April 25, 2023

Senator Robert Rodriguez
Chair, Senate Business, Labor, & Technology Committee
200 E Colfax
RM 346
Denver, CO 80203

Re: HB 1229 – Consumer Lending

Dear Chairman Rodriguez:

I write on behalf of the American Financial Services Association (AFSA)[1] to express our continued concerns with House Bill 1229, which would make drastic changes to Colorado's consumer lending laws. While the amended version of the bill that passed the House is a step in the right direction and a significant improvement over the draft that was introduced, HB 1229's proposed changes would create an uneven playing field between state and national banks and contribute to existing credit access issues in the state.

In particular, we are concerned about Section 2 of the amended bill, which would opt Colorado out of the rate preemption provision of the federal Depository Institutions Deregulation and Monetary Control Act (DIDMCA). When first enacted in 1980, DIDMCA leveled the playing field between national banks—which charge rates governed by the National Bank Act, not each state's law—and state-chartered banks subject to each state's existing rate cap. These changes brought more competitive balance to industry as a result. As amended, HB 1229 would return Colorado to the multi-tiered credit market between national banks and state-chartered banks that existed prior to DIDMCA and undermine that competitive balance that has been in place for decades since.

Shortly after DIDMCA's passage, Colorado was among a handful of states that opted out of DIDMCA's rate preemption.[2] However, Colorado lawmakers realized the negative effects the opt-out had on access to financial services and in 1994 repealed the state's opt out of the interest rate preemption provisions to restore competitive balance.[3] In fact, every state, with one exception, that initially opted out of DIDMCA subsequently opted back in because of the harmful effects of opting out. Leaving certain segments of the market subject to significant restrictions creates an uneven playing field with the rest of the market and would limit competition in the state for certain companies, leaving consumers with fewer choices and worse off as a result. Additionally, the difference could prove

[1] Founded in 1916, the American Financial Services Association (AFSA), based in Washington, D.C., is the primary trade association for the consumer credit industry, protecting access to credit and consumer choice. AFSA members provide consumers with many kinds of credit, including direct and indirect vehicle financing, traditional installment loans, mortgages, payment cards, and retail sales finance. AFSA members do not provide payday or vehicle title loans.
[2] The Federal Deposit Insurance Corporation (FDIC) recently issued regulations clarifying the DIDMCA interest rate preemption and provided a comprehensive historical view of this provision. This summary rules can be found on the Federal Register at https://www.federalregister.gov/documents/2020/07/22/2020-14114/federal-interest-rate-authority
[3] See SB 94-176 / Ch. 272.

confusing for consumers who may seek credit at multiple types of financial institutions and see drastically different offers.

Importantly, DIDMCA's federal preemption was also intended to alleviate pressure from the high interest rate environment that existed in the late 1970s. Prior to DIDMCA's passage, interest rates were rising, but some rate caps prevented state-charted banks from adjusting their own interest rates commensurate with their significantly higher cost of funds. DIDMCA's changes provided relief to prevent market disruptions that could have limited credit accessibility. More than 40 years later, we are in another high interest rate environment and the cost of funds for lenders has risen quickly. Opting Colorado out of DIDMCA at this time would limit flexibility at a time when it is crucial for credit markets.

Given that Colorado consumers may already have difficulty obtaining small dollar credit we respectfully request that you not move forward with HB 1229 at this time. Thank you for your consideration of our comments. If you have any questions or would like to discuss this further, please do not hesitate to contact me at (202) 469-3181 or mkownacki@afsamail.org.

Sincerely,

Matthew Kownacki
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American Financial Services Association
919 Eighteenth Street, NW, Suite 300
Washington, DC 20006-5517

cc: Members of the Senate Business, Labor, & Technology Committee

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

Colleen Honigsberg
Robert J. Jackson, Jr.
Richard Squire

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August 2017

ABSTRACT

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

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

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1. Introduction

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

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

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

nonbanks are no longer exempt from state usury law. Although *Madden* applies to a wide range of loans and likely has effects beyond the marketplace-lending context, we focus on this relatively narrow setting because we obtain high-quality data from marketplace lending platforms that allow us to trace the loan process through different points in time. A few previous papers have used publicly available data from a single marketplace platform (e.g., Rigbi, 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.

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

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

Second, lenders responded to the decision by extending relatively less credit to borrowers

in Connecticut and New York. Not only did lenders make smaller loans in these states post-*Madden*, but they also declined to issue loans to the higher-risk borrowers most likely to borrow above usury rates. Our sample contains hundreds of loans issued to borrowers with FICO scores below 640 in Connecticut and New York in the first half of 2015, but no such loans after July 2015. These findings are consistent with basic economic intuition, as well as with prior literature showing a negative association between credit availability and usury law (e.g., Benmelech and Moskowitz, 2010).

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

Our study contributes to the literature on the influence of legal institutions on behavior. Legal theorists have long debated whether legal enforcement mechanisms are necessary to ensure

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

contractual performance, or whether reputational sanctions, the parties' taste for fairness, and other factors can be effective substitutes (e.g., Schwartz and Scott, 2003; Rabin, 1993). Recent work has tested these questions empirically by studying strategic default in the context of mortgages (e.g., Foote, et al., 2008; Guiso, et al., 2013; Mayer et al., 2014). We extend these studies by examining strategic default in a new setting: consumer lending—a market that, despite its very significant size, has been difficult to study due to data limitations (Tufano, 2009; Campbell, 2006).

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

Finally, our findings contribute to the literature on law and debt contracting more generally. A large body of prior literature has studied how legal institutions are related to corporate debt contracts and loan syndication (e.g., Qian and Strahan, 2007; Lerner and Schoar, 2005). Although these papers encompass a broad range of subject areas, from corporate law (Wald and Long, 2007) to bankruptcy law (Davydenko and Franks, 2008), they focus almost exclusively on statutory law.[2] By contrast, our paper examines the effects of a decision by a significant federal court. Judicial

[2] One exception is Honigsberg, Katz and Sadka (2014), which incorporates both statutory law and judicial decisions.

decisions are critical for debt contracting in the United States, but they are difficult to study empirically because economically meaningful changes in the law governing debt contracts are rare. *Madden* provides a unique opportunity to understand how parties incorporate judicial opinions into the contracting process. For example, as we discuss below, we find that marketplace-lending platforms took roughly two months to adjust their lending practices to the decision. From a methodological perspective, this finding suggests that researchers should be cautious when running event studies to evaluate the effects of unexpected court decisions and should set the event window carefully.

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

2. Legal and Institutional Background

A. State Usury Statutes and Federal Preemption

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

Whatever the merits of this debate, most American states have adopted usury statutes that expressly cap interest rates. Penalties vary. Most statutes require lenders to return interest paid

above the limit; some reward borrowers three times this amount.[3] Perhaps most severe are the laws of states such as Connecticut and New York, which declare usurious loans 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.[4] 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.[5]

Despite their pervasiveness, usury laws have very little effect on modern American lending markets. The reason is that federal law preempts state usury limits, rendering these caps inoperable for most loans. For loans made by national banks, the National Bank Act ("NBA") establishes a usury limit equal to the limit of the state in which the bank is "located."[6] Loans made by state-chartered banks can preempt usury limits through a similar preemption in the Federal Deposit Insurance Act.[7] These preemptions explain why many banks, and particularly those that engage in significant consumer lending, are located in states such as South Dakota and Utah that have no usury limit. Banks in those states can charge whatever the market will bear, even if the borrower lives in a state whose laws deem the rate usurious (Smith, 2009).

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

[4] 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.").

[5] 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).

[6] 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).

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

Federal preemption in this area invites legal inquiries because banks that originate consumer loans often do not hold them until maturity. Rather, they sell much of the debt to nonbank investors such as hedge funds (Buhayar, 2016). Further, consumer loans are often securitized (i.e., converted to marketable securities and resold to other investors). Such practices present the legal question whether a loan issued by a national bank continues to be exempt from the usury laws of the borrower's state after the loan is sold to a nonbank. The traditional rule under usury law is that a loan is "valid when made," meaning that a change in the identity of the lender or residence of the borrower does not alter its enforceability. Sometimes called the "cardinal law of usury," the valid-when-made rule is well-established, and before 2015 courts followed it consistently when determining the NBA's preemptive scope.[8] For example, in the 2000 case *Krispin* v. *May Department Stores Co.*, the US Court of Appeals for the Eighth Circuit held that debt owed on credit cards issued by a national bank continued to be exempt from the usury laws of the borrowers' state even though the bank had sold the receivables to a department store.[9]

B. The Second Circuit's *Madden* Decision

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

[8] The cases brought by the Consumer Finance Protection Bureau (CFPB) against CashCall in California and West Virginia are arguably exceptions to this rule. The CFPB alleged that CashCall, a California finance institution, violated usury laws by purchasing loans issued by state-chartered banks and Native-American lending institutions (which also enjoy preemption of state usury laws) and immediately reselling those loans to consumers. In 2014, the Supreme Court of Appeals of West Virginia decided in *CashCall Inc. v. Morrisey* that Section 27 of the FDIA did not preempt claims against the defendant for violations of the West Virginia Consumer Credit Protection Act. And in 2016, the United States District Court for the Central District of California held in *Consumer Financial Protection Bureau v. CashCall, Inc., et al.* that the usury laws of the borrowers' home states should be applied. However, commentators have opined that these cases are not reflective of current law. In addition to legal technicalities that may limit their precedential value, CashCall, a payday lender that issued loans with rates above 300%, is not typical of nonbank lenders.

[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.

issued by Bank of America, and her account was originally serviced by FIA Card Services, a national bank based in Delaware, a state that permits banks to charge rates that would be usurious in New York. After Madden defaulted, FIA sold the receivable to Midland Funding, a debt collector. Midland sent Madden a collection notice seeking repayment of a balance calculated at 27% annual interest, the rate specified in her cardholder agreement. Madden declined to pay and sued Midland in federal court on behalf of herself and other New Yorkers. She claimed that the interest rate violated New York's usury laws, which set a civil cap of 16% and a criminal cap of 25%. In September 2013, the district court ruled for Midland, holding that the loan was valid when issued and remained so after its transfer to a nonbank.[10]

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.[11] The NBA only preempts state laws whose application might "significantly interfere" with the exercise of the national banking power, and the court found that this requirement was not met in Madden's case. The court thus held that Madden's debt was subject to New York's usury laws. Because New York law renders usurious loans void, the holding would seemingly cancel Madden's outstanding credit-card balance.

C. Subsequent Legal Developments

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

[10] 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.

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

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

The ensuing legal developments in Second Circuit have not been favorable for Midland or other nonbank debtholders. First, in April 2016, a proposed class-action lawsuit seeking damages for usurious lending was filed on behalf of consumers who borrowed through the Lending Club platform, an event that may lead to more widespread consumer knowledge of *Madden*.[12] Second, in June 2016, the Supreme Court followed the Solicitor General's advice and declined to hear *Madden*. Third, in February 2017, the lower courts rejected Midland's argument that the agreement should be governed by Delaware law[13] and agreed to certify a class of plaintiffs (a crucial step in class action litigation that is often not met). The case has now been cleared for discovery and seems destined for trial or, more likely, settlement.

Although these recent developments in the Second Circuit have not been favorable to nonbank lenders, two new avenues have opened that may ultimately overturn the decision. First, the Financial CHOICE Act proposed by the House Financial Services Committee includes

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

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

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

D. Marketplace-Lending Platforms and State Usury Law

Madden casts a shadow on debt markets in which originators do not hold loans to maturity but rather follow an originate-to-distribute business model. Marketplace lending is one such market (Treasury, 2016). The industry has grown quickly as consumers have sought new sources

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

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

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

of credit in the years following the financial crisis. While marketplace-lending platforms originated $5.5 billion in loans in 2014 (SBA, 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 (PWC, 2015).

While details vary across platforms, the general framework for marketplace lending is as follows. A borrower submits an application with standard information, including her credit information, employment history, and the purpose of the loan. The platform uses a proprietary algorithm to assign a risk grade to the proposed loan and then posts the loan request on the platform's website, where investors can search for specific loans that meet their desired risk characteristics. Upon finding a match, investors have the option of offering to fund the loan in full or in part. When one or more investors have offered to fund a proposed loan in full, the loan is issued by an affiliated bank pursuant to an agreement between that bank and the marketplace platform. The bank used by a number of marketplace platforms, WebBank, is located in Utah[17]—a state with no usury limit (Treasury, 2016). The originating bank promptly transfers its interest in the loan to the investors who have agreed to fund it. The platform generally receives an origination fee upon the initiation of the loan and a servicing fee over its lifetime.

Several commentators have celebrated the emergence of marketplace lending as a means of providing additional competition for consumer credit (e.g., Economist, 2014). These platforms can save borrowers money, as most loans are used to repay higher-interest forms of debt such as

[17] Some marketplace-lending platforms rely on state-chartered banks, others on national banks. WebBank is a state-chartered bank, so it relies on the preemption in the FDIA rather than the NDA.

credit cards (Economist, 2014; Vermont Dept. of Fin. Reg., 2015; PWC, 2015).[18] Especially for higher-risk, lower-quality borrowers, the difference in rates can be significant.

These marketplace lending platforms rely on federal banking law to avoid the application of state usury laws. For example, because these loans are immediately sold to nonbank investors, platforms rely on the valid-when-made doctrine to shield their loans from usury caps. Further, marketplace loans, like other forms of consumer credit, are often securitized—according to one estimate, some $5 billion in notes based upon marketplace consumer loans was issued in 2015 alone (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.[19]

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

Madden was a surprise to market participants and has significant implications for a wide range of loans. However, although *Madden* cast doubt on the legal enforceability of certain consumer loans, the case's ultimate disposition and practical significance were uncertain during the period we study and many questions remain unanswered even today. As noted above, it still was possible at the end of 2015 that the Supreme Court would ultimately reverse the decision or that the defendant-debtholder would prevail on other theories of enforceability. And the possibilities remain today that Congress will overturn the decision or that the Supreme Court will overrule it while reviewing a different case.

From a debtholder's perspective, there are two straightforward predictions. First, observers anticipated that *Madden* would disrupt secondary-market trading of above-usury loans issued to

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

[19] Accordingly, the *Madden* decision is disclosed as a risk factor in prospectuses for notes backed by platform-originated loans (e.g., Prosper Funding LLC, 2016).

borrowers in affected states because investors would be reluctant to invest in loans that were potentially uncollectible. Indeed, in the flurry of law-firm memoranda that followed *Madden*, counsel warned investors that the Second Circuit's decision "could significantly disrupt the secondary market for bank loans originated by national banks" (Ropes & Gray, 2015).[20] Similarly, Midland's petition for certiorari in the Supreme Court argued that the Second Circuit's decision "threatens to inflict catastrophic consequences on secondary markets that are essential to the operation of the national banking system and the availability of consumer credit."[21]

Second, consistent with prior literature on the effects of usury laws, another prediction is that *Madden* would, within the affected states, reduce credit availability for higher-risk borrowers likely to borrow above usury rates (e.g., Goudzwaard, 1968; Shay, 1970; Greer, 1974; Rigbi, 2013; Melzer and Schroeder, 2015). 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 loan volume and/or loan size.

In terms of borrower impact, the effect of *Madden* is not as clear. Although *Madden* provides borrowers in Connecticut and New York with incentives to default on their above-usury loans, there are many reasons to expect that borrowers will not engage in such action. First, they may be unaware of the ruling.[22] Second, borrowers might not engage in strategic default for non-

[20] 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, 2015).

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

[22] We think the two most plausible channels through which borrowers would learn of the case are plaintiffs' attorneys, who might publicize the case to search for clients, and bankruptcy attorneys, who might advise clients considering a bankruptcy filing to default on loans affected by the decision while continuing to pay their other debts. Although we searched for evidence that the case has been publicized through these channels, we have yet to find any. However, this may change if any *Madden*-related class action lawsuits are resolved favorably for the borrowers or their attorneys.

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

Finally, and perhaps most importantly, legal uncertainty around *Madden* might reduce strategic defaults. Borrowers might have expected that the Supreme Court would overturn the decision, that Midland (the debt-collector) would prevail on other theories of enforceability, or that lenders would find ways to evade the decision. For example, it is unclear whether an above-usury loan held by a nonbank investor can regain its enforceability if resold to a national bank.[23] If so, this would negate the benefits of strategic default. Such uncertainty likely increases the expected costs of defaulting strategically, as borrowers may fear that they will become defendants in potentially costly lawsuits if they default.

3. Methodology and Descriptive Statistics

A. Research Design

For two reasons, the *Madden* decision offers a unique empirical setting in which to examine how law affects consumer lending. First, the decision was by all accounts a surprise, offering a plausibly exogenous shock to market expectations about the state of the law. Second, the decision applies in only a subset of the country: Connecticut, New York and Vermont, the states subject to

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

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.[24]

First, we consider the proper treatment group. Our most obvious treatment group would be borrowers in the three Second Circuit states. However, that group would have a heterogeneity problem, as the states differ in their treatment of usurious loans. While usurious loans are void in Connecticut and New York, they remain valid in Vermont, where the borrower is excused only 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.[25] Because the laws of the three states award very different damages, we are hesitant to group these three states for empirical purposes. Hence, we use only Connecticut and New York in our treatment group, and our Vermont loans are dropped from the tests. As a practical matter, including Vermont makes very little difference in our results, as we have relatively few observations in that state.

Second, we consider the proper control group. Our primary control group contains all loans whose borrowers live outside the Second Circuit, as such loans are not directly affected by the *Madden* decision. However, this group also has a heterogeneity problem. The heterogeneity results from uncertainty about the ultimate disposition of the *Madden* case during our sample period. In 2015, it was unclear whether the Supreme Court would affirm, reverse, or refuse to review the decision. In states outside the Second Circuit that have their own usury laws, the mere possibility that the Supreme Court would affirm *Madden*—making it applicable nationwide—could affect lender willingness to issue loans at above-usury rates. Further, even if the Supreme Court denied

[24] 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.
[25] Vt. Stat. Ann. tit. IX, § 50(a)(2016).

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.[26]

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

B. Descriptive Statistics

Studying *Madden*'s impact requires data on loans that were originated by banks in accordance with federal preemption of state usury laws but were sold to nonbank investors. Because loans issued through marketplace-lending platforms fit this description, we targeted these

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

platforms. We were able to execute agreements with three of the largest marketplace lending platforms in the United States, pursuant to which the platforms agreed to share loan-level data with us for purposes of this study.[27] The firms provided two types of data: (1) information on loans arranged through their platforms ("primary loan dataset"), and (2) information on secondary-market trading of notes backed by loans arranged on the platforms ("secondary-market dataset"). We use the aggregated data from all three platforms for our analysis.

Our primary-lending dataset contains data on almost 950,000 loans, with a total principal amount of nearly $12 billion.[28] All loans were issued in 2015. They range from $1,000 to $35,000 in principal amount, with a mean (median) principal amount of about $12,500 ($10,500). The interest rates range from 5% to 66%, with a mean (median) value of 18% (15%). Figure 1 presents the total value of loans in this dataset for each month of 2015. The trend line included in the figure shows the overall growth of the market.

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

Overall, the borrowers in the primary-lending dataset tend to be in the same credit range as the average American borrower. The mean (median) FICO score is 684 (681.5). By comparison,

[27] Our nondisclosure agreements prohibit us from identifying the firms by name, but we note that all three are among the largest—if not the largest—marketplace-lending platforms in the United States (Federal Reserve Board, 2014).
[28] One of the three marketplace platforms included in our study offers both a "market-based" program, in which investors can select the loan they wish to fund, and a smaller "take it or leave it" program, in which investors must accept a full package of loans on an all-or-nothing basis. Because only one of the marketplace platforms we worked with offers this "take it or leave it" program, we omit the loans from this program from our analysis.

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" (Experian, 2015).) Our borrowers—like the majority of marketplace borrowers—cite debt consolidation and repayment of credit card balances as the most common reasons for borrowing through a marketplace platform.[29]

Tables 1 and 2 provide summary statistics for our primary-lending dataset. Table 1 compares loan and borrower characteristics for the treatment and control groups, while Table 2 breaks down each group to show characteristics for loans issued before and after *Madden*. Table 2 suggests that borrower quality increased post *Madden* in Connecticut and New York but not outside the Second Circuit. For example, average borrower annual income rose significantly in Connecticut and New York but not elsewhere. We also see a much larger increase in average FICO scores in Connecticut and New York than in either of the control groups in the table.

Table 3 presents descriptive statistics for our secondary-market dataset. Two of the marketplace platforms in our sample not only initiate loans directly but also allow investors to trade notes based on those loans—or an increment thereof—on a secondary-market trading platform. Our secondary-market dataset contains data provided by these two platforms and includes more than 1.3 million trades, in sizes ranging from $25 to $12,000. Each note traded is backed by a single loan (only loans originated through that specific platform may be traded).[30] 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.

In Table 3, Panel A compares our treatment group with the non-Second Circuit control group, Panel B compares the treatment group with the no-usury control group, and Panel C

[29] Other listed reasons range from home improvements to special events such as weddings.
[30] Although some marketplace lenders sell notes based on bundled loans, we analyze trading only of notes backed by individual loans. The investors in these notes, which primarily are institutions such as hedge funds, are able to identify the underlying borrower's state of residence.

compares the treatment group with the PSM sample. Because the change in law may have disparate effects on notes backed by non-current and current loans, we analyze each population separately. We create the PSM samples by estimating the probability that the note traded will be based on a loan made to a borrower in New York or Connecticut, where the prediction model includes the variables included in Table 3. As noted, we match the observations using nearest-neighbor matching without replacement.

4. Empirical Results

This section presents our empirical results. As described below, we separately analyze *Madden*'s impact on lenders and on borrowers. We find evidence that debtholders are aware of the decision, and that they respond to the legal limbo in two ways. First, by analyzing secondary-market trading, we see that investors discount notes backed by above-usury loans to borrowers in Connecticut and New York. Second, we show that lenders reduced the flow of credit for the higher-risk Connecticut and New York borrowers most likely to have loans above usury caps. However, we find no evidence that the decision induced borrowers to default strategically.

A. Secondary-Market Trading

We begin with our analysis of whether *Madden* affected secondary-market trading of notes backed by marketplace loans to Connecticut and New York borrowers. As noted previously, notes traded on secondary markets can be backed either by non-current loans, where the borrower is late on her payments but has not yet defaulted, or by current loans, where the borrower is current on her payments. We expect that the effect of *Madden* will be most prominent for notes backed by non-current loans, where the risk of nonpayment is especially high. Using the trading data we collected, we calculate the discount that investors apply to each note based upon the difference

between the price paid for the note and the value of the underlying loans if paid in full. Following investors in this field, we refer to that difference as the spread.[31] After controlling for other relevant variables, higher spreads indicate greater discounts, as higher values reflect the market's perception that the projected payout is insufficient to compensate for the time value of money plus the perceived nonpayment risk.

Because of the risk that the underlying loans may be uncollectible in Connecticut and New York after *Madden*, we expect that the spread on notes backed by above-usury loans increased after the decision. Table 4 presents the results of a series of triple difference regressions testing this hypothesis. Panel A presents results for notes backed by non-current loans, while Panel B presents results for notes backed by current loans. The variable of interest is Above16*Post-*Madden**NY_CT, which represents the interaction between Above16 (an indicator for whether the underlying loan has an interest rate above 16%, the civil usury cap in New York),[32] Post-*Madden* (an indicator for whether the trade occurred after *Madden*), and NY_CT (an indicator for whether the borrower resides in Connecticut or New York). Each panel has three columns, reflecting our three control groups. All models control for principal outstanding on the note traded, full loan amount, loan age, ask price, loan duration, loan interest rate, borrower FICO score, and whether

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

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

the loan underlying the note was issued within the fifteen months prior to the trade date.[33] Because the ratio of current loans to non-current loans traded varies over our sample period—and across lending platform—we also control for the daily ratio of current to non-current loans traded on the platform in question. Fixed effects are included for the grade the lending platform originally assigned the loan, and standard errors are clustered by the borrower's state of residence.

The results in Table 4 provide evidence that *Madden* reduced the price of notes backed by above-usury loans to borrowers in Connecticut and New York. Panel A analyzes notes backed by non-current loans and shows that spreads on notes backed by loans to Connecticut and New York borrowers were higher than expected following *Madden*. (One model is not statistically significant, but the other two are significant at the 5% level.) In terms of economic magnitude, the coefficient on the triple-interaction term in column (1) is 0.387, and the Stata margins command suggests that, at the mean, the spread for above-usury notes in the Second Circuit post-*Madden* is approximately 0.25 higher than expected. To put this result in perspective, the mean (median) spread for notes backed by non-current loans in our sample is 2.35 (1.29), and the standard deviation is 3.54. Column (3) uses the PSM control sample presented in Table 3 and shows a similar result.

Panel B in Table 4 analyzes notes backed by current loans. Although it also shows that spreads increased post-*Madden* on notes backed by above-usury debt owed by Connecticut and New York borrowers, the magnitude of the increase is much smaller. The variable of interest is significant at 5% across the three models, but the economic magnitude of the increase is virtually zero. The smaller discount has a clear explanation, as current loans present lower risks of nonpayment than non-current loans. Accordingly, the mean (median) spread on notes backed by current loans is -0.018 (-0.0158). Nonetheless, the economic magnitude of roughly zero suggests

[33] All trades are conducted at the ask price. Following industry practice, we refer to the trade price as the ask price.

that lenders expect borrowers who are making their payments on time to continue to do so despite the *Madden* decision. In other words, investors do not expect *Madden* to trigger widespread strategic defaults.

B. Credit Availability for Riskier Borrowers

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

i. *Madden*'s Effect on Loan Volume

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

New York seemed stunted.

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

There are two possible reasons why lenders made relatively fewer higher-interest loans in Connecticut and New York after *Madden.* One is that they curtailed lending to higher-risk borrowers; the other is that they charged less interest, holding borrower quality constant. To distinguish between these possibilities, Table 5 presents results of difference-in-differences regressions examining the relative change in credit quality, as measured by FICO score, for borrowers in Connecticut and New York after *Madden*. The table shows that average credit scores in Connecticut and New York rose significantly after *Madden* relative to either of the control groups.[34] (This finding is consistent with the descriptive statistics in Table 2.) Average FICO scores for Connecticut and New York borrowers increased roughly 2.6 to 3.0 FICO points more than expected based on the trend for borrowers outside the Second Circuit generally and in no-usury states specifically. All models in Table 5 control for the loan's interest rate, amount, and term, as well as the borrower's annual income, debt-to-income ratio, number of recent delinquencies, total credit availability, and years of employment at her current position (all variables are defined in Table 1). As before, we include fixed effects for each lending platform, and standard errors are clustered by the borrower's state of residence.

To further investigate this increase in FICO scores in Connecticut and New York, we assign borrowers to buckets based on FICO score and examine the growth in loan volume by bucket. The results, presented in Figure 3, indicate that the FICO increase was caused by a decline in lending

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

to lower-quality borrowers. Outside the Second Circuit, loan volume to borrowers in all FICO buckets increased substantially after *Madden*. However, although growth rates for loans issued to borrowers in Connecticut and New York are roughly comparable to growth rates outside the Second Circuit for higher-quality borrowers, growth in new loans was dampened—or even declined—for lower-quality borrowers. The pattern is most obvious for the lowest-quality borrowers—those with FICO scores below 625. The growth rate for these borrowers in Connecticut and New York was negative 52%—meaning that, in absolute numbers, loan volume to these borrowers *declined* after *Madden*. Outside the Second Circuit, loan volume for these borrowers after *Madden* grew by 124% (that is, loan volume in absolute numbers more than doubled).

We see a similar trend in Figures 4 and 5, where we plot the distribution of new loans by FICO score before and after *Madden*. In Figure 4, the first pair of histograms includes all non-Second Circuit borrowers and shows a post-*Madden* increase in new loans to borrowers with FICO scores below 670.[35] This is consistent with anecdotal evidence that marketplace lending to these borrowers grew during this period. The next pair of histograms, which includes only borrowers in Connecticut and New York, shows a different trend. Loans to riskier borrowers appear to decline, and loans to borrowers with FICO scores below 644 virtually disappeared.

Figure 5 zooms in on the lowest-quality borrowers in our sample, those with FICO scores below 640. As it shows, there was only one new loan to such borrowers in Connecticut and New York in July 2015, and none thereafter. By contrast, loan originations to such borrowers outside the Second Circuit were roughly 50% greater in the second half of 2015 than in the first half.

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

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

The finding that usury laws decrease credit availability is consistent with much prior work (e.g., Goudzwaard, 1968; Shay, 1970; Greer, 1974; Rigbi, 2013; Melzer and Schroeder, 2015). However, most of these earlier studies rely on associations, whereas we show the effects of usury laws in a more tightly identified setting. As a caveat, we note that our findings do not establish that these higher-risk borrowers were unable to borrow altogether. Because we look only at loans issued through marketplace-lending platforms, we cannot rule out the possibility that these borrowers substituted into other sources of credit, including those, such as credit cards, that charge more interest.

iii. Changes in loan size

Credit availability is affected by the availability of new loans and by the terms of available loans (e.g., Ghosh, Mookherjee and Ray, 1999; Stiglitz and Weiss, 1981; Melzer and Schroeder, 2015). Although most marketplace-lending platforms use standardized loan terms—for example, loans must be unsecured and have terms of either 36 or 60 months—loan size can range from $1000 to $35,000. It is therefore possible that *Madden* affected loan size in our sample.

Table 6 presents the results of difference-in-differences regressions testing this possibility. The table indicates that average loan size fell roughly $400 more than expected in Connecticut and New York following *Madden*,[36] with the greatest decreases for lower-quality borrowers. As before, we present results for tests using our non-Second Circuit control group (Panel A) and no-usury group (Panel B). The first column in each panel shows results for the full set of borrowers, the second for the subset of borrowers with FICO scores below 750, and the third for the subset of borrowers with FICO scores below 700. The interaction term is statistically significant at 1% across all models, and the change in loan size decreases monotonically with FICO scores. This result suggests that *Madden* not only constrained credit availability by reducing loan volume, but also by reducing loan size.

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

C. Strategic Default

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

[36] This result does not appear in basic descriptive statistics. It is driven by the inclusion of control variables.

for that and all subsequent months.[37]

Table 7 provides the results of triple-difference regressions used to test for strategic default. As in Table 4, the variable of interest is Above16*Post*NY_CT, which represents the triple interaction between Above16, Post-*Madden*, and NY_CT. Because we have repeat observations for the same loan, all standard errors are clustered by loan. All models include the same control variables as in Table 6 as well as platform fixed effects. All control variables are based on borrower and loan information at the time a borrower applied for a loan and do not update throughout the loan period.

Table 7 offers no evidence that borrowers engaged in strategic default after *Madden*; the coefficients on the variable of interest—the triple interaction term—are not significantly different from zero in any of the models. Panel A shows results from tests in which we keep delinquent borrowers in the sample in months after they miss a payment. Thus, if a borrower misses a payment in September 2015, she will also show up, with a Delinquent score of 1, in October through December. Panel B shows results in which we remove borrowers from the data after they first miss a payment. All models are Cox proportional hazard models.

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

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

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

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

D. 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 prior to the shock examined. Hence, in this section we report the results of parallel trends analyses. We show monthly trends for each of the significant results presented in our main regressions: discounts on secondary-market trading, FICO scores, and loan size.

i. Secondary-Market Trading

Figure 6 presents parallel trends analyses corresponding to our regressions analyzing *Madden*'s impact on the trading price of notes backed by current and non-current loans. Panel A shows the results for non-current loans, and Panel B shows the results for current loans. The figures in each panel plot the trend lines for two regressions, one using borrowers from Connecticut and

New York, and the second using borrowers outside the Second Circuit. The regressions are the same as those used in Panels A and B of Table 4, except that NY_CT, Post-*Madden*, and the triple interaction term are replaced with monthly indicators reflecting the month in which the trade occurred. The figure plots the coefficients on the interactions between Above16 and each monthly indicator.

Interestingly, Panel A indicates that it took several months for the full effect of *Madden* to materialize. Although the pre-*Madden* spread on notes backed by non-current loans in Connecticut and New York was slightly higher than the spread on notes backed by non-current loans outside the Second Circuit, the deviation between these lines widened significantly starting only in September. We do not see a similar trend in Panel B for notes backed by current loans. However, the lack of a visual trend in Panel B is not surprising given Table 4's finding that the economic magnitude of the discount applied to above-usury loans made to borrowers in New York and Connecticut post-*Madden* is very close to zero.

ii. Borrower Quality

Figure 7 presents the parallel trends analysis for the regression analyzing *Madden's* effect on FICO scores. The regression specification is the same as that in Table 5, except we replace the prior variables of interest—NY_CT, Post-*Madden*, and the resulting interaction term—with monthly indicator variables reflecting the month in which the loan was issued. As before, the first line presents results of a regression using borrowers from Connecticut and New York, and the second is for borrowers from outside the Second Circuit. The figure plots the coefficients on the monthly indicators. Although FICO scores for Connecticut and New York borrowers were higher than for those outside the Second Circuit throughout the year, the difference is roughly constant until September, when it widens significantly. This result is consistent with Figure 6, and with

anecdotal evidence, both of which indicate that it took several months for *Madden* to have its full impact on markets.

iii. Loan Size

Figure 8, which presents an analysis of *Madden*'s effect on loan size, shows a similar trend. Panel A shows results for the full set of borrowers, while Panel B includes only the subset of borrowers with FICO scores below 700. The regression specification is the same as in Table 6, except we replace the prior variables of interest with monthly indicator variables. As before, the indicators reflect the month in which the loan was issued; the first regression uses only loans to borrowers in Connecticut and New York, and the second uses only loans to borrowers outside the Second Circuit. Interestingly, the figure suggests that relative loan size in Connecticut and New York fell as early as June, suggesting that lenders initially responded to *Madden* by making smaller loans and only later reduced loan volume.

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

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

probabilities they assigned to these possible outcomes; those who assigned high probabilities might have felt that the potential returns from lending above 16% continued to justify the risks.[38]

5. Conclusion

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

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

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Table 1. Descriptive Statistics: Loan and Borrower Characteristics. This table presents characteristics of the loans and borrowers in our primary-lending dataset. Panel A compares loans to borrowers in Connecticut and New York (our treatment group) with loans to all borrowers outside the Second Circuit. Panel B compares loans in our treatment group with loans to borrowers in states lacking usury caps. Panel C compares loans in our treatment group to our propensity score matched (PSM) sample used in Panel A of 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: Outside the Second Circuit			Panel B: No Usury States			Panel C: PSM		
	NY & CT	Outside the 2nd Circuit	*t-test*	NY & CT	No Usury States	*t-test*	NY & CT	PSM	*t-test*
Loan Amount	14,206	12,598	*-49.10*	14,206	12,695	*-33.13*	13,934	14,052	*-4.98*
Term (Months)	43.26	43.65	*8.82*	43.26	43.88	*10.30*	42.94	43.36	*-12.83*
Interest Rate	13.80%	18.58%	*123.73*	13.80%	18.56%	*109.66*	12.94%	13.00%	*-3.75*
Annual Income	77,714	65,821	*-14.32*	77,714	65,694	*-28.12*	78,463	74,104	*20.03*
Debt-to-Income	19.39%	24.65%	*-45.52*	19.39%	25.36%	*-45.40*	19.70	21.33	*-53.68*
Delinquencies	0.31	0.25	*-20.12*	0.31	0.24	*-14.37*	0.36	0.35	*2.88*
Available Credit	19,138	14,894	*-44.13*	19,138	15,345	*-24.29*	18,103	17,000	*13.95*
Employment (Years)	7.11	5.32	*-69.39*	7.11	5.38	*-48.15*	7.03	6.93	*5.52*
FICO Score	696.22	682.82	*-87.60*	696.22	682.92	*-67.41*	695.48	694.64	*8.82*
Num. Obs.	*66,437*	*841,446*		*66,437*	*63,942*		*57,654*	*57,654*	

Table 2. Descriptive Statistics: Borrower and Loan Characteristics Before and After *Madden*. Using our primary-lending dataset, this table compares loans issued before and after *Madden*. Panel A reflects loans to borrowers in Connecticut and New York. Panel B reflects loans to all borrowers located outside of the Second Circuit. Panel C reflects loans to borrowers located in states without usury limits. All variables are as defined in Table 1, and all values are presented at the mean.

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

Table 3. Descriptive Statistics: Characteristics of Notes Underlying Secondary-Market Trades. This table presents descriptive statistics for notes traded on the secondary-market exchanges run by the marketplace platforms in our sample. Panel A compares our treatment group (notes traded based on loans in Connecticut and New York) with our main control group (notes traded based on loans outside the Second Circuit). Panel B compares our treatment group with our no usury control group (notes based on loans in states lacking usury caps). Panel C compares the treatment group with our propensity score matched sample. Each panel divides the notes based on whether they are backed by loans to borrowers who are no longer current on their payments or by loans to borrowers who are current on their payments. Principal Outstanding reflects the outstanding principal on the note at the time of the trade. Loan Amount is the total value of the loan underlying each note. 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 term of the underlying loan and is expressed in months. Loan Age reflects the number of months between the loan's issue date and the trading date. 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 fifteen months of the trading date. All values are presented at the mean.

Panel A: Outside the Second Circuit

Notes Backed by Non-Current Loans

	CT & NY	Outside the 2nd Circuit	*t-score*
	(1)	(2)	
Principal Outstanding	30.73	31.15	*0.53*
Loan Amount	20,169	20,506	*3.60*
FICO Score	690	689	*-0.14*
Ask Price	13.53	13.76	*0.32*
Loan Duration	50.06	50.68	*5.16*
Loan Age	16.94	16.28	*-6.30*
Interest Rate	19%	19%	*0.84*
Fifteen	0.51	0.48	*-4.99*
Num. Obs.	*10,543*	*84,675*	

Notes Backed by Current Loans

	CT & NY	Outside the 2nd Circuit	*t-score*
	(3)	(4)	
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*
Loan Duration	47.93	48.43	*14.38*
Loan Age	14.24	13.75	*-16.69*
Interest Rate	17%	17%	*-7.59*
Fifteen	0.41	0.40	*-10.87*
Num. Obs.	*130,092*	*1,226,167*	

Panel B: No Usury States

Notes Backed by Non-Current Loans

	CT & NY	No Usury	*t-score*
	(1)	(2)	
Principal Outstanding	30.73	31.09	*-0.39*
Loan Amount	20,169	20795	*-4.65*
FICO Score	690	689	*0.97*
Ask Price	13.53	13.08	*0.40*
Loan Duration	50.06	50.88	*-4.41*
Loan Age	16.94	16.57	*2.39*
Interest Rate	19%	19%	*-1.24*
Fifteen	0.51	0.48	*3.15*
Num. Obs.	*10,543*	*7246*	

Notes Backed by Current Loans

	CT & NY	No Usury	*t-score*
	(3)	(4)	
Principal Outstanding	33.23	34.49	*-3.57*
Loan Amount	19,736	20406	*-17.35*
FICO Score	695	693	*13.15*
Ask Price	33.60	34.90	*-3.56*
Loan Duration	47.93	48.70	*-15.21*
Loan Age	14.24	13.58	*14.89*
Interest Rate	17%	17%	*-1.85*
Fifteen	0.41	0.40	*11.14*
Num. Obs.	*130,092*	*94440*	

Panel C: PSM Sample

Notes Backed by Non-Current Loans	CT & NY (1)	PSM Sample (2)	*t-score*
Principal Outstanding	30.73	31.01	*-0.29*
Loan Amount	20,169	20,008	*1.29*
FICO Score	690	690	*-1.42*
Ask Price	13.53	13.84	*-0.36*
Loan Duration	50.06	50.21	*0.94*
Loan Age	16.94	17.01	*-0.48*
Interest Rate	19%	18%	*3.15*
Fifteen	0.51	0.51	*0.19*
Num. Obs.	*10,543*	*10,543*	

Notes Backed by Current Loans	CT & NY (3)	PSM Sample (4)	*t-score*
Principal Outstanding	33.23	33.42	*-0.58*
Loan Amount	19,558	19,513	*1.26*
FICO Score	695	695	*-3.97*
Ask Price	33.60	33.78	*-0.55*
Loan Duration	47.93	48.41	*10.25*
Loan Age	14.24	14.30	*-1.43*
Interest Rate	17%	17%	*6.00*
Fifteen	0.41	0.41	*-0.23*
Num. Obs.	*124,000*	*124,000*	

Table 4. Triple Difference Results: Change in Secondary-Market Trading Prices Post-*Madden*. The table presents results of triple-difference regressions testing *Madden*'s impact on the pricing of notes backed by above-usury loans to borrowers in Connecticut and New York. The dependent variable is note spread, defined as yield to maturity based on the note's trading price minus the underlying loan's interest rate. Panel A uses only notes backed by non-current loans, and Panel B uses only notes backed by current loans. In each panel, Model (1) uses all borrowers outside the Second Circuit as the control group, Model (2) uses borrowers in states lacking usury caps, and Model (3) uses the PSM control group. All regressions control for principal outstanding, accrued interest, loan age, loan term, the borrower's FICO score, whether the underlying 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 underlying 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.

$$Spread = \alpha + \beta_1 Post\text{-}Madden + \beta 2 \quad NY\_CT + \beta_3 Above16 + \beta_4 Post{*}NY\_CT + \beta_5 Above16{*}Post + \beta_6 Above16{*}NY\_CT + \beta_7 Above16{*}Post{*}NY\_CT + Controls + \varepsilon$$

	Panel A: Notes based on Non-Current Loans			Panel B: Notes based on Current Loans		
	Outside the 2nd Circuit	No Usury States	PSM Sample	Outside the 2nd Circuit	No Usury States	PSM Sample
	(1)	(2)	(3)	(1)	(2)	(3)
Post-*Madden*	-0.02	-0.21	0.04	0.002***	0.002***	0.003***
	(0.07)	(0.15)	(0.13)	(0.00)	(0.00)	(0.00)
NY_CT	0.08	-0.29	0.14	0.001***	0.00	0.00
	(0.15)	(0.19)	(0.17)	(0.00)	(0.00)	(0.00)
Above16	-0.14	-0.547**	0.01	-0.001***	0.00	0.00
	(0.09)	(0.22)	(0.14)	(0.00)	(0.00)	(0.00)
Post*NY_CT	-0.16	0.07	-0.17	-0.00**	-0.00*	0.00
	(0.26)	(0.28)	(0.27)	(0.00)	(0.00)	(0.00)
Above16*Post	-0.08	0.31	-0.15	0.003***	0.001*	0.001**
	(0.08)	(0.20)	(0.13)	(0.00)	(0.00)	(0.00)
Above16*NY_CT	-0.19	0.37	-0.18	0.00	-0.001*	-0.001*
	(0.11)	(0.21)	(0.14)	(0.00)	(0.00)	(0.00)
Above16*Post*NY_CT	0.387**	-0.03	0.433**	0.001**	0.002**	0.001**
	(0.18)	(0.24)	(0.20)	(0.00)	(0.00)	(0.00)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan Grade FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	95,218	17,789	21,086	1,356,259	224,532	248,000
R-squared	0.060	0.059	0.064	0.110	0.126	0.127

Table 5. Difference-in-Differences Results: Change in Borrower FICO Scores Post-*Madden*. The table presents results of difference-in-difference regressions analyzing changes in FICO scores for Connecticut and New York borrowers post *Madden* relative to changes for all borrowers (1) outside the Second Circuit and (2) in no-usury states specifically. All regressions control for the loan's interest rate, amount, and term, as well as the borrower's income, debt-to-income ratio, number of recent delinquencies, total credit availability, and months of employment at her current position. Fixed effects are included for each marketplace lending platform. Standard errors are clustered by the borrower's state of residence, and statistical significance of 10%, 5%, and 1% is indicated by *, **, and ***, respectively.

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

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

Table 6. Difference-in-Differences Results: Change in Loan Size Post-*Madden*. The table shows regression results testing post-*Madden* changes in loan size for Connecticut and New York borrowers relative to borrowers in our two main control groups. Panel A presents results using all borrowers outside the Second Circuit as the control group, and Panel B uses only borrowers from states without usury caps as the control group. In each panel, Model (1) uses the full set of borrowers, Model (2) uses only borrowers with FICO scores below 750, and Model (3) uses only borrowers with FICO scores below 700. 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.

$$Loan\ Amount = \alpha + \beta_1 Post\text{-}Madden + \beta_2 NY\_CT + \beta_3 Post{*}NY\_CT + Controls + \varepsilon$$

	Panel A. Outside the Second Circuit			Panel B. No Usury States		
	(1) All Borrowers	(2) Sub750	(3) Sub700	(1) All Borrowers	(2) Sub750	(3) Sub700
Post-*Madden*	324.5***	357.7***	524.6***	304.7***	326.5***	475.8***
	(24.68)	(25.46)	(33.79)	(37.09)	(33.68)	(53.20)
NY_CT	125.4	107.7	220.8*	142.0	115.7	215.2
	(110.3)	(107.85)	(116.49)	(165.8)	(144.02)	(136.37)
Post* NY_CT	-394.2***	-406.1***	-562.6***	-376.9***	-375.4***	-504.5***
	(33.98)	(42.24)	(74.26)	(37.56)	(48.95)	(76.76)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	907,883	857,544	635,219	130,379	122,147	85,672
R-squared	0.346	0.353	0.357	0.335	0.340	0.347

Table 7. Triple Difference Results: Change in Borrower Delinquencies Post-*Madden*. The table presents results of triple-difference regressions testing post-*Madden* changes in delinquencies on above-usury loans to borrowers in Connecticut and New York relative to delinquencies on such loans in other jurisdictions. The dependent variable, Delinquent, is given a monthly value of 0 until a borrower misses a payment; it then becomes 1 in that and all subsequent months. Panel A keeps borrowers in the sample after they are delinquent, while Panel B includes only through the borrower's initial delinquency. In each panel, Model (1) uses all borrowers outside the Second Circuit as the control group, Model (2) uses all borrowers in states without usury caps as the control group, and Model (3) uses the PSM matched-sample as the control. The analysis is presented using the Cox proportional hazard model. All models 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.

$$Delinquent = \alpha + \beta_1 Post\text{-}Madden + \beta_2 NY\_CT + \beta_3 Above16 + \beta_4 Post{*}NY\_CT + \beta_5 Above16{*}Post + \beta_6 Above16{*}NY\_CT + \beta_7 Above16{*}Post{*}NY\_CT + Controls + \varepsilon$$

	Panel A: All Borrower Months			Panel B: Through Initial Default Only		
	Outside 2nd Circuit	No Usury State	PSM Sample	Outside 2nd Circuit	No Usury State	PSM Sample
	(1)	(2)	(3)	(1)	(2)	(3)
Post-*Madden*	-0.006***	-0.008***	-0.006***	-0.001***	-0.002***	-0.001**
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
NY_CT	-0.000	-0.000	0.000	-0.000	-0.000	0.001*
	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Above16	0.007***	0.005**	0.005***	0.001***	-0.000	0.000
	(0.001)	(0.002)	(0.002)	(0.000)	(0.001)	(0.001)
Post*NY_CT	-0.001	0.001	-0.001*	-0.000	0.000	-0.001*
	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Above16*Post	-0.010***	-0.007***	-0.01***	-0.002***	-0.001	-0.002
	(0.001)	(0.002)	(0.002)	(0.000)	(0.001)	(0.001)
Above16*NY_CT	-0.001	0.002	0.001	-0.000	0.001	0.000
	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)
Above16*Post* NY_CT	-0.001	-0.004	-0.003	0.000	-0.001	-0.000
	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Lender FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,366,222	389,339	452,092	2,351,868	386,706	449,140

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

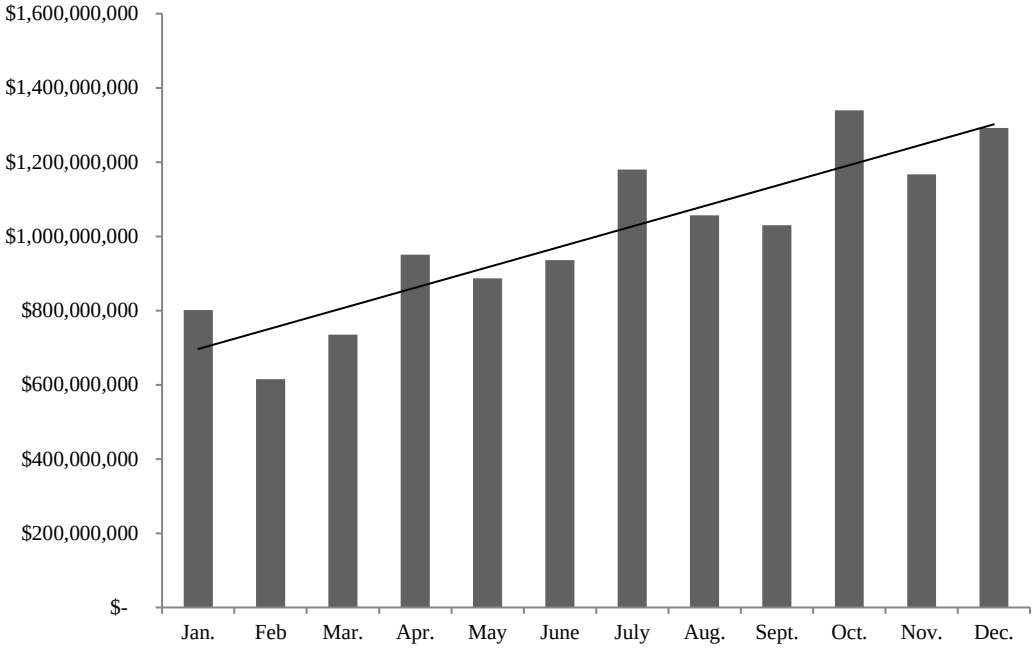


Figure 2. Summary Statistics: Distribution of Interest Rates Before and After *Madden*. The histograms include all loans issued through our marketplace lending platforms and show the distribution of interest rates before and after *Madden*, comparing Connecticut and New York with states outside the Second Circuit. All histograms use a bin width of four percentage points.

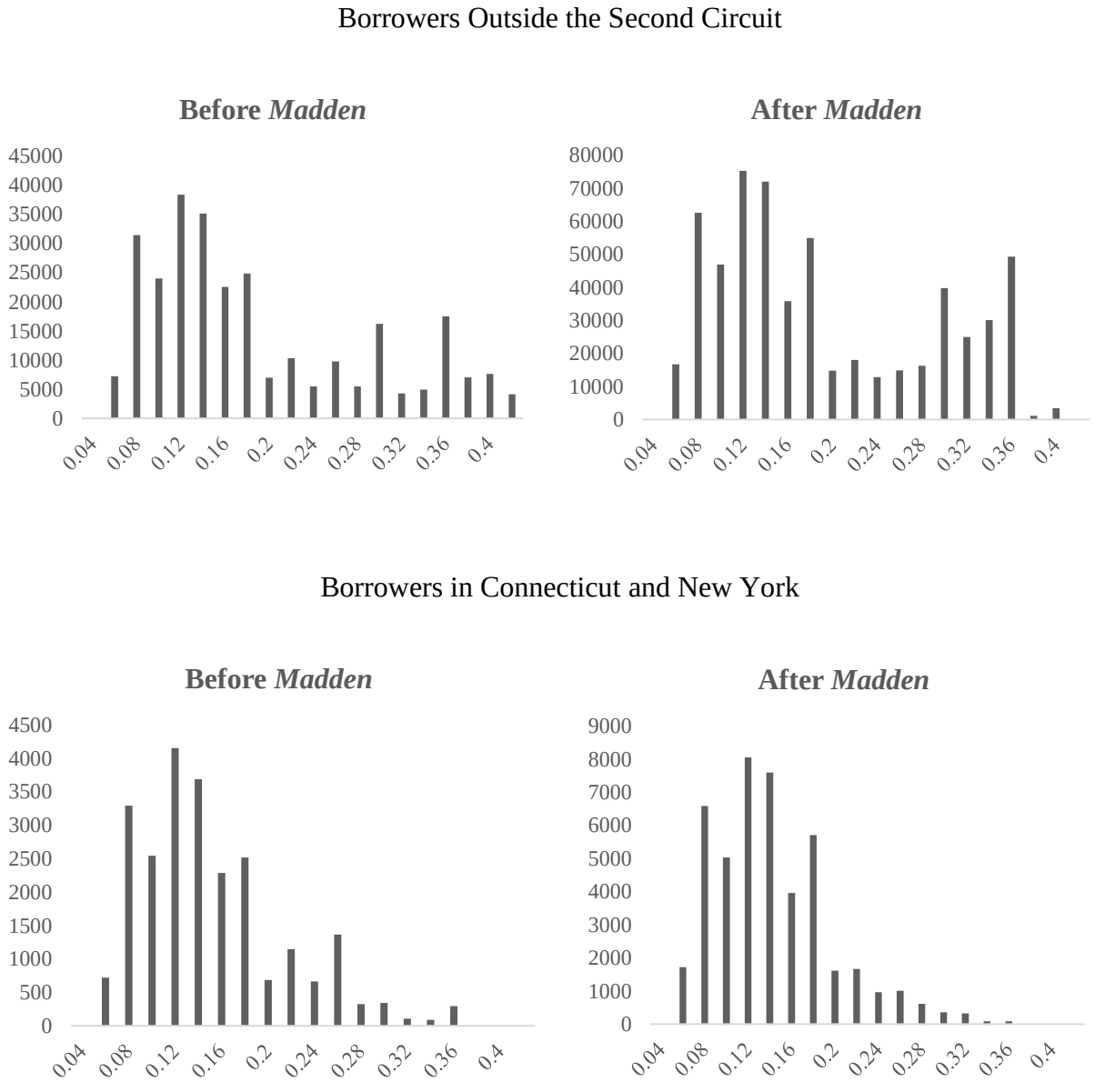


Figure 3. Summary Statistics: Growth in Loan Originations Post-*Madden*. The figure compares post-*Madden* growth in loan originations, broken down by borrower FICO score, in Connecticut and New York relative to growth outside the Second Circuit (a value of 100% would reflect that the same number of loans were issued before and after *Madden*). 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.

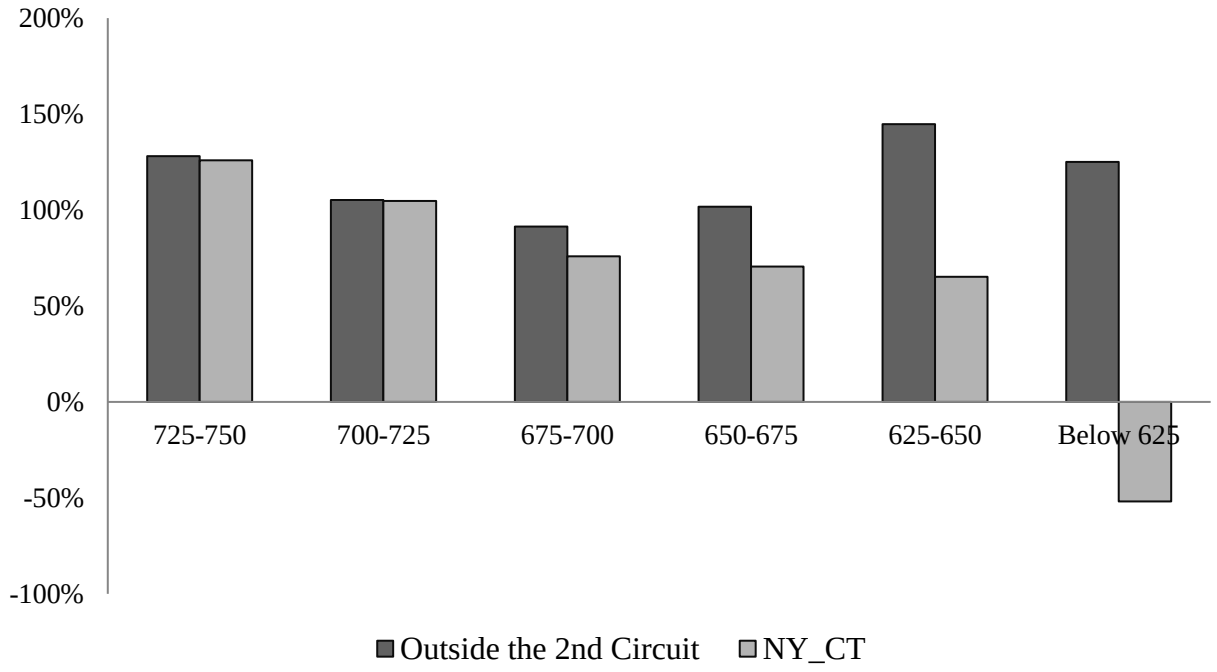


Figure 4. Summary Statistics: Distribution of FICO Scores Before and After *Madden*. The histograms include all loans issued through our marketplace lending platforms and show the distribution of FICO scores before and after *Madden*, comparing Connecticut and New York with states outside the Second Circuit. All histograms use a bin width of four FICO points.

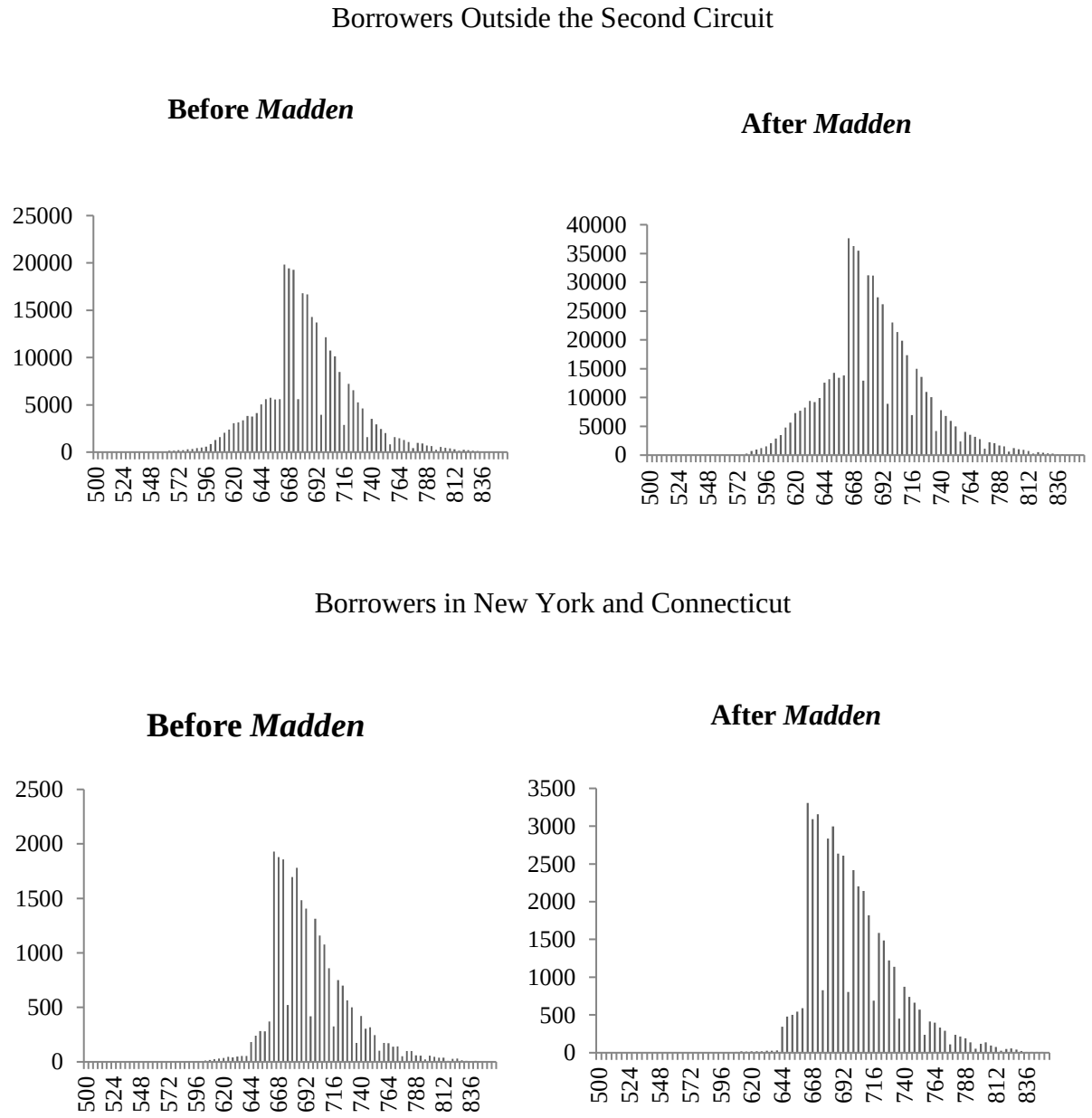


Figure 5. Summary Statistics: Loan Originations to Lower-Quality Borrowers in Connecticut and New York. The figure shows the number of new loans issued in 2015 to borrowers in Connecticut and New York with FICO scores below 640. There were hundreds of such loans from January through June 2015, but only one in July and none thereafter.

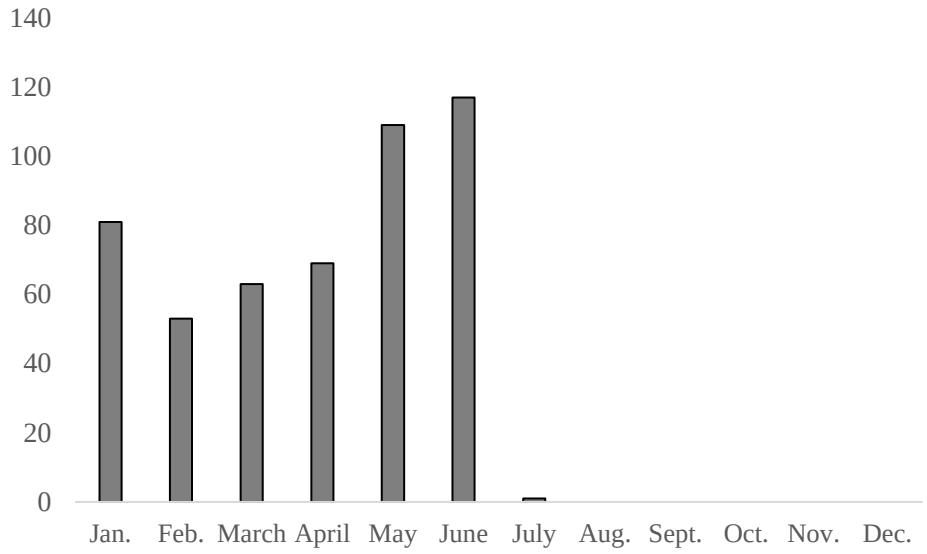
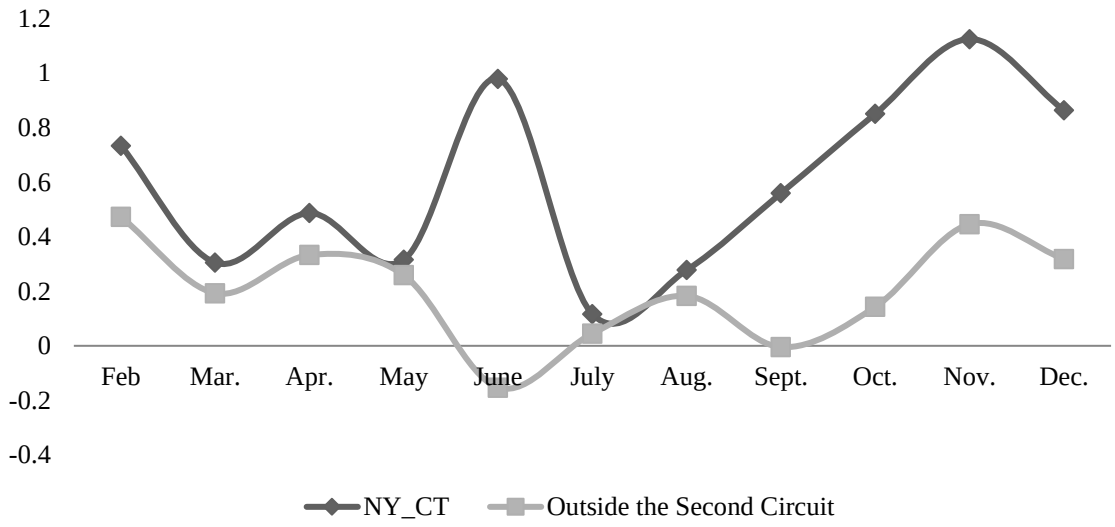


Figure 6. Parallel Trends Analysis: Discount on Traded Notes by Month. Each figure below presents the coefficients on monthly interaction terms from a pair of regressions. Panel A includes only notes traded based on non-current loans, and Panel B includes only notes traded based on current loans. In each panel, the first line represents results for notes backed by loans to borrowers in Connecticut and New York, and the second is for notes backed by loans to borrowers outside the Second Circuit. The sample and regression specification are the same as in Table 4, except that we replace the prior variables of interest with dummy variables for each month from February through December and interact those dummies with Above16, an indicator for whether the loan's interest rate is above 16%. The monthly indicators reflect the month in which the trade occurred.

Panel A. Notes backed by non-current loans



Panel B. Notes backed by current loans

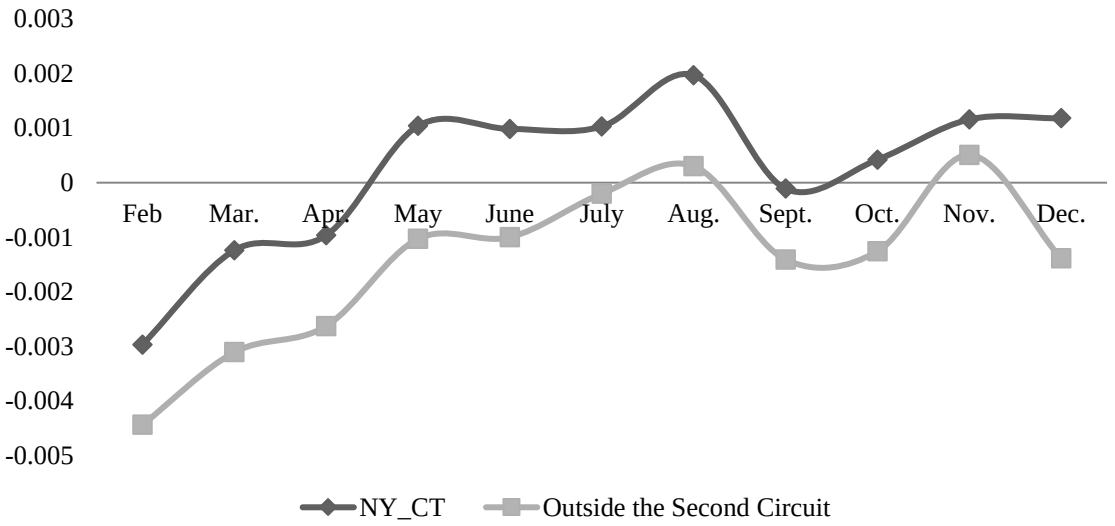


Figure 7. Parallel Trends Analysis: FICO Score by Month. The figure below presents the coefficients on monthly indicators from two regressions. The first regression includes only borrowers located in New York and Connecticut, and the second includes only borrowers located outside of the Second Circuit. The sample and regression specification are the same as 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.

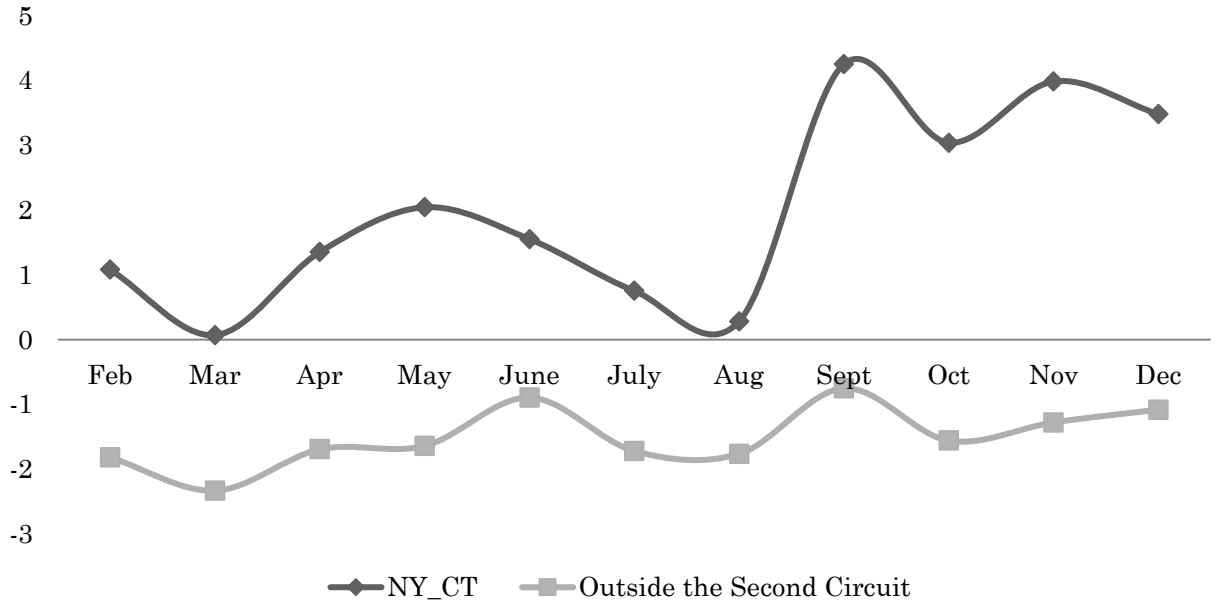
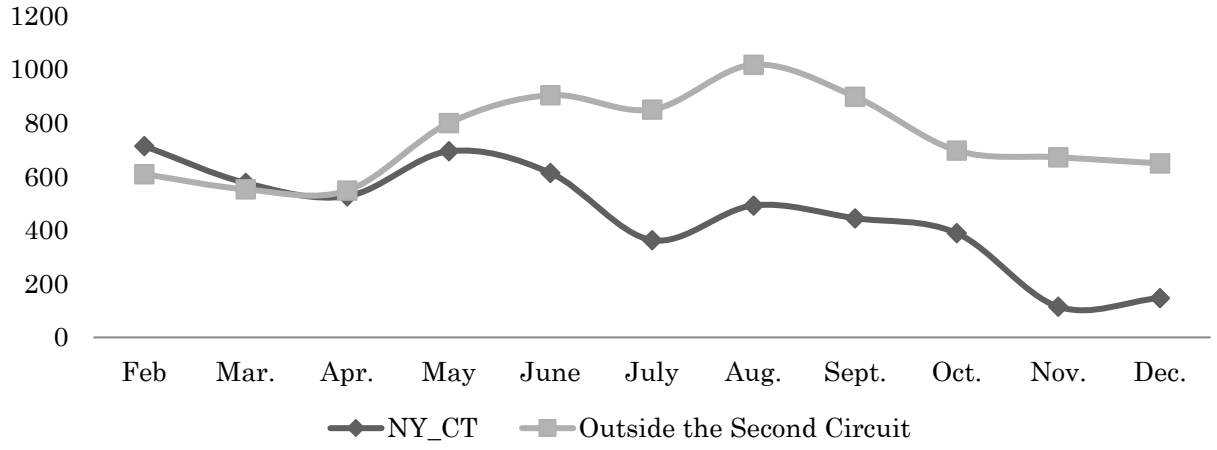


Figure 8. Parallel Trends Analysis: Loan Size by Month. The figures below present the coefficients on monthly indicators from two regressions. Panel A includes the sample of borrowers, and Panel B includes the sample of borrowers with FICO scores below 700. In each panel, one line shows the result from a regression for borrowers in Connecticut and New York, while the second shows the result for borrowers outside of the Second Circuit. The sample and regression specification are the same as in Table 7, except that we replace the prior variables of interest (NY_CT, Post-*Madden*, and the resulting interaction term) with dummy variables for each month from February through December. The monthly indicators reflect the month when the loan was issued.

Panel A. All Borrowers



Panel B. Borrowers with FICO Scores below 700