took roughly two months to fully incorporate the decision into their lending processes and decisions.

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

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

2. Legal and Institutional Background

A. State Usury Statutes and Federal Preemption

Dating back to the Old Testament, usury laws cap the interest rate that lenders may charge on loans. The policy merits of such caps have been debated for generations (e.g., Leviticus; Shanks, 1967; Homer & Sylla, 2005). Opponents argue that usury limits exclude higher-risk borrowers from credit markets—or, worse, require them to resort to more expensive, and even black-market, sources of credit (Bentham, 1787; Ryan, 1924). On the other hand, supporters of usury caps argue that they reduce the market power of lenders and prevent naïve borrowers from agreeing to loan terms on which they may eventually default (NCLC, 2016).

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

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

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

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

Despite the ubiquity of these laws, they are largely irrelevant to modern American lending markets. The reason is that the National Bank Act ("NBA") preempts state usury limits, rendering these caps inoperable for most loans. For loans made by national banks, the NBA establishes a usury limit equal to the limit of the state in which the bank is "located."[8] This is one reason many banks, and particularly those that engage in significant consumer lending, are chartered in states such as South Dakota, which has no usury limit. Pursuant to NBA preemption, banks chartered in these states may charge rates that would otherwise be usurious in the borrower's home state (Smith, 2009).

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

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

these claims preempted by the NBA because the fees were not usurious under the laws of the state in which the originating bank was located.[9]

B. Marketplace Lending and State Usury Law

Consumers have increasingly sought new sources of credit in the years since the financial crisis, and one source of such credit is marketplace lending: platforms that match willing lenders with borrowers to facilitate loans. Marketplace lending platforms issued some $5.5 billion in loans in 2014 (SBA, 2015), but the market is growing quickly; the three marketplace platforms we study here alone issued more than $12 billion in loans in 2015. The entire market is expected to grow to more than $150 billion in annual loan originations over the next decade (PWC, 2015).

The general idea of marketplace lending is to match prospective borrowers to willing lenders through a simple online platform that enables rapid funding decisions (Treasury, 2016). Although there are differences in procedure across platforms, the general framework for marketplace lending is as follows. First, a borrower submits an application with standard information, including her credit information, employment history, and the purpose of the loan. The platform uses a proprietary algorithm to assign a risk grade to the proposed loan and then posts the loan request on the platform's website, where investors can, in turn, search for specific loans that meet their desired risk characteristics. If enough investors are willing to fund the loan, the loan is then originated by a federally insured national bank pursuant to an agreement between that bank and the marketplace platform. The bank used by a number of marketplace platforms,

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

WebBank, is located in Utah—a state with no usury limit (Treasury, 2016). The originating bank then sells pieces of the loan to the investors that have agreed to fund the commitment. The platform generally receives an origination fee upon the initiation of the loan and a servicing fee over its lifetime.

Several commentators have celebrated the emergence of marketplace lending as a means of additional competition for providing consumer credit (e.g., Economist, 2014). The platforms typically charge lower rates, on average, than those charged by traditional banks for credit cards or installment loans—and their existence creates competition that may result in lower rates[10] (Economist, 2014; Vermont Dept. of Fin. Reg., 2015). Because a majority of consumers who borrow through marketplace platforms use the loan to consolidate or repay higher-interest credit card or installment debt (PWC, 2015), the argument goes, the availability of marketplace lending effectively saves consumers the difference between prevailing credit-card rates and marketplace lending rates. Especially for higher-risk, lower-quality borrowers, this difference can be significant.

These marketplace lending platforms rely on the common law of NBA preemption to avoid the application of state usury laws.[11] For example, since marketplace loans are initiated by a national bank but then immediately change hands, platforms rely on the valid-when-made doctrine to shield marketplace loans from usury caps. And marketplace loans, like other forms of

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

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

consumer credit, are often securitized—that is, transferred to an entity that issues notes to investors—so that investors can diversify their exposure to these loans. Indeed, according to one estimate, some $5 billion in notes based upon marketplace consumer loans was issued in 2015 alone (PeerIQ, 2015). Investors in these notes, too, rely upon NBA preemption to ensure that the loans underlying the notes are not subject to state usury laws.[12]

C. The Second Circuit's *Madden* Decision

Until last year, commentators and counsel relied upon prior legal precedent to conclude that marketplace loans, and notes based upon such loans, were not subject to state usury laws by operation of NBA preemption. In May 2015, however, the Second Circuit stunned markets in *Madden* v. *Midland Funding LLC,* concluding that National Bank Act preemption does *not* apply to loans initiated by a national bank but later sold to a nonbank third party.

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

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

which sets a civil cap of 16% and a criminal cap of 25%. The district court held Madden's claim preempted by the National Bank Act.[13]

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

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

> Perhaps most troubling about the opinion . . . is a cursory statement, which was made without explanation or supporting data, indicating that application of state usury laws to third-party assignees of bank-originated loans would not prevent or "significantly

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

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

> interfere" with the exercise of national bank powers . . . . Inexplicably, the court failed to realize the significance that its ruling would have on the ability of banks to sell their loans in the secondary market. Given that non-bank purchasers will be unable to enforce the terms of a loan according to the original agreement between the bank and borrower, [the decision] will undoubtedly chill the market for . . . . securitizations and bank loan programs with third parties [such as marketplace lending] (Paul Hastings, 2015).

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

Unfortunately for Midland, in June 2016, the Supreme Court followed the Solicitor General's advice and declined to hear the case. The case has now been remanded to the trial court, as the parties attempt to resolve two independent legal bases on which the lenders in *Madden* itself, and cases like it, may be able to avoid invalidation of their loans. First, the parties will address whether choice-of-law provisions in the agreement at issue in *Madden*, which point to Delaware, should be given effect. Although these provisions are almost always enforced in commercial agreements between sophisticated parties, their enforcement is less consistent in the consumer context (Honigsberg et al., 2014). If the court concludes that the loans should be

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

governed under Delaware law—under which the loan in question is not usurious—Madden's claims will likely be dismissed. Second, even if the court concludes that the loan is governed by the law of Madden's home state of New York, the parties will debate whether the common law of New York might separately embrace the valid-when-made doctrine. Again, if New York law itself incorporates the valid-when-made rule, Madden-like claims that loans can be rendered usurious by virtue of the identity of the lender will likely be dismissed.

3. Data

To study how the *Madden* decision affected consumer lending, we executed agreements with three of the largest marketplace lending platforms in the United States, pursuant to which the platforms agreed to share loan-level data with us for purposes of this study.[16] These firms agreed to share two types of data with us: information on primary lending activity—that is, loans arranged through their platforms—and information on secondary trading of notes based on those loans. We use the aggregated data from all three platforms for our analysis.

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

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

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

of loans originated by the three platforms we study for each month in 2015 and shows the overall growth in this market.

[Insert Figure 1 Here.]

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

For two reasons, the *Madden* decision offers a unique empirical setting in which to examine the effects of changes in common law on consumer lending. First, the decision was by all accounts a surprise, offering a plausibly exogenous shock to market expectations about the state of the law. Second, the decision today applies only to a subset of the market—Vermont, Connecticut, and New York, the states subject to the Second Circuit's jurisdiction. This offers us a plausible set of treatment and control states that permit us to examine the effect of the decision.

First, we consider the proper treatment group. *Madden* applies in Vermont, Connecticut, and New York, but these three states differ significantly in their treatment of usurious loans. In particular, as noted above, such loans are void in Connecticut and New York. By contrast, in Vermont, the borrower's recovery is limited to the interest above the permissible rate, interest

thereon, and reasonable attorney's fees.[18] Because the law awards very different damages—and therefore creates differing incentives to strategically default across states—we are hesitant to empirically group these three states together. Hence, our empirical analysis below includes only New York and Connecticut as the "treatment" group, and Vermont is dropped from the tests. As a practical matter, however, we note that the inclusion of Vermont makes very little difference as we have relatively few observations in that state.

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

We note, however, that loan activity for borrowers in states *without* usury limits should be unaffected regardless of *Madden*'s ultimate disposition. Therefore, loans made to these borrowers likely reflect the cleanest control group for our empirical analysis. As such, although our first control group includes all non-Second Circuit borrowers, our second control includes only borrowers from states that lack usury laws.[19] Finally, when appropriate, we include a third

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

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

control group created using propensity score matching (PSM)—a statistical technique that allows us to match the loans made to borrowers in New York or Connecticut with a comparable set of loans made to borrowers outside the Second Circuit. The PSM sample is created using nearest-neighbor matching without replacement, meaning that we match each treatment loan-borrower pair with the most similarly situated control loan-borrower and do not reuse observations. However, as we describe below, the type of borrowers obtaining loans after *Madden* significantly changed in New York and Connecticut, making it difficult to match observations in these states with observations in other states. Because of this, the matched sample is not well-balanced across the control variables. As such, although we include the PSM sample for completeness, we note the limitations of the analysis and include a robustness section with additional tests.

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

[Insert Table 1 Here.]

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

repayment of credit card balances as the most common reason for borrowing through a marketplace platform.[20]

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

[Insert Table 2 Here.]

Panel A of Table 2 presents characteristics for the full sample, and Panel B presents characteristics for the PSM sample. Because the change in law may have disparate effects on notes backed by both non-current and current loans, we present the characteristics separately for each sample. We create the PSM sample by estimating the probability that the note traded will be based on a loan made to a borrower in New York or Connecticut, where the prediction model includes the variables included in Table 2. As noted, we match the observations using nearest-neighbor matching without replacement.

4. Methodology & Results

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

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

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

A. Strategic Default

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

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

above 16%—the usury cap in New York[23]—to determine whether borrowers in New York or Connecticut were relatively more likely to be delinquent based on trends among borrowers in the control groups.

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

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

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

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

[Insert Table 3 Here.]

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

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

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

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

investors were aware of the decision.[27] Second, borrowers might not engage in strategic default because of non-pecuniary factors. Guiso et al. (2013), for example, find that 82.3% of survey respondents indicated that it is morally wrong to walk away from a house when one can afford to pay the monthly mortgage. Third, borrowers may be concerned that their reputation (i.e., credit score) would suffer, despite the fact that it is unclear whether borrowers may be penalized by credit agencies for defaulting on a loan that is legally void. Finally, borrowers may be concerned that there will be future legal ramifications if they deliberately default on these loans. Not only are borrowers and lenders waiting for the courts to resolve the remaining questions raised by the case, but borrowers may be concerned that aggressive debt collectors will bring actions against them even if the loans are legally void, causing them to incur the costs of defending such actions.

B. Secondary Market Trading

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

Using the trading data we obtained from these platforms, we begin by calculating the discount that investors apply to each note: that is, the difference between the price paid for the

[27] We note that, in April 2016, a proposed class-action lawsuit seeking damages for usurious lending was filed on behalf of consumers who borrowed through the Lending Club platform, an event that may lead to more widespread consumer knowledge of *Madden* and its implications. See *Bethune v. Lending Club Corp. et al.*, No. 1:16-cv-02578-NRB (S.D.N.Y. April 6, 2016). Because our data extend only through the end of 2015, however, it is possible that consumers remained unaware of the decision during the period we study here.

note and the value of the underlying loans if paid in full.[28] Following investors in this field, when a loan trades at a discount, we refer to that difference as the "spread." Such a discount reflects the market's perception that the projected payout is insufficient to compensate the debtholder fully for the time value of money plus the perceived nonpayment risk. Because of the risk that underlying loans may be uncollectible in New York and Connecticut after *Madden*, we expect that the spread for loans above usury caps will increase after the decision—reflecting purchasers' insistence that they be compensated for the legal risk created by the decision.

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

Further, because we have no theoretical reason to expect that loans below usury caps traded at a greater discount after *Madden*, we separately analyze loans above and below usury

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

caps. The table thus divides our sample into the set of loans with interest rates over 16% (the usury limit in New York), and the set of loans with interest rates under 16%. All models control for the principal outstanding, loan amount, loan age, ask price, loan duration, loan interest rate, the borrower's FICO score, and whether the loan underlying the note was issued within the previous fifteen months. Because the ratio of current to non-current loans traded varies over our sample period—and across lending platform—we also control for the daily ratio of current to non-current loans traded on the platform in question. Fixed effects are included for the grade the lending platform originally assigned the loan, and standard errors are clustered by the borrower's state of residence.

[Insert Table 4 Here.]

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

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

Finally, we note with interest that Panel B of Table 4 provides only limited evidence that *Madden* produced an increase in spreads on notes backed by *current* loans—that is, loans where the borrower is paying on time. Because Panel A of Table 4 shows that investors are aware of the *Madden* decision and its implications for their ability to collect these loans, we would predict a decrease in prices of current loans to borrowers in affected states, reflecting investors' expectation that some such loans may eventually enter default—and then, by reason of *Madden*, be uncollectible. Of course, we would expect this price decrease to be smaller than that observed for non-current loans, which generally present higher risks of nonpayment—but we would still expect to detect *some* decrease in the prices of notes backed by usurious loans in New York and Connecticut.

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

C. Credit Availability for Riskier Borrowers

Finally, a straightforward prediction is that *Madden* will reduce the volume of new loans issued with interest rates above usury limits. We find clear evidence of this result. For loans

below New York's usury cap of 16%, growth in loan volume in New York and Connecticut after *Madden* was statistically comparable to growth outside the Second Circuit. In New York and Connecticut, the number of loans issued with rates below 16% increased by 97% (from 16,683 to 32,937). Outside the Second Circuit, we find a similar increase of 95% (from 158,288 to 308,855). These growth rates do not differ at statistically significant levels (t=1.18).

By contrast, growth rates for loans above New York's usury cap of 16% are highly disparate. Outside the Second Circuit, the number of loans issued with rates above 16% increased by 125% (from 124,340 to 280,313). In New York and Connecticut, however, growth was just 65% (from 7,537 to 12,425). This relatively lower growth rate in New York and Connecticut is highly statistically significant (t=-20.96).

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

[Insert Figure 2 Here.]

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

price).[30]

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

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

[Insert Table 5 Here.]

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

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

[Insert Table 6 Here.]

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

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

To further investigate this increase in average FICO scores, we assign borrowers to buckets based on FICO score and examine the growth in loan volume by bucket. The results, presented in Figure 3, provide summary evidence that the increase in FICO scores was caused by a decline in loan volume to lower quality borrowers.

[Insert Figure 3 Here.]

Figure 3 shows that, outside the Second Circuit, loan volume for borrowers in all FICO buckets increased substantially after *Madden*—and that a significant portion of the growth was driven by lower-quality borrowers. In New York and Connecticut, however, growth after *Madden* appears comparable to other circuits only for borrowers with FICO scores over 700. For borrowers with FICO scores under 700, Figure 3 shows, growth in New York and Connecticut appeared to lag behind growth in other circuits. The pattern is most obvious for the very lowest-

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

quality borrowers—those with FICO scores below 625. Outside the Second Circuit, loan volume for these borrowers after *Madden* grew by 124% (that is, loan volume in absolute numbers more than doubled). By contrast, the same statistic for borrowers in New York and Connecticut was negative 52%—meaning that, in absolute numbers, loan volume to these borrowers *declined* after *Madden*.

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

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

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

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

ii. Lower interest rates for comparable loans

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

iii. The market's incorporation of *Madden*

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

caps in New York and Connecticut were issued shortly after *Madden*—indeed, we observe *zero* loans to borrowers with credit scores below 625 in New York or Connecticut after July 2015.

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

D. Robustness

For a difference-in-differences analysis to produce a valid estimate of the treatment effect, the treatment and control samples need not be identical, but the difference between the two groups should be consistent prior to the shock examined. Our primary analyses attempt to address potential concerns regarding the differences between the treatment and control groups through multiple control groups, but in this section we also examine the monthly trends in our regression variables.

To compare the difference between the treatment and control samples over time, Figure 5 plots the coefficients on monthly indicators from three regressions in which the dependent variable is the borrower's FICO score. The indicators reflect the month in which the loan was issued. The first regression uses only borrowers from New York and Connecticut; the second regression includes only borrowers from outside the Second Circuit; and the third regression

includes only borrowers from states with no usury limits. The regression specification is the same as that presented in Table 6, except that we replace the prior variables of interest—NY_CT, Post Madden, and the resulting interaction term—with monthly indicator variables.

[Insert Figure 5 Here.]

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

Figure 6 presents a similar analysis for the trading spread on notes backed by non-current loans in the treatment and control samples. As in Figure 5, we plot the coefficients on monthly indicators from February through December for three separate regressions. The indicators reflect the month in which the trade on the relevant note occurred. The regression specification is the same as that presented in Table 4, except that we replace the prior variables of interest—NY_CT, Post Madden, and the resulting interaction term—with the monthly indicator variables.

[Insert Figure 6 Here.]

Like Figure 5, Figure 6 indicates that it took several months for the full effect of *Madden* to materialize. Although the spread on notes backed by loans in New York and Connecticut is generally slightly higher than the spread on notes backed by loans outside the Second Circuit

before *Madden*, the figure shows that spread increased significantly in August and, with the exception of October, remained significantly higher thereafter.

5. Conclusion

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

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

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

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

Panel A: Full Sample

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

Panel B: PSM Sample

Notes Backed by Non-Current Loans				Notes Backed by Current Loans			
	NY & CT	PSM	*t-score*		NY & CT	PSM	*t-score*
	(1)	(2)			(3)	(4)	
Principal Outstanding	30.73	31.01	*0.29*	Principal Outstanding	33.19	33.58	*1.11*
Loan Amount	20,169	20,008	*-1.29*	Loan Amount	19,723	19,658	*-1.78*
FICO Score	690	690	*1.42*	FICO Score	694	695	*4.03*
Ask Price	13.53	13.84	*0.36*	Ask Price	33.59	33.98	*1.08*
Loan Duration	50.06	50.21	*0.94*	Loan Duration	48.59	48.11	*9.95*

Loan Age	16.94	17.01	*0.48*	Loan Age	14.49	14.60	*2.52*
Interest Rate	19%	18%	*-3.15*	Interest Rate	17%	17%	*-7.59*
Fifteen	0.51	0.51	*-0.19*	Fifteen	0.42	0.42	*0.35*
Num. Obs.	*10,543*	*10,543*		*Num. Obs.*	*124,000*	*124,000*	

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

$$Delinquent = \alpha + Post\text{-}Madden + NY\_CT + Post*NY\_CT + Controls + \varepsilon$$

Panel A: All Monthly Defaults

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

Panel B: Through Initial Default

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

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

$$Spread = \alpha + Post\text{-}Madden + NY\_CT + Post^{*}NY\_CT + Controls + \varepsilon$$

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

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

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

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

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

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

Figure 1. Summary Statistics: Value of Loans Originated by Three Marketplace Platforms in 2015. The figure below presents the value of all loans originated by the three lending platforms in our study in each month of 2015. The trend line, which is plotted on the figure, reflects the growth in the industry.

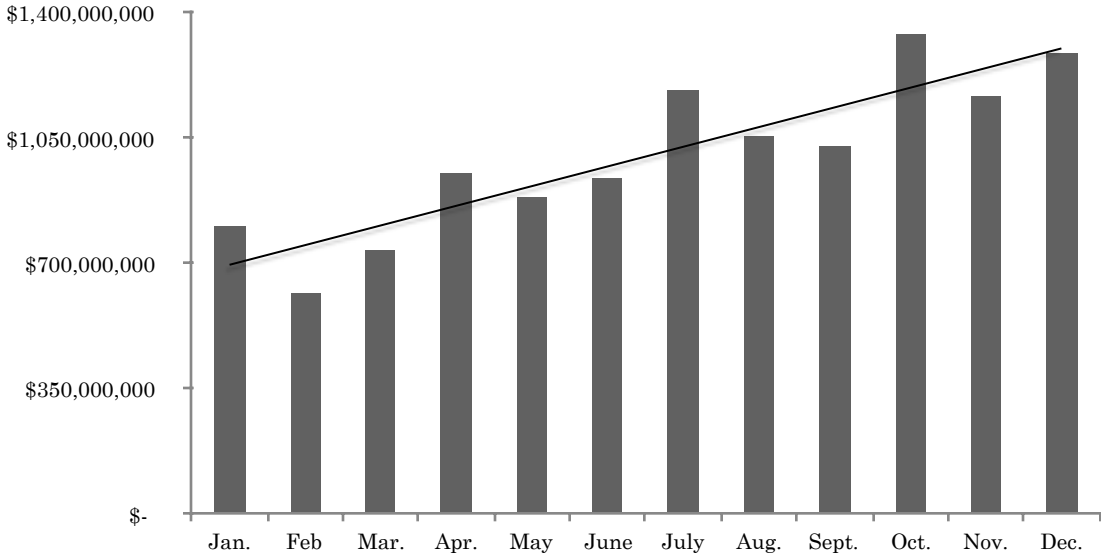


Figure 2. Summary Statistics: Distribution of Interest Rates Before and After *Madden*. The histograms below present the distribution of interest rates for all borrowers who were issued loans in the marketplace data we study. The first set of histograms include all borrowers from outside the Second Circuit and show a relative increase in interest rates after the *Madden* decision. The next set of histograms include all borrowers in New York and Connecticut and show a different trend—if anything, the percentage of loans issued at the highest interest rates appears to decrease. All histograms use a bin size of four percentage points.

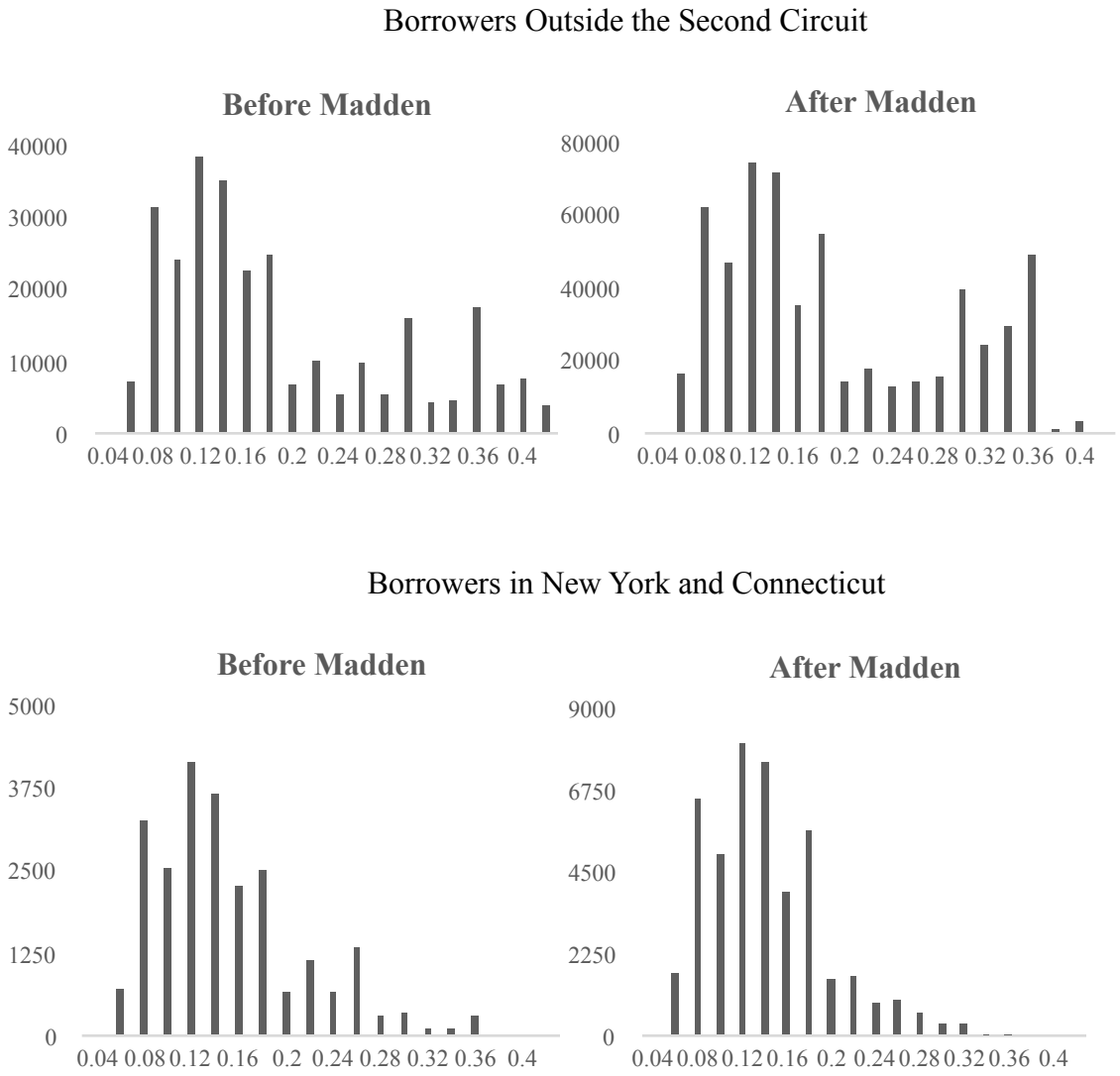


Figure 3. Summary Statistics: Growth in Loan Volume Post-*Madden*. The figure below shows the growth in loan volume for loans issued to borrowers in New York and Connecticut relative to loans issued to borrowers outside the Second Circuit. The borrowers are broken down into buckets by FICO score, and the sample includes all loans issued during calendar year 2015 (i.e., the "before" period includes all loans issued in 2015 before *Madden*, and the "after" period includes all loans issued in 2015 after *Madden*). The figure shows that growth rates for loans issued to borrowers in New York and Connecticut are roughly comparable to loan volume nationwide for high-quality borrowers, but that growth in loans was dampened—or even declined—for lower-quality borrowers.

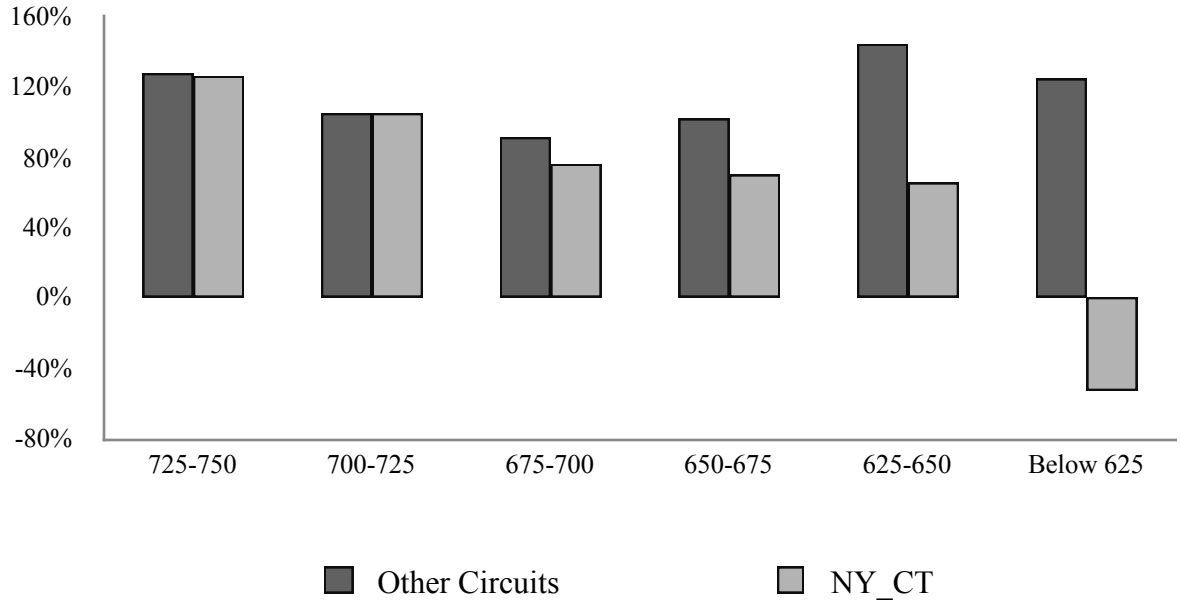


Figure 4. Summary Statistics: Distribution of FICO Scores Before and After *Madden*. The histograms below present the distribution of FICO scores for all borrowers who were issued loans in the marketplace data we study. The first set of histograms include all borrowers from outside the Second Circuit and show a relative increase in borrowers with FICO scores below 670 after the *Madden* decision. The next set of histograms include all borrowers in New York and Connecticut and show a different trend—if anything, loans to riskier borrowers appeared to decline, as loans to borrowers with FICO scores below 644 were virtually nonexistent. All histograms use a bin size of four FICO points.

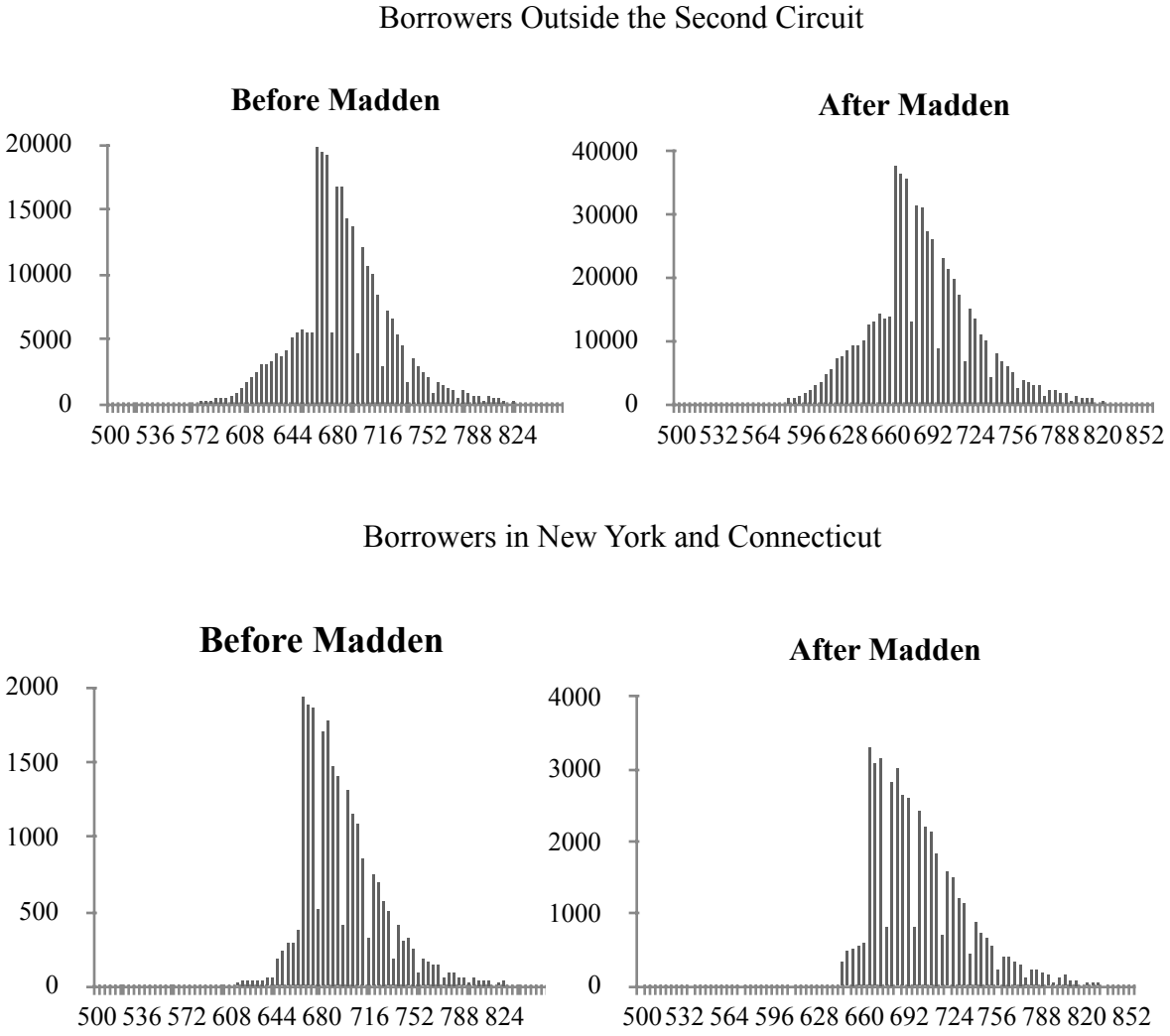


Figure 5. Coefficients on Monthly Indicator Variables: FICO Score by Month. The figure below presents the coefficients on monthly indicators from three separate regressions. The first regression includes only borrowers located in New York and Connecticut, the second regression includes only borrowers located outside of the Second Circuit, and the third regression includes only borrowers located in states that lack usury limits. The regression specification is the same as that presented in Table 6, except that we replace the prior variables of interest (NY_CT, Post *Madden*, and the resulting interaction term) with dummy variables for each month from February through December. The monthly indicators reflect the month when the loan was issued.

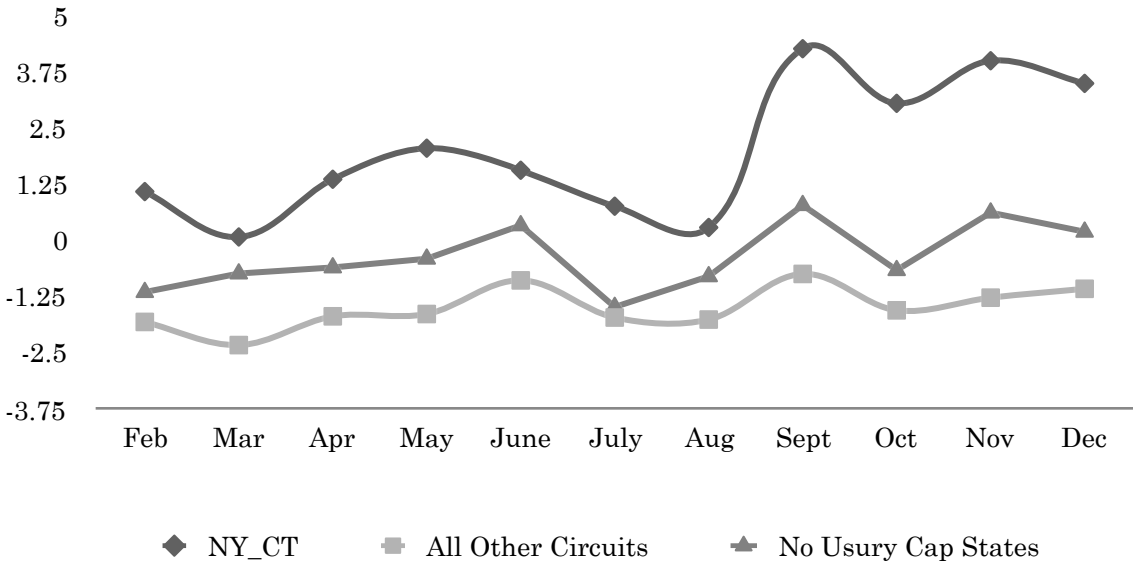
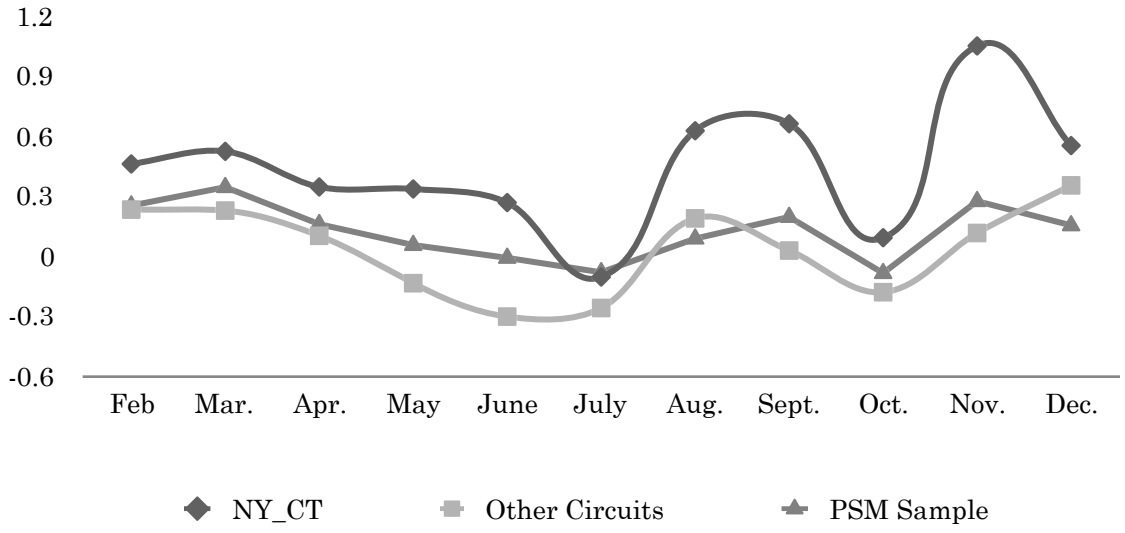


Figure 6. Coefficients on Monthly Indicators: Discount on Non-Current Loans. The figure below presents the coefficients on monthly indicators from three separate regressions. The first regression includes only notes traded based on loans to borrowers located in New York and Connecticut, the second regression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



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